Comment ID	From Page	From Line	To Page	To Line	Comment	Response
25714	0	0	0	0	Throughout, use "warming" consistently; see defintion at page 7-5, line 7; It is widely used to mean increase in GMST. [Stephen E Schwartz, United States of America]	Noted.
48026	0	0	0	0	Scoping Outline Check: All bullets from approved outline are covered in the first order draft but please note there is an overlap with radiative forcing and aerosols with chapter 6. [WGI TSU, France]	Noted, thanks. Coordination with Ch. 6 was strengthened for the SOD.
47810	0	0	0	0	Chapters 2, 5 and 7 class methane as long-lived but chapter 6 classes it as short-lived. [WGI TSU, France]	Taken into account. Thanks for pointing this out. A clear definition of what we consider short-lived and long-lived in this report is presented in Ch. 6, and terminology has been harmonized across chapters.
41676	0	0	0	0	I commend the authors on this excellent FOD, which I found comprehensive, topical and very interesting to read. Unfortunately the quality of some figures was rather poor which limited my ability to evaluate their suitability. [Amanda Maycock, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Thank you for the positive feedback! We have worked to improve the figure quality for the SOD.
41678	0	0	0	0	assessment of future ERF for different scenarios appears to not be assessed; this is important for chapter 4 and the SOD should ensure this is covered somewhere in the assessment [Amanda Maycock, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Given that Ch. 7 is a "process understanding" chapter, we argue that future ERFs have no place in Ch. 7 and should rather be assessed in Ch. 4.
48124	0	0	0	0	Response to CDR/SRM: the FOD does not cover the potential RF of SRM. Should this be covered? [WGI TSU, France]	Taken into account. Given that Ch. 7 is a "process understanding" chapter, we argue that the potential radiative influence of SRM or CCS should be covered elsewhere in the report.
14304	0	0			A very well written chapter, well done all. Perhaps you can helpout on the other chapters now! In places, extensive reviews could be reduced to an assessment of what is key information for policy makers (what was known in AR5 and what has advanced that is policy relevant) although I admit as a scientist the overviews are extremely useful and well written in this chapter (e.g. feedback methodology) [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Thank you for the positive feedback! We have worked hard to make the chapter more concise for the SOD.
14306	0	0			Additional refernces to consider (including my brief summary line) if relevant for the key messages (sorry for the long list): Dieng et al. 2017 IJOC doi:10.1002/joc.4996: Energy imbalance 2003-2013 0.5 W m-2 (in situ measurements), 0.68 W m-2 (ocean reanalysis) & 0.65 W m-2 (global sea level budget, all +/-0.1 W m-2). Storto et al. (2017) GRL doi:10.1002/2017GL075396: assimilation of CERES data to constrain ocean heat content data (but large heating below 1500m looks suspicious?) Hedemann et al. (2017) Nature Clim., doi:10.1038/nclimate3274: modest changes in upper ocean mixed layer heat budget due to ocean/atmosphere fluctuations can generate significant slowing in global surface warming and may elude observational detection Johnson and Birnbaum (2017) GRL doi:10.1002/2016GL071767: building El Nino temporarily increases ocean heat uptake (1oC warming in Nino3.4 over a year increases Earth's energy uptake by 0.2 Wm-2) Cuesta-Valero et al. (2016) GRL doi:10.1002/2016GL068496: many CMIP5 models underestimate observed continental heat accumulation of 0.01 Wm-2 (for global area, 1950-2000) by nearly factor of 10. Xie et al. (2015) Nature Geosci, doi:10.1038/ngeo2581: top-of-the-atmosphere radiation and global mean surface temperature less tightly coupled for natural decadal variability than for greenhouse-gas-induced response. Burgman et al. (2017) GRL doi:10.1002/2016GL071978: shortwave low cloud feedbacks in E Pacific explain much of SST/circulation variability of last 16 years Oka & Watanabe (2017) GRL, doi:10.1002/2016GL072184: Ekman transport from tropics to subtropics enhanced after 2002 increasing heat storage below 700m, explaining contribution to post-2002 global warming slowdown. Bellamo et al. (2016) GRL, doi:10.1002/2016GL069961: cloud feedback amplifies Atlantic Multidecadal Oscillation by 10-31% Oudar et al. (2018) GRL, doi:10.1029/2018GL078841: little impact of anthropogenic aerosols on global temperature trends after the late 1990's in large initial condition ensemble Monerie et al. (2017) ERL doi:10.1088/1748-9326/aa6cb5: Volcanic forcing explains slow 0.08oC/decade surface warming trend 2003-2012, cooling 0.04o/decade during 2008-2012 and more noticible in northern hemisphere but not affecting continued increases in global heat content. Dewitte & Nevens (2016) ERL, doi:10.3847/0004-637X/830/1/25: alternative total solar irradiance estimate, 1363 Wm-2 at solar minimum Checa-Garcia et al. (2016) ERL, doi:10.1088/1748-9326/11/9/094018: CFC decline + less growth in methane & low-level ozone pollution contributed to slower global surface warming rate	Taken into account. Thank you, the papers have been considered and some of them are now cited where relevant.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
14308	0	0			I notice there is no mention of the warming slowdown, whch I guess will be touched on in Chapter 2, although a sgtrong statement noting that multiple ocean in situ and satellite datasets indicate continued heating of the planet and that rather than any slowdown in the 2000s, the energy imabalnce has if anything increased. [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Excellent comment. Yes, we have made sure to emphasize this more also in our chapter in the SOD.
53780	0				A conceptual figure explaining ERF would be useful in ch7. Also some discussion of the idealized ERF vs the practical way to derive it (i.e. how surface temp changes are handled and effects of that) [Jan Fuglestvedt, Norway]	Accepted. This has been added to Section 7.1
56340	0				The tropical RRC results of LC11 and MS15 have been brought together in Table 1 of Bates (2016) (hereafter B16). This table clearly shows how the GCMs substantially underestimate the magnitude of the longwave RRCs by comparison with observations. It also shows that this underestimation occurs regardless of whether the GCMs are run in AMIP mode (SST prescribed) or CMIP mode (SST evolving interactively). An illustration of the underestimation in the CMIP case is shown in Figure 1 below. [J. Ray Bates, Ireland]	Taken into account. The "tropical anvil area feedback", "tropical high cloud amount feedback" or "iris effect" was not assessed in the first order draft, but is now assessed in Section 7.4.2. The fact that models do not exhibit an appreciable anvil area feedback has no influence on the updated process-level ECS assessment and therefore on the assessment as a whole.
6421	0				Introducing responses (Section 7.6) before feedbacks (Section 7.4) would give the chapter a more logical organization. [Stephanie Fiedler, Germany]	Not applicable - the latter part of the chapter has been restructured
56342	0				Figure 1. Illustration of the underestimation by CMIP GCMs of the tropical longwave radiative response coefficient (RRC) by comparison with observations. The GCMs used by Lindzen and Choi (2011) were CMIP3, those used by Mauritsen and Stevens (2015) were CMIP5. [J. Ray Bates, Ireland]	Not applicable - figure 1 has been extensively revised.
53790	0				In order to strengthen the coordination with ch6 on ERF and related concepts, you could involve ch6 authors as CA for the box 7.1 [Jan Fuglestvedt, Norway]	Taken into account. Thanks for the suggestion, we have asked Ch. 6 authors for input on Box7.1
52026	0				This chapter is very clear and well written. The boxes seem very detailed and long and I wonder whether that approach is helpful to overall flow or whether some of the detail should instead be in the main text. The boxes would likely extend considerably beyond two-pages in final layout which would inhibit readability potentially. There is also what feels to the reader like a degree of repetition and sometimes it reads as more textbook / review than assessment. There is undoubtedly scope to tighten the text and it would likely be worth critically reflecting upon where there appears to the reader to be repetition and considering whether alternative structures may diminish this. [Peter Thorne, Ireland]	Taken into account. We agree that the boxes were too long. This happened partly due to some misplaced text in the First order draft. Boxes and text were carefully revised for the second order draft in order to limit repetitions.
52050	0				Several places GCMs are referred to but in other chapters ESM has been used pretty consistently. Does this need changing for consistency? [Peter Thorne, Ireland]	Noted. In the FGD ESM is now consistently used throughout the chapter.
53852	0				The utility and applications of ECS and TCR could be made more clear in the introduction part of the chapter. This would strengthen and and make the motivation for the chapter clearer. To scientists it is obvious why we focus so much on this, but still it would be useful to explain in more plain language the importance of having good estimates of ECS and TCR and how we use these. This could fit in on page 8. (See also specific comment on section 7.5.1.) [Jan Fuglestvedt, Norway]	Taken into account. We have moved the discussion of the relevance of TCR and ECS up to introduction.
53668	0				A very nice chapter in good shape. Well written and well structured. But some parts are quite heavy and long, and sometimes a bit like a text book. So as a general comment I would ask the authors to consider what levels of details are needed. The summaries in the end of the sections are very useful for the reader. [Jan Fuglestvedt, Norway]	Taken into account. Thank you for very constructive and useful comments. We have made the text more concise and simplified it to the extent possible for the SOD.
9402	0				Congratulations to the progress made in science and the great clarity in summarizing the significant findings. It is well recognized that the uncertainty of the climate sensitivity could be reduced significantly. [Klaus Radunsky Radunsky, Austria]	Noted. Thank you for the positive feedback!
53698	0				It would be good if the chapter could be consistent in use of the units pmol mol-1 or ppt (and similar). I suggest the ppt, ppb and ppm units for consistency with previous reports [Jan Fuglestvedt, Norway]	Taken into account.
8660	0				At present I find it a bit of a long and winding tale that is not totally internally consistent. I hope that the Sherwood et al assessment, that is now quite close to submission, will help with this chapter. In that context, it seems wise to not say too much here, and to take a closer look at the SOD stage. [Julia Hargreaves, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Thank you for the constructive comments. In the FGD the Sherwood et al. paper is now cited a number of times in the section, which has undergone substantial revisions since the FOD.
48600	0				The comments here refer to the lack of adequate discussion of the poor representation of the longwave radiative response characteristics of the climate system in the tropics in GCMs as compared with observations. [J. Ray Bates, Ireland]	Noted. See answer to #56340

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
48602	0				The focus of Chapter 7 is to document advances in scientific understanding of radiative forcing, climate feedbacks and climate sensitivity, covering related observational, theoretical and modelling developments. A substantive development in recent years, having implications ranging over most of these areas, is the finding that in the tropics the CMIP3 and CMIP5 GCMs substantially underestimate the magnitude of the Earth's longwave radiative response to surface temperature perturbations as measured by satellite. This response has been quantified in recent studies in terms of a linearized radiative response coefficient (RRC) measured in units of Wm-2 K-1 and obtained as a regression slope. [J. Ray Bates, Ireland]	Noted.
48604	0				A recent paper confirming that this underestimation is robust is Mauritsen and Stevens (2015) (hereafter MS15). This paper is cited in Chapter 7 (page 7-60, lines 4 & 5; page 7-89, line 28), though without stress on the RRC underestimation in question here and its possible consequences. A previous paper highlighting this underestimation, not cited in Chapter 7, is Lindzen and Choi (2011) (hereafter LC11). [J. Ray Bates, Ireland]	Noted.
48606	0				The tropical RRC results of LC11 and MS15 have been brought together in Table 1 of Bates (2016) (hereafter B16). This table clearly shows how the GCMs substantially underestimate the magnitude of the longwave RRCs by comparison with observations. It also shows that this underestimation occurs regardless of whether the GCMs are run in AMIP mode (SST prescribed) or CMIP mode (SST evolving interactively). [J. Ray Bates, Ireland]	Noted.
48608	0				It is to be expected that such a significant underestimation of the observed RRC by the GCMs will have a significant influence on the accuracy of equilibrium climate sensitivity (ECS) estimates given by the GCMs. This issue was studied theoretically in LC11 by taking the RRCs as given by observations and by the GCMs and inserting them into an energy balance model (EBM). The results indicated that ECS estimates derived using the GCM RRCs are considerably larger than those obtained using the observed RRC. [J. Ray Bates, Ireland]	Noted.
48610	0				It was mistakenly stated in the IPCC AR5 WGI report (Section 10.8.2.2) that the simple EBM used by LC11 in obtaining the above results was limited to the tropics. Consequently, the report did not give due consideration to the issue highlighted by LC11. However, it has been shown in B16 (see in particular Appendix B) that LC11's EBM was, in fact, a global two-zone (tropical/extratropical) model. It corresponds to Model A of B16 with the extratropical RRC set to its Planck value. [J. Ray Bates, Ireland]	Noted.
48612	0				The influence of an underestimated tropical longwave RRC on estimates of ECS has been further studied theoretically by B16 using an extended EBM (Model B) that allows free variation of the tropical and extratropical RRCs and includes dynamical heat transport. B16's results using Model B indicate that any underestimation of the tropical longwave RRC in GCMs, such as shown to exist in the CMIP3 and CMIP5 GCMs by LC11 and MS15, respectively, may result in an even more serious overestimation of ECS by the GCMs than had been indicated by LC11. [J. Ray Bates, Ireland]	Noted.
48614	0				It is important that subsequent drafts of Chapter 7 address the question of whether the CMIP6 GCMs give any better agreement with the observed monthly-mean value of the tropical longwave RRC than do the CMIP3 and CMIP5 GCMs. If they do not, this should be stated and the possible implications of such a defect for the ECS values given by the CMIP6 GCMs should be discussed. [J. Ray Bates, Ireland]	Noted.
48616	0				In the above comments, only the tropical longwave RRC has been considered. Cho et al. (2012) and Choi et al. (2014) have shown that observational estimates of the tropical shortwave RRCs are dominated by noise due to random changes in clouds not caused by surface temperature changes. This indicates that tropical shortwave RRCs are not suitable for use in estimating ECS. [J. Ray Bates, Ireland]	Noted.
48618	0				References [J. Ray Bates, Ireland]	Noted.
48620	0				Bates, J. R. (2016), Estimating climate sensitivity using two-zone energy balance models, Earth and Space Science, 3, 207–225, doi:10.1002/2015EA000154. [J. Ray Bates, Ireland]	Noted.
48622	0				Cho, H., C. H. Ho, and Y. S. Choi (2012), The observed variation in cloud-induced longwave radiation in response to sea surface temperature over the Pacific warm pool from MTSAT-1R imagery. Geophys. Res. Lett.,39 , L18802, doi:10.1029/2012GL052700. [J. Ray Bates, Ireland]	Noted.
36592	0				General Comment: Very informative and comprehensive. As indicated below, I think that a couple more references can be added to Arctic amplification. One topic not addressed is whether Arctic Amplification is expected to teleconnect to other regions. Another topic not addressed is the interannual SST variability known as "blob events", which persist for unusually long times near continents (e.g. Pacific coast of North America or PWB) and have produced significant impacts on regional climate and ecosystems (Bond et al., 2015; Figure 1.2.4). Myers et al. (Climate Dynamics, 2018) on the basis of an observation analysis have suggested that a positive cloud-surface temperature feedback was key to the extreme intensity of the oceanic heatwave off Baja California associated with the PWB. It is, therefore, a great example of feedbacks but perhaps the chapter [Carlos Mechoso, United States of America]	Noted. Comment has been cut off. This discussion is too detailed for the Chapter
48624	0				Choi, Y.-S., H. Cho, C.-H. Ho, R. S. Lindzen, S.-K. Park, and X. Yu (2014), Influence of non-feedback variations of radiation on the determination of climate feedback. Theor. Appl. Climatol., 115, 355–364. [J. Ray Bates, Ireland]	Noted. This is a reference linked to the comments above

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36594	0				In terms of style, I find another feature that is present in several other chapters on the repetition of references. For example, when writing about low cloud feedbacks and their role in climate an important paper is referenced as it should be. The same reference is added every time the topic is mentioned, which may be more than once in the same paragraph. The many parenthetical remarks make for a tedious reading. [Carlos Mechoso, United States of America]	Taken into account. Thank you for the comment. We have in the FGD been careful not to reference the same papers repeatedly.
48626	0				Lindzen, R. S., and Y.-S. Choi (2011). On the observational determination of climate sensitivity and its implications, Asia-Pacific J. Atmos. Sci., 47, 377–390. [J. Ray Bates, Ireland]	Noted. This is a reference linked to the comments above
44532	0				(Comment submitted to C6, C7 and C10.) The treatment of the processes behind aersol-climate interactions needs to be strengthened through the report. Currently, processes are introduced in Chapter 6 (6.3.1), but only briefly. Then ERF is assessed in Chapter 7, but only globally. In Chapter 10, many regional studies and processes are discussed that rely e.g. on aerosol-precipitation interactions (such as Sahel precipitation trends), but they do not assess the progress in the underlying understanding. My suggestion would be that the process description is strengthened in Chapter 6, up to and including assessments of implications for estimates of regional ERFs and weather/climate interactions. The final assessments for ERF and regional climate can still reside in chapters 7 and 10, but can then refer back to the most recent process litterature in Chapter 6. However other divisions are of course possible, which is why I submit this comment to all three chapters. [Bjorn Samset, Norway]	Taken into account. The division of material on aerosol-climate interactions between chapters has been made much clearer in the SOD.
48628	0				Mauritsen, T. and Stevens, B. (2015). Missing iris effect as a possible cause of muted hydrological change and high climate sensitivity in models. Nat. Geosci. 8, 346–351. doi:10.1038/ngeo2414. [J. Ray Bates, Ireland]	Noted. This is a reference linked to the comments above
42036	1	1	1	1	Compared to some other chapters I have looked at, this is in very very good shape (and hence reviewing is a much more rewarding exercise). Congratulations to all [Keith Shine, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Thank you for the positive feedback!
25716	1	1	1	1	Title: Strike "The"; simply Earth's energy budget, climate feedbacks, and climate sensitivity. We don't say The Jupiter; we say Jupiter. [Stephen E Schwartz, United States of America]	Rejected - title is already approved by the IPCC.
13234	1	1	1	1	Cloud, as a separate chapter in AR5, why does this chapter (also the next chapter) not show that much information/progress about the cloud? I would like to suggest adding more knowledge and progress that we have done since AR5. [Chuanfeng Zhao, China]	Noted. We write quite extensively about recent progress in the understanding of clouds. If the reviewer feels that material is missing in the SOD, it would be most helpful with comments that specify which studies/topics are lacking.
55518	1	1	1	2	Chapter 8 is very well written; other chapters should be shaped to be more like this one. [Wesley Fraser, United Kingdom (of Great Britain and Northern Ireland)]	Noted.
46302	1	1			The chapter needs more conceptualization of the topics and providing a conceptual framework. There is more focus on process-based estimates and may need other estimates. The simple climate models have been discussed but needs to discuss more complicated models and also future potential opportunities. Validation and calibration of the model results and uncertainty analysis needs some pages to be mentioned since there is high uncertainty with the climate model results. Some results and figures are based on informal survey which needs more attention (Figure 7.25). [sadegh zeyaeyan, Iran]	Taken into account. Thank you for the comment, we included only limited GCM validation in the FOD because the CMIP6 simulations were generally not available at that point. We have expanded on our model validation material in the FGD.
57578	1	1			The chapter needs more conceptualization of the topics and providing a conceptual framework. There is more focus on process-based estimates and may need other estimates. The simple climate models have been discussed but needs to discuss more complicated models and also future potential opportunities. Validation and calibration of the model results and uncertainty analysis needs some pages to be mentioned since there is high uncertainty with the climate model results. Some results and figures are based on informal survey which needs more attention (Figure 7.25). [Sahar Tajbakhsh Mosalman, Iran]	Taken into account. Thank you for the comment, we included limited GCM validation in the FOD because the CMIP6 simulations were generally not available at that point. We have expanded on our model validation material for the FGD.
27248	1	6	202	13	While the coordinating lead author, P. Forster, is self-cited in the chapter not less than 36 times (!), prominent scientists like J. Christy, C. Wunsch, S. Schneider, R. Lindzen, R. Pielke Sr, and many other who reported important conclusions in the field of this chapter (https://notrickszone.com/2018/12/10/the-list-grows-now-85-scientific-papers-assert-co2-has-a-minuscule-effect-on-the-climate/) 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 is an amazing example of cherry picking which decredibilizes the entire chapter. The observation of (i) +0.4°C since 1945 (Figure 2.12), 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 that 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]	Taken into account. We have assessed the recommended papers and cover the topics listed to the extent that they pertain to the material covered in this chapter. However, several of the topics should rather be covered in other chapters rather than in Ch. 7 (Ch. 2 for observed changes, for example).
13510	1	15	1	15	"Govindasamy Bala (India)" should be changed to "Govindasamy Bala (India/USA)" [Govindasamy Bala, India]	Editorial
46448	1	17	1	17	Chang "Zhanqing Li (China)" to "Zhanqing Li (USA)" [Zhanqing Li, United States of America]	Taken into account.
11660	1	17	1	17	Zhanqing Li (China)' should be 'Zhanqing Li (USA)'. [Chuanfeng Zhao, China]	Taken into account.
33224	1	26	1	26	Robert Colman's name is misspelled [Mark Zelinka, United States of America]	Editorial

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
40716	1				This chapter is in good shape for a FOD. I like the amount of physical reasoning, not just review. [Daniel Murphy, United States of America]	Noted. Thanks for the positive feedback!
45248	1				The content could be more comprehensive. For example, in the section 7.3.3, the discussion is divided by methods. However, some critical findings are found by multiple methods. In addition, the discussion only focuses on global scale studies. But global studies may not be able to resolve small-scale quantities, such as cloud, that needs regional studies. The discussions are suggested to include regional studies' findings. [Jianping Guo, China]	Noted. We have chosen to divide the aerosol ERF material such that Ch. 6 covers individual species and processes governing regional forcing, Ch. 7 assesses evidence relevant for the global ERF, and Ch. 8 covers processes relevant to the hydrological cycle. .
45252	1				The results reported in literature are discussed. However, the uncertainties are not explicitly discussed such as line 10 in page 64. The lack of uncertainties makes the conclusion weak and make Figure 7.12 hard to integrate all the findings into one figure. [Jianping Guo, China]	Taken into account. Thank you, we have made sure to discuss the uncertainty associated with reported estimates more fully for the SOD.
14018	5	1	5	2	The introductory para of the executive summary providing a brief on the chapter and its contents is missing. It needs to be inserted appropriately. [Nikhil Kant, India]	Taken into account. Paragraph added.
53670	5	1	7	23	The chapter has a very good ES. [Jan Fuglestvedt, Norway]	Noted. Thank you for the positive feedback!
48002	5	1	7		Executive Summary formatting. Please arrange points/paragraphs under subheadings and provide a summarising paragrah explaining the purpose of the chapter (see SR1.5 for guidance). [WGI TSU, France]	Taken into account. Have provided a summarising paragraph and arranged the points under subheadings.
57358	5	1	113	35	This is a great chapter, well done! Best chapter I've reviewed -- and you're in luck, because I exhausted myself commenting on chapter 1 (that is, after all, what chapter 1s are for) so have run out of steam a bit. But really good to see all this material drawn together, and work on greenhouse gas emission metrics integrated with work on metrics of climate response. My only suggestion is a positive one, that there is a nice simple equation relating cumulative and short-lived climate pollutants that could be included in the metrics section: it's simpler than the equations you give (and I fear that any equation involving an integral sign may be a bit useless for many readers -- so it might be worth thinking hard about the presentation). With this, and your values of AGWP and TCR which together determine the TCRE, you can take over the entire report. Go chapter 7! [Myles Allen, United Kingdom (of Great Britain and Northern Ireland)]	Noted, thank you. Equations are no longer presented in the chapter text, but rather in the appendix.
52020	5	1			The ES is very accessible. Use of bold is a good rhetorical twist but at variance with other chapters. Perhaps some consideration should be given to other chapters adopting this approach? [Peter Thorne, Ireland]	Noted. Thank you!
15012	5	3	5	3	Relative to the energy budget, a substantial change cause by man is contradicted elsewhere in this chapter. Based on Figure 7.11, the effect to the energy budget from CO2 emissions has been about 2 W/m^2 since industrialization began. Including other anthropogenic effects and whether it's a fraction of the 240 W/m^2 of average solar forcing, the 390 W/m^2 of average surface emissions, or even more when including the non radiant energy leaving the surface, the total anthropogenic impact to the energy budget is less than 1% distributed over more than a century. If what's being referred to as substantial is the resulting temperature change and this is to be connected to a change in the energy budget, it must be made clear that the large predicted change in temperature arises from a small change to the energy budget and that relative to the average amplification boosting 240 W/m^2 of solar input into 390 W/m^2 of net surface emissions, the extraordinarily large incremental amplification required must be supported with extraordinary proof. [George White, United States of America]	Rejected. We have kept the wording. The relative quantity here that controls the total heat content of the system is the net radiative imbalance at the top of the atmosphere. A stable mean climate state (like that which characterized Earth in the centuries leading up to industrialization) requires a nearly perfectly balanced energy budget. The ~2Wm-2 ERF contributed by changes to atmospheric CO2 since pre-industrial times is in that context truly substantial.
51658	5	3	5	3	Can you find a clearer word than 'perturbed' for the non-native English speaker, as will many of the readers be? [Lindsey Cook, Germany]	Rejected. 'Perturbed' is believed to be well-known to most readers.
15336	5	3	5	3	Define Earth's energy budget in a sentence as part of this opening sentence "Human activity has substantially perturbed Earth's energy budget [very high confidence], ….[insert definition]". This will help lay people better understand this chapter. [Lia Cairone, United States of America]	Taken into account. This bullet has been changed but and we define on first use
18302	5	3	5	6	The second sentence may be confusing for the non-scientific reader. If the feedback understanding since AR5 (released in 2014) has improved, why is the energy budget deemed closed only up to 2014, and not up to the time of the current AR6? [Gwenaelle GREMION, Canada]	Taken into account. Good point. This is simply a consequence of there being a lag between the collection of observations and the associated data-processing and revised analysis. The statement has now been revised to be consistent with the updates in the report itself.

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29320	5	3	5	7	I don’t agree that the ERB is closed using nearly constant aerosol radiative forcing. In [Dewitte et al, 2019], the new result was found that the EEI seems to be decreasing since 2000. This can only be explained by an increase in aerosol radiative forcing. [Steven Dewitte, Belgium]	Noted. It appears that the EEI in Dewitte et al. decreases partly because of increased OLR, which is partly cancelled by reduced reflected shortwave radiation. This is not inconsistent with a nearly constant aerosol forcing in recent decades. Cloud changes unrelated to aerosols could easily be responsible for the slight reduction in reflected Shortwave radiation.
18300	5	3	5	9	The bolded sentence for this paragraph does not seem to connect with the remainder of the paragraph if the main takeaway point is that human activity is a major factor in our energy budget, i.e., the rest of the paragraph does not discuss the human element of warming, just that there is warming. [Gwenaelle GREMION, Canada]	Rejected. The estimated ERF is predominantly of anthropogenic nature, so therefore the bolded sentence does in fact reflect what comes after.
15096	5	3	117	14	The bottom up methodology of calculating the net incremental effect from equivalent forcing by adding together many small individual effects without bounding the result by a first principles model of the response to the net accumulated forcing is fundamentally flawed. This flaw enables predicting an incremental effect that's so far beyond the average effect, it becomes obviously meaningless, even as it seems plausible to anyone with a limited understanding of the macroscopic constraints imposed by the laws of physics. The predicted range of ECS has significant uncertainty because it lacks any theoretical support from those laws, which would otherwise constrain the ECS to within much tighter bounds. Even with all of its uncertainty, the presumed lower bound is beyond what first principles physics can even support. The closest thing to theory is how feedback applies, yet feedback analysis, as defined by Bode, was so horribly misapplied to the climate system by Hansen and others, there's no correlation between the climate feedback model and the laws of physics or the ground truth. [George White, United States of America]	Noted. Comment contains no concrete suggestions.
15098	5	3	117	14	There's a lot of discussion about uncertainties, but little of this uncertainty is reflected in the declared levels of confidence. For example, the ECS has error bars of about +/- 50% which as a factor of 3 between the minimum and maximum possible and is definitely not characteristic of a high confidence metric whose presumed mean is declared as settled science per the consensus surrounding IPCC reports. Confidence in a mean arising from excessive uncertainty is an illusion easily reinforced by confirmation bias and is an inappropriate conclusion for a scientific assessment. [George White, United States of America]	Rejected. It is unclear what revision is requested here.
15100	5	3	117	14	It the many serious errors in this report are not addressed, it will inevitably become a legal conundrum, even if it was true that in the past that these errors were not known. Failing to address them now combined with the substantial negative economic impact of the policy goals justified by knowingly and willfully misrepresenting the science will constitute a financial fraud against humanity. I understand that it’s hard to accept that ostensibly intelligent scientists can be so wrong about something so important, but it’s equally undeniable how wrong so many of those cited in this report are and it’s equally inconceivable why so many obvious errors have been allowed to persist for so long. It's my obligation to humanity, science and the IPCC to point out that having the courts decide what is and what is not science is certainly not an outcome that anyone wants to see, but sadly, this is the direction we seem to be heading and the evidence is overwhelming that if the laws of physics are to be accepted, the IPCC’s assessment of climate science will become moot. The destiny of science is predictable and the scientific truth will always prevail. The more this truth is resisted, the more it will impact its detractors when it’s inevitably accepted. There's only one logical, ethical and responsible way forward, which is for the IPCC to decouple the science from policy goals and present objective rational science. If the IPCC doesn’t do this on its own, it will inevitably be forced to do so, or be dissolved. All that needs to be done is to bound the problem by the known laws of physics which otherwise need no further justification and eliminate references to the many obviously flawed papers that defy those immutable laws. Then the important question becomes, how and why did peer review fail us so miserably? [George White, United States of America]	Noted. Unclear what revision is requested here. The IPCC does not make policy recommendations.
19060	5	4	5	4	What "closed" means here? [Gwenaelle GREMION, Canada]	Noted. "Closed" in this context means that there is no missing energy source or sink. We believe that readers will generally understand what a "closed budget" means.

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45534	5	4			There is a troubling point of confusion about "time varying feedbacks" that this chapter should straighten out. A feedback, by definition, is a causal loop where (in our case) a change in global-mean temperature T causes some radiative change dN, which subsequently acts on T. If the equilibrium warming occurs in a particular pattern (say, stronger at higher latitudes) then the feedback includes the impact of this pattern on dN. If the early transient response dN(T) differs from that at equilibrium due to lags (say ocean thermal inertia), this could arguably be called a time-varying feedback, although a purist might say the true feedback is the equilibrium change, and the transient responses are not addressed by the paradigm. In a third case, however, where natural pattern variability that has nothing to do with forcing or global-mean T is affecting dN, these variations should not be considered time-varying feedbacks since they are not part of a causal loop. They are simply noise in dN. However here and elsewhere, and in some of the literature, variations in $\Delta N/\Delta T$ are all lumped together as "time varying feedbacks" regardless of provenance. This is I fear impeding clear thinking and making the system sound more unpredictable than it may be. [Steven Sherwood, Australia]	Taken into account. There are indeed multiple ways this topic could be framed, as seen in the somewhat messy literature on this. We've chosen to frame it in terms of dN being a function of SST patterns, which can evolve over time under forcing or with internal variability. We prefer this framing because it is not clear what has drive the observed SST patterns, so both effects must be accounted for at once. But to help clarify our meaning, we're now careful not to refer to this as "time varying feedbacks" and to, instead, explicitly link dN to changing SST patterns while noting that multiple things could be influencing those patterns.
18304	5	5	5	6	It is not very clear what it means for the energy budget to be "consistent across observations and ECS". [Gwenaelle GREMION, Canada]	Noted. This ES bullet is based mainly on the content of Box7.1, which shows that observations of surface, atmosphere and ocean warming, along with sea ice and ice sheet melt, add up to an energy amount that is equal (within uncertainty) to the energy that Earth's has been accumulating through the top-of-atmosphere radiative energy imbalance.
15014	5	6	5	6	It's inappropriate to infer that the ECS is a high confidence metric, as its range of values is the cause of the climate science controversy. The reason for the controversy is that despite the +/- 50% uncertainty in the sensitivity, there's almost no overlap between the range asserted here and what has been reported by many credible scientists whose work is excluded from this report, moreover; the high confidence expressed herein ignores the high uncertainty that arises even without giving serious consideration to any of this other work. Furthermore, the large uncertainty infers false confidence since what it really means is that the science is far from settled and nobody has a clue about the actual effect of CO2 emissions, or even incremental solar energy based on Figure 7.8. Denying the uncertain and controversial nature of the ECS or using one extreme example to denigrate all possible challenges, as was done on page 94, only serves to deprecate the scientific method as the true arbiter of what is and what is not science. [George White, United States of America]	Taken into account. The ECS assessment is based on a very large body of literature, and supported by different and independent lines of evidence. However, we certainly may have missed papers, and would be thankful if the reviewer could point us to the specific one(s) he feels that we have omitted. But given the large number of papers already considered for each line of evidence, a handful of additional studies are unlikely to lead to substantial revisions to the assessment.
19062	5	7	5	7	the specification "(i.e. …heat content change)" is more general than "Earth system warming". Shouldn't it be the other way way around? Or maybe just change "change" for "increase". [Gwenaelle GREMION, Canada]	Taken into account. Thank you, we have followed your suggestion.
25718	5	7	5	7	Warming is defined here as "total earth system heat content change", (better "increase in total Earth system heat content" thereby specifying the sign of the change). However not clear whether the quantity is a time integral, e.g., J, or a rate J yr-1 (or, if normalized to surface area, J m-2; W m-2)? Such a definition is extremely useful but should be clarified. [Stephen E Schwartz, United States of America]	Taken into account. We now write: "Total earth system warming, i.e., the total change in 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." Thus making it clear that the quantity is a time integral.
18306	5	8	5	9	It might be worth considering whether the total earth system heat content change is a robust metric of climate change or just of global warming. Preciptation, storm intensity, drought frequency and other important climate changes should have stronger functional dependence on the average surface temperature than the ocean heat content (which dominates the system heat content). It is the case that the heat content could be a good metric of the amassed energy imbalance (how perturbed the earth system has been) but not the degree to which the climate in the sense of expectation weather has changed. This point is not addressed directly by Schuckmann et al, 2016, which is the sourced cited in the body of the report on the topic of surface temperature change vs heat content. [Gwenaelle GREMION, Canada]	Taken into account. This is a good point. Heat content has advantages, but global mean surface temperature is what we really want to predict for these and other reasons. Text has now been clarified
18308	5	11	5	11	"best estimate of 297 ZJ" should be clarified to indicate that this an independent observation-based assessment, to better line up with the discussion in Box 7.2 and not GCM estimate [Gwenaelle GREMION, Canada]	Taken into account. Good suggestion, we have added that this is an observation-based number.

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50834	5	11	5	11	the use of the wording 'best estimate' is not clear: what is the scientific rationale to jusdge a 'best estimate'? I suggest to remove tis, particularly in the ES [Karina von Schuckmann, France]	Accepted: This is quoted as a range.
15016	5	11	5	11	A substantial change in stored energy can only be supported when the change is shown to be sufficiently large relative to the uncertainty in the total energy stored. Citing an average imbalance of 0.43 W/m^2 doesn't do this, as it's less then 0.25% of the energy budget. Relative to the ground truth, the stated imbalance is indistinguishable from noise, uncertainty and the natural chaotic variability around the mean response to solar energy. Furthermore, the global monthly average imbalance varies over a 20 W/m^2 range during the year, while the peak to peak imbalance per hemisphere is about 160 W/m^2 in the N and 180 W/m^2 in the S. Half a Watt per meter squared is meaningless relative to the dynamic imbalance which for half the year in each hemisphere is positive and for the other half, is negative. The net imbalance arises by adding many large and somewhat uncertain positive numbers with many equally large and uncertain negative numbers. While the sum should be zero in the steady state, a small fraction of a percent residual is not statistically significant relative to the uncertainty in its constituent parts. Inferring an average imbalance indirectly is subject to even greater errors. [George White, United States of America]	Noted. Note that the statement about changes to the overall heat content of the system relies on multiple independent observations (ocean heat content, surface and atmospheric temperatures, sea ice and ice sheet melt, etc.), and that the these observations are consistent with each other and with the estimates of ERF and ECS - the imbalance in Earth's radiation budget is thus only one out of several lines of evidence that the amount of stored heat has changed substantially.
15018	5	11	5	11	Supporting data for the dynamic global and per hemisphere imbalances is here. They're reported as monthly averages spanning 3 decades of weather satellite data originating from GISS (ISCCP). While a net global imbalance appears, it's magnitude is statistically irrelevant when compared to the uncertainty in the components comprising it and is equally likely to be negative or zero. http://www.palisad.com/co2/plots/wbg/g/flux.png http://www.palisad.com/co2/plots/wbg/nh/flux.png http://www.palisad.com/co2/plots/wbg/sh/flux.png Yellow is the solar input before reflection, blue is the net input power from the Sun (Pi), brown is net LWIR output power at TOA (Po), red dashed is the BB surface emissions (Ps) at its reported temperature and red dash-dot is (Pi – Po) or the instantaneous imbalance at TOA (forcing). The components in each plot are centered on their average and displayed with unique Y axis limits. The averages and Y axis limits per component are shown. Neither Pi or Po are explicit in the data set, however, Pi is trivially calculated from the solar energy and albedo, both of which are available. Calculating Po from the reported data is more complicated and based on the emissions at the reported surface and cloud temperatures, cloud properties and HITRAN driven radiant models of the atmosphere. The fact that the net imbalance is so small is a strong indicator that the calculation of Po is relatively accurate, although the uncertainty is still larger than the net imbalance. The reported time constants (tau) are calculated based on the ratio of the peak to peak Po to the peak to peak Pi relative to its period as would be done for an RC circuit. The time constant is the amount of time it takes to achieve 63% of the final equilibrium given no further variability in Pi. Unlike the time constant of an RC circuit, the climate system time constant is not a constant and has a 1/T^3 dependency, decreasing as the temperature increases, thus the actual time to achieve 63% of equilibrium will likely be shorter than reported. Many similar plots aggregating smaller slices of latitude and displaying other variables can be found starting from here: http://www.palisad.com/co2/plots/wbg/plots/html [George White, United States of America]	Noted. The statement is based upon in-situ measurements of ocean temperature profiles, which are used to determine the tendency in ocean heat content anomalies for the period from 1971-2014. The statement has nothing to do with weather satellite data originating from GISS (ISCCP), as suggested by the reviewer.
25720	5	11	6	54	As in AR5 (as pointed out by Schwartz et al., 2014) there is apparent inconsistency in the summary data for Delta T, N, Forcing, and ECS. I propose that this at least be examined, if not actually resulting in changes in the best estimates presented. I have prepared some material for consideration for inclusion in the report, including a figure. As the figure cannot evidently be pasted here, I make the entire document available at https://www.bnl.gov/envsci/schwartz/more/ECSvsF-N.pdf . I also paste in here, succeeding comment, the proposed text. I would welcome the opportunity to prepare such a figure and work with the lead authors on incorporating this material into the text of the document. Alternatively and perhaps more fruitful would be a re-examination of the several recommended values to achive better consistency. Schwartz, S.E., Charlson, R.J., Kahn, R. and Rodhe, H., 2014. Earth's climate sensitivity: apparent inconsistencies in recent assessments. Earth's Future, 2, 601-605. http://onlinelibrary.wiley.com/doi/10.1002/2014EF000273/full The formatted text with figure is available at https://www.bnl.gov/envsci/schwartz/more/ECSvsF-N.pdf [Stephen E Schwartz, United States of America]	Noted. Multiple recent studies suggest that pattern-effects (Section 7.5.3), which temporarily dampen global warming, is sufficient to explain the apparent inconsistency.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
25722	5	11	6	54	suggest add a figure and the following language The relations between the best estimates and uncertainty ranges of top-level climate quantities are examined in Figure xx, similar to that presented by Schwartz et al. (2014). According to Eqn xx, the equilibrium climate sensitivity ECS is related to the total effective forcing over the industrial era F and net flux into Earth's climate system N as ECS = F_2x*DT/(F-N) (xx) where F_2× is the forcing for doubled CO2, shown by the green diagonal line (slope = -1 on log-log plot) drawn for best estimate values of the increase in global mean surface temperature ΔT - 1.1 K, N = 0.42 W m-2, and F2× = 4.0 W m-2. Ranges and best estimates for ECS and F - N are shown by the line segments and symbols at the right and the top of figure, respectively. As shown by the drop lines in the figure the best estimate for F - N, 2.06 W m-2 (for best estimate F = 2.48 W m-2) corresponds to an equilibrium climate sensitivity 2.1 K, well below the best estimate for this quantity, 3 K, and the lower limit of the likely range for this quantity, 2.5 K, and is close to the lower limit of the very likely range, 2.0 K. This graph thus illustrates considerable apparent inconsistency among the best estimates of the several quantities. Also shown in Figure xx are the values of (ECS, F - N) for CMIP6 models, evaluated from the data for the several models presented in Table 7.13 of the first order draft. Although for the most part the values of ECS and F - N for the several models fall within the very likely ranges of the two quantities, with a single exception the data points lie systematically above the green line representing the observed increase in GMST, implying that these models exhibit higher sensitivity and/or greater forcing (or lower N) than would be consistent with equation xx. [Insert Figure xx here] Figure xx. Equilibrium climate sensitivity ECS versus total effective forcing over the industrial period F minus Earth's net heat flux N, based on best estimate values and ranges for the several quantities in AR6 First Order Draft. Forcing for doubled CO2 F2× is	Noted. Comment contains no concrete suggestions.
25724	5	12	5	12	Give range for 0.42 W m-2 rate of heat increase [Stephen E Schwartz, United States of America]	Taken into account. Range added, thanks for the comment
18310	5	12	5	12	"Period length" reads confusingly. Perhaps "...ZJ, or an average of 0.42 W/m^2 over the surface of the Earth" would be simpler. [Gwenaelle GREMION, Canada]	Taken into account. "Period length" is removed from the sentence.
13362	5	13	5	13	Change "imbalance" to "gain" as the sign of imbalance is not conveyed now [Govindasamy Bala, India]	Taken into account. Thanks for the comment, the ES bullet has been reworded to clarify that the energy imbalance is in fact positive.
50836	5	13	5	13	… 'was larger' … is very vague, and a more concrete information should be added here. [Karina von Schuckmann, France]	Taken into account. This has been made more specific
29322	5	13	5	14	This is true, but it is a simplication compared to the results of [Dewitte et al, 2019]. The EEI seems to have have been rising steadily between 1982 and 2000, and to have been declining since 2000. [Steven Dewitte, Belgium]	Taken into account. The study was considered in our assessment but the more compelling evidence comes from assessment of the observed changes in earths energy inventory.
27230	5	14	5	15	Please cite and discuss Wunsch, C., Heimbach, P., 2014, Bidecadal thermal changes in the abyssal ocean. J. Phys. Oceanogr. 44, 2013, who estimate the heat content down to abyssal depths and who question this claim since the heat content is found of approximately 4E22 J in 19 years, for a net heating of 0.2 W/m2, smaller than some published values. [François GERVAIS, France]	Noted. The paper does not feature in our assessment because it is an outlier in the literature and because we primarily draw upon ocean heat content papers published since the IPCC SROCC assessment.
50838	5	16	5	16	the depth interval for 'upper' needs to be clarified: for the 'upper' ocean discussed in chapter 02, AR5, and 1.5° reports, there is 'high confidence' - consistency / or rationale/quantification is needed if confidence language changes [Karina von Schuckmann, France]	Not applicable. Sentence is no longer included in the Executive Summary in the FGD.

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42102	5	18	5	20	"will continue until 2300" : why 2300 specifically ? Could it rather be "at least 2300" ? Could you consider adding "under the strongest mitigation scenarios currently regarded as plausible", as the biggest uncertainty here could be that we know little about humanity in 2250 (they might perhaps bring CO2 concentraiton back to 280ppm if they so desire, getting rid of all the 'extra' CO2 with techinques as evident to us as a silicon-based computer would have appeared 200 years ago) ? [Philippe Marbaix, Belgium]	Taken into account. Text has been revised in the FGD.
18498	5	20	5	20	a short subordinate clause or parantheses about what "under strong mitigation" means, would be beneficialsince this it the executive summary [Gwenaelle GREMION, Canada]	Taken into account. We have added more explicit sign-posting to the relevant projections in Chapter 9.
19064	5	20	5	20	Not clear, from Box 7.2 sea level rise will continue increasing until 2030 under strong mitigation, but not temperature. [Gwenaelle GREMION, Canada]	Taken into account. We have more clearly sign-posted projections of sea-level rise in Chapter 9.
18922	5	21	5	24	I think some context should be provided about the dimming and brightening in surface solar radiation, for example what causes those changes, what is their net effect (if this is known). [Gwenaelle GREMION, Canada]	Taken into account, we have added a sentence about the most likely cause of the dimming and brightening: "Decadal variations in aerosol forcing are considered major contributors (medium confidence), but internal variability in cloudiness may also have played a role."
40732	5	22	5	22	I've always been uncomfortable with the terms "dimming" and "brightening", finding them misleading to non-specialists in places like this summary where they don't have context. Unfortunately, I don't have a better suggestion other than expanding them to awkwardly long phrases, like "reductions or increases in surface solar flux due to changes in scattering aerosols or other causes." [Daniel Murphy, United States of America]	Taken into account. We have added a little more context to this ES for the SOD, as follows: "Multidecadal dimming and brightening trends in incoming solar radiation at the Earth's surface occurred at widespread locations. These trends are neither a local phenomenon nor a measurement artefact [high confidence]. Since AR5, additional evidence for a widespread decline in surface solar radiation is found in the observational records between the 1950s and 1980s ("dimming"), with a partial recovery at many observational sites thereafter ("brightening") (high confidence). Decadal variations in aerosol forcing are considered major contributors (medium confidence), but internal variability in cloudiness may also have played a role."
18314	5	22	5	23	The use of "which are" doesn't fit subject verb agreement for the bolded sentence. Could be clearer, maybe "indicating that these trends are not a local phenomenon…" [Gwenaelle GREMION, Canada]	Accepted. This was rewritten for clarity.
13196	5	22	5	23	This sentence is confusing. What is meant by "dimming and brightening"? Does "surface solar radiation" refere to "net", "incoming", or "reflected" solar radiation? [Nora Richter, United States of America]	Taken into account, we have added some clarifications and context to this ES bullet, see response to comment ID 40732. Also we refer to this term now as "incoming solar radiation at the Earth's surface"
29324	5	22	5	23	The dimming and brightening trends should be the movitation to better investigate total aerosol radiative forcing and its time variation. It is not credible that during recent decades aerosol radiative forcing has been varying monotonuously with time as shown in Box 7.2 Figure 1 e). [Steven Dewitte, Belgium]	Noted.
13364	5	22	5	24	Specify the period during which this first statement is valid. Also, it would be clearer if the other components are explicitly specified. [Govindasamy Bala, India]	Taken into account. Approximate periods od dimming and brightening have been added. There is no room to mention all other components in the surface energy budget individually where trends are much more uncertain. An exception is the surface downward longwave radiation, which we now explicitly mention here.

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19312	5	22	5	24	Are these “trends” forced or just due to internal variability (or is that known). Please clarify. [Norman Loeb, United States of America]	Taken into account. We have added a statement about the likely cause of the trends. See response to comment ID 18922
18312	5	22	5	24	Given that solar signals are often mis-interpretted as the cause of climate change, it would be useful to make clear in this paragraph that the surface solar radiation varies due to atmospheric composition (aerosols) and cloud cover. [Gwenaelle GREMION, Canada]	Taken into account. We have added a statement pointing to the importance of aerosol and clouds. See response to comment ID 18922
53672	5	22	5	24	This statement may seem a little conflicting with the strong opening statements of the ES. [Jan Fuglestvedt, Norway]	Taken into account. Text now reworded to make clearer
27094	5	22	5	24	The language used here conflaits the temporal and spatial variability in the surface solar radiation, and it is hard to understand the scale (temporal or spatial) on which these variations have been detected. I think it would be better to split this exectutive summary point into two, one concerning the temporal variability, and the other the spatial variability, if possible, and expand on both aspects. This is a format which is used for some of the other summary points (e.g. lines 47-51) where two or more components are addressed separately. [Chris Satow, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We have expanded considerably on this ES bullet (see response to comment ID 40732), and hope that your concerns have been addressed with the revised text.
58020	5	22		24	The current phrasing doesn't make clear the temporal evolution of the dimming and brightening trends. Replace with 'Dimming trends from xxx to yyy following by brightening trends from yyy to zzzz occurred at widespread locations…'. [Nathan Gillett, Canada]	Taken into account. Changed accordingly, thanks.
18316	5	24	5	24	Maybe the phrase "surface energy imbalance" would be more clear than "surface energy budget". [Gwenaelle GREMION, Canada]	Rejected. In our opinion "…trend estimates in components leading to surface energy imbalance" complicates the language here.
25726	5	26	5	26	The use of "warming" introduced at line 7 is contradicted at Line 26, "warming effect"; perhaps better "has resulted in an increase in global surface temperature" which is unambiguous. Similarly at line28 "greenhouse warming"; better "increase in global mean surface temperature due to forcing by incremental greenhouse gases" and similarly "decrease in ..." [Stephen E Schwartz, United States of America]	Taken into account. Warming effect has now been used and clarified
18318	5	26	5	27	I would insert "global" in between "induced" and "surface" [Gwenaelle GREMION, Canada]	Accepted.
48798	5	26	5	27	Inconsistent with Chapter 5 p66 line 33-34: We here apply a historical warming expressed in global average surface air temperatures (SAT) of 0.97°C between the 1850–1900 and 2006–2015 periods. And inconsistent with Chapter 2, p37 line 33-37: SR1.5 reported warming from 1850-1900 to 2006-2015 of 0.87 °C, with an 1880-2012 trend of 0.86°C and an 1880-2015 trend of 0.92°C. A table with the different historic warming estimates, an explanation of the terms, why each is used where and the origine of the differences would help. [Birgit van Munster, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. There are no inconsistencies, warming is here reported for a different period than in the other instances (1750 - 2017). Note also that changes in GMST and SAT differ.
9640	5	26	5	27	Here, the text says that "Human-induced surface temperature rise for the period 1750-2017 is 1.1 degree C", whereas in SR1.5, it is extimated as 1.0 degree C above pre-industrial level (p. 6 of the SR1.5 SPM). In SR 1.5 report, however, pre-industrial period is defined as 1850-1900 (p. 26 of SR 1.5 SPM). Does the difference of 0.1 degree C come from the difference of the definition of 'pre-industrial period'? Please explain for readers who are familiar with SR 1.5. [Mitsutsune Yamaguchi, Japan]	Noted. Yes, the 0.1 degrees C stems from the warming from 1750 to 1850.
9642	5	26	5	27	Does human-induced surface temperature here mean surface air temperature (SAT) and not global mean surface temperature (GMST) as in SR 1.5 (p. 6 of the SR 1.5 SPM). If yes, clear explanation of why SAT was used is necessary as, In SR 1.5 report, global average temperature is defined as GMST. Also please explain the reason of difference between human-induced temperature rise in AR6 (1.1 degree C) and SR 1.5 (1.0 degree C.) whether this is caused by the difference of either the definition of 'pre-industrial level' and or 'global aberage temperature". [Mitsutsune Yamaguchi, Japan]	Taken into account. We are now clear that we consider GSAT within the Chapter
27232	5	26	5	30	This claim is most questionable since, as seen in Fig. 2.12, the increase of temperature since 1945, beginning of acceleration of emissions, the increase has been only of about 0.4°C up to the plateau precceding the last El Niño. [François GERVAIS, France]	Rejected. This claim is supported by all available observations, as assessed in the chapter. It would be helpful to know whether the reviewer wants to challenge the reported observed temperature change, or the attribution of this warming to anthropogenic activity, and even better if relevant references in the peer reviewed literature could be provided.
53674	5	26	5	31	The bold part is a repetiton of ch2. You may consider turning the para around and start with the warming and cooling components. [Jan Fuglestvedt, Norway]	Taken into account. Good suggestion, we have rearranged this bullet for the SOD

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
9166	5	26	5	31	There is some disagreement amongst the various Chapters as to by how much global temperature has risen since pre-industrial; and based on comments 2 and 3 above, there should be recognition of a significant natural influences. [Jim O'Brien, Ireland]	Taken into account. We tried to make sure all statements of observed warming are consistent with each other
14820	5	26	5	31	This statement doesn't belong here. Chapter 3 is where the attribution assessment needs to be made bringing in a wider variety of information. The information provided here needs to be incorporated into the overall snythesis provided in Chaoter 3. The assessment in this chapter is based on a simple climate model while the overall attribution assessment needs to take account of other sources of information. I suggest the chapter body still includes this information but this ES paragraph is removed and a synthesis version drawing in more sources of information is provided in Chapter 3. [Peter Stott, United Kingdom (of Great Britain and Northern Ireland)]	Noted, to be discussed in Ch. 3
49130	5	26	5	31	This attribution on historical GSAT changes gives a much more in-depth statement than the detection & attribution assessment in Chapter 3. Please consider describing the methodology/rational in this statement to clarify difference from from the Chapter 3 methodology. [Yu Kosaka, Japan]	Taken into account. This has now been coordinated with Chapter 3
58022	5	26		31	Make clear that these temperature changes are inferred temperature changes based on assessed ERF changes, not an attribution of observed temperature changes. [Nathan Gillett, Canada]	Taken into account, thank you.
56662	5	26			Very nice that the headline result uses 1750 as reference point. [Malte Meinshausen, Australia]	Noted. Thank you for the positive feedback.
36240	5	26			Note that Chapter 5 only assesses that 'It is virtually certain that the accumulation of CO2, CH4 and N2O in the atmosphere is the result of human activities' (i.e. P>= 99%) (ch5, pg 6, ln 36-37). If it there is up to a 1% chance that the increase is not due to human activities based on the Chapter 5 assessment, how can it be certain that human activity has had a warming effect? [Nathan Gillett, Canada]	Taken into account. Corrected, thanks.
37796	5	26			Chapter 3 (page 3-6, line 6) states that it is "virtually certain that human influence has warmed the climate system". "Virtually certain" is not quite the same as "unequivocal", the word used here. A related comment is no. 13 on Chapter 1 (page 1-4, line 30) where the word "fact" is used. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Good point.. Corrected to "virtually certain".
29326	5	28	5	30	An aerosol cooling that has remained relatively constant over the last 20 years is not consistent with the decline of the EEI since 2000, see [Dewitte et al, 2019]. Also it is not consitent with the measured TOA ERB changes following air pollution reduction in China and the US after 2013, see [Loeb et al, 2018]. [Steven Dewitte, Belgium]	Rejected. As per the response to your previous comment, a relatively constant aerosol forcing in recent decades is NOT inconsistent with the decline in EEI found since 2000 in Dewitte et al. (2019). The OLR increase is, as the paper points out, due to increasing surface temperatures. The reported slight decrease in reflected shortwave radiation could easily be explained by cloud changes unrelated to aerosols (i.e. cloud feedbacks)
18980	5	28	6	6	The indication of the 5% to 95% range is confusing. There should be some divider between the range in degrees C and the "5% to 95% range" – whether it be a semicolon or the word "is". Perhaps it should be written more like line 53 – that's more understandable. Or maybe somewhere should state that all plus/minus values are a 5% to 95% percent range. [Gwenaelle GREMION, Canada]	Taken into account. The confidence ranges have been changed for the second order draft.
18500	5	29	5	29	it should be stated if the cooling of 0.6°C refers to aerosols in total or just aerosol-cloud or aerosol-radiation interaction [Gwenaelle GREMION, Canada]	Taken into account. The 0.6 degrees of cooling is due to the total aerosol effect (aerosol-radiation and aerosol-cloud interactions). This has now been explicitly stated.
19314	5	29	5	30	This is somewhat surprising given reductions in pollution from China and US. Are these reductions offset by increases in pollution from India? Please clarify. [Norman Loeb, United States of America]	Taken into account. Actually, according to Hoesly et al. (2018), global SO2 emissions plateaued in China in the early 2000s, while OC and BC continued to rise. Globally, there was a slight reduction in SO2 emissions he last couple of decades, while OC and BC emissions continued to increase. So according to our best information about emissions, there is no reason to expect a decline in aerosol forcing.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
40764	5	30	5	33	Can you say something about what this mid-latitude poleward heat transport means for mid-latitude weather? Are storms going to get stronger, weaker, move faster, move slower? I often get asked this. [Daniel Murphy, United States of America]	Rejected. This topics is covered in Ch. 8
41610	5	31	5	31	suggest "negligible long-term effect" [Amanda Maycock, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Suggestion taken, thanks.
46684	5	33	5	34	Assessment on modes of variability occurs in Section 1.3.3; Section 2.4; Section 3.7; Section 4.4.3, 4.5.3; Section 6.2.2.5.1; Section 7.1.1/2 ; Section 8.3.1.3.2, 8.3.2.2, 8.3.2.4.1, 8.3.2.9.1, 8.4.2.5,8.5.2.2.1, 8.3.2.9.2, 8.4.2.5, 8.3.2.9.3, 8.4.2.5, 8.3.2.9.4, 8.4.2.5, Figure 8.43, 8.5.2.2.1, 8.5.2.2.1; Section 9.2.2.1, 9.2.2.3, Section 9.4.3.2, BOX 9.2, 9.2.3.1, Table 9.1, Section 9.2.1, Cross-Chapter Box 9.1, BOX 9.2, 9.6.2.1.1, 9.6.2.1.2, 9.5.4.7, 9.2.5; Section 10.1.4.2, 10.4.2.2, 10.6.3.3; Section 11.3.1, 11.7.1.1, 11.6.2, 11.1.5,11.4.1, 11.6.1, Table 11.4; Section 12.4.1, 12.4.4.3, 12.5.2.3; Section Atlas.5.2.1.2, Atlas.5.3.1.1, Atlas.5.3.2.1, Atlas.5.5.1.1, Atlas.5.5.2.1, Atlas.5.6.2.1, Atlas.5.6.3.1, Atlas.5.10.2.1, Atlas.5.10.2.2. This topic is addressed in ES of Chapter 2, 3, 4, 7, 11, addressed in box in chapter 9, and broadly addressed in above-mentioned subsections in chapter 1, 2, 3, 4, 6, 7, 8, 9, 10, 11, 12. [WGI TSU, France]	Not applicable. This bullet is not about modes of variability
18320	5	33	5	35	I feel that there is a word missing between "Under" and "greenhouse gas forcing" that would increase clarification of this key point. Is this projected GHG? Current GHG? Observed? [Gwenaelle GREMION, Canada]	Taken into account. Thanks for the suggestion, we have added the word "projected" to clarify
18556	5	33	5	39	The present draft of AR6 claims that "Ocean heat transport changes arise from a reduction in the Atlantic Meridional Overturning Circulation, and from the transport by ocean currents of excess heat taken up at the surface.", which highlights the special importance of the Atlantic Meridional Overturning Circulation (AMOC). However, a recent study by Forget and Ferreira (2019) suggests that the global ocean heat transport is dominated by heat export from the tropical Pacific Ocean. Though the Atlantic and Indian oceans have unique ability to transport heat across the Equator, the two ocean basins imports only about 25% as much as heat as is exported from the Pacific. As Forget and Ferreira (2019) estimated (their Fig. 1), the tropical Pacific Ocean obtains large amounts of heat through the sea surface and "most of this heat uptake (1.20 PW) appears to be transported to the Indian Ocean via the Indonesian through flow (ITF)." Plain OHT by the ITF is 12 PW, while 0.29-0.84 PW by the AMOC, indicating that the ITF has a greater meridional heat transport that the AMOC. The heat and freshwater transports by the ITF show strong and long-term change as well (e.g., Hu and Sprintall, 2017; Sprintall et al., 2019), which exerts important influence on the Earth's heat budget (0.1 PW of "effective OHT", Forget and Ferreira, 2019) and climate (e.g., Hu et al., 2015; Lee et al., 2015). So, it is essential to include the ITF if list the AMOC in the summary of the AR6 in page 7-5. References: Forget, G., and D. Ferreira (2019), Global ocean heat transport dominated by heat export from the tropical Pacific, Nature Geoscience, 12, 351–354. Hu, D., et al. (2015), Pacific western boundary currents and their roles in climate, Nature, 522, 299-308. Hu, S., and J. Sprintall (2017), Observed Strengthening of Interbasin Exchange via the Indonesian Seas Due to Rainfall Intensification, Geophysical Research Letters, 44(3), 1448-1456. Lee, S.-K., W. Park, M. O. Baringer, A. L. Gordon, B. Huber, and Y. Liu (2015), Pacific origin of the abrupt increase in Indian Ocean heat content during the warming hiatus, Nature Geoscience, 8(6), 445-449. Sprintall, J., et al. (2019), Detecting Change in the Indonesian Seas, Frontiers in Marine Science, 6, 257. [Gwenaelle GREMION, Canada]	Not applicable. The paragraph has been removed in the FGD.
58024	5	34		35	Say which sign these 'modest changes in poleward heat transport are'. Or if the sign is uncertain, better to write 'small changes in poleward heat transport' or similar. [Nathan Gillett, Canada]	Not applicable. Paragraph has been removed in the FGD.
16132	5	35	5	39	The statement about AMOC seems too strong, thus perhaps in a slight disagreement with those about AMOC in Chapter 1 and 2. Namely, the latter imply the weakening of AMOC might be an interanual variation, i.e., climatically-temporal decline [my interpretation]. [Branko Grisogono, Croatia]	Taken into account. This text has been moved to Chapter 9. The statement there should clarify that it is referring to projections rather than observed AMOC changes.
56146	5	38	5	38	SRM will have many more implications than water cycle responses: this should be refelcted here and elsewhere in the document [Rolf Müller, Germany]	Not applicable. This is now taken up by Chapter 8 and not considered here

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
18924	5	41	5	45	The ERF is introduced here for the doubling of CO2, while in the next paragraphs the ERF is referring to the period 1750-2017; this might be confusing, so I;d suggest either the lines 41-45 to be removed or the ERF for CO2 to be also given for the same period. Since a value is already given in line 54 (not only for CO2 but for all GHGs) I'd suggest to remove altogether lines 41-45 [Gwenaelle GREMION, Canada]	Rejected. We respectfully disagree. The forcing associated with a doubling of CO2 is of particular interest for estimates of ECS and TCR, and so belongs in the ES. We explicitly state what CO2 changes we are reporting ERFs for in the various bullets, so don't expect this to be confusing to the readers.
57854	5	41	5	45	Missing cross reference with Chapter 1? [Catia Domingues, Australia]	Taken into account. The Executive Summary has been revised (and will continue to be done so throughout the drafting process) - we will keep a close eye on relevant cross-references with other chapters.
18322	5	41	6	13	Include a reference to Figure 7.11 for those paragraphs that discuss ERFs in the end of paragraph brackets [Gwenaelle GREMION, Canada]	Rejected. Referring to figures in the Executive summary is opposing the IPCC AR6 style guide.
58026	5	41			Was the ERF framework really 'introduced' in AR5? Wasn't it introduced in the published literature, and assessed in AR5? [Nathan Gillett, Canada]	Noted. Here we are referring to ERF as it was defined in AR5. You are correct that the concept was introduced in the literature that preceded AR5, but here we specifically want to refer to the exact defintition of AR5.
13366	5	43	5	43	What is the reason for the increase in ERF of doubling CO2 to 4.0 Wm-2.? I recall it used to be 3.5 Wm-2. [Govindasamy Bala, India]	Noted. The ERF has increased since AR5, partly due to revised line-by-line calculations, but also due to better understanding of the associated rapid adjustments. This is stated in Section 7.3.2.1.
18324	5	43	5	43	Clarify this sentence to indicate that double CO2 is a theoretical scenario to provide stronger context for the observed ERF values in the following paragraphs so as to strengthen the "Earth has gained substantial energy" key point [Gwenaelle GREMION, Canada]	Taken into account. This is not the main purpose of providing an ERF estimate - it is a crucial quantity for the calculation of ECS/TCR. This has now been explicitly stated.
25728	5	43	5	55	The uncertainty on ERF for CO2 is 17.5%; how can the uncertainty in GHG ERF be 11.5% and in wmggg 11.8%. [Stephen E Schwartz, United States of America]	Taken into account: These values have been revised in SOD

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
25730	5	43	5	55	For situations of precisely specified aerosol properties (diameter, relative humidity, refractive index) and surface reflectance comparison of radiation transfer codes shows that the radiative effect of aerosols, quantified as forcing per optical depth, can be calculated with model diversity of as low as about 10% (for particle radius 100 - 700 nm, increasing outside that range); Boucher et al., 1998. However forcing is highly sensitive to uncertainty in optical depth, single scatter albedo, asymmetry parameter, surface reflectance (McComiskey et al., 2008). For forcing per AOD of 38 W m-2 (24 hr average at equinox at midlatitude continental site), de-rated by a factor of 2 for clouds, then an uncertainty in AOD of 0.01 corresponds to uncertainty in total aerosol radiative effect of 0.19 W m-2. For uncertainty in global mean AOD from satellite of 0.03 from Modis (Levy et al., 2013) this translates to 0.6 W m-2, just based on uncertainty in AOD alone. This is augmented by uncertainty on single scattering albedo, asym param, and surface reflectance approximately 50% of the ARE. For global mean AOD 0.14 (Levy et al. 2013), the ARE is 2.7 W m-2, of which 50% is 1.3 W m-2. McComiskey, A., Schwartz, S. E., Schmid, B., Guan, H., Lewis, E. R., Ricchiazzi, P., & Ogren, J. A. (2008). Direct aerosol forcing: Calculation from observables and sensitivities to inputs. Journal of Geophysical Research: Atmospheres (1984–2012), 113(D9). Boucher O., et al., Intercomparison of models representing direct shortwave radiative forcing by sulfate aerosols. J. Geophys. Res. 103, 16979-16908 (1998). Levy, R.C., Mattoo, S., Munchak, L.A., Remer, L.A., Sayer, A.M., Patadia, F. and Hsu, N.C., 2013. The Collection 6 MODIS aerosol products over land and ocean. Atmospheric Measurement Techniques, 6(11), p.2989. [Stephen E Schwartz, United States of America]	Not applicable. This comment doesn't seem to refer to the text indicated by the page/line numbers
18502	5	45	5	45	20% may still sound like a lot: mention also the respective value from AR5, especially since in line 44 it is said that the models improved [Gwenaelle GREMION, Canada]	Taken into account. AR5 value added, thanks for the suggestion.
27234	5	47	5	48	To give numbers with 3 digits with an uncertainty of a factor of 2 is physically dubious. Based on infrared spectra of the atmosphere, what is surprisingly missing in the AR6 report, http://dx.doi.org/10.1155/2013/503727 concludes to a radiative forcing of 2.6 W/m2 at doubled CO2 concentration which is 1.1W/m2 smaller than that of Myhre et al (1998). This expert reviewer recommends that this work together with subsequent works by the same author, H. Harde, that report a climate sensitivity of 0.7°C should be cited and discussed. [François GERVAIS, France]	Rejected: Understanding of the radiative transfer calculations have uncertainties far less than a factor of 2. Evidence for this is provided in the chapter text. The Harde study referred to by the reviewer does not include sufficient atmospheric processes compared to the sophisticated radiative transfer models discussed in the chapter text.
33422	5	47	6	13	Total ERF, GHG ERF, and aerosol ERF are all very useful metrics. I think that one additional metric would be an important complement here, namely the estimated continued radiative imbalance in the atmosphere. I believe Medhaug et al. (2017) (Reconciling controversies about the “global warming hiatus”) assessed this imbalance to be between 0.75 and 0.93 W/m2, but I don't know if there is a superior or more recent analysis of this quantity. It looks like in the present chapter there is an estimate of 0.71 +- 0.1 W/m2 from 7.2.2, so maybe that could rise to the level of a key finding? [Marcus Sarofim, United States of America]	Taken into account. Thank you for the suggestion. In SOD we have cited estimates of Earth's energy imbalance (with uncertainties) for two periods: (1) the first would be 1971-present, based on the assessment presented in cross-chapter box 9.2 on the combined Energy + sea level budgets; (2) a more recent period based on Argo from about 2006-present.
25732	5	48	5	48	This is an 8% increase over AR5 estimates for 1750-2011 in the best estimate value. [Stephen E Schwartz, United States of America]	Noted. This ES bullet has been reworded completely in the FGD.
18982	5	48	5	50	Some may be confused about what comprises “total anthropogenic ERF”. The second sentence specifies greenhouse gases, but upon first reading, I though the “total anthropogenic” was greenhouse gases, and so the 8% v. 14% increase was confusing. Maybe add some statement about the total anthropogenic ERF includes both aerosols, greenhouse gases, etc, before stating their quantities would clarify. [Gwenaelle GREMION, Canada]	Taken into account. We have added a clarification of what forcing agents contribute to the total anthropogenic ERF

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19066	5	48	5	50	If there is an 14% increase and a 22% more negative, how can be an 8% increase? Is this counterintuitive? [Gwenaelle GREMION, Canada]	Taken into account. No, this is not counterintuitive, the GHG forcing is larger in magnitude that the aerosol forcing, so a relative change in GHG forcing translates to a larger absolute forcing change that for aerosol forcing. We have added the absolute changes to avoid confusion.
13368	5	49	5	49	Why would the increase in forcing efficiency increase the ERF? It should only increase the response in temperature. [Govindasamy Bala, India]	Rejected. No, forcing efficiency refers to forcing per unit change of the relevant constituent, so highly relevant for ERF.
19304	5	50	5	50	The stated 14% 22% increase in ERF should be emphasized that they are increases in the estimated ERFs, which means that it is a better understanding of their ERFs, rather than just a change in the CO2 or aerosol concentration [Gwenaelle GREMION, Canada]	Rejected. We believe the text already states that the new estimates result from a combination of increased GHG concentrations and increased knowledge. The change in ERF estimates since AR5 is due to a combination of further changes in atmospheric GHG concentrations and revisions of their forcing efficiencies, e.g. better understanding.
44776	5	53	6	4	Provide ERF number for ozone as well [Astrid Kiendler-Scharr, Germany]	Accepted.
18984	5	54	5	54	Ozone “changes” --> ozone “decrease”. It’s also unclear what the difference between well-mixed greenhouse gases and not well-mixed greenhouse gases are at this point. [Gwenaelle GREMION, Canada]	Rejected - tropospheric ozone increases are the main influence on ERF, while stratospheric depletion is expected to contribute much less (+0.37 vs. -0.08 Wm-2). Both effects are accounted for here and "changes" is therefore the correct term to use.
13370	6	1	6	1	Can the actual value of ERF due to CO2 increase be stated here? [Govindasamy Bala, India]	Taken into account. Yes, certainly. Now added.
51660	6	2	6	4	Is there anyway you could put this more in layman's terms, i.e: 'negative rapid adjustment' and what actually means? [Lindsey Cook, Germany]	Taken into account. Removed the word "rapid" to make it more accessible.
18926	6	2	6	5	I assuime “total ERF” refers to the aerosol contribution to ERF. If so, it should be mentioned. [Gwenaelle GREMION, Canada]	Not applicable. There is no mention of "total ERF" on the line numbers cited, so there appears to be a confusion of page/line numbers here
18986	6	4	6	4	What is an upwards revision? [Gwenaelle GREMION, Canada]	Rejected. We believe the meaning of an upwards revision is well known.
9168	6	6	6	13	The fact that the ERF from aerosols has changed 100% since AR5 underlines the continuing degree of uncertainty in climate models. [Jim O'Brien, Ireland]	Rejected. No, that is an oversimplification. The estimate from climate models has not changed much between the two reports. What is different is mainly the method of assessment (i.e. what lines of evidence are given more weight, etc.). This is all explained in detail in Section 7.3.3.4, which should make it easy to trace what has changed since AR5.
19238	6	6	7	13	The volcanic radiative forcing were not mentioned in this summary, and also not well discussed in section 7.3.4.5. After IPCC AR5, studies, such as Santer et al. (2014, Nature Geosciences) and Solomons et al. (2011, Science), have augured the missing volcanic radiative forcing in CMIP5 is responsible to the warming hiatus in the last decade. Their findings are on debating. I suggest the leading authors should do a comprehensive assessment on the impact of volcanic radiative forcing on short-term climate change. [Gwenaelle GREMION, Canada]	Noted. The so-called hiatus and its plausible causes is addressed in cross-chapter box 3.1, including the potential contribution from volcanoes
53676	6	7	6	7	80% of the total ERF for all aerosols, I guess? Make that clear? [Jan Fuglestvedt, Norway]	Taken into account. Yes, this has been clarified, thanks.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
18928	6	7	6	7	I assuime “total ERF” refers to the aerosol contribution to ERF. If so, it should be mentioned. [Gwenaelle GREMION, Canada]	Accepted. Text altered
18988	6	7	6	13	“total ERF” or "total aerosol ERF"? [Gwenaelle GREMION, Canada]	Taken into account. Text edited to include "aerosol" for clarity.
25734	6	9	6	9	but --> and [Stephen E Schwartz, United States of America]	Accepted
45554	6	11			Please be clear whether this estimated forcing uses, or does not use, the historical warming record as part of the evidence. From the later text it appears it does, but as far as I remembver, in AR5 it did not. If you use this, then the estimated range is not independent of the warming, and cannot be used to constrain climate sensitivity (since the climate sensitivity was already implicitly used to constrain the forcing). If inverse estimates are an important source of information, you should probably give two likely ranges: one based only on process understnding and another that takes on board the past climate change. The main reason we care about the aerosol forcing is due to its implications for ECS, and for this, the former range would have to be used. The latter range would be useful only for projecting future aerosol forcing reductions if the atmosphere is cleaned up again. [Steven Sherwood, Australia]	Taken into account. This is now clarified and an independent estimate is made.
18930	6	12	6	13	Since the aerosols have a negative contribution to ERF, these revisions (100% up and 50% downward) do they imply a warming and cooling effect compared to the previous estimations? [Gwenaelle GREMION, Canada]	Noted. Yes, relative to the AR5 estimates, that is correct, but the estimates themselves show cooling contributions from aerosols both for cloud-aerosol interactions and from aerosol-radiation interaction, both in AR5 and AR6. The text has been rephrased for clarity.
13372	6	14	6	14	What is the forcing from the biophysical land cover changes? A ES statement would be good [Govindasamy Bala, India]	Taken into account. Yes, this has been added to the bullet on the total anthropogenic ERF.
18932	6	15	6	22	What is the cloud contribution to the ERF? If the range is known it should be laso mentioned, as it is done for the GHG and aerosols in the previous paragraphs. [Gwenaelle GREMION, Canada]	Rejected - Anthropogenic aerosols and GHG affect the radiation budget. Clouds on their own is a natural phenomenon and does not induce an ERF. Cloud changes due to aerosols is treated.
51496	6	24	6	26	The dominant source of uncertainty notion relating to cloud feedback and aerosol ERF "under scenarios" is kind of diminishing the dominant uncertainty in GHG and methaneemissions. I can see it is implicit in the term under scenarios, but should still be mentioned as dominant uncertainty. I wonder if there is a sentence in addition needed on eg methane feedbacks, and sceanario uncertainties. [Michael Schulz, Norway]	Taken into account. The intention here is to make statements about sources of uncertainty for a given scenario, we have reworded the bullet to clarify this. We consider the methane feedback to be included in the carbon cycle feedbacks that are acknowledged as an important source of uncertainty in the last sentence of the bullet.
18504	6	24	6	26	the "dominant" should be quantified [Gwenaelle GREMION, Canada]	Taken into account. "dominant" has been replaced by "largest"
13198	6	24	6	26	This seems to contradict the previous key point (lines 15-22), which states that we have much better constraints on our cloud feedbacks. I would suggest either modifying the wording of the last key point or changing the wording of lines 24-26 by being more specific about what the current uncertainities for cloud feedbacks in "increasing and stable emission scenarios" . [Nora Richter, United States of America]	Taken into account. There is no contradiction. The uncertainty associated with cloud feedbacks was very large in the past, and remains large even if it has now been considerably reduced. This is still the most uncertain of the climate feedbacks. We have added a sentence to clarify this.
18326	6	24	6	28	It is not clear the connection between the sentence in bold and the following sentence. The first talks about the cloud and the latter about ocean heat uptake and carbon cycle. The take home message is not clear. [Gwenaelle GREMION, Canada]	Taken into account. The overall bullet deals with sources of uncertainty in projections of future climate. We have reworded the bullet to make this clearer.
44778	6	30	6	40	Make more explicit statement that ECS depends on initial state. [Astrid Kiendler-Scharr, Germany]	Accepted.
51498	6	31	6	32	"than that inferred from historical records. ...reconciles disparate estimates of ECS...." - I think the paleo evidence for lower ECS is more important to reconcile with. Also - the "reconcile" sentence could be more explicit in that it could point to what is found in the next next paragraph, namely the lower end of the ECS has risen, [Michael Schulz, Norway]	Noted.
51662	6	42	6	42	Sorry, lost here - ECS and 3C - gained, lost, again, non-specialists may be lost as to what this is and what it means. Simpler language, less assumptions that we know what you are talking about? [Lindsey Cook, Germany]	Rejected, This is a summary and we cannot explain in full.

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27236	6	42	6	43	This claim ignores a list of not less than 85 peer-reviewed papers which reports climate sensitivity equal or lower than 1°C: https://notrickszone.com/2018/12/10/the-list-grows-now-85-scientific-papers-assert-co2-has-a-minuscule-effect-on-the-climate/ This expert reviewer recommends a discussion of why those findings are ignored in the AR6 report [François GERVAIS, France]	Taken into account. A blog-post would nominally be considered non-peer-reviewed literature, prone to obvious mistakes that would be caught in peer-review, and also this one contains a series of serious errors. Examples include studies that consider only part of the feedback, e.g. Planck feedback and CO2 forcing which naturally yields ECS ~ 1C as per our process-level assessment, some studies are really concerned with TCR, which is of course smaller than ECS, and some of the cited studies are either mis-interpreted or mis-represented, e.g. mistaking specific climate sensitivity with ECS which is a factor 4 smaller. A specific mis-representation is of Möller 1963 who found ECS ~ 10C when accounting for water vapour feedback, i.e. an order of magnitude higher than the claim of the blog. Generally, a number of studies conducted in the mid-20th Century, Möller 1963 included, apply a surface energy balance perspective. It is, however, well-known that this can lead to erroneous results, and instead the top-of-atmosphere energy
41608	6	42	6	43	ensure that ahead of SOD assessed range of ECS is well communicated in a timely manner to other chapters that use this information (e.g. chapter 4) [Amanda Maycock, United Kingdom (of Great Britain and Northern Ireland)]	Noted.
27314	6	42	6	48	Understanding of ECS remains poor. The best estimate of 3°C has not changed the last 40 years (Charney 1979) . Feedback of clouds and aerosols remain a key unknown. The estimated likely range change from 1,5-4,5 (AR5 with no best estimate given) to 2,5-4 °C is difficult to justify in view of literature with large variations for ECS . A more balanced literature summary for ECS could be 1,5-5 °C. This rather broad range for ECS estimates , and its consequences for establishing a maximum allowable "safe" temperature target as in the Paris Agreement, is not enough communicated to policymakers. Using TCR has an extra problem with the ocean initial conditions, and just like ECS measures the respons to CO2 forcing only (Knutti 2017) [ferdinand meeus, Belgium]	Rejected. An assessment range (likely or very likely) need not span all numbers of ECS found in the literature. Please also see the Synthesis section.
57856	6	42	6	54	ECS and TCR: Missing cross reference with Chapter 1? [Catia Domingues, Australia]	Noted. Same as #57854
9170	6	42	6	54	It is surprising that the IPCC estimation of ECS and TCR has not significantly tightened since the Charney estimate of 1979, despite $40bn having been spent on climate research over the last 40 years; other researchers have come to significantly lower figures, not yet reflected in IPCC analysis, as in comment 5 above. [Jim O'Brien, Ireland]	Noted.
19240	6	42	7	49	The likely range of ECS between 2 to 5℃ should be updated, several CMIP6 models, such as NCAR CESM and UK UKESM show ECS higher than 5℃, but the results have not been included in Table 7.13 and this Chapter. The likelihood of high climate sensitivity is very important for political decisions in avoiding, or adapting to climate change. The lead authors should do a thorough and careful assessment of these results. CMIP3 and CMIP5 models show ECS lower than 4.5℃. Why the ECS in CMIP6 model is much higher than CMIP5 and CMIP3 models? [Gwenaelle GREMION, Canada]	Taken into account. We do not rely on CMIP6 models for our ECS range, text now clarified
58028	6	46		47	The statement that there is 'a high level of agreement among the different lines of evidence' seems to gloss over the fact that several CMIP6 models have ECS outside the very likely ECS range assessed here. The authors argue correctly in the text that if one piece of evidence limits the upper end of the very likely range, then this should be enough to constrain the overall assessed range. But they should add more expalantion on this to this ES bullet, since readers will be interested in the consistency or otherwise between CMIP6 models, and the range on ECS assessed here. [Nathan Gillett, Canada]	Taken into account. In response to this and other comments, raw climate model ECS has been removed as a line of evidence.
18328	6	48	6	49	A reference to a figure or table that illustrates the notable asymmetry would be helpful here in the reference brackets. Maybe Table 7.15 [Gwenaelle GREMION, Canada]	Taken into account. Added, thanks for the suggestion.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
27238	6	51	6	53	Again this claim ignores a list of not less than 85 peer-reviewed papers which reports climate sensitivity equal or lower than 1°C: https://notrickszone.com/2018/12/10/the-list-grows-now-85-scientific-papers-assert-co2-has-a-minuscule-effect-on-the-climate/. Among the authors are prominent climate scientist like S. Schneider or R. Lindzen, the works of which cannot be ignored. This expert reviewer recommends a discussion of why those findings are ignored in the AR6 report. [François GERVAIS, France]	Noted, see also reply to comment above which deals generally with the blog post. Here two specific studies are referred to which we handle in more detail. The study be Rasool & Schneider 1971 which estimated ECS=0.8 K based on a 1D climate model was later corrected by Schneider 1975 to 1.5-3 K – in the absence of cloud and surface albedo feedbacks – which is fully in line with the AR6 process-level assessment. Among other things, the earlier study neglected the well-known stratospheric adjustment as well as solar IR absorption by H2O and CO2. The study by Lindzen et al. 2001 constructed a two-zone model wherein tropical feedback was calibrated using natural variations in the tropical top-of-atmosphere energy budget which is found to be most negative when lagging tropical surface temperatures. Feedback in the extra-tropics was assumed to be that of the Planck feedback. By ignoring positive feedbacks from water vapour, clouds and surface albedo the study obtains low-biased estimate of ECS. Moreover, Mauritsen and
19246	7	1	15	1	The author information of the citation of "Loeb et al.,, 2018a" is wrong here. [Gwenaelle GREMION, Canada]	Rejected. Unclear what is wrong here.
13200	7	2	7	7	What are the major uncertainties of polar amplification? [Nora Richter, United States of America]	This text has been revised to discuss mechanisms.
40718	7	2	7	13	You could combine these two summary points about polar amplification. [Daniel Murphy, United States of America]	Accepted.
58030	7	2		13	Note that polar amplification is also assessed in depth in 4.5.1.1. I suggest cross-referencing and avoiding duplication. [Nathan Gillett, Canada]	Taken into account. The polar amplification section has been shortened and cites Section 4.5.1.1 where relevant.
13374	7	3	7	3	Change "poles" to "polar regions". There is no ocean at the south pole. [Govindasamy Bala, India]	Accepted.
18330	7	4	7	5	"a variety of factors" is too vague, which factors contribute to the Arctic amplification? For the reader an example would be nice. [Gwenaelle GREMION, Canada]	Taken into account. This text has been revised to discuss mechanisms.
13202	7	4	7	7	Consider briefly listing/summarizing what factors contribute to Arctic amplification that are not observed in the Antarctic. [Nora Richter, United States of America]	Taken into account. This text has been revised to discuss mechanisms.
19068	7	6	7	7	might consider to highlight (bold) this lines that are "high confidence" instead of lines 2-3 [Gwenaelle GREMION, Canada]	Not applicable. This text has been revised.
27042	7	7	16	17	Pronounced changes were ....termed "brightening" Reference needed for this statement [Mansour Almazroui, Saudi Arabia]	Taken into account. Agree reference added
41612	7	9	7	9	can you quality "eventual surface warming" e.g. by a time frame or at equilibrium [Amanda Maycock, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The polar amplification summary points have been merged and the text revised to note the timescale.
15020	7	9	7	13	Polar amplification is an artifact of expressing the sensitivity as a linearized temperature change, rather than as a surface emissions sensitivity expressed as the demonstrably linear ratio of the change in radiant surface emissions per W/m^2 of forcing. One W/m^2 at the poles will have a larger effect on the temperature than it would at the equator owing to the T^4 dependence between temperature and W/m^2. [George White, United States of America]	Noted. The impact of the T^4 dependence of outgoing longwave radiation (the Planck response) on polar amplification has been quantified in Pithan & Mauritsen (2014) and is included in the assessment of mechanisms in Section 7.4.4.1.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
15022	7	9	7	13	The results of repeatable tests on ISCCP satellite data supporting the relative linearity between forcing and surface emissions as well as the unavoidable T^4 relationship are shown here: http://www.palisad.com/co2/sens/se/po.png http://www.palisad.com/co2/tp/fig1.png The first is a scatter plot of emissions at TOA vs. emissions by the surface as a consequence of its reported temperature. Each little dot represents the average relationship for 1 month of data for each 2.5 degree slice of latitude from pole to pole. The larger dots are the per slice averages over the entire data set. The average linearity shown by this data is undeniable. Many similar plots testing the linearity of different climate variables to each other can be found here: http://www.palisad.com/co2/sens The second scatter plot is of the surface temperature vs. the emissions at TOA. The green line is the prediction based on a gray body whose emissivity is 0.62, the slope of which is the sensitivity factor of about 0.3C per W/m^2 corresponding to an alpha of 3.3 W/m^2 per degree K. The blue line illustrates how the sensitivity factor presumed in this report aligns with the actual data when compared at scale. [George White, United States of America]	Noted. The page/line numbers the reviewer is referring to addresses polar amplification. The ISCCP satellite data has nothing to do with polar amplification. Polar amplification is largely associated with feedbacks associated with the loss of sea ice and snow.
18332	7	10	7	10	"polar amplification" should likely be changed to "Arctic amplification" in this instance to distinguish between polar (which could be arctic or antarctic) and Antarctic amplification. [Gwenaelle GREMION, Canada]	Rejected: "polar" is used here as an encompassing term and is not incorrect as Antarctic amplification is an identified phenomenon.
47572	7	15	7	18	It is unclear what "ambiguous" indicates (line 15). I do not understand "uncertainty in future warming" (line 17) in the text. As I comment in Section 7.7, there are two recent studies related to these statements (Tanaka and O'Neill, 2018, Nature Climate Change, doi:10.1038/s41558-018-0097-x; Fuglestvedt et al., 2018, Phil. Trans. R. Soc. A., doi:10.1098/rsta.2016.0445), although these studies are not discussed in the current draft. The Tanaka paper shows that, if GWP100 is used to implement the net zero GHG emission target by 2060, it leads to declining temperatures below 2C. If GTP100 is used instead, it gives almost stabilized temperature at 2C. If GWP20 is used, it is not possible to reach net zero because the residual CH4 emissions are weighed too high to be compensated by negative CO2 emissions that are assumed possible in the model. Could the outcome of these studies be also considered in this statement? [Katsumasa Tanaka, Japan]	Not applicable. We have revised this text. We now make the point that explicitly treating GHG differently allows a better (i.e. improved) quantification of the warming effects of a portfolio of gases, compared with the use of CO2-equivalent bundling. We expand on this by highlighting innovations in emissions metrics work.
57352	7	15	7	23	Good to see this said very clearly. Perhaps worth stating that the specific problem is aggregating cumulative and shortlived pollutants, not just general uncertainty in metrics (and this ambiguity would be almost entirely resolved if countries aggregated cumulative pollutants and SLCFs separately). [Myles Allen, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We have revised this text, because the old text probably had too much material at a level beyond that considered appropriate in ES statements. We retain the point that separate treatment of gases allows better quantification of the warming effects of different GHG, and we retain the point that innovation in emissions metrics has created approaches which do a better job than CO2-equivalence.
57354	7	15	7	23	Could we be more positive and say that cumulative CO2-equivalent emissions of cumulative pollutants (all those with lifetimes longer than the 100-year GWP time-horizon) over a multi-decade time-period, Ctot, can be combined with cumulative emissions of short-lived forcers Stot, also expressed at CO2-e using GWP100, and the change in SLCF agregate CO2-e emission rate ΔS, like this: Ctot + alpha Stot + (1-alpha) x H x ΔS where H is the GWP time-horizon (100 years). The value of alpha depends on how fast radiative forcing needs to decline to maintain stable tempertures: following a 1% ramp-up this is given by (ECS-TCR)/(d2 x TCR), where d2 is the slow thermal adjustment time, or about 0.3%/year. So alpha/(1-alpha)=0.003 x H and alpha=0.25. Despite being such a simple equation, this converts cumulative emissions and SLCFs into CO2-forcing-equivalent emissions with quite reasonable precision, and you can just multiply the result by the TCRE to get future warming. [Myles Allen, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We have revised this text and opted for a more high level statement. We will discuss the equation in LAM4.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
33418	7	15	7	23	A couple of comments on this key finding: first: I believe this key finding reads too negatively regarding the value of GWPs. GWPs are certainly not going to be provide a perfect measure of net warming at any given point in time, and in my opinion, that's not their purpose. In my opinion, the purpose of a GWP is to provide an approximate equivalence in terms of climate impact, recgonizing that some gases will have greater near-term impact and some greater long-term impact, and, in my opinion, the 100 year GWP does a reasonable job in that regard (see, e.g., Sarofim and Giordano 2018, Reisinger et al. 2013, Smith et al. 2013). On the other hand, the GWP* is in fact a much better measure of temperature equivalence at any time scale, but is not going to be suitable for many of the purposes for which GWPs are used (e.g., comparing relative impact of emissions pulses, or looking at single year inventories). I also dislike the use of the word "should" for the concept of using multiple baskets: "could" would be better. As the IPCC has been careful to note in previous assessments, there is no absolute scale on which metrics can be judged, and policymakers may have different purposes. The IPCC can assess the implications of different metrics in different usage modes, but categorical "shoulds" and "appropriates" should be used with care. [Marcus Sarofim, United States of America]	Taken into account. We have revised this text. We now make the point that explicitly treating GHG differently allows a better (i.e. improved) quantification of the warming effects of a portfolio of gases, compared with the use of CO2-equivalent bundling. That is unambiguously correct, and policy-relevant. We then point to the emissions metric work in section 7.6.
56664	7	15			IPCC fails on its mandate to provide policy relevant information, if it provides such kinds of executive summary that basically say to move from GWP to GWP* or MGTP. While yes, GWP* or MGTP gives better comparability in terms of specific targets and settings, it does so in a completely unrelevant setting for policy makers. The basic premise of "permanent step changes of emissions", which is assumed by the GWP*/MGTP star concept as the promise against which governments get rewarded or penalized, is as far removed from reality as a net-zero global target by 2020. Thus, if the IPCC now touts GWP* as the newly favourite metric value, it fails to take into account the actual real-world setting in which climate policies are done (and for which metric comparison values are needed). As discussed with the proponents at multiple occasions, GWP* also has the opposite practical effects. Just as an example, GWP would lead to an equivalent value of, say, 3000 for comparing a step change of CH4 emissions to a pulse emission of CO2. Thus, in the next accounting period until an NDC, countries would solely focus on CH4 (as they get hugely rewarded under the GWP* concept). In contrast, whenever methane emissions would increase, countries would likely be catapulted out of compliance with their NDC. Under a GWP approach, methane would only have a value around 30 for the next 5-10 years, similary (+-) for GTP with a time horizon that is roughly in line with 1.5C or 2.0C peak warming... Anyway, if IPCC were to propose GWP* in the manner that this paragraph does, it would certaintly create a lot of debate (and it would be very unhelpful from my point of view). GWP* can be mentioned as an option next to GWPs and GTPs, but only if its merits and drawbacks are discussed by people other than the small circle of very vocal proponents. [Malte Meinshausen, Australia]	Taken into account. See revised text. We do not make a recommendation regarding policy. We point out that explicitly treating GHG differently allows a better (i.e. improved) quantification of the warming effects of a portfolio of gases, compared with the use of CO2-equivalent bundling. This is a science point. We expand on it by highlighting innovations in emissions metrics work.
30296	7	16	7	20	This statement is unbalanced, and in part unsupported by the evidence in the chapter. By trying to make a too general statement I think it unfortunately is also misleading or wrong. It should be reworded to reflect the implicit assumptions and value judgments made by the authors. For example: The first sentence is unnecessariy singling out GWPs for a deficiency that is equally present in other metrics. The chapter text is correct here in noting that "In effect, metrics are used as an imperfect summary of the “exchange rates” between different forcing species." This is true for any metric and should be appropriately communicated. However, some metrics are better for specific uses than other. The current statement doesn't reflect the fact that uncertainty in climate outcome is simply transfered from an ambiguity in the instantaneous trend of warming (rates of temperature decline can vary when assuming net zero GWP) to a larger uncertainty in the absolute level of warming and the trend over longer timescales when using the proposed solution of GWP*. Just to clarify: Imagine a country setting an NDC target in GWP* for 2030 or 2050, the uncertainty in the amount of warming this would imply would be very large, as a one-off decline in short-lived forcers relative to the preceding year can allow a country to reach virtually any level of GWP* in a specific year. I provide a suggestion for more balanced wording in a separate comment. The use of the word "appropriate" is policy prescriptive as the authors make a clear value judgment of what is appropriate without making the criteria on which their judgment is based explicit. [Joeri Rogelj, Austria]	Taken into account. We have revised this text. We now make the simple and obvious point that explicitly treating GHG differently allows a better quantification of the warming effects of a portfolio of gases, compared with the use of CO2-equivalent bundling. We then point to the emissions metric work in section 7.6.
30298	7	16	7	20	See other comments on this topic for the reasons why this statement is unbalanced and unsupported. A corrected wording for this statement could be (just a suggestion, with html flags): Aggregating short and long-lived greenhouse gases with warming metrics into CO2 equivalent emissions always results in a certain degree of ambiguity in the resulting surface warming. The uncertainty in future warming resulting from targets expressed in CO2 equivalence can be reduced by considering short and long-lived greenhouse gases separately. New metrics comparing pulse emissions of long-lived greenhouse gases with sustained emission changes in short lived gases show a closer proportionality between cumulative CO2 equivalent emissions and surface warming but result in large uncertainties when used for emissions targets in a single year. Annual CO2 equivalent emission benchmarks using Global Warming Potentials (GWPs) result in less potential variation in the total surface warming. Long-lived greenhouse gas emission metrics are larger compared to AR5, due to the methodological change of accounting for carbon-cycle responses becoming standard. [high confidence] [7.7.1, 7.7.2] [Joeri Rogelj, Austria]	Taken into account. We have revised this text. We only have one statement on metrics now.
30300	7	16	7	20	The reference to NDCs is not supported by any evidence in the underlying chapter. Please provide evidence or remove this statement. (see suggested rewording for this para in separate comment) [Joeri Rogelj, Austria]	Taken into account. We have revised the text and do not refer to NDCs.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
47574	7	20	7	21	I do not think this statement reflects the current state of the debate. Many papers suggest that multiple metrics should be used complementary for the same basket of forcers, without separating the forcers into multiple baskets. Here are some examples: Cherubini et al. (2016, Environmental Science & Policy, doi:10.1016/j.envsci.2016.06.019); Levasseur et al. (2016, Ecol. Indicators, doi:10.1016/j.ecolind.2016.06.049); Ocko et al. (2017, Science, doi:10.1126/science.aaj2350); Tanaka et al. (2019, Nature Climate Change doi:10.1038/s41558-019-0457-1). [Katsumasa Tanaka, Japan]	Taken into account. We have revised the text and make a high level statement about the limitations of CO2-equivalence, along with a pointer to the emissions metrics work in 7.6.
53678	7	21	7	21	You may add "...and not aggregated to a common unit" after "separately" [Jan Fuglestvedt, Norway]	Not applicable. We have revised this text and this text has been deleted.
33420	7	21	7	23	I would note 2 issues in this last sentence: the first is that they key advancement is the _consistent_ accounting for carbon-cycle responses for both CO2 and non-CO2 gases - based on Gasser et al., the exclusion of carbon-cycle responses in both leads to very similar results (at least for GWP100) as does inclusion in both. The second note is that AR5 _did_ assess the consistent use of these responses in an alternate set of metrics, so I would note that: e.g., "Self-consistent inclusion of carbon-cycle responses for both carbon dioxide and non-CO2 GHGs has become standard, and as a result, metrics reported here are closer to the GHG emission metrics that AR5 presented using a similar methodology. These metrics are larger than the metrics calculated based on including carbon-cycle responses for only CO2". \ [Marcus Sarofim, United States of America]	Taken into account. We have revised this text. The new text aims for a high level treatment of why CO2-equivalence may not be a desirable choice if warming is the variable under consideration, followed by a pointer to the relevant section in the chapter. We briefly mention the carbon cycle in the new ES statement - more is said in section 7.6.
27040	7	25	11	25	This Figure 7.3 (left) has already been published in AR5 Chapter 2: Observations Atmosphere and Surface, page 181 [Mansour Almazroui, Saudi Arabia]	Taken into account. We agree, this shows now an update with latest data, reference now added
50840	7	26	7	26	Why is the focus on AR5 only? Information in the chapters' topic have been also assessed in the AR6 special reports. Shlpoudn't they be incliuded here as well? [Karina von Schuckmann, France]	Taken into account. We agree and this is now included
18334	7	28	8	41	Motivating why it is useful scientifically significant to accurately map the energy budget might improve the readability of this chapter for non-scientists. The second paragraph (7-7, line 35) explains that perturbations in the energy budget cause global warming or cooling, but that does not itself establish that it is important scientifically to be able to attribute the imbalance/warming to a specific feature of the budget. The rest of that paragraph lists the connected concepts, but not the importance of this line of study. Is it that closure of energy budgets confirms the quality of our understanding (models) of the climate? Is it that it facilitates estimates of climate impacts via the climate sensitivity? The carbon budget implied by temperature limits? That it gives another mode of confirmation that climate change is occuring? Making these sorts of framings apparent could improve the effective information content of the rest of the chapter. [Gwenaelle GREMION, Canada]	Taken into account. A really good idea - we have revised the text - thank you
18336	7	28	8	41	This section implicitly takes the approach that energy imbalances are the culprit in climate change, which is potentially conceptually misleading. For example the phrase "climate change arises when the Earth's top of atmosphere energy budget is perturbed" gives a different emphasis from "when the Earth's top of atmosphere energy budget is perturbed, the climate changes". This may seem trivial, but it could be important to how a non-expert reads this chapter, particularly with respect to solar radiation management. The climate changes when the local details of how energy flows through the Earth system changes, which is often manifest in a TOA globally-averaged alteration, but need not necessarily be. The climate can be changed while TOA energy budgets are balanced, which is crucial to understanding the challenges/risks of SRM. [Gwenaelle GREMION, Canada]	Taken into account. Agree, and combined with the above and rewritten with your framing
57858	7	35	7	36	Should specificy timescale? To separate energy imbalance due to climate change from short term variability? [Catia Domingues, Australia]	Agree. Timescale added
19242	7	39	13	44	The authors have carefully assessed CMIP5 model on simulating atmospheric cross equatorial heat transport here. What about CMIP6 models? Are CMIP6 models better or worse? [Gwenaelle GREMION, Canada]	Not applicable. Text has been deleted to avoid excessive chapter length.
6245	7	42	7	42	General: energy consumption pattern also should be considered.(Ref. Jafari, M., Smith, P., (2018). Climate Change as a Driving Force on Urban Energy Consumption Patterns. In Encyclopedia of Information Science and Technology (4th ed., pp. 7815-7830). IGI Global. https://doi.org/10.4018/978-1-5225-2255-3.ch680 [Mostafa Jafari, Iran]	Rejected. This reference is not relevant here
53680	7	43	7	48	Not sure if this is needed. Delete? [Jan Fuglestvedt, Norway]	Accepted.
18338	7	50	7	52	"[total earth system warming] is a masure of global warming that provides a more robust indication of Earth's energy imabalance than globally averaged surface teperaure". Is it worth explaining that total earth system warming is the time-integral of the TOA imbalance? This makes the nature of the robust relationship more clear, and the primacy of total earth system warming over global average temperature self-evident. On the other hand, it might also be worth noting that surface average temperature more directly feeds back into the TOA energy imbalance than the OHC does, which explains why it features in the subsequent definition of ECS. [Gwenaelle GREMION, Canada]	Taken into account. Agree, we have reconsidered this terminology and explained more carefully
19244	7	50	14	22	In this subsection, the authors have discussed the observed TOA flux from satellite data, atmospheric reanalyses and climate model. But how well are the CMIP6 or CMIP5 model in reproducing TOA flux as compared to satellite data or other observational data? I think this is an important question which should be further addressed here. [Gwenaelle GREMION, Canada]	Taken into account. Agree, model data is now discussed

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
6423	7		9		Box 7.1 uses the near-surface air temperature while section 7.1 introduces total earth system warming as a better metric for estimating climate warming. [Stephanie Fiedler, Germany]	Taken into account. These two metrics are clarified here
19248	7.37	18	7.37	23	The author lists of Loeb et al. (2018a) and Loeb et al. (2018b) are incorrect. [Gwenaelle GREMION, Canada]	Taken into account. Thank you for pointing this out. The error in Loeb et al. (2018b) was corrected. Cannot find an error in the 2018a reference.
18492	8	2	8	5	This sentence is confusing and needs more clarity. [Gwenaelle GREMION, Canada]	Taken into account. Rewritten the sentence.
18340	8	3	8	5	"so that climate sensitivity has become…" - this wording in this sentence seems too colloquial, and also a bit unclear. The phrase "climate sensitivity" is not very explicitly defined in this section and can be easily confused with ECS, so contrasting the two might not make immediate sense to a reader. [Gwenaelle GREMION, Canada]	Taken into account. Agree, reworded
9302	8	4	8	4	While ECS is defined unambiguously in box 7.1 page 10 lines 27-28, what is meant by climate sensitivity? Logically that would be a value of Delta_T out of the equilibrium situation. Should then some other condition be specified?. This may not be easy; on page 84 line 31 climate sensitivity is estimated assuming equilibrium! However on p85 line 13 the text clearly discusses situations departing from equilibrium; reference is unfortunately made to the (non existent) denominator of equation 7.1. Later on, although the text stipulates "climate sensitivity", one may sometimes wonder whether actually it might not mean "equilibrium climate sensitivity"; see for example p88 lines 13-22. Please clarify. [philippe waldteufel, France]	Taken into account. We now introduce 'effective climate sensitivity' in the introduction. We have also added a conceptual figure with curvature to illustrate many of the points we are making.
41614	8	4	8	6	"which isn't immediately relatable to the ECS" do you mean the traditional concept of a single value for ECS? The same issue of time-dependent feedbacks also affects ECS estimates, as you state in the next sentence. [Amanda Maycock, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We now introduce 'effective climate sensitivity' in the introduction. We have also added a conceptual figure with curvature to illustrate many of the points we are making.
58034	8	4		5	It is not clear 'which isn't immediately relatable to the ECS' means. [Nathan Gillett, Canada]	Taken into account. Agree, reworded
58032	8	4			Replace 'has become' with 'is now known to be'. ECS itself has not changed since AR5, only our understanding of it. [Nathan Gillett, Canada]	Taken into account. Agree, reworded as suggested
40720	8	5	8	5	Maybe use "conceptual update" in place of "change" to clarify that the estimates haven't changed so much as the way terms like "transient" and "feedback" are defined. [Daniel Murphy, United States of America]	Taken into account. We now introduce 'effective climate sensitivity' in the introduction. We have also added a conceptual figure with curvature to illustrate many of the points we are making.
53682	8	9	8	9	Good that you point to WGIII here. Need to connect to authors there. [Jan Fuglestvedt, Norway]	Noted. Thank you
53684	8	27	8	42	Very useful to have this clarification of uses of the word "metric" [Jan Fuglestvedt, Norway]	Noted. Thank you
53686	8	31	8	33	As far as I can see, nothing on emission metrics in the figure, even if it says so here in the text [Jan Fuglestvedt, Norway]	Taken into account. The figure has been remade and now does
27098	8	33	8	36	The two sentences covering these two lines should be one single sentence; i.e. The climate metrics used in this report typically evaluate how the Earth system response varies with atmospheric concentration or radiative forcing change, whereas emission metrics evaluate how radiative forcing or climate is affected by the emissions of a certain amount of gas [Chris Satow, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. While not combining to one sentence, the text has been rephrased to make it easier to follow.
51500	8	37	8	38	"climate metrics are useful for adaptation decisions" - I think the distinction sought wrt to emisison metrics is misleading. ECS estimates do inform and rightly should inform policy decisions on mitigation in a direct way, not just adaptation decisions. [Michael Schulz, Norway]	Taken into account. Agree, text now reworded for clarity
9604	8	38	8	39	Perhaps instead of 'TCR to emissions' refer to it as TCRE, which would be consistent with terminology in Chapter 5. [Katarzyna (Kasia) Tokarska, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Agree, TCRE used
53688	8	38	8	39	You may write TCRE [Jan Fuglestvedt, Norway]	Taken into account. Agree, TCRE used
53690	8	38	8	39	The driver part in the figure is too short. I suggest you add more; like population technology etc. See WGIII [Jan Fuglestvedt, Norway]	Taken into account. Agree, figure improved
58036	8	38		39	As defined and used in Chapter 5, Transient Climate Response to Emissions is abbreviated 'TCRE' rather 'TCR to emissions'. [Nathan Gillett, Canada]	Taken into account. Agree, Chapter 5 TCRE adopted for consistency
13376	8	46	8	48	Figure 7.2: The long term earth sensitivity should also include deep ocean warming, in addition to ice sheet and veg changes [Govindasamy Bala, India]	Taken into account. The figure was updated for the second order draft.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
40722	9	1	10	54	This box is both very important and very frustrating. It is a very good idea to set out definitions of these terms in a box. But the box ends up as much too long and too technical. I'd strongly suggest you rethink the box as defining these terms to a wider audience – maybe a goal is that a non-specialist atmospheric chemist who is a little lost in much of the chapter can still understand the boxes. My experience is that few people who aren't deeply involved in climate work really understand what effective radiative forcing means. There is also a missing concept: that adjustments which don't depend on surface temperature tend to be specific to particular forcing agents and that is part of why it makes sense to fold them back into forcing. In all places please redefine acronyms inside the box (e.g. line 50 should read the" Transient Climate Response or TCS is defined as…"). Comment continues… [Daniel Murphy, United States of America]	Taken into account. We thank you for this comment and have thought about this hard. We have revised this carefully taking the spirit of your idea on board. Some discussion has moved to chapter 2 on surface temperature and some moved to the main section as suggested. However, a further schematic has been added to explain forcing and feedback and text on rapid adjustments added. We prefer one box over two. We have tried to be more explicit on definitions
40724	9	1	10	54	My suggestions: Consider breaking the box into two: one for one for feedbacks and sensitivity and one titled "What is the difference between instantaneous and effective radiative forcing?". Just breaking it up will make it more reader-friendly, although I recognize Equation 7.1 is common to both topics. For ERF, details of how to handle land/ocean and stratosphere (lines 35 to just page the page break) could be moved to the main text. The discussion of the difference between surface and near-surface temperature (7-9 line 43 ff) is terrific, but maybe doesn't belong in this box. For feedback parameter lines 19-20 could be moved to text. For ECS the "Previous IPCC reports …" could be deleted out of this box. I also question if these sentences are rigorously correct - previous IPCC reports have included some atmospheric chemistry changes. My suggestions for a more reader-friendly definition of ECR continues below…. [Daniel Murphy, United States of America]	Taken into account. We thank you for this comment and have thought about this hard. We have revised this carefully taking the spirit of your idea on board. Some discussion has moved to chapter 2 on surface temperature and some moved to the main section as suggested. However, a further schematic has been added to explain forcing and feedback and text on rapid adjustments added. We prefer one box over two. We have tried to be more explicit on definitions
40726	9	1	10	54	Suggested box: What is the difference between instantaneous and effective radiative forcing? Instantaneous radiative forcing, IRF, simply calculates the change in incoming and outgoing energy at the top of the atmosphere without considering the Earth's responses. The Earth system responds to a radiative perturbation, such as changing CO2, in many ways. It turns out that it makes sense to group responses into responses that depend on average surface temperature and those that don't. Because the ocean takes time to warm, surface temperature responses tend to be slow. They also tend not to depend on the forcing agent. For example, more water vapor in a warmer atmosphere [or pick another example] does not depend much on whether the cause of the warmth was CO2, CH4, or BC. These surface-temperature responses are feedbacks in Equation 7.1 [Daniel Murphy, United States of America]	Taken into account. We thank you for this comment and have thought about this hard. We have revised this carefully taking the spirit of your idea on board. Some discussion has moved to chapter 2 on surface temperature and some moved to the main section as suggested. However, a further schematic has been added to explain forcing and feedback and text on rapid adjustments added. We prefer one box over two. We have tried to be more explicit on definitions
40728	9	1	10	54	Suggested box continued: In contrast, responses that do not depend on surface temperature tend to be rapid and depend on the forcing agent. For example, the change in vertical temperature structure after an infrared forcing such as CO2 is imposed is different than it is for a shortwave forcing such as scattering aerosol [or pick your example]. Because these responses are different for every forcing agent, it makes sense to modify the forcing. Effective radiative forcing, ERF, includes these adjustments. Accounting for such processes.. {text and refs from 7-9 lines 26-28). Formally, ERF is calculated by keeping surface temperature constant. Details are in section 7.3.1. [along with a shortened version in the box]. My suggested text here is not a finished product, instead view it is an effort to communicate at a less specialist level WHY surface temperature is used to divide ERF from feedback. Again, although the chapter authors clearly understand, lots of readers won't. [Daniel Murphy, United States of America]	Taken into account. We thank you for this comment and have thought about this hard. We have revised this carefully taking the spirit of your idea on board. Some discussion has moved to chapter 2 on surface temperature and some moved to the main section as suggested. However, a further schematic has been added to explain forcing and feedback and text on rapid adjustments added. We prefer one box over two. We have tried to be more explicit on definitions
53692	9	1	11	5	useful box [Jan Fuglestvedt, Norway]	Noted. thank you
55516	9	2	11	5	This box contains some really good information - keep as is. [Wesley Fraser, United Kingdom (of Great Britain and Northern Ireland)]	Noted. thank you
42034	9	9	20	41	I don't disagree with anything here, but I think perhaps a bit more could be indicated about the relative uncertainties in the rapid adjustments coupled to the fact that for many of them, we don't really have a solid conceptual view of what drives them (unlike, for example, stratospheric temperature adjustment). (actually, I see this point is addressed partially at 22:33, although that really addresses the methodogical difficulty rather than the fact that there is no robust theoretical basis for predicting these other rapid adjustments). [Keith Shine, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Agree, more has been added in Section 7.3.1 on this, which the box now refers to

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
56344	9	13	10	39	Comments on Box 7.1: Forcing, feedbacks and sensitivity definitions. [J. Ray Bates, Ireland]	Not applicable.
38588	9	16	9	16	Why use global mean near-surface air temperature when, as the 2018 IPCC SR1.5 Report states (page 1-12), "The IPCC has traditionally defined changes in observed GMST [global mean surface temperature] as a weighted average of near-surface air temperature (SAT) changes over land and sea surface temperature (SST) changes over the oceans"? Particularly when near-surface air temperature is less well measured than SST, is of less environmental relevance and, in models, is an approximately diagnosed variable not a prognostic grid level variable. [Nicholas Lewis, United Kingdom (of Great Britain and Northern Ireland)]	Noted. We choose to have both projections and historical simulations on a common metric and after careful consideration selected global surface air temperature. This is discussed extensively in Chapter 2 in the revised SOD
38590	9	16	9	16	The standard negative sign for the alpha deltaT term in Equation 7.1 is reversed here, so that the value of alpha is negative rather than, as in previous standard use (e.g. Forster et al 2013; Andrews et al 2012), positive. In AR5 lambda was used for the feedback parameter and it was defined as positive. Defining alpha to be positive is preferable as it conforms with previous usage and avoids confusion when writing about higher or lower, or stronger or weaker feedback. Also a more negative effect is a stronger effect, not a weaker effect: the usage of stronger and weaker throughout this chapter in relation to feedback is simply wrong. E.g, a correlation of -0.7 is stronger than a correlation of -0.3, not weaker, Strength/weakness is a measure of absolute value, not signed value. [Nicholas Lewis, United Kingdom (of Great Britain and Northern Ireland)]	Noted. We retain the sign conventions. Different conventions are used across the published literature. We are more careful on the wording choice accordingly
25736	9	18	9	18	In the eqn, I would recommend changing the sign of the last term; this would make alpha a positive quantity. One notes the widespread use of alpha as a positive quantity, as in Table 7.13. At the very least the report should be consistent in definition and use. [Stephen E Schwartz, United States of America]	Taken into account. We agree and have switched our sign conventions as suggested and improved our wording to make it more consistent
15024	9	18	9	18	By assuming that W/m^2 of forcing are linear to temperature, equation 7.1 has no relationship to the laws of physics. The only relevant relationship between W/m^2 and temperature is the Stefan-Boltzmann Law, where W/m^2 are proportional to the temperature in degrees Kelvin raised to the fourth power. The T^4 relationship is immutable and independent of the spectral composition of the emissions or the effective emissivity of the radiating body. The Stefan-Boltzmann Law precisely quantifies the emissions from matter consequential to its temperature, including non ideal systems like the Earth. All deviations from ideal are exactly quantifiable by applying a dimensionless emissivity between 0 and 1. A non unit emissivity does not affect the T^4 dependence of a body radiating energy and there's no known physics than can. The underlying assumption for equation 7.1 is approximate linearity over a narrow range of average temperature, however; this is wildly insufficient for applying linear feedback amplifier analysis to the climate system. A necessary condition is that the system must be linear over the entire range of inputs and outputs, not just around the average, which for solar forcing is from 0 W/m^2 a night up to 1300 W/m^2 at high noon on the equator and across all possible temperatures found across the planet. This serious error appeared in the first IPCC report, has never been corrected and is one of the primordial errors driving the climate science controversy. [George White, United States of America]	Noted. We do not assume the earth behaves linearly - in fact we show quite clearly that is does not. We rather frame the chapter as showing and discussing how good (or not) the linear model is. We go onto show non linearities in forcing 92xCO2 is different than 4xCO2) and discuss how effective climate sensitivity varies with time and state in great detail. We just introduce the linear model as one way of describing the Earth and then test it with far more complex models, observations and theory. We don't apply a "linear feedback amplifier" to the climate system, nor did the first IPCC report, so there is no error. Projections are always made with far more complex models. The linear framework is purely diagnostic.
15028	9	20	9	20	The deltaF term is said to arise from a perturbation, such as a change in solar input or a change in atmospheric absorption resulting from a change in CO2 concentrations. For an instantaneous change, as was the case for the original definition of forcing in AR1, these two are not equivalent on a Watt by Watt basis. All of any incremental solar input that's not reflected away heats the planet, while geometry dictates that about half of any incremental surface emissions absorbed by atmospheric GHG's and clouds must ultimately escape into space and only the remaining energy is available to heat the surface. Only the incremental absorption of surface emissions matters and any incremental re-absorption isn't adding new energy to the atmosphere and is just increasing the delay before the previously absorbed surface energy is ultimately emitted into space or returned to the surface. The definition of ERF states that the short term adjustments are accounted for, apparently in order to get around this Issue. However, based on HITRAN driven simulations of a standard atmosphere with average clouds, the 4 W/m^2 of equivalent ERF said to arise from doubling CO2 is the incremental absorption by the atmosphere upon instantly doubling CO2 and which doesn't account for the half that would eventually leave TOA within milliseconds to seconds and is definitely not equivalent to 1 W/m^2 of solar input after the atmosphere has adapted to any short term change. [George White, United States of America]	Noted. References cited in this an earlier IPCC report show that the radiative forcing concept at the top of atmosphere and not the surface provides the best measure for eventual surface temperature response. It is generally also used as a diagnostic, so in complex models solar and CO2 forcing behave very differently, but still lead to similar global surface temperature responses for an equivalent top of atmosphere forcing.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
15026	9	20	9	41	Bode considers the forcing to be the entire range of input to the system, which for Earth is all of the power arriving from the Sun. Best practices modeling considers a change to the system, for example changing CO2 concentrations, to be equivalent to a change in forcing keeping the system (CO2 concentrations) constant. Considering forcing to be an arbitrary change at TOA, rather than all of the solar input, incorrectly decouples the incremental effect from the average effect which must otherwise be the same since COE requires that all Joules are equivalent relative to the energy balance and that all Joules are equivalent with regard to the work done to heat the surface. [George White, United States of America]	Noted. The terminology adopted follows our equation 7.1
38592	9	28	9	29	This sentence is misleading. The most important and best known rapid adjustment, of the stratosphere, has a timescale of several months (and so is not complete for up to a year or so). [Nicholas Lewis, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Agree, this has been reworded for accuracy
18342	9	31	9	31	The phrase "not associated with" to describe the rapid adjustements with respect to surface temperature change might be confusing, since rapid and longer timescale adjustements are certainly "associated" with each other in some sense. Perhaps "not contingent on" or "not reliant on" might be more clear. [Gwenaelle GREMION, Canada]	Not applicable. Box 7.1 has been rewritten for the second order draft.
44780	9	32	9	34	Give more explanaition on "climatological values" - whic ones/implications? [Astrid Kiendler-Scharr, Germany]	Taken into account. Agree, extra detail added
38594	9	34	9	37	It is not valid to quantify ERF in fixed SST simulations by assuming land surface temperature changes to be small, since that assumption is obviously false. Not adjusting for these changes materially biases ERF estimation. On the other hand, fixing SSTs prevents accounting for rapid adjustments in SST patterns (with unchanged global SST), which arguably biases up fixed SST ERF estimates. Regression over year 2-10 or 2-20 of abrupt CO2 increase simulations could be suggested for estimating ERF as it does allow for SST pattern adjustments as well as for land warming, while avoiding bias from the incomplete rapid adjustments that affect year 1. [Nicholas Lewis, United Kingdom (of Great Britain and Northern Ireland)]	Noted. ERF can be defined in several ways in the literature, including fixed SST. There are pros and cons with each method. We have carefully considered these and added more details to the text. It is always imperfect, when trying to fit reality with a simple linear equation. The bias in the chosen approach is now discussed in more detail
18344	9	34	9	38	This sentence is difficult to follow. [Gwenaelle GREMION, Canada]	Not applicable. Box 7.1 has been rewritten for the second order draft.
55734	9	37	9	39	Given the well-documented change in feedback parameter (α), i.e., the slope of N vs. T, with time in the abrupt-4xCO2 simulations, a better assessment of the methods for calculating ECS from models, and the associated uncertainties, is needed. For instance, would using years 51-150 instead of 1-150 give a more accurate estimate of the true model ECS? [Larry Horowitz, United States of America]	Taken into account. This is now addressed in section 7.5
18346	9	39			"small forcing agent" - unclear what this is. [Gwenaelle GREMION, Canada]	Not applicable. Box 7.1 has been rewritten for the second order draft.
25738	9	43	9	43	Do you mean "warming" here in the sense defined at Chapter 7, page 5 line 7? maybe better "temperature increase" [Stephen E Schwartz, United States of America]	Taken into account. Agree, surface temperature increase used
36596	9	43	9	44	Don't you think that near-surface temperature may be strongly model-dependent (it depends of the PBL parameterization)? [Carlos Mechoso, United States of America]	Not applicable. This discussion has been moved to chapter 2 where it is covered in more detail
37798	9	47	48		This statement is too specific.As pointed out in several earlier comments, the FOD of Chapter 2 is far from error-free in this regard. Reanalyses provide observationally-based estimates of globally averaged trends for surface air temperature, and have been shown in the published literature to be fully competitive with the traditional gridding methods for trends over the past forty years at least, with competitiveness likely to be extended back to seventy years ago within a year, once ERA5 results are complete. To date it is the new versions of the traditional datasets that are coming closer to the reanalyses, not vice versa. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We agree and have clarified, including moving material to chapter 2
37800	9	48			"and reanalysis" could be added after "Modelling" if 40-year trends can be regarded as "historical". [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. Agree, but this section now refers to chapter 2 so text removed
38596	9	50	9	52	The 16% difference in Richardson et al 2016, 2018a appears to be greatly exaggerated in respect of the blending effect, quite possibly because the warming patterns and wind changes simulated by CMIP5 models have, for whatever reason, proved unrealistic. ECMWF homogenised ERA-interim reanalysis data, using model-diagnosed rather than analysed tas, showed only a 2% excess of tas warming over blended land tas & SST global temperature over 1979-2014. See analysis in, and evidence cited by, Lewis and Curry 2018 (section 7e). NB This paper has the two authors' names duplicated in the Ch. 7 reference list. Note also that the excess of the global mean increase from 2006-25 to 2080-99 in near-surface air temperature (tas) over that in ts (SST over ocean, skin temperature over land) in RCP8.5 simulations, averaged across all CMIP5 models, was only 2%, indicating only a minor blending effect. [Nicholas Lewis, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. This discussion has been moved to chapter 2 where it is covered in more detail

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
37802	9	50			What is written here is factually correct as a summary of the Richardson et al.'s work, but perhaps a little misleading, as the discussion concentrates on the GMST/GSAT difference, whereas it is the incomplete data coverage that accounts for the larger share of the 16% quoted. The data-coverage issue has been known for some time from comparisons of tradtionally-gridded datasets and reanalyses. But even then, the net figure of 16% looks rather high, as discussed further in comment 282 below. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. Agree, but this section now refers to chapter 2 so text removed
37804	9	52			More of the same, I'm afraid. Models indeed give larger trends. But reanalyses tend to also, especially in recent years when the Arctic has been warming sharply, though not by as much as 16% if taken over the past 40 years. Chapter 2 is currently relevant to this (table 2.3 for example), but it promises for the SOD to have new versions of several datasets, with better geographical coverage, for the second draft. This may in any case necessitate a rewriting of this paragraph. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. Agree, but this section now refers to chapter 2 so text removed
56346	9				Equation (7.1) of Box 7.1 (page 7-9) shows that the energy balance model (EBM) used in the discussion of feedback and sensitivity definitions is a zero-dimensional model (ZDM), i.e., one in which all quantities are global and annual means. This EBM is also the one used in estimating the equilibrium climate sensitivity (ECS) using GCM output (taken as half the value of the point where the linear slope of ΔN against ΔT over the first 150 years of an abrupt 4×CO_2 climate model simulation crosses the x axis at $\Delta N = 0$; page 7-10, lines 37-39). [J. Ray Bates, Ireland]	Rejected. An EBM is not used to assess feedbacks, rather a zero-dimensional model is implied when assessing feedbacks from the climate systems so this paper is not relevant here
56348	9				It has been shown in Bates (2016) (hereafter B16) that a ZDM is not always a realistic zero-order model of the climate system; a criterion is given there for determining when it may be regarded as realistic (i.e., $X<<1$ in the terminology of B16). A discussion of this issue in Box 7.1 would be appropriate. [J. Ray Bates, Ireland]	Rejected. We use the zero-dimensional model to assess the magnitude of different feedback processes not for making projections
56350	9				Bates, J. R. (2016), Estimating climate sensitivity using two-zone energy balance models, Earth and Space Science, 3, 207–225, doi:10.1002/2015EA000154. [J. Ray Bates, Ireland]	An emulator box has been added which tests these simple models
38598	10	2	10	2	This statement is untrue. Almost all observational previous estimates of climate sensitivity have used blended SST/near-surface air temperature, not global near-surface air temperature. AR6 should do likewise, not break with previous practice (as confirmed in IPCC SR1.5). [Nicholas Lewis, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We choose to use surface air temperature here but now clarify. The choice is explained in Chapter 2
25740	10	6	10	6	The quantity alpha is denoted here as a feedback parameter. It is really a response parameter. A feedback assumes as a base state a no-feedback response (here the planck response), the feedbacks modifying the response from what it would be in the absence of a feedback. I am not sure how entrenched the language is becoming calling this quantity a feedback parameter. But in general it is better not to muddy concepts that are well established. I refer to Hansen et al 84 (cited in this chapter): Delta Teq is the equilibrium change of global mean surface air temperature and DeltaTo is the change of surface temperature that would be required to restore radiative equilibrium if no feedbacks occurred. Bony et al 06 (cited in this chapter) in an otherwise very nice discussion (Appendix) muddied the situation using "feedback parameter" for the quantity here called alpha. Schwartz (2011) tried to correct the situation. I recommend calling the quantity alpha an "aggregated response parameter" rather than an "aggregated feedback parameter". The point "there is no standardised notation or sign convention for feedbacks in the literature" is well taken. Perhaps a box is called for. I would be happy to prepare such a box, based on my 2011 paper: Schwartz S. E. Feedback and sensitivity in an electrical circuit: An analog for climate models. Climatic Change 106, 315-326 (2011). doi: 10.1007/s10584-010-9903-9 [Stephen E Schwartz, United States of America]	Taken into account. We prefer the conventional feedback parameter but have been clearer on the wording

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
15032	10	6	10	25	The correct way to apply feedback analysis is in the power domain where all W/m^2 of solar forcing are the input to the feedback amplifier model and all W/m^2 of surface emissions are its output. Temperatures are then determined from W/m^2 of surface emissions using the SB Law. This model considers the zero feedback case to be an ideal black body whose surface emissions consequential to its steady state temperature are exactly 1 W/m^2 per W/m^2 of forcing power and is unconditionally independent of the amount of power arriving. This model also encapsulates whatever net effect non radiant energy leaving and then returning to the surface has on the steady state. The ratio of the surface emissions to the planet emissions is the surface emissions sensitivity and is a dimensionless ratio as Bode's definition of gain requires. Note that Bode's definition of the sensitivity is not the same as the climate definition, where what Bode calls the gain is what the climate model refers to as the sensitivity and what Bode calls the sensitivity is unrelated to the formulation of the climate sensitivity although some of of the attributes of Bode's sensitivity have been inappropriately attributed to what's called the climate sensitivity. The most important attribute is that both the gain and sensitivity are constant, dimensionless ratios and this was not honored by the application of feedback analysis to the climate. The current climate feedback analysis models a change in surface temperature as the output which both obfuscates and disconnects from the COE requirement between the input forcing and the output surface emissions. Applying it incrementally was a failed attempt to get around the linearity constraint. Expressing the sensitivity in the non linear units of degrees per W/m^2 is absurd and should have never gotten past peer review. The disconnect from COE arose because Bode's simplifying assumption of an implicit power supply precludes the requirement to conserve energy between the input and output of the amplifier model, which otherwise significantly complicates the analysis. For the climate system, the origin of the output Joules are the input forcing Joules and COE must be honored between the input and output of the feedback model, but is not. The fact that it's not lends false plausibility to the idea that the next W/m^2 of forcing can result in as much as an infinite amount of W/m^2 in the 'runaway' case, while each of the other W/m^2 from the Sun contributes only 1.62 W/ w^2 to the result. [George White, United States of America]	Noted. There are many ways to apply feedback, we choose the well established and proven methods for the climate literature. The argument in the review comment is not well supported. The linear model of forcing and feedback is for small perturbations around a mean state. It is used as a diagnostic to gauge processes related to Earth's energy budget. Climate models fully evaluate the radiative transfer of energy the Earth's atmosphere to diagnose forcings and feedbacks.
15034	10	6	10	25	The Earth is not an ideal black body and its surface is warmer than an ideal BB would be by emitting more than 1 W/m^2 per W/m^2 of forcing. Given average surface emissions of about 390 W/m^2 at 288K and an average solar input of about 240 W/m^2 at about 255K, each W/m^2 of solar input uniformly contributes about 1.62 W/m^2 +/- < 5% to the surface emissions, where the additional 620 mw per W/m^2 is the net, steady state excess warming from all feedbacks, positive, negative, known and unknown. There's no legitimate reason why the next W/m^2 of non reflected solar input considered as 1 W/m^2 of ERF will contribute more than 1.62 W/m^2 to the surface emissions, yet the nominal ECS assumes that it will increase the surface emissions by more than 4 W/m^2. Furthermore, only feedback expressed in the units of W/m^2 makes any sense as only W/m^2 of feedback can be added to W/m^2 of forcing. Dimensional constants converting temperature into W/m^2 for the purpose of establishing feedback are as meaningless as the presumed linear sensitivity converting W/m^2 into a temperature and neither has any foundation in physics. [George White, United States of America]	Noted. The theory presented in this comment is not supported in the peer reviewed literature and it is not clear what text this refers to or if any text is inaccurate
15036	10	6	10	25	Venus needs a better explanation then runaway GHG effects which arose only as a failure to acknowledge the missing power supply. Relative to its surface temperature, the behavior of Venus is closer to that of an ideal insulating container (a white body) whose emissivity is zero and whose constant temperature is arbitrary. Unlike Earth, whose atmospheric transparency is chaotically semi-transparent in both the LWIR and visible bands, the Venusian atmosphere is nearly completely opaque in both. The Venusian surface in direct equilibrium with the Sun is high up in its clouds where the temperature of the solid surface below is a function of the equilibrium temperature of the clouds and the PVT profile of the dense CO2 'ocean' between the clouds and the solid surface below. Venus is not a case of runaway GHG's, but one of runaway clouds, where the clouds became a thermodynamic system decoupled from the solid surface below owing to an extremely dense atmosphere. This is analogous to the temperatures of Earth's deep oceans, which are also decoupled from the temperature of Earth's virtual surface in direct equilibrium with the Sun, which like Venus, is something other than the solid surface of the planet. For Earth, this virtual surface is the top of the oceans and bits of solid surface that poke through. Unlike Venus, Earth's clouds are not an independent thermodynamic system and are tightly coupled to the surface by the hydro cycle. The dense CO2 atmosphere of Venus has more in common with Earth's oceans then with Earth's atmosphere. It's even acts like a fluid at the surface (a supercritical fluid). [George White, United States of America]	Noted. Venus is not discussed

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
15038	10	6	10	25	The theoretical emissions sensitivity limit for a planet with a semi-transparent atmosphere is 2 W/m^2 of surface emission per W/m^2 of forcing. Any predicted W/m^2 beyond the theoretical maximum can only come from the implicit and infinite power supply required for applying feedback analysis and which is not actually part of the climate system. The implicit power supply can't be the Sun, as the solar forcing Joules are already the explicit input to the model and can't be both the forcing input and the implicit power supply. The idea that the average W/m^2 is the power supply and the incremental W/m^2 is the signal still doesn't answer the question about how the climate system can distinguish the next W/m^2 from the others. Some are confused by the concept of the 'small signal' gain, but the small signal attribute has nothing to do with the size of a change relative to the size of the signal and in this case, the 'small signal' is the entire range of solar input from 0 W/m^2 up to its maximum and the model must be unconditionally linear across the entire dynamic range of the signal. [George White, United States of America]	Noted. The theory presented in this comment is not supported in the peer reviewed literature and it is not clear what text this refers to or if any text is inaccurate
25742	10	17	10	17	Do not specialize to CO2; Eq7.1 refers to a forcing, not to CO2. keep the forcing general; suggested language: Respond to a forcing such as that induced by an increase in atmospheric CO2 concentration. In principle there could be feedbacks on other forcers, for example an increase in temp and water vapor could increase OH and decrease methane, a feedback on methane. An increase in temp could result in more precip, diminishing aerosol lifetime. So perhaps better to treat CO2 as an example not as the only such feedback on forcer amount. [Stephen E Schwartz, United States of America]	Taken into account. Agree, wording changed to be general
18348	10	17	10	18	"All earth system feedbacks that do not alter the atmospheric…". This sentence may be misleading. It might be better as "All earth system feedback mechanisms should be included in the sum except those which act by altering atmospheric CO2 concentration" - need to make clear that the vegetation change is included for the non-CO2-altering aspects, even though that feedback also alters CO2. [Gwenaelle GREMION, Canada]	Taken into account. Agreed wording changed
27100	10	17	10	19	These lines state that an example of an Earth System feedback that does not alter the atmospheric CO2 concentration is the vegetation response, yet vegetation cover and type do change in response to CO2 concentrations and also affect CO2 concentrations. This is covered in section 7.4.2.5.2. I suggest that this example is removed from here. [Chris Satow, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Agree, this example is removed
58038	10	17			Replace 'change in CO2 concentration' with 'change in ERF'. [Nathan Gillett, Canada]	Accepted.
15030	10	23	10	23	The basic analysis cited directly or indirectly in all climate feedback related papers is Bode's book on linear feedback amplifier design. This reference is missing from the report and is otherwise crucial for understanding the definitions of linear, forcing, feedback, gain and sensitivity relative to the application of feedback analysis to the climate system. This reference is indirectly mentioned here and elsewhere when Hansen's 1984 feedback paper is cited. Bode's book is the only reference cited by any feedback related paper that defines the math and logic behind the linear feedback amplifier analysis applied to the climate system. The first two paragraphs of Bode's book outline the two simplifying assumptions necessary for applying linear feedback amplifier analysis. One is strict linearity and the other is an implicit source of Joules powering the gain. Neither of these were honored when Bode's feedback analysis was first applied to the climate system by Hansen, modified by Schlesinger and cited as the theoretical rationalization for the ECS reported in AR1. This is yet another fatal error that has been inappropriately canonized as 'settled science'. [George White, United States of America]	Noted. The Bode, industrial feedback framework is not used. This is not an error but rather a choice dating back to early Hansen papers as you cite - see 1991 paper by Schwartz that compares legitimate approaches. The method used here is well proven and established
18350	10	25	10	25	"Note that there is no standard notation..." - add explanation that there is a standard used within the section/chapter. [Gwenaelle GREMION, Canada]	Taken into account. The paragraph has been revised.
18484	10	27	10	27	Box 7.1 defined ECS for a CO2 doubling experiment in Line 27. Later in Line 39 the definition is provided for 4CO2 experiment. It may be useful to provide similar definitiom for 2CO2 here, which is perhaps equal to the 2XECS calculated for 4XCO2. [Gwenaelle GREMION, Canada]	Taken into account. We agree, this is not exclusively doubling
18486	10	27	10	27	It may be useful to mention the rationale/reference behind choosing the first 150 years. In the intital 10-20 years of the climate model run, the scatter is much higher between dN vs. dT, and the relationship is not so linear as compared to the later years. Wouldn't it be useful to discard first few years when defining ECS and use only those years when the model is approaching equlibrium state? Also how different is the value of ECS if one uses all available years in place of first 150 years? [Gwenaelle GREMION, Canada]	Not applicable. This text has now changed to define ECS as the ideal and the regression as a poor approximation of this
25744	10	27	10	27	Suggest let it read: "the value of ΔT at steady state in response to a sustained doubling, commonly denoted "equilibrium" (ΔN = 0), ..." We really need to underscore that the climate system is not an equilibrium system. [Stephen E Schwartz, United States of America]	Taken into account. Agree, quotes added
15040	10	27	10	27	The climate system can't distinguish one Joule of forcing from any other. All Joules, including the next one, must contribute equally to the result, which in this case, is the average surface emissions resulting in its average temperature. For this reason, the feedback must apply to all W/m^2 equally and not just the next one. If the next W/m^2 results in more than 3 W/m^2 of feedback power, as predicted by the nominal ECS inferred in this chapter, all 240 W/m^2 from the Sun must do the same and the surface temperature would be close to the boiling point of water. For all intents and purposes, this failed prediction of the nominal ECS falsifies it as a legitimate hypotheses. [George White, United States of America]	Rejected. This is not true, responses are well known to be non linear in many situations, the forcing feedback framework is only true for perturbations around am mean state. Each Wm-2 is produced by a process and the processes can all be affected by the climate state

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
25746	10	27	10	33	Suggest phrase "also known as the Charney sensitivity" right after first use of ECS at line 27, if that is what is meant. [Stephen E Schwartz, United States of America]	Not applicable. Box 7.1 has been rewritten for the second order draft.
44784	10	27	10	39	Consider to make figure to illustrate the way ECS is estimated and provide additionial discussion/framing which assumptions are made by this and why/in how far this gives approximately ECS for doubling of CO2. [Astrid Kiendler-Scharr, Germany]	Taken into account. We agree, this figure has been added
18352	10	27	11	5	It would be worth mentioning how longer than longer-than-150-year feedback processes such as methane release are (not) accounted for. The name "equilibrium climate sensitivity" suggests that it captures the full extent of the warming the climate system might experience due to a CO2 perturbation. This might be helped by clarifying the meaning of "equivalent" in 10 lines 43-46. [Gwenaelle GREMION, Canada]	Taken into account. We agree and now define ECS on longer timescales
13378	10	30	10	33	Does ECS include deep ocean feedbacks? It is not clear as the time scale quoted here is 150 yrs where deep ocean changes could be important. [Govindasamy Bala, India]	Taken into account. Yes it does as revised. Text has been revised
25748	10	30	10	38	The 150 years would seem to need some justification. Are the 150 in lines 30 and 38 manifestations of the same physics, or is it a coincidence? 140 yr I would understand as twice 70 yr, where 70 yr = 1/(0.01 yr-1)* ln2; but 150?. [Stephen E Schwartz, United States of America]	Taken into account. This has changed so ECS is now a true equilibrium value
13380	10	35	10	39	What is the reason for the choice of 150 years? [Govindasamy Bala, India]	Taken into account. This has changed so ECS is now a true equilibrium value
56666	10	35	10	47	If the ECS from CMIP6 runs is inferred via a 4x abrupt run, discuss the systematic (likely upward) shift of the ECS values in comparison to when ECS is inferred from the 2x CO2 climate state… [Malte Meinshausen, Australia]	Taken into account. This is now addressed in the Box and Figure has been added
38600	10	36	10	39	It is unscientific and inconsistent to estimate ECS by halving an estimate of ultimate warming in a 4xCO2 simulation when the ratio of CO2 radiative forcing at 2x and 4x preindustrial concentration is quite accurately known, and is non-negligibly different from 0.5. The ratio given by the expression in Etminan et al (2016), as adopted elsewhere in Chapter 7, should be used, which gives a ratio of 0.478. Careful ECS studies published since 2016 have done so, e.g. Lewis and Curry (2018). Estimating ECS from 4x CO2 simulations also involves assuming no climate state dependence of sensitivity between 2x and 4x CO2, however in CMIP5 models this assumption this assumption does appear to be true on average (Lewis and Curry 2018 Table S2 cols 3 and 5 show that CMIP5 mean/median ECS_hist estimates derived from abrupt4xCO2 and 1pctCO2 simulation data are nearly identical). [Nicholas Lewis, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We agree, text has now been added on this and the 2xCO2 ECS used ,Lewis and Curry is now cited
25750	10	37	10	39	I would hope that there is a figure showing this, and there should be an internal reference to that figure. I am a pretty knowledgable reader and I have a tough time parsing what is meant by that. [Stephen E Schwartz, United States of America]	Taken into account. Agree, figure now added
18354	10	37	10	39	Add discussion of accuracy/limitations of the appoximation for ECS from shorter climate model runs. What are the errors it introduces? [Gwenaelle GREMION, Canada]	Taken into account. This is too detailed here but a figure has been added for clarity
18488	10	38	10	38	Can someone use climate runs less than 150 years to calculate the ECS? [Gwenaelle GREMION, Canada]	Taken into account. Effective ECS estimates are now discussed. Yes they can but not well
38602	10	41	10	41	Please ensure that, unlike in AR5, "climate state" is defined in the Glossary. It is unclear what exactly it includes, and whether it refers only to an equilibrium situation. [Nicholas Lewis, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. We need to keep it undefined here as term is descriptive and used differently in the report
25752	10	42	10	44	Again, why 150 yr? Needs to be motivated. That said, I think that a time scale of order 100 yrs is a very sensible number to pay attention to. [Stephen E Schwartz, United States of America]	Taken into account. This has changed so ECS is now a true equilibrium value
38604	10	46	10	48	Agree that it is important to translate evidence onto a common standard, but the standard should be whatever the best observational evidence is available in respect of, not whatever is convenient or usual practice in models, and the usual model definition of ECS may not be the best standard. Translation can be accomplished accurately in models, but not for observations (where the relationship between variables may differ from that in models). [Nicholas Lewis, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We agree, and in light on the comment This text has changed so ECS is now a true equilibrium value
25754	10	50	10	54	The reason that TCR defined as the increase in temperature for a 1% yr-1 increase in CO2 at time of doubling (70 yr) is a meaningful number, and closely approximates the Transient Climate Sensitivity, TCS, is that the time lag for reaching quasi steady state in the atmosphere–mixed-layer-ocean system (circa 4 - 8 yr) is short compared to 70 yr, so that unrealized increase in GMST, the increase that would ensue if the increase in CO2 were abruptly halted at 70 yr, maintained thereafter at 2X, on the centennial time scale thereafter is quite small. As this unrealized committed temperature increase, estimated as the transient sensitivity (K /W m-2) times the rate of increase of forcing (W m-2 yr-1) times the time constant to achieve quasi steady state (yr) is small compared to the realized increase at year 70, its neglect is not of great consequence. This is discussed by Schwartz (2018), Appendix A. Schwartz, S. E. (2018). Unrealized global temperature increase: Implications of current uncertainties. J. Geophys. Res. Atmospheres, 123, 3462–3482. https://doi.org/10.1002/ 2017JD028121 [Stephen E Schwartz, United States of America]	Rejected. This is too detailed a point for the box

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
14310	10	51			correction: "doubling (year 70 in a climate model simulation in which CO2 increases at 1% yr-1 from pre-industrial)" [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Rephrased the sentence for clarity.
57356	10	54	11	5	Since metrics are also covered in this chapter, you should note that TCRE is also a function of TCR and AGWP of CO2: TCRE = TCR x H / F2x / AGWP_H -- and it is definitely worth checking these numbers all stack up. [Myles Allen, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. This is a good point and now added
25756	11	13	11	13	Let it read " _Virtually_ all the energy that enters or leaves the system..." There are other minor sources of energy . See . "Where does Earth's atmosphere get its energy?," A. C. Kren, P. Pilewskie, and O. J. Coddington, Space Weather Space Clim. 7, A10 (2017). Systematic examination of sources of Earth's energy. [Stephen E Schwartz, United States of America]	Accepted.
58040	11	13		14	It is not true that all the energy that enters or leaves the climate system does so through TOA radiation. Geothermal heat flux at the ocean floor is estimated to be ~0.1 W/m^2, which is larger than some of the radiative forcing terms considered in the chapter, and is important for the temperature and circulation of the deep ocean (e.g. Hofmann and Maqueda, 2009 https://doi.org/10.1029/2008GL036078). [Nathan Gillett, Canada]	Taken into account. Since geothermal heating is time invariant we consider the system to be in equilibrium relative to that heat input. Therefore, it drops out of the energy budget framework considered in Chapter 7 and we omit its discussion for brevity.
25758	11	16	11	16	Again, steady state, not equilibrium!!! [Stephen E Schwartz, United States of America]	Accepted.
18356	11	16	11	16	"in an equilibrium climate, the outgoing and incoming components are in balance". Two issues: firstly, equilibrium is an odd word to use to describe and out-of-equilibrium system such as the planet. Perhaps steady? However, secondly, the incoming and outgoing fluxes are not in balance on seasonal timescales as well (only internal variability is mentioned). [Gwenaelle GREMION, Canada]	Taken into account. Text has been revised accordingly.
15042	11	18	11	22	The idea that there's a persistent average imbalance at TOA is misleading. There's certainly a periodic imbalance per hemisphere where in the spring and summer Pin > Pout and hemispheres warm, while in the fall and winter, Pin < Pout as they cool. There's also a yearly imbalance that varies on either side of zero as the yearly average temperature increases or decreases. The per hemisphere peak to peak imbalance between winter and summer is well over 100 W/m^2, moreover; owing to a longer time constant, the variability in the S is much larger than that in the N and overall, about 20 W/m^2 peak to peak from the S hemisphere is not 'canceled' and appears as the global seasonal imbalance. To be clear, hemispheric imbalances don't actually cancel relative to the response of the surface to solar energy since given how little energy crosses the equator relative to the energy absorbed and emitted per hemisphere, the 2 hemispheres operate largely independently of each other. It's the same with hemispheric emissions and temperature, where the seasonal temperature response in the N is larger than in the S and when averaged together, about 2-3C of peak to peak variability in the N hemisphere emerges as the apparent global seasonal temperature response which coincidentally is 180 degrees out of phase with the 20 W/m^2 difference between perihelion and aphelion and is another strong indication that the hemispheres do indeed act independently of each other. The degenerate example of this kind of independent behavior would be a planet tidally locked to its Sun. [George White, United States of America]	Noted. The CERES observations permit monitoring on both hemispheres simulataneously at monthly timescales. We accept that EEI is highly variable on these timescales, but it is also clear that the Earth is accumulating heat over time and therefore on longer timescales EEI is positive (this is very clear in estimates of ocean heat content gain and associated time series of global sea-level rise).
15044	11	18	11	22	Supporting data for the global hemispheric yearly temperature variability is here. The data is reported as monthly averages calculated over 3 decades of weather satellite data. http://www.palisad.com/co2/plots/wbg/g/temp.png http://www.palisad.com/co2/plots/wbg/nh/temp.png http://www.palisad.com/co2/plots/wbg/sh/temp.png Yellow is the solar input before reflection, blue is the surface temperature, green is the cloud top temperatures, magenta is the fraction of the surface covered by clouds and red dash-dot is the instantaneous energy balance, (Pi-Po). The components in each plot are centered on their average and displayed with unique Y axis limits. The averages and Y axis limits per component are shown. The plotted variables are either directly or derived from the variables reported in the ISCCP aggregation of weather satellite data. Pi and Po resulting in the imbalance were calculated based on reported values of solar input, albedo, the temperatures in this plot and a HITRAN based 3-d radiant model of the atmosphere. Many similar plots aggregating smaller slices of latitude and displaying other variables can be found starting from here: http://www.palisad.com/co2/plots/wbg/plots.html Since temperature is linearly proportional to stored Joules and Watts are a rate of delivered Joules, the rate of temperature change towards a new steady state is proportional to the instantaneous imbalance and given the very large peak to peak diurnal and seasonal imbalances, adapting to a milliwatt or so per month of absorption by incremental CO2 will occur at a far faster rate than it would if the only variability was from an increase in CO2. The bottom line is that the only 'persistent' imbalance is what little effect hasn't already been realized by the CO2 emitted in the very recent past and even this is superimposed on a very large p-p swing in the instantaneous imbalance. [George White, United States of America]	NOTED
25760	11	19	11	19	"known as"; better "denoted", or just omit. [Stephen E Schwartz, United States of America]	Accepted.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
50842	11	20	11	22	A J. Hansen paper could be ciuted here: either Hansen et al., 2005, DOI: 10.1126/science.1110252; or Hansen et al., 2011: doi:10.5194/acp-11-13421-2011 [Karina von Schuckmann, France]	Noted.
38606	11	20	11	22	The Earth's energy (power) imbalance is not the main metric determining the present rate of global warming. That metric is the rate of change in forcing. Moreover, an increase in energy imbalance (with unchanged forcing) reduces the rate of global warming, contrary to the impression given by this sentence. [Nicholas Lewis, United Kingdom (of Great Britain and Northern Ireland)]	TAKEN INTO ACCOUNT. We have revised this text in light of the reviewer's comment.
25762	11	21	11	21	Is it "the fundamental driver" (as stated) or a response? I would have thought the forcing is the fundamental driver. The imbalance is simply the difference between the forcing and the radiative response. [Stephen E Schwartz, United States of America]	TAKEN INTO ACCOUNT. We have revised this text in light of the reviewer's comment.
19316	11	24	11	32	It’s not at all obvious why the clear-sky energy budget diagram is provided in Fig. 7.3 (right), nor is it explained how it is derived. The text merely refers to it in passing as “a base state to enable an estimation of the effects of clouds on the energy flows in the climate system”. This doesn’t strike me as being particularly useful or informative. I would suggest either removing the clear-sky energy budget diagram entirely or expanding the discussion to describe what are the effects of clouds on the energy flows within the climate system. [Norman Loeb, United States of America]	Taken into account, we added information and a reference on the derivation of the clear sky diagram and expand the discussion to describe the effects of clouds in modifying the energy flows in the climate system.
19318	11	24	11	32	The preceding paragraph provided a nice description of the TOA energy budget but a similar description at the surface is missing. For consistency, it would make sense to provide a similar introductory paragraph to introduce Fig. 7.3. [Norman Loeb, United States of America]	Taken into account, we expanded the description of the energy flows within the climate system and at the surface.
15046	11	43	11	43	Figure 7.3: The 398 W/m^2 of surface emissions corresponds to a temperature of 289.5K which is higher than the generally accepted average of about 288K whose emissions are only 390 W/m^2. I realize that this has been increasing a little in every AR, but excluding the effects of the most recent El Nino, the net temperature increase since the first AR is less than the uncertainty, somewhere between negligible and imperceptible and no where near 1.5C. It's unnecessary to include non radiant energy transported by matter in the radiant balance, for example, latent heat and thermals or convection, plus the return of that energy to the surface. Thermals only affect the kinetic temperature of the gases in the atmosphere and neither O2 or N2 emit any LWIR that contributes to the radiant balance as a consequence of their translational energy. It's unnecessary to separate the solar energy absorbed by the atmosphere from that absorbed by the surface. Only the liquid and solid water in clouds absorbs any appreciable amount of solar energy and this water is quickly and tightly coupled to the surface by the hydro cycle. Since the climate averages being considered are over periods of time much longer than the hydro cycle, absorption and emission of solar energy by clouds can be considered a proxy for absorption and emission by the surface. When the return of non radiant energy entering the atmosphere is subtracted from the back radiation term, all that's left are the W/m^2 replacing the emissions of the surface at its average temperature. Nothing in this report explains how the non radiant energy entering the atmosphere plus its return to the surface effects the average surface temperature and its corresponding radiant emissions beyond the effects they're already having which is fully accounted for by the 1.62 W/m^2 of surface emissions per W/m^2 of forcing. The basic error this illustrates is conflating the energy transported by photons which is responsible for the radiant balance, sensitivity, net surface emissions and its temperature with the energy transported by matter which only redistributes the energy already stored by the planet’s thermal mass. [George White, United States of America]	Noted, the surface thermal upward flux contains both the surface thermal emission and a small contribution from the reflected part of the downward thermal radiation (the thermal emission from the atmosphere directed towards the surface), since the Earth's surface is not a perfect blackbody. Also, the surface thermal emission cannot be calculated solely from the global annual mean surface temperature, but has to take into account the nonlinearities arising from the spatial and temporal (seasonal, diurnal) variations of the surface temperature patterns over the globe. The nonradiative fluxes of sensible and latent heat compensate for the radiative energy surplus at the Earth surface and the radiative deficit in the atmosphere, and the latent heat flux is a key element to link the global energy and water cycles. The relative amount of absorbed solar energy in the atmosphere and at the Earth's surface is relevant, as it is a determinant factor for example for the intensity of the global water cycle or the vertical stability of the atmosphere. Solar energy is also absorbed in the cloud free atmosphere, e.g. by water vapour and absorbing aerosols. The best
19320	11	46	11	46	The discussion in this subsection is mainly focused on uncertainties in determining the mean quantities (especially the surface energy budget) as opposed to discussing the mean quantities themselves. Are the uncertainties so large that it’s not worth even describing the mean state of Earth’s energy budget? There is also a lot of discussion about climate model representation of the mean fluxes. I wonder if that’s appropriate here given that the section is about the present state of the energy budget as opposed to climate model evaluation, which I believe is covered elsewhere in AR6. [Norman Loeb, United States of America]	Taken into account, we added related statements which discuss the mean quantities. Since there is no longer a specific model evaluation chapter in AR6, the representation of these aspects in climate models needs to be covered here as well.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
19322	11	51	11	52	I did not see in Section 2.2 any discussion about recent (present-day) variations in solar irradiance (e.g., from the SORCE-TCTE-TSIS record). Section 2.2 focuses more on historical pre-satellite solar irradiance reconstructions. Given that the present section (7.2.1) focuses on “present-day energy budget”, shouldn’t present-day solar irradiance changes be discussed here (if nothing else to acknowledge that there have been no surprises in solar irradiance etc.)? [Norman Loeb, United States of America]	Taken into account, this has now been incorporated in Section 2.2.
50844	12	1	12	2	This sentece can be mis-leading (the ay it is written): the 'anchoring' to OHC is done for satellite adjustment reasons. [Karina von Schuckmann, France]	Taken into account. Text has been revised in the FGD to avoid the mis-leading statement.
14312	12	2			"monitor" is the wrong word as CERES is capable of monitoring fluctuations in the EEI to high accuracy, "quantify" is perhaps more accurate. [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Accepted, "monitor" has been replaced by "quantify"
19324	12	3	12	3	Please add “reflected” before “solar” and “emitted” before “thermal. [Norman Loeb, United States of America]	Accepted.
51504	12	3	12	4	Would be nice to asses dominant uncertainties for the TOA flux components [Michael Schulz, Norway]	Taken into account, uncertainties in global mean TOA flux of 1.7% (1.7 Wm-2) for reflected solar and 1.3% (3.0 Wm-2) for outgoing thermal radiation at the 90% confidence level are added in Section 7.2.1.
29328	12	3	12	8	The value of the EEI of 0.71 +/- 0.1 W/m² of [Johnson et al, 2016] for the first ten years of Argo data seems to be somewhat low compared to the value of 0.9 +/- 0.3 W/m² from [Trenberth et al, 2016], which seems to be confirmed by the recent results of [Meyssignac et al, 2019] who find a value of 0.85+/-0.26 W/m². [Steven Dewitte, Belgium]	Taken into account. All the quoted numbers are consistent with our assessment for the period 2006-2018 when accounting for estimated uncertainties.
19326	12	5	12	5	The 0.71 Wm-2 value is inconsistent with the value of 0.6 Wm-2 in Figure 7.3 (left). [Norman Loeb, United States of America]	Taken into account.
25764	12	5	12	5	0.71; compare to 0.42 at page 5,line 12; I recognize that these are different time periods, so if the use of the larger value here is impt then the reason for the difference should be explictly noted. [Stephen E Schwartz, United States of America]	TAKEN INTO ACCOUNT. The text in this section has been revised substantially for the SOD.
51510	12	5	15	13	an imbalance of +0.71 is mentioned for 2005-2015, then on page 15 two other numbers can be found: global mean reflectance all sky solar +0.57 (2000-2015) with no significant thermal trend and, by another author, a total imbalance of +0.59 (2000-2015). Two questions: Can these numbers be reconciled? and is there really no thermal flux trend? is the latter consistent with increasing GHG impact on radiative budget and why? [Michael Schulz, Norway]	Taken into account. The text in this section has been substantially revised.
41616	12	8	12	9	Check for consistency if this applies in CMIP6 [Amanda Maycock, United Kingdom (of Great Britain and Northern Ireland)]	Noted, this statement applies to both CMIP5 models and the CMIP6 models available so far, thus no immediate action is required.
19328	12	8	12	11	Is this section the appropriate place to evaluate climate models? The section title is “Earth’s energy budget and its changes through time”. Why are climate models even mentioned in this section? How is this relevant? [Norman Loeb, United States of America]	Noted, there is no longer a specific model evaluation chapter in AR6, the representation of these aspects in climate models needs to be covered here as well.
9964	12	8	12	11	Disagree with the statement. It should be more nuanced. Lucarini et al. 2014 Rev Geophys shhe energy budget of some AR5 models is problematic to say the least. Inconsistencies emerge not only for climate as a whole, but for the atmosphere and the ocean separately. [Valerio Lucarini, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account, here we talk about the absolute magnitudes of the individual energy flux components (not the imbalance which is almost two order of magnitude smaller), and that after tuning they mostly agree with the CERES reference values within their uncertainties. We revised the statement to make this more clear: "Since climate models are typically adjusted to match the magnitudes of their global mean solar and thermal fluxes at the TOA with corresponding satellite references from CERES-EBAF, they often do not greatly deviate from those values on a global mean basis."

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
51502	12	13	12	32	This paragraph is a bit too general, could be merged, and this shortened with the next paragraph. No substantiation with eg numbers found here. It is interesting to discuss the uncertainty and data base for all components of the surface energy budget. [Michael Schulz, Norway]	Taken into account, we added concrete numbers on the magnitudes of the fluxes under discussion, to make this paragraph more specific.
46450	12	16	12	16	before "Kato et al. (2018)", add the citation of the latest review article by Huang et al. (2019): Huang, G., Z. Li, X. Li, et al., 2019: Estimating surface solar irradiance from satellites: Past, present, and future perspectives, Remote Sensing of Environment, revised. [Zhanqing Li, United States of America]	Accepted, the reference has been added.
19330	12	21	12	25	As written, this sentence is a bit awkward. Perhaps it would be better to state that surface energy budget closure from satellite-derived surface radiation and turbulent heat flux (precipitation/evaporation and sensible heat) data products is achieved within the uncertainties of the individual components, but the uncertainties are as large as 15-20 Wm-2. [Norman Loeb, United States of America]	Taken into account. The sentence has been reformulated as follows: "Inconsistencies in the quantification of the global mean energy and water budgets discussed in AR5 (Hartmann et al., 2013) have been reconciled within the (considerable) uncertainty ranges of their individual components (L'Ecuyer et al., 2015; Wild et al., 2013, 2015). "
44786	12	26	12	32	Conclusions? [Astrid Kiendler-Scharr, Germany]	Taken into account, we changed to "However, on regional scales.." to make the difference between global and regional closures more apparent.
18358	12	28	12	31	The sentence about how the ' (L'Ecuyer) methods seems out of place, too specific compared to the other information offered in the paragraph. [Gwenaelle GREMION, Canada]	Accepted, we removed the description referring to this method and reformulated the sentence to make it more concise.
44788	12	34	13	3	Consider compilation of information in table and comparing observations vs models. [Astrid Kiendler-Scharr, Germany]	Noted, it is difficult to create a table with such heterogeneous information and severe space limitations.
19332	12	41	12	45	The last part of this rather long sentence, "and are consistent with the uncertainty in daily mean latent and sensible heat fluxes measured at a buoy observatory", seems entirely out of place. Daily uncertainty in turbulent heat fluxes at once site probably shouldn't be compared with global (and presumably) long-term mean uncertainty. [Norman Loeb, United States of America]	Accepted, we removed that part of the sentence.
19334	12	45	12	48	Please specify the temporal and spatial resolution of the uncertainties and whether these are bias (systematic) or random errors. [Norman Loeb, United States of America]	Taken into account, we reformulated the sentence to clarify these points. The values correspond to root mean squared differences.
13982	12	46			There is no mention of sea spray and its potential role in heat fluxes over the ocean. [Steven Siems, Australia]	Taken into account. Constraints on chapter length prevented discussion of this topic.
18360	12	48	12	49	"The uncertainty stems from the retrieval" - what about the retrieval? [Gwenaelle GREMION, Canada]	Noted, comment not clear
19294	12.4	28	12.4	28	The text cites the paper of Haywood et. al (2013), but there are two references in the bibliography. Is it 2013a or 2013b ? [Gwenaelle GREMION, Canada]	Editorial: updated in SOD
18362	13	5	13	10	In page 12, line 9 it states that the GCM TOA radiative flux is often tuned to match oservatinos. This makes the fact that the discrepency at surface flux is larger than at TOA in climate models relatively trivial, which should be mentioned in this paragraph. It might also be worth emphasising that an inabiliy to correctly capture energy flows which have not been tuned in models suggests model error which might be is not accounted entirely as an uncertainty by considering the spread of climate modes. [Gwenaelle GREMION, Canada]	Taken into account, we expanded this statement to cover this concern.
13086	13	10	13	10	To improve the comprehensiveness of the main text, text on the simulated radiations by reanalysis products will be added after '...(Wild et al., 2015).': [More importantly, the current reanalysis products have important biases in the modelling trends of the downward thermal and solar radiation since 1979 (by -1.61~-0.25Wm-2decade-1 and 0.00~3.93Wm-2decade-1), which can explain approximately 60% of the modeling biases in regional warming (Zhou et al., 2017; Zhou et al., 2018).] References: Zhou, C., Wang, K., and Ma, Q., (2017). Evaluation of eight current reanalyses in simulating land surface temperature from 1979 to 2003 in China. J. Clim., 30, 7379-7398. doi: 10.1175/jcli-d-16-0903.1. Zhou, C., He, Y., and Wang, K., (2018). On the suitability of current atmospheric reanalyses for regional warming studies over China. Atmos. Chem. Phys., 18, 8113-8136. doi: 10.5194/acp-2017-966. [Zhou Chunlüe, United States of America]	Taken into account, In this section, the focus is on the mean climatologies (section "present day energy budget"), the trends are covered in a subsequent section (section 7.2.2.3), where this reference has been included.
44790	13	14	13	17	provide explanaition for "more outgoing LW radiation in NH". [Astrid Kiendler-Scharr, Germany]	Taken into account. The text has been modified to provide an explanation.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
51506	13	15	13	16	"northward coean heat transport as reason for northerly rain maximum" - Is that really so clear? What about the role of the land mass distribution? What about the higher outgoing LW radiation in the north? Could that be substantiated a bit more? [Michael Schulz, Norway]	Taken into account. The text has been modified to clarify that ocean heat transport is a primary cause of the ITCZ location but that other factors contribute as well.
18558	13	17	13	31	The draft describes in details on the cross-equatorial heat transport referring the studies by Loeb et al. (2016), Liu et al. (2015, 2017a) and etc. The cross-equatorial energy transport in the ocean estimated by Loeb et al. (2016) was 0.44 PW as shown in Figure 7.4. However, more recent studies indicate that the cross-equatorial energy transport in the ocean is much smaller than 0.44 PW. For example, Figure 7.5 shows that it is very small (the figure provided for review is very fuzzy) and, an independent work by Forget and Ferreira (2019, their Fig. 2) also suggests that global mean cross-equatorial heat transport is close to zero. The cross-equatorial heat transport in the ocean is an important part in the total energy transport. Therefore, such a significant contradiction should be clearly declared and its influence on the confidence of the studies by Loeb et al. (2016, Fig. 7.4) should be reappraised. References: Forget, G., and D. Ferreira (2019), Global ocean heat transport dominated by heat export from the tropical Pacific, Nature Geoscience, 12, 351–354. [Gwenaelle GREMION, Canada]	Forget and Ferreria find an "effective" cross equatorial heat transport of 0.08 PW +/- 0.5 PW (5-95% uncertainty) across all basins. This value is similar to their "plain" cross-equatorial heat transport as well. It suggests that the ECCO state estimate used has a hemispheric asymmetry in the net surface heat flux, and thus cross-equatorial heat transport, that is somewhat at odds with the estimates in Loeb and other papers, but within the range of uncertainty. Note that using air-sea heat fluxes, they find a value 0.48 +/- 0.3 PW, consistent with Loeb. The text has been modified to include a citation to Forget and Ferreira.
8596	13	20	13	20	This is also one of the main results of our Lembo et al. 2019 paper (which is mentioned later in the same page) [Valerio Lembo, Germany]	Accepted, the reference of Lembo et al. 2019 is added
45536	13	34			This statement confidently attributing the ITCZ asymmetry to one process (Atlantic heat transport) (a) seems inconsistent with the subsequent discussion noting that in models it depends also on the hemispheric albedo contrast (which happens to be small in observations), and (b) implies that errors in the ITCZ would have to be caused by errors in the Atlantic which does not seem to be the case. Please clarify. [Steven Sherwood, Australia]	Taken into account. The text has been modified to clarify that ocean heat transport is a primary cause of the ITCZ location but that other factors contribute as well. Note that there is no inconsistency between these statements about the role of ocean heat transport (which refer to observed ITCZ location) and statements about models where biases in the ITCZ location are driven by biases in hemispheric gradients in TOA radiation.
40730	13	38	13	41	I wasn't clear here on whether this sentence refers to all cross equatorial heat transport or only atmospheric transport. Overall I learned a lot from this paragraph. [Daniel Murphy, United States of America]	Taken into account. This text has been modified.
41618	13	41	13	44	see also Hawcroft et al (2016, doi: 10.1007/s00382-016-3205-5) and Stephens et al (2016, doi: 10.1007/s40641-016-0043-9) [Amanda Maycock, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. References added.
36598	13	43	13	44	There are too few references to this model problem. At least you should include Wang et al. (2014, Nature climate change) [Carlos Mechoso, United States of America]	Taken into account. Reference added.
24478	13	51	13	53	Caption does not sufficiently describe figure. First explain its content, symbols etc, then give data sources. [Joanna Haigh, United Kingdom (of Great Britain and Northern Ireland)]	Noted, Fig. 7.4 is no longer shown in the SOD due to space limitations.
14314	13	51			Fig.7.4 can be updated to account for ocean heat uptake that is predominantly in the southern hemisphere. [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Noted, Fig. 7.4 is no longer shown in the SOD due to space limitations.
18632	13.4	21	104	24	This sentence is about proxy data for the whole Pliocene. However, the text previously mentioned that we were interested in the MPWP in concrete. [Gwenaelle GREMION, Canada]	Rejected. Yes, that is why we call it Pliocene rather than MPWP.
19292	13.4	35	13.4	35	It would be more accurate to say "MPWP" than "Pliocene", and "Late Quaternary" instead of "modern" because those are the periods which have been compared in the literature you have reviewed. [Gwenaelle GREMION, Canada]	Accepted. Yes, we are now more consistent here.
29330	14	2	14	9	These results have been contradicted by our recent study [Dewitte et al, 2019]. Following our assessment there has been a slight ageing of the Ceres Ebaf reflected solar fluxes, which is consistent between the all-sky and clear-sky case. The ageing corrected Ceres is consistent with the ERA5 reanalysis. Consequently we find that the EEI has been decreasing since 2000, not increasing as suggested by the [Loeb et al, 2018] study. This result is confirmed by the analysis of independent measurements of the Ocean Heat Content Time Derivative (OHCTD). From 1982 to 2000 we find an increase of the OHCTD. [Steven Dewitte, Belgium]	TAKEN INTO ACCOUNT. The papers we considered as part of our assessment but we were unable to present a full discussion due to space limitations.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
41620	14	4	14	15	unclear how much of this was already known in AR5. What is new? [Amanda Maycock, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Some of this was published just before AR5, but was not discussed in this level of detail there.
9966	14	7	14	7	Authors should make reference to Lucarini and Ragone Rev. Geophys 49, RG101 (2011) for CMIP3 and Lucarini et al. Rev Geophys.52, 809-859 (2014) [Valerio Lucarini, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. References added.
8598	14	10	14	10	Maybe a bit more could be said on the systematic underestimation of oceanic poleward heat transport in the SH. This is probably related to the reproduction of the upwelling branch of the Deacon cell, feeding the Antarctic Circumpolar Circulation (ACC) by storing potential energy into the ocean's surface [Marshall and Speer, 2012]. Marshall J, Speer K (2012) Closure of the meridional overturning circulation through Southern Ocean upwelling. Nat Geosci 5:171-180. doi: 10.1038/ngeo1391 [Valerio Lembo, Germany]	Not applicable. The discussion of mechanisms of ocean heat transport has been moved to Chapter 9.
14316	14	12			The cloud biases develop rapidly in CMIP5 models (e.g. Hyder et al. 2018 Nature Comms, doi:10.1038/s41467-018-05634-2 [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. Indeed. However, we are not sure how to fit this reference into this particular paragraph.
14320	14	12			Some reference to or signpost to consistency with ocean heat uptake discussed in the next section could be made here (e.g. Cheng et al. 2017 Sci. Adv., doi:10.1126/sciadv.1601545) [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. This text has been moved to later in Section 7.2, so it now follows the discussion of ocean heat uptake.
14318	14	16			Some assessment should be made of the large increase in outgoing longwave found by Dewitte and Clerbaux (2018) which seems at odds with other observations and AMIP simulations [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	TAKEN INTO ACCOUNT. Unfortunately, we did not have space in our chapter for a discussion of the Dewitte papers - but they were considered in our overall assessment.
51508	14	18	14	19	"independent estimates of radiative components have converged" - not very clear what exactly has converged [Michael Schulz, Norway]	Taken into account, this sentence has been revised to be more clear.
36600	14	19	14	21	Are you referring to climate models in general or to full GCMs? There is some understanding that climate models with a too simplified ocean have their own problems. [Carlos Mechoso, United States of America]	Noted, also climate models with sophisticated ocean models do have problems to accurately reproduce the surface energy budgets as well as aspects of the meridional transports.
29332	14	19	14	22	I don't agree that the solar reflectance has decreased – see above. Concerning the variation of the EEI, the formulation is dubious. It suggests that the EEI was at different constant levels during the 90'ies and after 2000, which is not the case following our analysis. The EEI has been rising steadily from 1982 to 2000, and has been decreasing since 2000. [Steven Dewitte, Belgium]	TAKEN INTO ACCOUNT. See our response to the previous comment (ID 29330) .
45538	14	19			Confusing to use "high confidence" for a statement about the data rather than the actual climate system—are you saying you are confident that the data agree, or that you know the actual budget? The former does not automatically imply the latter. [Steven Sherwood, Australia]	Accepted, the statement has been reformulated.
37806	14	26			"Observationally-based" would be better than "Observed". [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Accepted, replaced as suggested
45540	14	50	15	17	It is not always clear how this material is relevant to any important quesiton about climate change. [Steven Sherwood, Australia]	TAKEN INTO ACCOUNT. We have revised the text in this section substantially, and included a new figure that compares TOA radiative fluxes between CERES and climate models.
19336	14	51	14	52	This statement is a bit misleading. Variations in TOA energy fluxes since 2000 are overwhelmingly due to internal variations of the climate system. The above sentence doesn't make this clear. [Norman Loeb, United States of America]	Taken into account. Rewritten to highlight importance of internal variability.
18364	15	1	15	2	Sentence beginning "Substantial" should include a specific citation. [Gwenaelle GREMION, Canada]	Accepted, reference of Loeb et al. (2018) has again been added
41622	15	4	15	6	reference needed for this [Amanda Maycock, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The related reference is Loeb et al (2018a). As we already refer to this reference twice in this paragraph within a few lines around this sentence, we feel that this is enough referenced.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
40734	15	6	15	8	It is important to clarify this statement. Taken out of context, it sounds like evidence against our understanding of climate forcing and feedbacks. My understanding from Loeb 208a is that there was no significant linear trend in global mean thermal TOA radiation versus time. This does not mean there were no changes, only that the data record was too short. For example, CERES global monthly mean outgoing thermal is highly correlated with global mean surface temperature (e.g. my Murphy et al. 2009 JGR paper although there may be better, newer references using the now longer data record). [Daniel Murphy, United States of America]	Noted, we no longer discuss the trends in the CERES fluxes due to the shortness of the records and severe space constraints.
40736	15	9	15	16	I had real difficulty following this argument, and I'm pretty familiar with the underlying science. [Daniel Murphy, United States of America]	Taken into account, the sentence has been rewritten to improve the clarity.
33424	15	9	15	17	I would suggest adding Medhaug et al. (2017) (https://www.nature.com/articles/nature22315#f8), as they presented another analysis of the TOA imbalance ("with the most likely value around 0.75–0.93 W m−2") [Marcus Sarofim, United States of America]	Noted. The section in question addresses changes in TOA radiative fluxes. Medhaug et al. (2017) only discuss the mean value of EEI, not how it has changed.
18366	15	16	15	17	This reads as being contradictory information to the steadiness of TOA thermal outgoing radiation mentioned in line 7. [Gwenaelle GREMION, Canada]	Noted. The sentence in question refers to the period from 1985 onwards while the former is for March 2000-September 2016. After 2000 the OLR record from CERES shows no longer a significant linear increase. However, we no longer discuss the trends in the CERES fluxes due to the shortness of the records and severe space constraints.
51512	15	19			"energy exchange can be accurately tracked" .. maybe better "changes in the energy exchange" [Michael Schulz, Norway]	Taken into account.
13382	15	20	15	20	Change "millennium" to "20th century"?. Millennium has a long term connotation. [Govindasamy Bala, India]	Taken into account. Rewritten for clarity.
52022	15	21			Strictly speaking is higher right here given that it's a negative imabalance as I understand the preceding assessment upon which it is based? [Peter Thorne, Ireland]	Rejected. This is a positive imbalance and higher is the correct term.
53694	15	37	15	37	Check SROCC. [Jan Fuglestvedt, Norway]	Taken into account. The assessment of ocean heat content change appears in chapter 2.
14322	15	37			An assessment of the consistencey with EEI changes would be useful, including account for uncertain sampling of shallow tropical and Arctic oceans [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We have included a brief discussion of comparisons between TOA measurements and observed ocean heating rates in Section 7.2.2.1 of the FGD.
51514	15	39	16	48	I think there are numbers missing here on the actual W m-2 taken up by the ocean, as compared to TOA imbalances. There is only an uncertainty mentioned. [Michael Schulz, Norway]	NOT APPLICABLE. The text has been removed from this section.
18378	15	41	15	42	It might be useful to include the size of the trend here along with its uncertainty for scale. [Gwenaelle GREMION, Canada]	NOT APPLICABLE. The text has been removed from this section.
58042	15	41		42	Are these measures of the observational uncertainty in OHC, or the interannual and decadal internal variability? [Nathan Gillett, Canada]	NOT APPLICABLE. The text has been removed from this section.
19338	15	42	15	46	This sentence appears to imply that the satellite observations are only useful for constraining short-term variability in Earth's energy imbalance (EEI) while in-situ OHC data are better suited for monitoring its longer-term changes. There is no evidence of either of these statements given the short observational record of CERES and Argo. I would consider modifying the sentence to reflect this. [Norman Loeb, United States of America]	NOT APPLICABLE. The text has been removed from this section.
50846	15	43	15	43	Add 'The satellite measurements for TOA"… Because otherwise the sentence can be misleading [Karina von Schuckmann, France]	Rejected. Although measured from space the OHC satellite measurements are not valid for the TOA.
50848	15	44	15	46	The sentence is misleading, because this does not take inot accoiunt what has been said before n line 39, same page, i.e. that this 'constraint approach' holds only at time scales longer that annual. Moreover, it is also important to assure that those terminologies like 'long-term' and 'short-term' are used in a coherent manner - or better time scales should be always precised. [Karina von Schuckmann, France]	NOT APPLICABLE. The text has been removed from this section.
18368	15	48	15	52	The framing of the accumulated excess heat being "stored" in the oceans implies a sink or disappearance. Would it be more direct to explain here about the heat capacity of the oceans dominating that of the climate system? [Gwenaelle GREMION, Canada]	NOT APPLICABLE. The text has been removed from this section.
57864	15	51	15	52	What does consistent mean? Trend period? Variability? What is the current uncertainty? [Catia Domingues, Australia]	NOT APPLICABLE. The text has been removed from this section.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
57862	15	52	15	52	Include cross chapter box reference [Catia Domingues, Australia]	Taken into account. Croos-chapter box reference has been added in the FGD.
44456	15	52	15	52	Does box 9.2 ("Key processes driving sea-level change") support this statement on closure of the sea level budget? Perhaps a table in chapter 9? [Anne Marie Treguier, France]	TAKEN INTO ACCOUNT. We reference Cross-Chapter Box 9.2 in Box 7.2 and the Executive Summary.
57866	15	54	16	8	Although it is correct that in situ OHC estimates were the focus in past IPCC assessments and that they are limited by spatio-temporal availability in (meta)data and quality of historical subsuface observations, this continues to be true for this AR6 assessment, even though there are 3 new (trend) estimates independent from subsurface observations. Please refer to IQuOD effort/datasets. Please include Good reference. In addition, can a brief history of major advances in terms of OHC change over IPCC assessments be provided? Eg, AR4: only two or three estimates, mostly upper ocean, interdecadal variability not mached by CMIP3 models, closure sea level budget (SLB). AR5: more estimates in the upper ocean, deeper ocean, interdecadal variability due to XBT bias discovered and reduced, SLB closure. AR6: a larger number of estimates available; largest uncertainty due to choices in gridding methodology (Boyer et al., 2016) etc; despite that OHC trends from different groups agree with each other within uncertainties since 1993 and for different depth integrations (Johnson et al., BAMS 2019, submitted)... What are the limitations of the independent estimates? How can one rule out that the agreement between estimates based on direct ocean obs and independent methods is not fortuitous? Are their error bars too large? Good, S. INTERNATIONAL JOURNAL OF CLIMATOLOGY Int. J. Climatol. 37: 2260–2268 (2017) Published online 8 February 2016 in Wiley Online Library (wileyonlinelibrary.com) DOI: 10.1002/joc.4654 The impact of observational sampling on time series of global 0–700 m ocean average temperature: a case study [Catia Domingues, Australia]	Taken into account. The assessment of ocean heat content change appears in chapter 2.
18370	15	54	16	12	The first sentence of the paragraph "Past IPCC assessments of OHC" very closely mirrors the first two of the subsequent paragraph "In-situ subsurface" in a way which might be confusing because slightly different emphasis is given to the information (although the same pair of references are used). Might be worth considering rearranging these ideas so they only need to be delivered in one place? [Gwenaelle GREMION, Canada]	Taken into account. The assessment of ocean heat content change appears in chapter 2.
50850	15	54	16	48	Consistency check and exchange with chapter 2 needed- redundancy, and also inconsistencies in OHC assessment. [Karina von Schuckmann, France]	Taken into account. Revised text moved to chapter 2 ocean heat content assessment.
49132	15	54	16	48	This subsectoin is organized so that the assessment summary of global OHC changes appears earlier, followed by reviews of studies, and finally the assessment on top 700m OHC changes. It is unclear which evidence supports which assessment (i.e. global or top 700m). [Yu Kosaka, Japan]	Taken into account. Revised text moved to Chapter 2 in the FGD.
57868	16	1	16	2	Please include Meyssignac et al. reference (OCEANOBS'19) [Catia Domingues, Australia]	Taken into account. Text moved to Chapter 2.
52024	16	2	16	5	It feels like more details are required here as to what these approaches are. If, instead this is an extended introduction to following paragraphs consider whether it is required. As written this feels insufficiently detailed to justify the assessment so either remove or expand would be my suggestion here to avoid a potential accusation of a strawman argument here [Peter Thorne, Ireland]	Taken into account. Revised text moved to Chapter 2.
38608	16	2	16	7	Note that Nature has still not yet accepted the Authors' corrections to Resplandy et al 2018 (see Nature web page). It is evident that the correct uncertainty range for this study will be so large that its estimate will do almost nothing to strengthen evidence for OHC change. Moreover, its estimate is for OHC change over 1991 to 2016, so has no bearing on 1870s to 1971 or 1971 - present OHC change. And its data shows the rate of OHC change decreasing during the 1991 to 2016 period. Please make these points if Resplandy et al 2018 is cited. [Nicholas Lewis, United Kingdom (of Great Britain and Northern Ireland)]	Noted.
38610	16	2	16	7	It is simply not true that Zanna et al (2019) provides independent verification of the in-situ based estimates from 2005-present; its estimate is only half as great. The in-situ estimate of the Earth's energy imbalance over 2005-2015 given on page 7-12 is 0.71 W/m2, of which 0.03 W/m2 is non-ocean related, so 0.68 W/m2 for the full depth ocean. The Zanna et al full depth ocean heat content change estimate for that period is 0.35 W/m2 (linear trend) or 0.36 W/m2 (2015 to 2015 OHC change / 10). [Nicholas Lewis, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Revised text moved to Chapter 2 in the FGD.
19340	16	3	16	4	Given the significance of these new observational strategies it seems like it would be worthwhile devoting a sentence or two (or more) to describe them rather than merely referring the reader to journal articles. [Norman Loeb, United States of America]	Taken into account. Revised text moved to Chapter 2.
9312	16	7	16	48	There is near perfect duplication between lines 7-8 and 47-48 [philippe waldteufel, France]	ACCEPTED. We have revised the text.
38062	16	8	16	8	Confidence level inconsistent with chap 2, 3, 9 where it says virtually certain. Coordination needed. [Jean baptiste SALLEE, France]	ACCEPTED
57870	16	10	16	12	Please include Meyssignac et al. reference (OCEANOBS'19) [Catia Domingues, Australia]	Taken into account. Text moved to Chapter 2.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
41624	16	10	16	41	Some of these sections are quite review like - I suggest sharpening them up and focusing on what is new since AR5 [Amanda Maycock, United Kingdom (of Great Britain and Northern Ireland)]	TAKEN INTO ACCOUNT. We have substantially revised the text to make it more concise and focussed primarily on assessment.
57872	16	12	16	13	How is this accounted for the current ocean warming assessment? [Catia Domingues, Australia]	NOT APPLICABLE. The text has been removed from this section.
57874	16	19	16	22	New publicaton: F. K. Garry, E. L. McDonagh, A. T. Blaker, C. D. Roberts, D. G. Desbruyères, E. Frajka-Williams, and B. A. King. (2019) Model-Derived Uncertainties in Deep Ocean Temperature Trends Between 1990 and 2010. Journal of Geophysical Research: Oceans 124:2, 1155-1169. Online publication date: 22-Feb-2019. [Catia Domingues, Australia]	Taken into account. The assessment of ocean heat content appears in chapter 2.
57876	16	31	16	32	Not only this but also Argo float sensors are more precise than XBT sensors, however, high quality research cruise observations are still required to detect and remove Argo sensor drifts. Please refer to GO-SHIP program, for example, and related references. [Catia Domingues, Australia]	Taken into account. See discussion in chapter 2.
57878	16	34	16	41	What is the confidence that can be placed in ocean reanalyses compared to statistical analyses? Can all of them be used for climate change purposes? And, although in principle ocean reanalyses should provide a dynamically consistent estimate, their diversity is huge. Should they be used extensively along with statistical estimates in the AR6 assessment? [Catia Domingues, Australia]	Taken into account. The decision was taken not to include ocean reanalyses as a primary line of evidence in the assessment of ocean heat content change, which is presented in chapter 2.
18372	16	34	16	41	The word "however" is used a lot in this paragraph and others nearby and might be useful to revise it because it gives the impression that there is some disagreement or conflict in what is being described, when often it's just additional approaches or information. Line 36-37 is a good example of this. The sentence could perhaps read: "Recently, ocean state estimates and reanalyses based on dynamical ocean models have also been used to gain insights into O..", for example. The "However" in line 40 could be replaced by "It is important to note that...". [Gwenaelle GREMION, Canada]	Not applicable. Section has been rewritten.
58044	16	43		45	Given that these two studies are key to supporting the increased confidence in pre-1971 historical warming estimates more discussion and assessment of them should be added. [Nathan Gillett, Canada]	Taken into account. Revised text moved to Chapter 2.
8600	16	48	16	48	It could make sense mentioning here the model uncertainty on OHU, to be consistent with the previous sections. A reference for that could be Exarchou et al. 2015 Exarchou, E., Kuhlbrodt, T., Gregory, J. M., and Smith, R. S.: Ocean Heat Uptake Processes: A Model Intercomparison, Journal of Climate, 28, 887–908, https://doi.org/10.1175/JCLI-D-14-00235.1, 2015 [Valerio Lembo, Germany]	(considered in FGD). Not applicable. Revised text has been moved to Chapter 2.
42428	16	51			Once again, the ground heat component is being ignored. This is a mistake. Ground heat flux is important and this term is a fundamental compament of the surface energy baance. There are estimates for heat content as I mentioned in my comments to Chapter 2, and also the heat fluxes can be estimated from borehole temperature data and the short term may be likely estimated from the FLUXNET network database. data exist for this term and they must be included after all the ground heat flux will play a role in the estimate of the ECS. Again, the references for Box 7.2 Figure 1, pannel d are missing in the text. Please see above comment for references. There should be an updated paper from our team by the end of this year. [Hugo Beltrami, Canada]	Taken into account. References to the ground heat flux have been included in Section 7.2.2.2
14326	16	51			Some assessment of downward longwave radiation in Section 7.2.2.3 would be useful, even if referring to AR5, since this captures the temperature and water vapour signal of climate warming [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Accepted, a small paragraph with respect to downward longwave radiation has been added as follows: " AR5 reported indications for an increase in downward thermal radiation over recent decades, in line with expectation from an increasing greenhouse effect. Updates of the longest observational records from the Baseline Surface Radiation Network continue to show an increase at the majority of the sites, in line with an overall increase predicted by climate models on the order of 2 Wm-2 per decade over the coming decades"

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
45542	16	53	18	10	It is again not clear how a lot of this material is relevant to global warming. The authors need to think hard about how all material in the chapter links to important questions that the chapter needs to answer. Do these variations tell us anytihng about past limate change? Aerosol raditive forcing? ...? [Steven Sherwood, Australia]	Taken into account, the revised section places more emphasis on the links between the surface energy flux changes and climate change as well as aerosol forcing. The first paragraph of this subsection was expanded as follows: "Over the past decades, these changes may have substantially modified key elements of climate change, such as global and regional warming rates (Wild 2016, Li et al. 2016, Du et al. 2017), glacier melt (Ohmura et al. 2007, Huss et al. 2009), the intensity of the global water cycle (Wild 2012) and terrestrial carbon uptake (Mercado et al. 2009) . Further, these changes may also be used as emergent constraints to quantify the aerosol effective radiative forcing (see section 7.3.3.3)" It has already been stated in the FOD that the inability of climate models to reproduce the full exent of global dimming and brightening points to to inadequacies in the representation of aerosol mediated effects or related emission data, which is of key relevance for climate change.
57880	16		16		What about GOHC estimates for paleo timescales? [Catia Domingues, Australia]	Noted. See discussion in Chapter 2. The focus of section 7.2 is the instrumental period and comparisons of radiative foricng, radative response and increase in global energy inventory (see Box 7.2).
13386	17	1	17	30	Was there an increased aerosol emissions on the global level during the dimming period? [Govindasamy Bala, India]	Noted, yes, aerosol emissions have increased over this period
18374	17	1	17	30	Should the confidence in the trends in surface solar radiation between the 1950s -1980s discussed in AR5 be revised in light of the new information about the incoherence of tendancies in the more recent time-periods, where higher quaility/more data sources are available? Does it imply that there may have been an overconfidence in the past conclusions because of that limited information availability? Or is it that the coherent trend has become incoherent, and if so, what's the signifigance of this? It would also be useful to include a reference to the particular section of AR5 in which this brightening and dimming was discussed so that the reader can explore this comparison themselves. [Gwenaelle GREMION, Canada]	Taken into account. The reference to the specific section of AR5 has been added (2.3.3). The pattern has indeed been more coherent in the dimming period than in recent years, which is, however, not due to data quality/availability issues. Rather the introduction of air pollution regulations, which were not consistently implemented around the world may have contributed to the more incoherent pattern in recent years.
6896	17	10	17	10	I suggest that the authors add a reference: Wang, K., Q. Ma, Z. Li, and J. Wang (2015), Decadal variability of surface incident solar radiation over China: Observations, satellite retrievals, and reanalyses, Journal of Geophysical Research: Atmospheres, 120(13), 6500-6514. This paper provides comprehensive information on the data quality of surface solar radiation in China. [KAICUN WANG, China]	Accepted, reference has been added.
38612	17	14	17	17	Mention other possible reasons for climate models not reproducing dimming and brightening: a) non-aerosol related deficiencies in model cloud and circulation etc. representation generally and b) internal climate system multidecadal variability [Nicholas Lewis, United Kingdom (of Great Britain and Northern Ireland)]	Accepted, we added a related statement. "This does not rule out the possibility that non-aerosol related deficiencies in the representation of model-simulated clouds and circulation as well as an underestimation of natural variability could further contribute to the lack of dimming and brightening in the models."

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
58048	17	14		17	What does this tell us about aerosol forcing changes in climate models? Cross-reference Chapter 6 if this is discussed there. [Nathan Gillett, Canada]	Noted, It has already been stated in the FOD that the inability of climate models to reproduce the full extent in global dimming and brightening points to inadequacies in the representation of aerosol mediated effects or related emission data. See also response to review comment ID 38612.
6898	17	17	17	17	New generation of global atmospheric reanalysis (i. e., MERRA2) assimilated atmospheric aerosol optical depth from satellite observations does improve the variability of surface solar radiation in China. Reference: Feng, F., and K. Wang (2019), Does the modern-era retrospective analysis for research and applications-2 aerosol reanalysis introduce an improvement in the simulation of surface solar radiation over China?, International Journal of Climatology, 39(3), 1305-1318. [KAICUN WANG, China]	Accepted, we added a related statement: "The inclusion of assimilated aerosol optical depth inferred from satellite retrievals in the MERRA2 reanalysis helped to improve the variability of surface solar radiation in China (Feng and Wang 2019)"
6900	17	17	17	17	Trend of surface solar radiation is an essential determining factor of regional warming pattern (Du et al, 2017). The bias in simulated surface solar radiation generally explain the biases in regional warming pattern in the reanalyses (Zhou et al, 2017, 2018), in particular that of daily maximum temperature (Du et al., 2018). References: Du, J., K. Wang, J. Wang, and Q. Ma (2017), Contributions of surface solar radiation and precipitation to the spatiotemporal patterns of surface and air warming in China from 1960 to 2003, Atmos. Chem. Phys., 17: 4931-4944. Du, Jizeng, Kaicun Wang*, Jiankai Wang, Shaojing Jiang, Chunlue Zhou (2018), Diurnal Cycle of Surface Air Temperature within China in Current Reanalyses: Evaluation and Diagnostics, Journal of Climate, 31, 4585-4603 Zhou, Chunlüe, He Yanyi, and Wang Kaicun, 2018: On the suitability of current atmospheric reanalyses for regional warming studies over China. Atmospheric Chemistry and Physics, 18, 8113-8136. Zhou, Chunlüe, Wang Kaicun, and Ma Qian, 2017: Evaluation of eight current reanalyses in simulating land surface temperature from 1979 to 2003 in China. Journal of Climate, 30, 7379-7398. [KAICUN WANG, China]	Taken into account, see response to comment ID 45542. references have been added.
57860	17	19	15	20	Text suggestions: "In sum,…. since 2001" (to replace summation and turn of the millenium) [Catia Domingues, Australia]	Taken into account. Rewritten to "in summary…"
14324	17	24			A trend in surface radiation for 2000-2014 is difficult to interpret in view of the short record so this could be removed [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account, the shortness of the record is now emphasized.
44792	17	28	17	30	This needs more detail. [Astrid Kiendler-Scharr, Germany]	Taken into account, see response to comment ID 45542
38616	17	34	17	36	Storelvmo (2016) shows (Figure 1 data) insignificant (-0.21) correlation over the full period 1964-2010 between global downwelling solar radiation at the surface and global SO2 emissions (which other anthropogenic aerosol emissions are highly correlated with). That is evidence for little contribution to solar dimming/brightening from anthropogenic aerosols. [Nicholas Lewis, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. A new study (Julsrud et al., 2019) shows that the long-term surface solar radiation trend (dimming & brightening) is highly correlated with aerosol emissions if the interannual variability (which is well explained by variations in cloudiness) is removed. This paper has been cited in the SOD.
46452	17	36	17	41	after Wild et al. 2016 on line 36, and before Pfeifroth et al. 2018 on line 41, add the citation of Li, Z., et al. (2016) for it includes a comprehensive review of studies on the trends of surface radiation budget due to changes in aerosol and cloudiness across Asia. Li, Z., et al., 2016: Aerosol and monsoon interactions in Asia, Rev. Geophys., 10.1002/2015 RG000500. [Zhanqing Li, United States of America]	Accepted, reference has been added, but in the second paragraph of this subsection, where the reference fits better, and in addition also in the first paragraph of this subsection in response to review comment 45542.
51516	17	41	17	44	"the relative contribution of aerosol and clouds...depend on prevailing pollution levels": can the reported aerosol and cloud and interaction contributions be substantiated with numbers possibly? [Michael Schulz, Norway]	Noted, this is a conceptual framework which has not yet been quantified to the extent that numbers can be given.
44794	17	41	17	44	Check with Chapter 6 for links and refer to. [Astrid Kiendler-Scharr, Germany]	Taken into account, we made a link to section 7.3.3. where this is further discussed. See also response to comment 45542.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
18376	17	42	17	44	The cloud-mediated aerosol effects are not addressed very thoroughly in this section, although they are an important aspect of the surface solar flux response. It is possible that this is sensible given that the discussion will occur in a separate part of the report, but in that case it may be useful to include a reference. At the moment, it reads as if there is little understanding of the dependence of cloud behavior on aerosols. For example, see Gryspeerdt et al. 2017. Constraining the instantaneous aerosol influence on cloud albedo. PNAS, 114 (19) 4899-4904; DOI: 10.1073/pnas.1617765114. [Gwenaelle GREMION, Canada]	Taken into account, this discussion indeed takes place in detail in section 7.3.3.2. (Aerosol cloud interactions). A link is added to section 7.3.3
58054	17	46		51	Briefly describe the physical processes driving these trends. [Nathan Gillett, Canada]	Taken into account, the drivers of these trends have already been mentioned in FOD page 12, lines 48-51. We slightly revised this sentence in the 4th paragraph of Section 7.2.1 to make it clear "Turbulent flux uncertainty estimates consider the uncertainties in their major drivers, namely near surface wind speed, temperature and humidity,..."
13384	17	47	17	51	The trends per decade in latent and sensible heat fluxes seem too large. For instance, a trend of 5 Wm^-2 per decade in latent heat fluxes over 6 decades indicate a trend of 30 Wm^-2. What is the reason for such large trends? [Govindasamy Bala, India]	Taken into account, the 4–7 W m-2 decade-1 trend is for western boundary current regions in the reanalyses and not for global averages. There is no value provided for the global ocean in Gulev and Belyaev (2012), but other regions show negative trends, so the overall trend is likely smaller. We reformulated this statement to clarify this. The trends in Gao (2013)from a satellite-based product a quasi-global average (60°S – 60°N) of 5.15 W m-2 decade-1 from 1988 to 2008 indeed seem unrealistic and has been removed.
38614	17	47	17	51	Global ocean latent heat trends of ~5 W/m2/decade over 1948-2008 and 1988-2008 appear to be well in excess of that implied by the Clausius–Clapeyron relation, the average latent heat flux and the SST trend. Or am I missing something? [Nicholas Lewis, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account, see response to review comment ID 13384
51518	17	48	17	51	are these large ocean latent heat fluxes consistent with other heat trends? [Michael Schulz, Norway]	Taken into account, see response to review comment ID 13384. Moreover, as highlighted in FOD page 12 lines 41–43, the uncertainties are large and reduce confidence in the estimated trends.
58050	17	48			Clarify the direction of these flux trends - is this positive upward? [Nathan Gillett, Canada]	Noted, yes, this is positive is upward, we believe the term "increase" is unambiguous.
58052	17	49			What kind of extremes are these - daily, hourly? And rather than giving the largest trends ('can be higher than 15 W/m^2') consider specifying the global average change, or average change over certain regions. [Nathan Gillett, Canada]	Taken into account, we reformulated the paragraph to focus on trends of the average fluxes. Trends in extreme 6-hourly fluxes are not mentioned anymore.
18380	17	53	18	1	I could not find the values terrestrial latent heat flux change referenced here from Mueller 2013 in that cited paper. If it is the case that it has been derived directly by the author from the dataset the paper introduces, that might be useful to clarify that. The wording in the present report suggests a clear reversal of a decadel trend of terrestrial latent heat flux in 1997/1998 which would require futher examination. It is possible that the underlying data does not in fact suggest these two decadel-scale reversing trends and that they are instead artefacts of the analysis approach. [Gwenaelle GREMION, Canada]	Taken into account, Indeed Mueller (2013) provides trends in land evapotranspiration in units of mm year-2, which has been converted for this report to W m-2 decade-1 by multiplying by the latent heat of vaporization. The trend reversal in the reference is linked to enhanced soil moisture limitation particularly in the Southern Hemisphere as already mentioned in the FOD.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
18382	17	53	18	11	It may be useful to explain if/why it is important to be able to accurately measure turbulent heat flux, given that it is proving difficult to capture. Is this an issue for building other conclusions? [Gwenaelle GREMION, Canada]	Taken into account, the relevance of the components of the surface energy balance for various aspects of climate change are now emphasized in the introductory paragraph of Section 7.2
38618	18	15	18	18	It is very unclear whether anthropogenic aerosols have substantially contributed to surface solar radiation changes. See my comment on Ch.7 page17 lines 34-36. I think "have substantially contributed" should be changed to "might have substantially contributed". [Nicholas Lewis, United Kingdom (of Great Britain and Northern Ireland)]	Accepted, reformulated as suggested
40738	18	23			Box 7.2. I am really unsure of a box that extend for several pages plus figures. I would point out that the box contains two fairly distinct concepts, global heat balance and poleward heat transport. The box could be split, and significant amounts moved to plain text. For example I learned a lot from the paragraph starting page 20 line 51, but I have difficulty seeing why it is emphasized as a box. [Daniel Murphy, United States of America]	Taken into account. There was an error in Box 7.2 and it included more text than planned. This is now fixed.
25766	18	25	18	49	ZJ; give also in W m-2. Especially as different time periods are involved. [Stephen E Schwartz, United States of America]	Taken into account. We retain ZJ in the Box 7.2 text indicate equivalent W m-2 in the revised figure.
38622	18	26	18	27	The volcanic ERF component in Box 7.2 Figure 1e is shown as a cumulative negative ERF over 1971-2015. It should be almost zero, if (as seems reasonable) the 1750-2015 average is taken as the baseline average level of volcanic ERF. Has an allowance been made for lower stratospheric/ upper tropospheric volcanic AOD been made for recent years without a corresponding adjustment to the baseline period AOD, in Piers Forster's recent dataset? Since some 70% of the recent lower stratospheric/ upper tropospheric volcanic AOD is estimated to be natural (see, e.g, ftp://iacftp.ethz.ch/pub_read/luo/CMIP6/StratAerosols_CMIP6_Updates_v3.0.pdf), it is essential to make a corresponding adjustment to the baseline estimate of average preindustrial volcanic AOD and forcing. [Nicholas Lewis, United Kingdom (of Great Britain and Northern Ireland)]	TAKEN INTO ACCOUNT. The FOD was based on placeholders with all numbers updated for SOD.
38620	18	28	18	29	The figures of 1200, 860 and 1510 ZJ disagree to Box 7.2 Figure 1b. Have the Figure 1b data been changed from those per an earlier version and. if so, what forcing component was changed and why? [Nicholas Lewis, United Kingdom (of Great Britain and Northern Ireland)]	TAKEN INTO ACCOUNT. The FOD was based on placeholders with all numbers updated for SOD.
49134	18	28	18	40	I expect that the numbers will be updated for SOD, but why these numbers are smaller than those shown in Box 7.2, Figure 1b, c? [Yu Kosaka, Japan]	Taken into account. The FOD was based on placeholders and all assessment numbers have been revised for the final report.
18384	18	28	18	43	Is the net energy storage change listed on Line 43 340 ZJ not 360 ZJ (1200 ZJ estimate IN minus 860 ZJ OUT = 340 ZJ) [Gwenaelle GREMION, Canada]	TAKEN INTO ACCOUNT. The FOD was based on placeholders with all numbers updated for SOD.
18832	18	32	18	32	expound on the "Planck response" or if explained in the document, refer to which chapter. [Gwenaelle GREMION, Canada]	TAKEN INTO ACCOUNT. We refer to section 7.4.2, where the Planck response and other climate feedbacks are described and assessed.
38624	18	33	18	40	The radiative response estimated using AGCMs driven by datasets of observed SSTs and sea-ice concentrations varies hugely according to the AGCM used and the observational dataset used. Moreover there may be biases common to all the AGCMs used. Over the post-1971 period common to amipPiForcing simulations by all 8 AGCMs used in Andrews et al 2018 (which extended to 2004), based on the AMIP II SST and sea-ice dataset, the cumulative radiative response spanned a range of almost 2:1 (260-509 ZJ). Further, the cumulative radiative response using the more recent HadISST2 SST and sea-ice dataset evidently lay well outside this range, for at least one of two AGCMs for which it was tested (see their SI). And, given other uncertaintlies annd the small model set, it is unrealistic to conclude that the range in the Andrews et al (2018) data provides even a likely range, let alone a very likely range. [Nicholas Lewis, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The assessment has been substantially revised since the first order draft and results focus on the likely rather than very likely range.
25768	18	41	18	49	The assertions "agrees well" and "closed within the estimated uncertainties"seem hardly justified when the modeled quanttity is uncertain even in sign and when the measured quantity is given to 3 significant figures with no stated uncertainty. Perhaps better: "No inconsistency." [Stephen E Schwartz, United States of America]	Taken into account. The assessment has been revised substantially since the first order draft.
38626	18	43	18	43	Why is the implied energy storage 360 ZJ, not 1200 ZJ - 860 ZJ = 340 ZJ? [Nicholas Lewis, United Kingdom (of Great Britain and Northern Ireland)]	TAKEN INTO ACCOUNT. The FOD was based on placeholders with all numbers updated for SOD.
57882	18	45	18	49	Missing cross reference/boxes with relevant chapters? [Catia Domingues, Australia]	References to other chapters added.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
38628	18	47	18	49	The estimated uncertainties are far too low, and the agreement within true uncertainties between forcing plus response estimated by a small set of GCM amipPiForcing simulations (based on a single stitched together, old observational dataset) and observed storage change provides very weak evidence for the estimates of climate sensitivity in Chapter 7, which are not derived from the radiative response estimated in this Box 7.2. [Nicholas Lewis, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The assessment has been revised substantially since the first order draft, with particular emphasis on revised uncertainty estimates.
57884	18	51	19	5	Are the Cheng et al estimates truly independent of CMIP models? Do their mapping procedure rely on CMIP model information to infill gaps of the in situ observing system? Should these empirical results be considered for assessment? 2014, NCC, Quantifying underestimates of long-term upper-ocean warming Paul J. Durack1*, Peter J. Gleckler1, Felix W. Landerer2 and Karl E. Taylor1 [Catia Domingues, Australia]	NOT APPLICABLE. The text has been removed from this section.
45544	18				Box 7.2 seems very long and somewhat unfocused. Based on the title I expected it to concern the global-mean budget, not to cover also the latitudingal distribution and transport. Again, if you are going to cover that ,you need to flag to readers how it is relevant (for example does it relate to anticipated polar amplification?) [Steven Sherwood, Australia]	Taken into account. There was an error in Box 7.2 and it included more text than planned. This is now fixed.
38630	19	1	19	2	Historical climates have not evolved in the same way as reality in most CMIP5 models, having regard to warming patterns as well as global mean warming, not just "in some of the models". [Nicholas Lewis, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. Paragraph has been removed in the FGD.
57886	19	7	19	13	What is meant by good agreement and mains signals? This statement seems more optimistic compared to the differences stated in the paper, particularly in terms of the time evolution. [Catia Domingues, Australia]	Not applicable. Paragraph has been removed in the FGD.
49136	19	9	19	9	"ERB" is underfined. [Yu Kosaka, Japan]	Not applicable. Section has been rewritten.
18834	19	15	19	15	"all RCP scenarios" - but Box 7.2 shows only scenarios 2.6 and 4.5. What about 6.0 and 8.5? Especially 8.5 which is the high GHG scenario? [Gwenaelle GREMION, Canada]	Not applicable. The Figure has been removed.
9304	19	20	19	22	This sentence goes on to develop the explanation mentioned on p15 line 09. Now most readers will easily agree that the reduction of ice cover decreases the albedo. The fact that the role of water vapour increase has similar magnitude, on the other hand, is not expected. Indeed Donahue et al quote estimated feedback coefficients equal to 0,3 Wm-2K-1 for both cases. However in section 7.4.2.2 the WV feedback coefficient is estimated at 1.12 Wm-2C-1 (page 56 line 22). Hence there is a large apparent discrepancy which I was unable to understand.. Possibly, both are right but use the same word to designate differently quantities. [philippe waldteufel, France]	NOT APPLICABLE. The text has been removed from this section.
18838	19	20	19	22	"This arises because…." Make clear that this statement is a result of a feedback. I've just inferred it from the "reduced ice cover and increased atmospheric water vapour" [Gwenaelle GREMION, Canada]	NOT APPLICABLE. The text has been removed from this section.
14328	19	21			The increase in absorbed shortwave can also include a contribution from positive low altitude cloud feedback (e.g. Trenberth & Fasullo, 2009; doi:10.1029/2009GL037527) [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	NOT APPLICABLE. The text has been removed from this section.
18836	19	25	19	25	"GMST" - has not been spelled out prior to this instance in this chapter. [Gwenaelle GREMION, Canada]	Taken into account. Now defined
37808	19	25			Can "GSAT" replace "GMST" here? GSAT is a better-defined quantity than GMST. Detailed comments have been made on Chapter 2 in this regard. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. Paragraph has been removed in the FGD.
18840	19	27	19	27	"shorter" - as in annual/yearly? Shorter than decades? Clarify relative "shortness" of timescales. [Gwenaelle GREMION, Canada]	Not applicable. Paragraph has been removed.
38632	19	33	196	40	Please state the baseline anomalization periods or year used for each panel of Box 7.2 Fig1 and the overall period covered. [Nicholas Lewis, United Kingdom (of Great Britain and Northern Ireland)]	ACCEPTED
8602	20	4	20	4	I think somewhere about here the review paper by Lucarini et al. 2014 should be mentioned on the state-of-the-art of meridional heat transport uncertainty ranges in CMIP: Lucarini V, Blender R, Herbert C, et al (2014) Mathematical and physical ideas for climate science. Rev Geophys 52:809–859. doi: 10.1002/2013RG000446 [Valerio Lembo, Germany]	Taken into account. Reference added.
18848	20	5	20	8	"...where majority of models show increased poleward…" Explain why, or at least provide a hypothesis why this trend was observed. [Gwenaelle GREMION, Canada]	Taken into account. Section 9.2 is now referenced, where mechanisms for this are discussed.
18842	20	7	20	7	More like 60 degrees than 70 degrees? [Gwenaelle GREMION, Canada]	Rejected. 70 degrees is more accurate for this. See Figure 7.5f.
18844	20	8	20	8	"decreased poleward atmospheric heat transport" - refer to figure 7.5e [Gwenaelle GREMION, Canada]	Accepted.
18846	20	10	20	10	Armour, et al. 2019 has no available online resource or doi, nor volume or issue number. Is this just submitted, ASAP, in preparation, etc? [Gwenaelle GREMION, Canada]	Taken into account. Reference added.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
9968	20	11	20	11	Mechanism is explained in Lucarini and Ragone Rev. Geophys 49, RG101 (2011 [Valerio Lucarini, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. This sentence describes the causes for changes in the partitioning between moist and dry energy transports. As far as I can determine, Lucarini and Ragone only calculates and considers the net enthalpy transport and does not address this partitioning or its changes.
49138	20	13	20	30	Here the "improved process understanding" supports the high confidence assessment. The following statements in the paragraph, however, are descriptive and only explains how the changes in ocean heat transports are manifested (e.g. through AMOC slowdown), but processes on how global warming induces those changes are missing. [Yu Kosaka, Japan]	Not applicable. This text has been modified and moved to Chapter 9.
13978	20	15			There is a large disconnect between the behaviour of the Southern Ocean SST (and its bias), ocean freshening, precipitation and the biases in the radiation budget over the SO. These are unlikely to be independent of one another. The SW bias may be contributing to the SST bias. Precipitation changes may affect freshening. The chapter reads like these groups don't interact at all. [Steven Siems, Australia]	Not applicable. This text has been moved to Chapter 9 where biases in Southern Ocean SSTs, temperature trends, and salinity trends are discussed.
8604	20	17	20	18	The role of the Deacon cell should be addressed also here. [Valerio Lembo, Germany]	Not applicable. This text has been modified and moved to Chapter 9.
38634	20	18	20	21	Untrue. There is little observational evidence of weakening in AMOC strength to date. Moreover the source of most variability in AMOC strength/oceanic heat transport has recently been found(Lozier et al., Science 363, 516–521 (2019)) to lie in a quite different area of the ocean than previously thought and, I believe, implicated in model simulations. [Nicholas Lewis, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. This text has been modified and moved to Chapter 9.
8606	20	29	20	30	In Irving et al. 2019 the problem is addressed in terms of attribution and the poleward transport anomalies are related with asymmetries in the ocean heat uptake in the historical period and projections Irving, D. B., Wijffels, S., & Church, J. A. (2019). Anthropogenic aerosols, greenhouse gases, and the uptake, transport, and storage of excess heat in the climate system. Geophysical Research Letters, 46,4894– 4903. https://doi.org/10.1029/2019GL082015 [Valerio Lembo, Germany]	Not applicable. This text has been modified and moved to Chapter 9.
8608	20	35	20	36	A comparison of implied atmospheric/oceanic heat transports and transports inferred from direct computations of static energy is already found in Trenberth et al. 2001 and it would be would be worth mentioning (as setting the standard of the problem) Trenberth KE, Caron JM, Stepaniak DP (2001) The atmospheric energy budget and implications for surface fluxes and ocean heat transports. Clim Dyn 17:259–276. doi: 10.1007/PL00007927 [Valerio Lembo, Germany]	Taken into account. Reference added.
13388	20	36	20	36	"ERF from CO2 peaks…." Is this TOA ERF or atmospheric ERF? [Govindasamy Bala, India]	Noted. This refers to forcing at the TOA using the definition of ERF as defined in the chapter.
38636	20	38	20	40	Doesn't the Planck feedback preferentially remove energy from the tropical atmosphere? [Nicholas Lewis, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. With polar amplification, its radiative contribution ends up being comparable at high and low latitudes so it is not mentioned here.
14330	20	38			While water vapour increases add heat to the atmosphere, this is not necessarily the case for cloud based on O'Gorman et al. (2012) doi: 10.1007/s10712-011-9159-6, Fig.4. Even for water vapour, increases at low altitude increase cooling (to the surface) whereas there is a strong heating for increases in the upper troposphere (e.g. Previdi 2010 ERL 10.1088/1748-9326/5/2/025211 Fig. 2). [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Text added to clarify that this refers to top-of-atmosphere heating of the atmospheric column, and that this is the net effect of clouds from models.
8610	20	43	20	44	People often refer to this compensation as “Bjerknes compensation" [Valerio Lembo, Germany]	Taken into account. Text revised to note this.
17944	20	47	20	47	That degree of compensation… probably depends on a couple of more feedbacks, not only radiative; wind field structures, turbulence and moisture fluxes ought to play roles there. [Branko Grisogono, Croatia]	Noted. There are of course many factors that will impact this compensation. But a simplification is achieved by considering the top-of-atmosphere energy budget as has been done in the cited papers. Then, radiative feedbacks are seen as primary, permitting a mechanistic understanding of the degree of compensation. The compensation must be near-perfect in regions of weak radiative response to space with surface warming, for instance.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
14332	21	1		10	Box 7.2 could signpost discussion in 8.2 (including Siler et al. 2018a,b) or alternatively overlap/consistency can be checked and modified as appropriate [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Reference to 8.2 added.
8612	21	13	21	15	I would not enter into details of the dispute on maximization principles. There is quite some amount of literature providing sharply different thoughts about the maximum entropy production principle (MEPP) and its extensions (e.g. maximum power, maximum kinetic energy dissipation), while no unified vision has been yet achieved. If the problem is not addressed here in all its complexity (mentioning contributions by Ambaum, Goody, Dewar, Herbert, Paillard, Fraedrich, Lucarini), or the conclusion here drawn looks a bit specious… An updated discussion on the topic can be found in this paper under review on ESDD: https://www.earth-syst-dynam-discuss.net/esd-2019-6/ [Valerio Lembo, Germany]	Not applicable. Good points, thank you. We unfortunately do not have room to give discussion of this topic the space it deserves, so we have removed this text.
18850	21	15	21	15	Dyke and Kleidon, 2010 might be a questionable article in a questionable journal, although the other references seem credible enough. [Gwenaelle GREMION, Canada]	Not applicable. This text has been removed.
18852	21	15	21	15	Expound on the "maximization of entropy production" and how this relates to the observations. How is this theory applied to the physical observations, ex. Temperature gradients? [Gwenaelle GREMION, Canada]	Not applicable. We unfortunately do not have room to give discussion of this topic the space it deserves, so we have removed this text.
8614	21	22	21	27	I think that crucial is also here our understanding of the influence oceanic heat transports have on the atmospheric circulation, not only limiting the mentioned Bjerknes compensation (Rose and Ferreira 2013, again, but also e.g. Knietsch et al. 2015 dig quite deep in the topic). Knietzsch M-A, Schröder A, Lucarini V, Lunkeit F (2015) The impact of oceanic heat transport on the atmospheric circulation. Earth Syst Dyn 6:591–615. doi: 10.5194/esd-6-591-2015 [Valerio Lembo, Germany]	Noted. This is an interesting area of research, but we do not have space to give it adequate attention as this chapter is focused on Earth's energy budget. Atmospheric circulation and its changes are discussed in Chapters 2, 3, and 4.
38638	21	22	21	27	Make clear that these are changes in atmospheric circulation that models project will occur under CO2 forcing, and state to what extent they are validated by observational evidence. E.g., has a slowdown (as opposed to a poleward shift) in the Hadley circulation been robustly observed? [Nicholas Lewis, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Text revised to make clear that these are model projections. Observations of these phenomena are covered in Chapter 2.
41626	21	34	25	3	this section is rather technical and methodological but you could say more about what are the different adjustments and how do they compare across forcings (e.g. based on Smith et al (2018) and others) [Amanda Maycock, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account: Text added to explain this and smith reference added.
50942	21	36	21	46	Water vapor is of course an important GHG. The section lists two processes that could affect atmospheric water vapor as included in the ERF definition (dTè dWV, line 42, and CO2-physè evapotranspiration, line 44). However, also aerosol effects on clouds and precipitation (and thus evaporation of cloud droplets) could change WV and should be included in the definition. This might be more important if new research show that INPs are affected by anthropogenic activity. [Terje Berntsen, Norway]	Taken into account. Thanks, we have added a couple of sentence to address this.
13390	21	43	21	43	Reference should be included for microphysical effects of aerosols being considered as rapid adjustment or part of IRF. [Govindasamy Bala, India]	Rejected. We believe that this is known fact.
38640	21	44	21	44	How can, as a matter of definition, effects on clouds possibly be part of the initial IRF? [Nicholas Lewis, United Kingdom (of Great Britain and Northern Ireland)]	Accepted this phrase has been deleted
38642	21	44	21	46	Mention also that initial changes in SST patterns, with zero effect on GMST, can also be considered as components of rapid adjustments (Rugenstein et al 2016 DOI: 10.1175/JCLI-D-16-0312.1) [Nicholas Lewis, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account: The text has been clarified to state that SST adjustments should definitely not be considered part of the forcing, and Rugenstein et al. cited.
52028	21	44			Is this a typo and IRF meant to be ERF? Otherwse perhaps you need to define IRF as AFAICT this is its first use and you are defining remaining acronyms in this paragraph. [Peter Thorne, Ireland]	Not applicable - the section has been rewritten. IRF is now defined at first use.
51520	21	52	21	52	"ERFs represent the ultimate temperature response": represent is maybe not precisely the rigth word here. [Michael Schulz, Norway]	Taken into account. Rewritten to clarify.
47896	21		46		There is potential overlap in ERF topics between chapter 6 (Section 6.3, Section 6.3.1.4) and Chapter 7 (Section 7.3, Section 7.3.3), for example, methods of ERF, emergent constraints, SO2, methane, aerosols. Could all outs / cross references to each seciton be included. Furthermore chapter 7 hold the main assessment of ERF, including th indepth introduction, but ch6 comes before chapter 7. Could chapter 6 refer to chapter 7 with respect to this? Could assessments on ERF in both chapters be systemised to have similar approaches? This would make it easier for the reader. [WGI TSU, France]	Taken into account. Cross refencing with chapter 6 has been added following collaboration with Ch 6 on this.
13392	22	4	22	4	Tang et al. 2019 (JGR) discuss nine methods of calculating ERF [Govindasamy Bala, India]	Taken into account: Added Tang et al. citation to following paragraph

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
18854	22	4	22	6	"The first method is to regress…" simplify this whole sentence to something like, "a linear regression was performed" and show an equation of the parameters showing the slope and the y-intercept. Line 6, "intercept with the Y axis" is verbose and unnecessary if the equation is shown succinctly. [Gwenaelle GREMION, Canada]	Taken into account: Reworded
18388	22	4	22	15	The first ERF method (regression) is discussed in several-sentence detail while the second is discussed in 2, even though this second method is stated to be the more certain one. The first method detail can be likely reduced to condense these methods, and could potentially even combined with the following paragraph by inserting these methods after the first sentence of the paragraph starting on line 17. [Gwenaelle GREMION, Canada]	Taken into account: More detail provided on fixed-SST method
38644	22	4	22	15	There is no logic in excluding rapid SST pattern adjustments not associated with a change in GMST when measuring ERF. It is a conceptually much more appropriate method than that using fixed SSTs, as the whole point of ERF is to be able to apply the linear feedback - forcing framework, and for most real world purposes are measured over periods that rapid SST adjustments (as well as rapid atmospheric and land surface) adjustments are already complete. A regression based definition of ERF should therefore be used in AR6. Regressing TOA flux against GMST over years 2-10 or 2-20 of step CO2 change simulation data avoids influence from either the period during which rapid adjustments are very incomplete or curvature of the relationship, while including rapid adjustments in SST patterns. It has the further advantage over fixed SST simulations of avoiding uncertainty as to what feedback value to use to adjust for land surface warming. For the 20 CMIP5 models for which published fixed SST estimates of CO2 ERF (unadjusted for land warming) are available, the average is only 1% different from that estimated by regression over years 2-10 of abrupt 4x CO2. That suggests that adjustment for land warming cancels out with the effects of rapid SST pattern adjustment. [Nicholas Lewis, United Kingdom (of Great Britain and Northern Ireland)]	Rejected: The text has been clarified to state that SST adjustments should definitely not be considered part of the forcing, and to explain how the adjustment for land-surface warming is addressed. This section already assesses the methods for calculation of ERF and finds the fixed SST method the most appropriate (see text).
18386	22	4	22	27	I would rearrange these two paragraphs as ERF definitions are given after how it is calculated, which can be counter-intuitive. [Gwenaelle GREMION, Canada]	Rejected: More detail now provided to make it clearer how the two paragraphs are related.
58056	22	4			Insert 'from models' after 'calculate ERF'. [Nathan Gillett, Canada]	Taken into account: Reworded
13394	22	8	22	10	Excellent point. [Govindasamy Bala, India]	Noted. Thank you.
18856	22	10	22	10	"the curvature of the relationship" - it might be helpful to show a plot of this relationship rather than just describe its curvature. [Gwenaelle GREMION, Canada]	Accepted: We no show a Delta N vs Delta T plot in section 7.1.
6425	22	14	22	15	This is even the case for the prescribed aerosols in CMIP6 simulations (Fiedler et al., 2017) and is relevant here. Reference: Fiedler, S., Stevens, B., and Mauritsen, T. (2017), On the sensitivity of anthropogenic aerosol forcing to model-internal variability and parameterizing a Twomey effect, J. Adv. Model. Earth Syst., 9, 1325– 1341, doi:10.1002/2017MS000932. [Stephanie Fiedler, Germany]	Rejected: This reference does not directly relate to the fixed-SST vs regression issue being discussed here.
53696	22	19	22	19	It could be useful for the reader with a very brief explanation of what a radiative kernel is. [Jan Fuglestvedt, Norway]	Taken into account: This paragraph has been expanded to explain radiative kernels.
18858	22	24	22	24	"The definition adopted herein aims to…" [Gwenaelle GREMION, Canada]	Taken into account. "The definition of ERF in Box 7.1 aims to have the cleanest separation…"
13396	22	24	22	27	Does the term "The definition" here refer to regression method? [Govindasamy Bala, India]	Taken into account. No, " the definition" here refers to the definition of ERF adopted in this chapter. Rewrote this for clarity.
40740	22	29	22	51	This is a very well-argued section. Consider a paragraph break somewhere near line 51. The concepts shift from how to make calculations to how consistent the results are. Or delete after about line 51 for brevity. [Daniel Murphy, United States of America]	Accepted: Thank you.
44796	22	35	22	39	Consider new naming convention to avoid issleading towards timescale response. [Astrid Kiendler-Scharr, Germany]	Rejected: The term "Rapid adjustments" is in common use in the literature.
13398	22	36	22	36	"surface temperature" should be "surface temperature change" to be precise [Govindasamy Bala, India]	Not applicable. Section has been rewritten.
12438	22	37	22	39	Surface air temperature or surface temperature? Both are used synonymously here but it would be better to stick to one. [David Neubauer, Switzerland]	Taken into account. Rephrase to surface temperature throughout.
12440	22	44	22	45	Add reference to Mülmenstädt et al. (2019). Mülmenstädt, J., Gryspeerdt, E., Salzmann, M., Ma, P.-L., Dipu, S., and Quaas, J.: Separating radiative forcing by aerosol–cloud interactions and fast cloud adjustments in the ECHAM-HAMMOZ aerosol–climate model using the method of partial radiative perturbations, Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2018-1304, in review, 2019. [David Neubauer, Switzerland]	Accepted

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41632	22	45	22	47	can you assess the consistency of kernels computed from different models? [Amanda Maycock, United Kingdom (of Great Britain and Northern Ireland)]	Rejected: The uncertainty in the kernel analysis is already discussed here.
12442	22	48	22	52	The cited studies are not fully independent as they are based on the same set of model simulations. This needs to be mentioned explicitly. [David Neubauer, Switzerland]	Not applicable: The text referred to has been deleted.
44798	22				Consider conceptual figure to illustrate text. [Astrid Kiendler-Scharr, Germany]	Taken into account: New figure added in section 7.1
18860	23	2	23	2	"2xCO2" - expound. This means a doubling of the CO2 concentration - mention this in the body of text also. Since this is not mentioned until the figure caption, which is on another page, it might be helpful to the reader to expound on what this acronym means. [Gwenaelle GREMION, Canada]	Taken into account. Rewrote the text for clarity.
38646	23	3	23	6	Chung and Soden 2015 shows (fig 2) a nearly 2:1 range in 4x CO2 IRF across CMIP5 models 3.2 - 5.7 W/m2) using the accurate double call method, and a quite different range (4.6-6.8 W/m2) using the kernel method (4.6 - 6.2 for those models with double call results). This implies (a) that there are definitely major errors in many models' radiative transfer codes and (b) that possible errors from the kernel method are much larger than 10%. As they say: "The instantaneous forcing estimated from the radiative kernels is systematically larger than that for the double-call method" [Nicholas Lewis, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account: An explanation of the Chung and Soden analysis has been added to the text. The vertical resolution of the kernels and the methodology based on temperature regression are likely to be reasons for both the spread and the offset in their analysis.
38648	23	9	23	10	This is illogical. The narrow spread in Table 7.1 does not enable the far wider spread, and very different mean values, in Chung and Soden 2015. The very poor agreement between models in Chung and Soden 2015 and between that study and Richardson et al shows that climate models' representation of radiative forcing from greenhouse gases is in many cases poor and gives low confidence in its accuracy. [Nicholas Lewis, United Kingdom (of Great Britain and Northern Ireland)]	Rejected: The understanding of the kernel approach and application to fixed-SST experiments has improved since Chung and Soden.
38650	23	13	23	21	The mean values in Table 7.1 differ wildly from those in Chung and Soden 2015. Dividing their kernel estimates of ERF (Fig.1 b, Adjusted forcing, range 5.2-9.1 W/m2) for 4x CO2 by 2.092 to adjust to 2x CO2 (per Etminan et al 2016) gives a range of 2.5 - 4.35 W/m2, compared with 3.73 - 4.64 in Table 7.1. Moreover, their Fig. 1b shows that kernel estimates of SARF and ERF fall almost exactly on, or slightly to the left of, the 1 to 1 line indicating that on average ERF is slightly lower than SARF, not slightly higher as per Table 7.1. Further, the land change mean difference of 0.32 W/m2 disagrees to the ~0.19 W/m2 kernel-derived value shown in Smith et al Fig 3 / fig 7.7 of this chapter. Given that kernel derived errors are likely to exceed 5% and that Chung & Soden 2015's results are contrary to those in Smith et al 2018, I see no justification for assuming that ERF differs from (is 5% greater than per this assessment) SARF for CO2. I note also that Fig. 7.8 shows almost identical climate sensitivity estimates using ERF and SARF for CO2, implying that they hardly differ. [Nicholas Lewis, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account: The "ERF" column in table 7.1 included additional corrections (as in Tang et al. 2019), not just the land surface temperature. This has been replaced with just and land surface temperature corrected ERF. These values are now lower and more in alignment with Chung and Soden.
13400	23	15	23	15	Table 7.1: What is the method used to calculate ERF in the last column? This may be stated in the caption. [Govindasamy Bala, India]	Taken into account. The table and caption has been updated for the second order draft.
44296	23	15	23	21	In the table, I suggest adding another row at the bottom presenting the multi-model mean and s.d. but limiting the analysis to those models for which all forcing diagnostics are available. That'd allow us to see how the spread varies across the different forcings, whereas currently we can't tell as the models included varies from one forcing to another. [Drew Shindell, United States of America]	Taken into account: This table now include values in all columns for all models.
18864	23	16	23	16	Richardson, et al - there are two undated articles by Richardson. Specify or label which one. [Gwenaelle GREMION, Canada]	Not applicable. Table has been updated and reference removed.
18862	23	19	23	19	expound more on ERF with land change and ERF only. What causes the difference/discrepancy? [Gwenaelle GREMION, Canada]	Not applicable: The "with land change" values are no longer shown.
18866	23	19	23	19	To me, these averages seem to be in good agreement. However, as I am not a climate scientist, I do not have a good notion of a "good agreement" between these values. It would be useful to have a short description of the range of such values and what a unit change in such values mean, as well as references detailing the meaning of these values. [Gwenaelle GREMION, Canada]	Taken into account: The preceding text has been revised to fit more closely with the presentation in table 7.1.
38652	23	27	24	7	Fig. 7.7 (a) shows that 2x Solar ERF is only ~10% lower than IRF but section 7.3.4.4 assesses it to be 28% lower?? [Nicholas Lewis, United Kingdom (of Great Britain and Northern Ireland)]	Rejected: This difference is explained in the text of 7.3.4.4
42038	23	29	4	5	The Smith et al study is excellent, but one caveat I have is that the separation of stratospheric and tropospheric temperature changes is too idealised for full comfort, given the assumed tropopause pressure which is assumed to vary linearly with latitude. I think the caption could mention that this separation is approximate. [Keith Shine, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Caption revised to mention tropopause definition
25770	24	7	24	14	I am surprised that the uncertainty in F2x of CO2 is now 17.5% (5-95%) down from 20% in AR5. Many of the same uncertainties remain, especially those resulting from masking by clouds. It would seem that the advances in understanding leading to reduction in uncertainty in this key quantity should be discussed. [Stephen E Schwartz, United States of America]	Taken into account: The methodology for calculating the uncertainty in ERF of CO2 forcing has been added.

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42040	24	10	24	10	I feel this discussion is more optimistic than I would gather from the Ting et al 10.1029/2018JD030188 study where there are substantial differences in the ERF (e.g. 30% differences between regression and fixed SST methods for ghg forcings) [Keith Shine, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account: The reasons for selecting fSST methods have been expanded in the text.
13402	24	10	24	11	What is "α"? Box 7.1 could be referred [Govindasamy Bala, India]	Not applicable. Cannot find the noted "?" at or near this location in the text. Ignored.
18868	24	11	24	11	are alpha values the values in table 7.1? Clarify. [Gwenaelle GREMION, Canada]	Taken into account: This has been clarified to state climate feedback parameter
13512	24	13	24	14	Krishnamohan et al. (2019) in Earth System Dynamics Discussions also show that the α is different between CO2 and sulfate aerosol forcings (https://doi.org/10.5194/esd-2019-21). This work also shows that the effective radiative forcing depends on the height of the aerosols and may be cited. [Govindasamy Bala, India]	Accepted, reference has been added
18390	24	14	24	16	What kind of variations in the climate feedback parameter alpha is this referring to? Differences in spatial distribution? Or, is this referring to different values for the same plant type, as in the plant is reacting differently and this is resulting in a different alpha? How large are these variations? [Gwenaelle GREMION, Canada]	Not applicable. This discussion of plants has been removed.
18870	24	16	24	16	Richardson, et al - there are two undated articles by Richardson. Specify or label which one. [Gwenaelle GREMION, Canada]	Taken into account. Reference updated for the second order draft.
18872	24	16	24	16	The Shindell 2015 reference in the References section has a typo- doi has the word "Received" attached to the doi. [Gwenaelle GREMION, Canada]	Editorial
44298	24	17	24	30	The authors rely on the Richardson et al study of PDRMIP models for much of this section. An important thing to note, however, is that those experiments used prescribed distributions of aerosols in all models (well, most anyway, though a few ran with their own emissions). As the text describes earlier in this paragraph, studies have found that the latitudinal and altitudinal distributions affect the response in at least some models, and so the PDRMIP study, though having many models, doesn't sample the actua variability across those models since most were given aerosol distributions from a single model. The HadGEM3 model appears to show different adjustments when it uses its own aerosols rather than when it used the prescribed PDRMIP concentrations (Johnson et al., JGR, submitted - assuming I understood a recent presentation he gave correctly), and that's also the case for GISS (see Tables 4 & 5 in Conley et al., JGR, 2018; I'm trying to write up some additional similar results that also find an enhanced sensitivity as in the Conley et al paper when using our own sulfate forcing and explain why this may differ from the results using the more homogeneous distribution of PDRMIP and will send if/when sumbitted). So there is additional evidence that the PDRMIP experiments sampled a narrow range of cases (by design) that likely do not span the full range of possibilities in current generation GCMs. Note GFDL results in Conley et al appear to go the other way - weaker sensitivity to inhomogeneous sulfate than CO2 - but likewise suggest there's still a considerable spread in efficacies. [Drew Shindell, United States of America]	Taken into account. This paragraph has been re-written to take into account all information on variations in alpha.
41630	24	26	24	27	see also Persad and Kaldeira (2018) doi: 10.1038/s41467-018-05838-6 [Amanda Maycock, United Kingdom (of Great Britain and Northern Ireland)]	Accepted, reference added
13508	24	28	24	28	Krishnamohan et al. (2019) in Earth System Dynamics Discussions also show that the α is different between CO2 and sulfate aerosol forcings (https://doi.org/10.5194/esd-2019-21). This work also shows that the effective radiative forcing depends on the height of the aerosols and may be cited. [Govindasamy Bala, India]	Accepted, reference added
13404	24	29	24	30	Yes, it is correct that α varies a lot for localized forcings. In this context, it is worth citing the 2017 JGR paper by Stjern and others which showed the efficacy of BC ranges from 0.06 to 1.12 across models. This paper may be cited again at lines 41-42 [Govindasamy Bala, India]	Accepted, reference added
18874	24	39	24	39	Just checking: are forcing agents defined in previous chapters? If not, sections where it was previously discussed in the document should be referred to in this instance in this chapter. [Gwenaelle GREMION, Canada]	Rejected, "forcing agents" is clear from the context.
6257	24	41	24	41	General: Regional energy consumption pattern also should be considered.(Ref. Jafari, M., Smith, P., (2018). Climate Change as a Driving Force on Urban Energy Consumption Patterns. In Encyclopedia of Information Science and Technology (4th ed., pp. 7815-7830). IGI Global. https://doi.org/10.4018/978-1-5225-2255-3.ch680 [Mostafa Jafari, Iran]	Rejected, In this chapter we deal only with regional temperature changes, as they affect the feedback parameter. Implications for Urban Energy Consumption are out of scope.
42042	24	42	24	42	while I know it is mentioned in the box, the fact that ERF's cannot easily be derived for small forcings is an important one and should be mentioned here I feel. [Keith Shine, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. This point has been added to text earlier in this section

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15048	24	45	24	45	Figure 7.8: From this figure, the sensitivity as measured relative to a 2% change in solar energy (4.8 W/w^2) is about 0.6 C per W/m^2. Increasing the average surface temperature from 288K to 288.6K increases the average surface emissions by about 3.2 W/m^2, or about 2.2 W/m^2 more than the W/m^2 of forcing causing it. Each average W/m^2 from the Sun currently contributes 1.62 W/m^2 to the surface emissions, or about 620 mw/m^2 more than the forcing causing it. The method used by Richardson et all to establish the 0.6C per W/m^2 value for solar energy produced a value that was a factor of 2 too large . The uncertainty of the actual 0.3C per W/m^2 value for solar input is less than +/- 0.03C per W/m^2 which is much better bound than the error bars seem to suggest. [George White, United States of America]	Rejected. We don't quite follow the calculation here, but perhaps the reviewer is using surface emissions (from the Stefan-Boltzmann equation) rather than the effective radiative forcing, which is a much better predictor of surface temperature change. If we have misinterpreted the reviewer's point we would be grateful for a reference to the peer-reviewed literature. The Richardson values arise from a study of 10 coupled climate models and evaluate the surface temperature change per unit forcing which include climate feedbacks. This paper is now published.
42060	25	8	25	8	I know there is no contradiction between WMGHG's being also SLCFs but I think the assessment does muddy the water, especially given the Chapter 6 discussions. Some of the halocarbons included here (eg. HFC32 and HFC152a) are rather short-lived and arguably break the "WM" definition. [Keith Shine, United Kingdom (of Great Britain and Northern Ireland)]	Accepted: added "Many halogenated species have lifetimes short enough that they can be considered short-lived climate forcers (Chapter 6). As such, although there are considered here as WMGHGs, they are not completely "well-mixed" and their vertical distributions are taken into account when determining their radiative efficiencies."
44800	25	8	43	44	Refer to Chapter 6 for SLCF concentration changes. Check with Chapter 6 for consistent use of dimensions. [Astrid Kiendler-Scharr, Germany]	Accepted: Chapter 6 has been referred to
52030	25	8			It feels lto me like there is some need to cross-reference and assure consistency of this section with the discussions on carbon budgets undertaken in chapter 5. These seem to be looking at very similar issues through two distinct lenses. It is unclear whether there is a consistency issue but I suspect at a minimum there is a need to cross-referece these assessments better than is the case thus far. [Peter Thorne, Ireland]	Accepted. Chapter 7 is the definitive source of information on WMGHG forcing, but in the FGD chapter 5 is referenced extensively throughout the chapter where appropriate.
42046	25	20	25	20	Table 7.1 missing - note that the units for the constants given in Etminan werent all accurate and may need adjusting - can contact Shine for more info [Keith Shine, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Units are fixed in what is now supplementary table SM7.1
13406	25	20	25	20	Table 7.1 does not contain any simplified expression as stated here [Govindasamy Bala, India]	Taken into account. Added to supplementary.
42044	25	20	25	21	The "confirmation" from Collins is of course reassuring, but it is a partial one, since they "just" computed the instantaneous SW forcng, rather than including the effect of SW forcing on statospheric temperature adjustment, which accounted for a around half of the Etminan SW forcing [Keith Shine, United Kingdom (of Great Britain and Northern Ireland)]	Accepted: added "shortwave contribution to the instantaneous radiative forcing of methane "
44802	25	20	25	41	Provide "simplified expressions" and "formula". [Astrid Kiendler-Scharr, Germany]	Accepted: These formula have be added to supplementary material.
18876	25	21	25	21	"results for methane have been confirmed independently" - Clarify that it has been confirmed independently for non-Earth atmospheric systems, aka Jovian moons and planets, so as to be not misleading. [Gwenaelle GREMION, Canada]	Rejected: Although the paper title does mention Jovian observations, this is only a minor part of the paper.
18392	25	32	25	34	This sentence is difficult to read, mainly because of the first clause. I think greater clarification can be added by more explicitly stating what the "main information used" from Vial et al. 2013 was [Gwenaelle GREMION, Canada]	Taken into account: This sentence has been reworded.
18394	25	32	25	37	The sentence starting with "Since then, Zhang and Huang (2014)…" could be rewritten to better flow with the previous two sentences, which would help condense this section. [Gwenaelle GREMION, Canada]	Taken into account: This has been reworded.
18878	25	37	25	37	doi typo error. Should be: 10.1007/s00382-013-1725-9. [Gwenaelle GREMION, Canada]	Editorial
18396	25	37	25	38	"these earlier two" papers? Studies? Assuming this is referring to the Vial et al. 2013 and Zhang and Huang 2014 papers [Gwenaelle GREMION, Canada]	Not applicable. Section has been rewritten.
38654	25	39	25	41	Given that kernel derived errors are likely to exceed 5% and that Chung & Soden 2015's results are contrary to those in Smith et al 2018, I see no justification for assuming that ERF is 5% greater than SARF for CO2. Note also that Fig. 7.8 shows almost identical climate sensitivity estimates using ERF and SARF for CO2, implying that they hardly differ. See Nicholas Lewis comment on Table 7.1. [Nicholas Lewis, United Kingdom (of Great Britain and Northern Ireland)]	Rejected: As described in the text, we have less confidence in the Chung and Soden calculations.

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45548	25	43			It seems like this should be a probability, rather than a confidence, statement. [Steven Sherwood, Australia]	Rejected. This statement is useful in categorising the extent of our knowledge. Probability/uncertainty quantification is covered elsewhere in the text. No change made.
12444	25	47	26	2	Are the cloud adjustments in Table 2 in agreement with the assessment of cloud feedbacks in Section 7.4? [David Neubauer, Switzerland]	Rejected: The rapid cloud adjustments are not comparable to the cloud feedbacks.
42048	25	49	25	49	As I noted elsewhere, the separation of stratospheric and tropospheric temperature adjustment in Smith et al. depends on a non-ideal specifiction of the tropopause. One significant advantage (not noted elsewhere, so far, in the chapter) of ERF compared to SARF is that there is no need to specify the tropopause position; but when you attempt to understand the drivers of the ERF and separate out trop and strat T adjustments, this difficulty reemerges. It is especially important for forcing agents where much of the temperature change is in the lower stratosphere (which is the case, e.g. for methane when solar focing is included). [Keith Shine, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account: This has been included in the text.
18398	25	50	25	51	"excluded from the forcing in out definition" doesn't make sense in reference to "forcing in out" [Gwenaelle GREMION, Canada]	Taken into account. Correct spelling should be "in our..:"
41628	25	51	25	51	typo out --> our [Amanda Maycock, United Kingdom (of Great Britain and Northern Ireland)]	Accepted
33226	25	51	25	51	out should be our [Mark Zelinka, United States of America]	Accepted
25772	26	7	26	10	Not clear how 17.5% uncertainty in RF for CO2 is reduced to 10% for ERF. Explain or correct. Inconsistent also with Table 7.3 and table 7.4 [Stephen E Schwartz, United States of America]	Taken into account: This has been changed to 17%
27240	26	7	26	24	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 should be mentioned and discussed. [François GERVAIS, France]	Rejected: The paper cited by the referee does not include a detailed representation of the atmosphere as used by all other radiative transfer modelling.
58058	26	10			The uncertainty of 0.7 W/m^2 quoted on line 7 is more than 10% i.e. it is inconsistent with what is written here. [Nathan Gillett, Canada]	Accepted. This has been changed to 17%.
12446	26	11	26	12	Add references for the rapid adjustments. [David Neubauer, Switzerland]	Accepted: Reference to table 7.2 added.
38656	26	17	26	23	2x CO2 SARF per Etminan et al 2016 expression, based on 278 ppm CO2 and 270 ppm N2O PI concentrations, is 3.80 W/m2 not 3.81 W/m2. Have you tried calculating it? I suspect that you may instead have relied on Piers Forster's value. [Nicholas Lewis, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account: Value revised to 3.80.
12448	26	17	26	23	Are the cloud adjustments in Table 3 in agreement with the assessment of cloud feedbacks in Section 7.4? [David Neubauer, Switzerland]	Rejected: The rapid cloud adjustments are not comparable to the cloud feedbacks.
42050	26	28	26	30	This is well-caveated, but I think it needs a bit more. Only 4 of the 9 PDRMIP models include SW forcing and these probabily have some commonality in their radiation codes and so I question a bit the statement about robustness here. Perhaps RFMIP will be able to shed some light on whether these codes do work reasonably, but our (unpublished) experience of e.g. ECMWF and Met Office radiation codes is that they do not do a great job of this particular forcing. [Keith Shine, United Kingdom (of Great Britain and Northern Ireland)]	Noted: However the text explicitly refers to the robustness of the sign of the adjustments, which are also consistent with physical understanding of the processes.
38658	26	28	26	35	Please indicate here which of the 9 Smith et al models are used to derive the -14% adjustment for CH4. I note that for the average of all 9 models they derive a zero average adjustment, with an extremely narrow confidence interval, so it seems surprising that using a subset of the models gives a large negative mean adjustment. Please also make clear here that Modak et al 2018 is a single model study. [Nicholas Lewis, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account: The models are now identified. It is now clarified that Modak is a single model study.
14336	26	29			"shortwave, only" [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Accepted
38660	27	7	27	10	This paragraph is illogical. It is stated that the +15% response for CH4 ex shortwave gives an indication of the physical response for N2O, and then says "the adjustments to N2O are therefore assessed to be 0 +/- 8%. Surely, given this CH4 based indication, but no specific studies for N2O, the logical assessment would be an adjustment of +15% +/- 15%, thereby embodying the best indication of its likely value but also encompassing a zero value? [Nicholas Lewis, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account: This text on N2O adjustments has been removed.
41634	27	7	27	10	would the chemical effect of N2O on stratospheric O3 be counted as a rapid adjustment in the ERF framework? [Amanda Maycock, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account: We have clarified that chemical reactions can be accounted for in the ERF framework.
42078	27	15	27	15	Perhaps this needs a similar "there have been no studies of the rapid adjustments" as was in the N2O section. Since the halocarbons have a more marked effect on lower stratospheric temperatures than N2O, the effects might be larger. [Keith Shine, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account: This text has been revised to state this.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
45592	27	18	27	19	Please note that this list of studies is non-exhaustive, and there are several more studies that have provided new estimates of radiative efficiencies, both for existing and new compounds, since AR5 (e.g., Etminan et al., 2014, Atm., http://doi.org/10.3390/atmos5030473; Davis et al., 2016, ACP, http://doi.org/10.5194/acp-16-8043-2016; Wallington et al., 2016, JQSRT, http://doi.org/10.1016/j.jqsrt.2016.01.029; Kovács et al., 2017, ACP, http://doi.org/10.5194/acp-17-883-2017). [Øivind Hodnebrog, Norway]	Accepted: References added.
41636	27	22	27	22	"is estimated to 20%" do you mean assessed to be 20%? Since there is not a great deal of traceability of that value apart from the spectroscopic uncertainty. [Amanda Maycock, United Kingdom (of Great Britain and Northern Ireland)]	Accepted, change made.
56134	27	23	27	23	The recent decline in the CFC-11 rate (montze ka et al., Nature, 2018; see also chapter 2) should be relevant here. [Rolf Müller, Germany]	Rejected: Not relevant. The decline in the CFC-11 decay rate is accounted for in the observed concentrations so does not affect the ERF uncertainty.
18882	27	24	27	24	"replacement species" - specify. HCFCs? HFCs? [Gwenaelle GREMION, Canada]	Accepted, HCFCs and HFCs specified.
18880	27	25	27	25	Give a reference (website, report, even news article etc) for the Kigali agreement [Gwenaelle GREMION, Canada]	Taken into account: Explanation given.
18400	27	25	27	25	I would specify that the Kigali Agreement is an amendment to the Montreal Protocol, just to ensure that all readers (scientific and non-) are aware of the importance of this [Gwenaelle GREMION, Canada]	Accepted: change made
41638	27	25	27	27	"Kigali agreement" --> "Kigali Amendment to the Montreal Protocol" [Amanda Maycock, United Kingdom (of Great Britain and Northern Ireland)]	Accepted: change made
38662	27	43	27	54	I suggest that the 1990-2015 estimated +0.06 W/m2 change in tropospheric O3 forcing from Myhre et al 2017 be added to the mean Checa-Garcia estimate of 0.30 W/m2 for the 1980s and 1990s, giving a best estimate for 1860-2015 of 0.36 W/m2. Moreover, the Checa-Garcia estimate is for 2000–2014 relative to 1850–1860, and that the Myhre et al multimodel mean change from 2000-2014 mean to 2015 is ~0.015 W/m2, which would increase to ~0.02 W/m2 up to 2017, bringing the Checa-Garcia estimate up to 0.35 W/m2 when adjusted from 2000-2014 mean to 2017, implying 0.39 W/m2 for 1750-2017. Also, why do you ignore the AR5 estimate of 0.36 W/m2 for tropospheric O3 forcing over 1850-2011? It was based on a wider set of studies and (Conley et al., 2013; Lamarque et al., 2013; Stevenson et al., 2013). Adjusting it to 2017 would increase the forcing by ~0.01 W/m2 per Myhre et al 2017, to 0.37 W/m2, or 0.41 W/m2 over 1750-2017. Averaging this and Checa-Garcia estimate of 0.39 W/m2 adjusted post 1990 to match Myhre et al 2017 would give an O3 1750-2017 forcing estimate of 0.40 W/m2. [Nicholas Lewis, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account: The Checa-Garcia estimate (which is 1850-1860 to 2009-2014) has been extrapolated back to 1750 and forward to 2018 using the FaIR model.
41640	27	46	27	49	Checa-Garcia et al (2018) did not assess the O3 RF in the CCMI models. Rather they assessed the CMIP6 ozone forcing dataset, which is produced as a weighted average of two chemistry-climate models that were part of CCMI, but not the whole CCMI model set. [Amanda Maycock, United Kingdom (of Great Britain and Northern Ireland)]	Accepted: This has been clarified.
25774	28	0	28	0	I strongly recommend a figure here showing ERF vs Delta mixing ratio for at least CO2, N2O, CH4, F11, F12, similar to Schwartz (2018) Figure 3. Global average radiative forcing by long-lived greenhouse gases as a function of their mixing ratios (parts per million, ppm; billion, ppb; trillion, ppt) shown on abscissa beginning at preindustrial values and extending approximately to present values. Curves (red, online) forcing calculated by approximate formulas . Lines (blue, online) denote linear dependence of forcing on mixing ratio. Note differences in horizontal and vertical scales for the several gases.. Schwartz, S. E. The Greenhouse Effect and Climate Change: The Intensified Greenhouse Effect, Amer. J. Phys., 86, 645-656 (2018). https://aapt.scitation.org/doi/full/10.1119/1.5045577 [Stephen E Schwartz, United States of America]	Rejected: Thank you for this suggestion, however we consider that adding a figure would not provide additional information beyond that already contained in the text.
42052	28	2	28	2	"Stratospheric ozone has been observed since 1979" - presumably this means "observed globally from satellites since 1979". Measurements go back a bit further! [Keith Shine, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Change made
38666	28	2	28	9	Since tone of the three Hassler stratospheric O3 forcing estimates differs hugely from the other two (which are almost the same), it is inappropriate to use the mean as the central estimate; the median of -0.038 W/m2 should be used. That is very close to the Checa-Garcia central estimate of -0.03 W/m2. Moreover the Hassler estimates are only up to 2005, 2006 and 2010, and O3 forcing has been falling (less -ve) since then. That all appears to point to a fair 1750-2017 best estimate being somewhere in the range -0.03 to -0.038 W/m2. [Nicholas Lewis, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account: The Hassler study is no longer used for the central estimate.
56136	28	2	28	9	the discussion of changes in strstospheric ozone need to be consistent with other staements in this report, in particularly chapter 2 [Rolf Müller, Germany]	Taken into account: Reference to chapter 2 made.
38664	28	5	28	5	Hassler stratospheric O3 forcing range is wrong; it should be-0.03 to -0.12 W/m2. [Nicholas Lewis, United Kingdom (of Great Britain and Northern Ireland)]	Accepted: Typo corrected.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
42054	28	5	28	5	Phew! There is a typo and the 0.33 should be 0.033. I was worried for a minute. Given that the Checa-Garcia value (previous para) was -0.03 and for a longer (1850-2014) period than Hassler's (1979-2005), I wonder if the weighting of evidence here is the right one. Note that the Checa-Garcia forcing indicates some negative forcing prior to 1979 which would have to be taken into account when using the Hassler values for the longer period. [Keith Shine, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account: Typo corrected. The stratospheric ozone forcing has been revised.
56138	28	12	28	20	Note that the observational basis of stratospheric water vapour is not goof (see e.g. Müller, Kunz, Hurst et al., Earth's Future, 2016); see allo Chapter 2. The discussion in this chapter need to be consistent with Chapter 2 [Rolf Müller, Germany]	Rejected: Not relevant. This section considers only the chemically produced water vapour from methane oxidation, not meteorological changes in water vapour.
41646	28	12	28	20	you might add here that changes to the transport of water into the stratosphere are considered as a climate feedback and are considered later in the chapter [Amanda Maycock, United Kingdom (of Great Britain and Northern Ireland)]	Accepted: reference to 7.4.2.2 added.
58060	28	15		16	I would not expect that the stratospheric water vapour response to forcings that warm or cool the UTLS is very rapid - I would guess it could have a response time of years. Secondly, if stratospheric water vapour is included as a rapid adjustment, shouldn't the stratospheric ozone response e.g. to changes in ODSs, also be considered as a rapid adjustment in ERF? [Nathan Gillett, Canada]	Taken into account: The word "rapid" has been removed. The consideration of chemical effects has been added to the introduction in 7.3.1
42056	28	16	28	16	"should be included as part of the ERF for that species" presumably only correct if that temperature change is unrelated to surface temperature change, in which case it is a feedback? [Keith Shine, United Kingdom (of Great Britain and Northern Ireland)]	Rejected: This sentence is clear that it is discussing rapid adjustments and so by definition these are unrelated to surface temperature change.
18402	28	16	28	16	UTLS is not defined until page 44 of this chapter [Gwenaelle GREMION, Canada]	Taken into account: UTLS now defined.
38944	28	23	29	2	Table 7.4 and Chapter 7.3.2.7 Synthesis do not include ozone and stratospheric water vapor. These parts are not positioned well. [Yugo Kanaya, Japan]	Rejected: This section is only considering the direct forcing from WMGHGs
18884	28	25	28	25	Just a note: it might be helpful for other readers if it is explicitly noted in the text that umol/mol is ppm or ppmv. [Gwenaelle GREMION, Canada]	Not applicable. ppm now used throughout.
33426	28	25	28	30	This is a useful synthesis: I would add the caveat that this reflects only the effects of elevated concentrations of these gases, and not indirect effects such as the methane effects on ozone and stratospheric water vapor detailed above. [Marcus Sarofim, United States of America]	Taken into account: Discussion of chemical effects has been added to this section.
18404	28	25	28	30	The synthesis as is only covers WMGHG, and does not discuss the halogenated species, ozone, or stratospheric water vapor main points of section 7.3.2; ozone is the longest sub-section in 7.3.2 [Gwenaelle GREMION, Canada]	Accepted: The synthesis has been expanded to cover the halogenated species and ozone
14338	28	25			isn't ppm is usualy used rather than micro mol per mol? [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. ppm used.
37810	28	25			"ppm" rather than "µ mol mol-1" is used elsewhere in the FOD. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. ppm used.
58062	28	25			Why are units of micro mol per mol used, rather than ppmv, as in previous assessments? Also, if we really are to change these units in AR6, this would need to be done in agreement with the other chapters, including Chapter 2 which reports observed changes in concentrations. And the change should be explained and justified somewhere. [Nathan Gillett, Canada]	Accepted. ppm used.
13408	28	35	28	35	Table 7.4, column 3 and 4: I wonder if "vs" should be replaced by "relative to" [Govindasamy Bala, India]	Taken into account. Changed vs to with respect to.
25776	28	41	29	1	It seems astonishing and must be a mistake that the uncertainty given for the sum of ghg forcings of 0.24 W m-2 is actually less than that given for CO2 forcing alone, 0.36 W m-2. In a sum, uncertainties always increase. If uncertainties are uncorrelated then that yields 37.9 W m-2, but the assumption that the uncertainties are uncorrelated may not be correct if they arise from cloud masking. In any event this needs to be corrected and discussed. [Stephen E Schwartz, United States of America]	Accepted: The uncertainties have been recalculated.
18406	28	41	29	2	Table 7.4; I suggest flipping the columns in this table so that the years are presented as chronological in order [Gwenaelle GREMION, Canada]	Rejected: The current order has the most recent first.
58064	28				Table 7.4 Why are units of micro mol per mol used, rather than ppmv, as in previous assessments? [Nathan Gillett, Canada]	Accepted: change made
18886	29	1	29	1	Double check or explain species with zero concentration (So2F2, NF3, CH3CCl3). Is this zero, as in not detected, or just below limit of detection? [Gwenaelle GREMION, Canada]	Taken into account: This has been clarified in the text that the ERF for these species is less than 0.5 mW/m2.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
52032	29	5			This becomes a very very long section, disproportionate in detail to remaining subsections of 7.3. While it is undoubtedly the case that aerosol understanding changes have been substantive and should be afforded due attention thisends up being 14 pages which feels a lot. It risks losing, through detail, essential messages for the intended reader. I would try to shorten and simplify the text to support the main assessment finding at the end and better help the reader to navigate what is a highly complex subject via perhaps more use of figures instead of what can be highly detailed nad to a non-expert somewhat disorienting text. [Peter Thorne, Ireland]	Taken into account. We have shortened the section substantially and focused it on the material that supports the main assessment.
18894	29	7	29	7	Does the expression “substantial increase” relate to the percent of the aerosols in the atmosphere or to the effects? Could you provide some numbers (e.g. 20% or “that significantly altered precipitation patterns (citation)”) ? [Gwenaelle GREMION, Canada]	Rejected. "substantial increase" relates to the aerosol concentration in the atmosphere. The increase in aerosol concentration during industrial era has been discussed in chapter 6, as has been indicated in the parenthesis.
50944	29	7	29	28	Indirect aerosol forcing through changes in production and loss of aerosols from natural emissions. E.g. vegetation emits BVOCs and oceans DMS that form secondary aerosols. Changes in anthropogenic emissions of e.g. ozone precursors (NOx, CO and VOCs) change OH and nitrate radical levels and thus the oxidation capacity and the spatial pattern of aerosol formation. The report should be clear about how (and if) this mechanism(s) are included in the estimates for ERFari and ERFaci. [Terje Berntsen, Norway]	Taken into account. In most model-based estimates of ERFari and ERFaci, response of natural emissions to anthropogenic activities is not included. And in different models, the complexities of the chemical processes associated with aerosols are not the same. These uncertainties is now mentioned in Section 7.3.3
41642	29	13	29	13	typo thrroughout [Amanda Maycock, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
18560	29	14	29	14	Consider replacing “above” with “in Box 7.1” [Gwenaelle GREMION, Canada]	Accepted
18896	29	18	29	22	The sentence starting with “Likewise” seems quite long and complicated. Would it be possible to separate it into two sentences? Or create some bullet-points within the sentence? [Gwenaelle GREMION, Canada]	Accepted
18562	29	22	29	25	A citation related to the role of INP might be Kärcher, B. Curr Clim Change Rep (2017) 3: 45. https://doi.org/10.1007/s40641-017-0060-3 [Gwenaelle GREMION, Canada]	Rejected - appropriate citations regarding the role of INPs are given when this is discussed in more detail in section 7.3.3.2.
58066	29	23			Explain somewhere how INPs differ physically from CCN in general. [Nathan Gillett, Canada]	Taken into account. INPs are discussed in more detail in section 7.3.3.2
44804	29	28			Seems the following is not exclusively limited to use of satellite based observations. Change accordingly and make use of ground-based (e.g. LIDAR) data sets. [Astrid Kiendler-Scharr, Germany]	Not applicable - section has been rewritten.
50946	29				Section 7.3.3. Changes in CCNs and INPs do in principle change the structure and dynamics of the clouds which could cause changes in detrainment rates and thus cause a change in the source of water vapor. This kind of rapid adjustment is not included in the assessment so far I believe and should at least be mentioned. [Terje Berntsen, Norway]	Taken into account. It is now mentioned, thanks.
44806	30	8			Based on titel one would expect a more laboratory and field/case-study centered section [Astrid Kiendler-Scharr, Germany]	Not applicable - section has been rewritten.
51524	30	9	31	13	The discussion of the aerosol radiation interactions and associated uncertainties is not really structured and ordered by relevance and magnitude of the uncertainty of the aerosol processed involved. Hard to understand which are the important uncertainties (eg the pure aerosol loads) and which are the unimportant ones (eg shape of particles). Where is the uncertainty in RFari coming from? [Michael Schulz, Norway]	Taken into account - the whole section has been revised and reorganized.
25778	30	16	30	18	Distinguish between optical properties (properties not dependent on incident radiation) and radiative effects, the interaction with radiation. Distinguish also between single particle properties and aerosol properties (properties of the size distribution). Optical properties of single aerosol particles pertinent to their radiative influence can calclated with great accuracy for spherical particles of known size and wavelength-dependent complex refractive index. However even for spherical particles the radiative influence is not yet computed with sufficient accuracy to nail down the aerosol radiative effect to accuracy sufficient to constrain forcing to the degree necessary. Key issues are mixtures (homogeneous, individual particles consisting of multiple substances; inhomogeneous, particles of the same size or in different sizes exhibiting different composition); humidity dependence (of size and real and imaginary components of refractive index) for a given amount of nonvolatile aerosol; role of organics in humidity dependent growth. So the implication of the sentence that radiative effects of aerosols consisting of spherical particles can be computed with sufficient accuracy to meaningfully constrain their contribution to direct aerosol forcing cannot be supported. [Stephen E Schwartz, United States of America]	Not applicable - section has been rewritten.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
18904	30	18	31	13	Suggestion: to put all the information about different components influencing models uncertainties in estimation of RFari and ERFari in one table (e.g with the variable, estimated percent of contribution, and links to the studies). It would help a lot in understanding this information as well as compare magnitude of different uncertainties. [Gwenaelle GREMION, Canada]	Taken into account. Thank you for pointing out that these sections were not easy to follow. While we have not produced such a table, we have changed the structure of the subchapter in order to rectify this.
51522	30	20	30	21	Rfari uncertainty due to particle shape -15 to +100 % - thats just applying to mineral dust, not at all to anthropogenic aerosol Rfari. Not valid here. [Michael Schulz, Norway]	Not applicable - section has been rewritten.
50130	30	25	30	26	Please explain what is meant by "particle habit" or give a reference which uses this term. Ice crystal shapes are defined by habits, but not aerosols. [Joyce Penner, United States of America]	Not applicable - section has been rewritten.
18900	30	25	30	26	It seems that this sentence does not add additional information, because the previous paragraph was also about the influence of particle shapes on the ERFari. Furthermore, the term "habit" in this meaning is very specific to this area and not known to more broad readers. I would suggest to replace it by "shape" if it has the same meaning. [Gwenaelle GREMION, Canada]	Not applicable - section has been revised.
44808	30	26			What is an "aerosol paricle habit"? [Astrid Kiendler-Scharr, Germany]	Not applicable. Section has been rewritten.
12450	30	28	30	30	Also for ERFari the vertical aerosol distribtution is a large source of uncertainty (Allen et al., 2019). Allen, R. J., Amiri-Farahani, A., Lamarque, J.-L., Smith, C., Shindell, D., Hassan, T., Chung, C. E.: Observationally constrained aerosol-cloud semi-direct effects. Npj Climate and Atmospheric Science 2, 2019. https://doi.org/10.1038/s41612-019-0073-9 [David Neubauer, Switzerland]	Not applicable - The section has been revised and this is no longer applicable, but the uncertainty noted by Allen et al., 2019, is cited in the new section 7.3.3.1.2: model-based lines of evidence.
45250	30	28	30	36	It is true that Jo et al. (2016) used GEOS-Chem to simulate aerosol including brown carbon. However, GEOS-Chem was not used to simulate the aerosol-radiation effect. So, it is not fully correct to say "Similar results were found by Jo et al. (2016) using the GEOS-Chem model". [Jianping Guo, China]	Not applicable - section has been revised.
46454	30	29	30	29	The reference of Li et al. (2009) is missing. Pls add it to the reference: Li, Z., X. Zhao, R. Kahn, M. Mishchenko, L. Remer, K.-H. Lee, M.Wang, I. Laszlo, T. Nakajima, and H. Maring, 2009: Uncertainties in satellite remote sensing of aerosols and impact on monitoring its long-term trend: a review and perspective, Ann. Geophys., 27, 1–16. [Zhanqing Li, United States of America]	No longer applicable - section has been rewritten.
18564	30	29	30	30	The reference to Kahn et al. 2017 does not seem to be appropriate. The argument in the previous sentence can be traced in the importance of providing vertical spiral flights for aerosol measurements (option C), but there is no mention on the impact on RFari estimates. I think this reference is more appropriate: Kahn, R.A. Surv Geophys (2012) 33: 701. https://doi.org/10.1007/s10712-011-9153-z [Gwenaelle GREMION, Canada]	Not applicable - section has been revised.
18566	30	34	30	34	More recent reference is Feng, Y., Ramanathan, V., and Kotamarthi, V. R., Atmos Chem Phys (2013), 13:8607-8621, https://doi.org/10.5194/acp-13-8607-2013, 2013 and references therein (particularly Saleh et al 2014 and Saleh et al 2015) [Gwenaelle GREMION, Canada]	Not applicable - section has been revised.
50132	30	34	30	35	Lin et al. (JGR, 2014) uses an improved version of IMPACT to predict 20 - 40% of the total BC+BrC absorption [Joyce Penner, United States of America]	Not applicable - section has been revised.
13410	30	35	30	35	Expand "IMPACT" [Govindasamy Bala, India]	Not applicable - section has been rewritten.
46456	30	36	30	36	at the end, add "Since AR5, most efforts have been devoted to the estimation of ERFari in Asia, as summarized in the Table 1 (for China) and Table 2 (for India) of Li et al. (2016). In general, both regions have much smaller ARF at the TOA (0–2 W m-2) than at the surface (-14 to -20 W m-2) and inside the atmosphere (14–18 W m-2) due to high fractions of absorbing aerosols in the two regions (Ramanathan et al., 2001; Li et al., 2010). " [Zhanqing Li, United States of America]	Not applicable - section has been revised.
18902	30	38	30	38	When you have a list of different aerosol characteristics, it fits better when you say "mixing states" instead of "mixing state". Or did you mean the uncertainties within a mixing state, but across other characteristics such as distributions? [Gwenaelle GREMION, Canada]	Not applicable - section has been revised.
12452	30	38	30	41	Add reference to Zieger et al. (2013). Zieger, P., Fierz-Schmidhauser, R., Weingartner, E., and Baltensperger, U.: Effects of relative humidity on aerosol light scattering: results from different European sites, Atmos. Chem. Phys., 13, 10609-10631, https://doi.org/10.5194/acp-13-10609-2013, 2013. [David Neubauer, Switzerland]	Not applicable - section has been revised.
18568	30	40	30	40	The reference Zhou et al. 2017B is superseded by Zhou et al. 2018. All the mentioned references make use of Bcc-agcm2.0_CUACE/AERO. For sake of completeness, I would also add references to Suzuki et al. 2019 an Takemura and Suzuki 2019, using a modified version of MIROC: Suzuki K J Geophys Res: Atm (2019) 124:2194– 2209. https://doi.org/10.1029/2018JD029808 Takemura T Sci Rep (2019) 9:4419. https://doi.org/10.1038/s41598-019-41181-6 [Gwenaelle GREMION, Canada]	Not applicable - section has been revised.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
18570	30	40	30	41	A reference is missing here. Please add Petersik et al. 2018 (and Haywood and Shine 1997; Haywood and Ramaswamy 1998 references therein): Petersik P Atmos Chem Phys (2018) 18:8589-8599. https://doi.org/10.5194/acp-18-8589-2018, 2018. [Gwenaelle GREMION, Canada]	Not applicable. The section has been revised and this is no longer applicable, but the findings of Petersik et al. 2018 is now cited in section 7.3.3.1.2.
12454	30	43	30	45	Other studies found similar results e.g. Randles et al. (2013). Randles, C. A., Kinne, S., Myhre, G., Schulz, M., Stier, P., Fischer, J., Doppler, L., Highwood, E., Ryder, C., Harris, B., Huttunen, J., Ma, Y., Pinker, R. T., Mayer, B., Neubauer, D., Hitzenberger, R., Oreopoulos, L., Lee, D., Pitari, G., Di Genova, G., Quaas, J., Rose, F. G., Kato, S., Rumbold, S. T., Vardavas, I., Hatzianastassiou, N., Matsoukas, C., Yu, H., Zhang, F., Zhang, H., and Lu, P.: Intercomparison of shortwave radiative transfer schemes in global aerosol modeling: results from the AeroCom Radiative Transfer Experiment, Atmos. Chem. Phys., 13, 2347-2379, https://doi.org/10.5194/acp-13-2347-2013, 2013. [David Neubauer, Switzerland]	Not applicable - section has been revised.
12456	30	43	30	54	Furthermore the surface albedo and cloud distribution introduce uncertainty in RFari (Stier et al., 2013). Stier, P., Schutgens, N. A. J., Bellouin, N., Bian, H., Boucher, O., Chin, M., Ghan, S., Huneeus, N., Kinne, S., Lin, G., Ma, X., Myhre, G., Penner, J. E., Randles, C. A., Samset, B., Schulz, M., Takemura, T., Yu, F., Yu, H., and Zhou, C.: Host model uncertainties in aerosol radiative forcing estimates: results from the AeroCom Prescribed intercomparison study, Atmos. Chem. Phys., 13, 3245-3270, https://doi.org/10.5194/acp-13-3245-2013, 2013. [David Neubauer, Switzerland]	Taken into account. We now separate between clear sky and all sky RFari.
47904	30		32		There are overlaps on SLCF (inc aerosols) and the hydrological cycle in Chapter 6 (both sections 6.3.4.3 and 6.3.1.2 overlap with each other) and with chapter 8 (e.g. in Section 8.2.2) and with chapter 7 (7.3.3.1 - aerosol cloud interactions) [WGI TSU, France]	Taken into account. We are well aware of the FOD overlaps and have worked extensively with other chapters (particularly Chapter 6) in order to resolve these for the SOD.
12458	31	2	31	13	This is challenged by Allen et al. (2019) who use observational constraints for the aerosol distribution and find an amplification of RFari of black carbon by rapid adjustments. Allen, R. J., Amiri-Farahani, A., Lamarque, J.-L., Smith, C., Shindell, D., Hassan, T., Chung, C. E.: Observationally constrained aerosol-cloud semi-direct effects. Npj Climate and Atmospheric Science 2, 2019. https://doi.org/10.1038/s41612-019-0073-9 [David Neubauer, Switzerland]	Taken into account. Allen et al. (2019)'s work is added.
44300	31	3	31	13	The text here s drawing on the PDRMIP modeling that finds a fairly strong negative rapid adjustment in response to BC. The authors should be aware of Allen et al, in press, 2019 (in npjCAS) that finds that the BC distribution used In most of the PDRMIP modeling (the prescribed concentration models) is biased relative to obsservations in its vertical profile, and this leads to a biased rapid adjustment. In simulations constrained to match observed profiles, the rapid adjustment flips sign to positive, so enhances the RF from BC rather than weakening it. The fact that there was a large multi-model ensemble doesn't mean either that it's right or that the design really tested the possible range of rapid adjustments (and in this case, we have clear indications it didn't) so I don't think the conclusions here are necessarily valid. [Drew Shindell, United States of America]	Taken into account. Allen et al. (2019)'s work is also added in the discussion.
33428	31	4	31	7	As a complement to Stjern et al, I'd consider citing Yang Y. S.J. Smith H. Wang C.M. Mills and P.J. Rasch 2019. “Variability Timescales and Non-linearity in Climate Responses to Black Carbon Emissions” Atmos. Chem. Phys., 19, 2405–2420. https://doi.org/10.5194/acp-19-2405-2019. [Marcus Sarofim, United States of America]	Rejected. Even though in Yang et al. (2019), rapid adjustment was used in explaining the climate responses to BC, but it seems that rapid adjustment was not explicitly discussed or quantitively assessed in the paper.
13412	31	10	31	13	A brief discussion on how rapid adjustments (the mechanisms) increase the RF of sulfate aerosols is needed [Govindasamy Bala, India]	Rejected - we find this too detailed for the overview of progress listed here.
18572	31	11	31	13	It is not clear why Zhao and Suzuki work adds something to the discussion (besides a broad quantification of the shares of Rfari impacted by rapid adjustments to different species). Maybe it could be made more explicit. [Gwenaelle GREMION, Canada]	Taken into account. The emphasis of citing this work is on the discussion of the rapid adjustment of black carbon, considering the context of the paragraph. Therefore text was modified, only results related to BC were left.
12460	31	15	31	15	Change title of section to: "Observation-based lines of evidence" [David Neubauer, Switzerland]	Taken into account. Section title has been revised.
18906	31	16	31	16	In this sentence, you for the first time introduce the ERari abbreviation. It would be better to include the full phrase: “radiative effect of aerosol-radiation interactions” there in order to let reader better remember the abbreviation. [Gwenaelle GREMION, Canada]	Rejected. We find the sentence to be clear as it reads now.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
6427	31	16	31	17	Typos affecting meaning. Could be: "RFari, is easier to estimate from observations than RFaci, because the latter requires knowledge of pre-industrial aerosol distributions." [Stephanie Fiedler, Germany]	Rejected. REari and RFari, are what the authors intend to say in the sentence, This is not a typo.
58068	31	20		22	The authors ascribe a difference in global REari of -2.1 W/^2 and ocean-only REari in AR5 of -4 to -6 W/m^2 to REari being less negative over more reflective surfaces. Presumably land is more reflective. But since over 2/3 of the globe is ocean, this effect would have to be very strong to explain the difference. Is this the only reason for the difference? [Nathan Gillett, Canada]	Not applicable. Text has been removed in the FGD.
18908	31	21	31	21	It might be better to replace the phrase "That estimate is less negative than…" with "That estimate is higher than…", or with "That estimate is less in absolute value than…", because the phrase "less negative" is not common and can be misleading. [Gwenaelle GREMION, Canada]	Taken into account. Changed to "That estimate is smaller in magnitude…"
6431	31	30	31	31	Although re-analyses assimilate satellite data, I would rather interpret such estimates and the one by Kinne (2019) as lines of evidence from models incorporating observational data, rather than satellite-based lines of evidence. Consider revising the title of this sub-section. [Stephanie Fiedler, Germany]	Rejected. The subheading Satellite-based lines of evidence is kept as satellite data is incorporated in the Kinne study.
18574	31	30	31	34	Maybe it is worth explicitly mentioning what are the models that have been used to obtain pre-industrial AOD. In the case of Ma et al. 2014 this is GEOS-Chem-APM. In the case of Remy et al. 2018 the method by Bellouin et al. 2013B is used, in which natural and anthropogenic aerosol AOD are distinguished in MACC Reanalyses. This latter case is substantially different from the other case, where a pre-industrial model run is performed, and I think that the usage of a different methodology shall be emphasized. [Gwenaelle GREMION, Canada]	Taken into account. Some more information is given on the methods to obtain pre-industrial AOD estimates. Model names are, however not explicitly mentioned.
44490	31	30	31	34	Rémy et al. (2018) analyzed the trends in Rfari for the period 2003-2017 (not 2008-2014) using CAMSiRA data. The increase in Rfari in 2017 is linked to increased biomass-burning aerosols in the tropics not the increase of AOD over India. Rémy et al. (2018) clearly mentioned that the trends remain statistically fragile, however, because of large uncertainties in the estimates. The statements made based on reference Rémy et al. (2018) may please be carefully rewritten in whole chapter. [VIJAY SONI, India]	Taken into account. The discussion of Rémy et al (2018) has been revised in what is now section 7.3.3.1.1
14340	31	32			Murphy et al. (2013) Nature Geosci. doi.org/10.1038/ngeo1740 also find constant RFari from 2000 to 2012 in clear-sky satellite data [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Murphy (2013)'s result was added.
6429	31	34	31	36	Kinne (2019) is not a satellite-based line of evidence. He uses model results and AeroNet station data. Also see next comment. [Stephanie Fiedler, Germany]	Rejected. The subheading Satellite-based lines of evidence is kept as satellite data is incorporated in the Kinne study.
18910	31	41	31	41	"There are two challenges against that hypothesis." Is incorrect sentence. There could be evidences against the hypothesis or challenges to the hypothesis. [Gwenaelle GREMION, Canada]	Editorial
50134	31	41	31	43	The first point in this paragraph does not support the contention that constraining AOD narrows the uncertainty of Rfari. Just because different reanalyses get different results for AOD does not mean that doing a better job of constraining AOD would not improve RFare estimates. [Joyce Penner, United States of America]	Taken into account.
25780	32	10	32	10	Give 5-95% range in lieu of or in addition to likely. For a factor of 1.6 this would seem to be -0.4 ± 0.8 W m-2, but to my thinking this would allow +0.4, much too high in my opinion. [Stephen E Schwartz, United States of America]	Rejected. The likely range given here is based on the scientific results presented in the given section.
12462	32	10	32	11	The study by Kinne (2019) uses no satellite-data but surface observations. Therefore "Observation-based lines of evidence" is more appropriate. [David Neubauer, Switzerland]	Rejected. The subheading Satellite-based lines of evidence is kept as satellite data is incorporated in the Kinne study.
13414	32	18	32	18	Is there a way to distinguish rapid adjustments (of Rfari) from the cloud changes in Rfaci? Also, a reference for semi-direct effect could be included. [Govindasamy Bala, India]	Noted. The methodology of diagnosing rapid adjustments and the references, e.g., radiative kernels, partial radiative perturbation approach, were introduced in section 7.3.1. semi-direct effect is used in AR5 and AR4.
53700	32	18	32	18	this is the first time the semi-direct effect is mentioned. Needs explanation. Could this be presented by Ch6 with links both ways? [Jan Fuglestvedt, Norway]	Taken into account.
50136	32	37	32	38	I'm not quite sure why you are quoting this low value of forcing, since this paper uses very poor representation of refractive indices for brown carbon. Zhu et al. (Nat. Comm., 2019) find a radiative forcing of -0.38 W/m2. [Joyce Penner, United States of America]	Taken into account - the paragraph has been extended and more studies added to the discussion.
18576	32	37	32	45	For sake of completeness, the paper by Paulot et al. 2018 shall also be included, showing a SW RFari value for the 2000-2014 period of +0.03 W -2 (-0.09 W m-2 in 2001). This is obtained from simulations with GFDL AM3, whose radiative scheme is called twice, with and without aerosols. [Gwenaelle GREMION, Canada]	Taken into account. Paulot et al. (2018) is not cited in section 7.3.3.1.2
33228	32	49	32	49	should be approximate partial radiative perrurbation technique [Mark Zelinka, United States of America]	Accepted

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
6435	33	0	33	0	Table 7.5: Add ECHAM6.3 estimates (Fiedler et al., 2017): ERFari: -0.23 Wm-2, ERFaci: -0.27 Wm-2 to -0.8 Wm-2, ERFari+aci: -0.5 Wm-2 to -1.03 Wm-2; Final estimates from the pilot study for RFMIP-SPaer will also be published in time (Fiedler et al., in press): ERFari+aci: -0.9 Wm-2 to -0.4 Wm-2; Choose a more general title for the table since it lists not just ERFaci. References: Fiedler, S., Stevens, B., and Mauritsen, T. (2017), On the sensitivity of anthropogenic aerosol forcing to model-internal variability and parameterizing a Twomey effect, J. Adv. Model. Earth Syst., 9, 1325– 1341, doi:10.1002/2017MS000932. Fiedler, S., Kinne, S., Huang, W. T. K., Räisänen, P., O'Donnell, D., Bellouin, N., Stier, P., Merikanto, J., van Noije, T., Carslaw, K., Makkonen, R., and Lohmann, U.: Anthropogenic aerosol forcing – insights from multi-estimates from aerosol-climate models with reduced complexity, Atmos. Chem. Phys., https://doi.org/10.5194/acp-2018-639, in press. [Stephanie Fiedler, Germany]	Not applicable - the format of the table has changed only to include model estimates. Individual published literature is now cited in the text.
6433	33	4	33	4	Estimate of ERFari from ECHM6.3 (atmosphere component of MPI-ESM1.2) in Fiedler et al. (2017): -0.23 Wm-2 in all-sky at TOA for shortwave radiation. Reference: Fiedler, S., Stevens, B., and Mauritsen, T. (2017), On the sensitivity of anthropogenic aerosol forcing to model-internal variability and parameterizing a Twomey effect, J. Adv. Model. Earth Syst., 9, 1325– 1341, doi:10.1002/2017MS000932. [Stephanie Fiedler, Germany]	Not applicable - the format of the table has changed only to include model estimates. Individual published literature is now cited in the text.
51526	33	8	33	9	The smaller than AR5 ERFari should be revisited based on recent literature in 2019. So far it seems that the new assessment would require more arguments, or better argumentation. Its not clear to me how the value of 0.2 is derived. [Michael Schulz, Norway]	Taken into account. ERFari has now been revised upwards (in magnitude). Thanks.
25782	33	11	33	11	A summary para is reqd to synthesize observational and model results and give best estimate and associated 5-95% uncertainty range for ERFari. I don't see where the -0.2 ± 0.2 value given in Table 7 comes from, and it seems too low to me. [Stephen E Schwartz, United States of America]	Accepted. Synthesis assessment paragraph has been added and ERFari has now been revised upwards (in magnitude).
12464	33	21	34	2	Add values from Lohmann (2017a, 2017b), Neubauer et al. (2019). Lohmann, U. (2017a), Anthropogenic aerosol influences on mixed-phase clouds, Curr. Clim. Change Rep., 3(1), 32–44, doi:10.1007/s40641-017-0059-9. Lohmann, Ulrike. (2017b). Why does knowledge of past aerosol forcing matter for future climate change?: Anthropogenic aerosol forcing. Journal of Geophysical Research: Atmospheres. 10.1002/2017JD026962. Neubauer, D., Ferrachat, S., Siegenthaler-Le Drian, C., Stier, P., Partridge, D. G., Tegen, I., Bey, I., Stanelle, T., Kokkola, H., and Lohmann, U.: The global aerosol-climate model ECHAM6.3-HAM2.3 – Part 2: Cloud evaluation, aerosol radiative forcing and climate sensitivity, Geosci. Model Dev. Discuss., https://doi.org/10.5194/gmd-2018-307, in review, 2019. [David Neubauer, Switzerland]	Accepted. Values have been added to the table.
25784	33	24	33	24	Let the title read ERFari, ERFaci and ERFari+aci from GCM model simulations, not just aci. [Stephen E Schwartz, United States of America]	Accepted. Text has been revised accordingly.
18898	33	24	33	24	Please, indicate details about the table in the legend (e.g. confidence intervals ranges). For example, the table 7.6 has the full description in the legend. [Gwenaelle GREMION, Canada]	Accepted. Table legend has been expanded.
50138	33	24	33	25	This table needs to include whether this is only liquid clouds, or also includes mixed phase and cirrus clouds [Joyce Penner, United States of America]	Taken into account. Clarification has been added to the text.
18578	33				Table 7.5: being first mentioned at p. 39, it might be more appropriate to place it somewhere closer to the reference. [Gwenaelle GREMION, Canada]	Rejected. Table 7.5 is referred to on page 33, where the table is placed.
18580	33				The “Post-AR5” shall be provided with an uncertainty range. Further, the ERFari+aci is -1.16, rather than -1.17. [Gwenaelle GREMION, Canada]	Taken into account. Uncertainty ranges added. New data is in and the table has been updated.
11670	34	1	34	1	The study by Rosenfeld et al., (2019) is an excellent study, but it still remains controversial. Perhaps it's a little bit overcited (5 times) in this section. [Chuanfeng Zhao, China]	Taken into account. We have rewritten the section and made the assessment much more balanced.
14342	34	1			A balanced assessment is needed in 7.3.3.2.1 over and above the work of Rosenfeld et al. including for example the perspective of Malavelle et al. (2017) and McCoy et al. (2019) ACP doi:10.5194/acp-18-5821-2018 (although I appreciate there is discussion in 7.3.3.2.2). The assessment in 7.3.3.4 seems quite robust but I could not see a clear link between the discussion in 7.3.3.2.1 and the synthesis. [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We have made an effort to make the assessment of the process-based evidence for ERFaci more balanced.
17828	34	4	39	39	Section 7.3.3.2 has a strong focus on adjustments (LWP response to Nd) rather than radiative forcing (cloud albedo response to Nd). Page 7-36 line 20-22 addresses the albedo effect, but the accumulating evidence referred to here is not presented. [Frida Bender, Sweden]	Taken into account. We have now shortened the discussion on adjustments (in LWP and cloud cover) and added more evidence for the cloud albedo effect.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
17830	34	4	39	39	Section 7.3.3.2 Aerosol effects on boundary layer clouds relies very heavily on the single recent study by Rosenfeld et al. 2019, with only minor caveats that its results, showing substantially larger than previously estimated susceptibility of marine boundary layer cloud to aerosol, have yet to be confirmed by other studies. (Some examples: p7-34:line29-30, p7-34:line50-53, p7-35:line7-12, p7-35:line34-38, p7-36:line22-27) Less emphasis is placed on the large number of studies finding indication of weak or even negative relations between LWP and aerosol or LWP and Nd (Christensen and Stephens 2011, Lebsock et al. 2008, but also Bretherton et al 2007 GRL 34:L03813, Small et al. 2009 GRL 36:L16806, Chen et al. 2015 NatGeosci 7 643-646, Neubauer et al. 2017 ACP 17 13165-13185, Bender et al. 2019 ClimDyn 52 4371-4392). Although these studies do not use the CGT constraint suggested by Rosenfeld et al. 2019, they are consistent with suggestions of competing effects of droplet size on precipitation and entrainment on the cloud thickness (e.g. Wood 2007 JAS 54 2657-2669, Stevens and Feingold 2009 Nature 461 607-613), and there is not reason the believe that segregating by CGT would enhance or reverse the relations seen to a strong positive dependence. Bender et al 2019 for instance, shows opposite signs of correlations between LWP and Nd, in CMIP5 models and satellite observations respectively, in limited marine stratocumulus regions, in which CGT is not expected to vary largely. This rather indicates that the precipitation-related microphysical coupling between Nd and LWP is more clearly manifest in models than can be seen in observations. [Frida Bender, Sweden]	Taken into account. We have rewritten this section to make it a lot more balanced.
58070	34	6	39	39	Several subsections of this section are missing overall assessment statements at the end. [Nathan Gillett, Canada]	Taken into account. We have now added overall assessments to every section.
47966	34	7	36	31	LWP is covered in both ch7 and ch6 with respec to aerosol-cloud interactions. Please ensure no inconsistecies and avoid overlap where possible. Please also call out / cross reference to the appropriate sections. (Chapter 6 Section 6.3.2, Chapter 7 Section 7.3.3.2.1) [WGI TSU, France]	Taken into account. Processes governing aerosol-cloud interactions, and particularly cloud adjustments like LWP changes, are now assessed exclusively in Ch. 7 (as agreed at LAM3).
18582	34	9	34	10	It might helpful referencing here the beginning of sect. 7.3.3 (l. 20-21 p. 29) [Gwenaelle GREMION, Canada]	Accepted. Reference added.
12466	34	10	34	11	RFaci [David Neubauer, Switzerland]	Accepted
50140	34	11	34	11	RFaci not ari [Joyce Penner, United States of America]	Accepted
12468	34	16	34	18	Add reference to Toll et al. (2017). [David Neubauer, Switzerland]	Accepted. Reference added.
11662	34	17	34	17	have the opposite effect on LWP (i.e a reduction).' should be 'have a reduction effect on LWP'. [Chuanfeng Zhao, China]	Rejected. The current form underlines that the effect can be opposite to what has been expected.
12470	34	18	34	18	Cloud-top entrainment for stratocumuls clouds (Wang et al., 2003; Ackerman et al., 2004; Bretherton et al., 2007; Hill et al., 2009) and cloud lateral entrainment for trade wind cumulus clouds (Xue and Feingold, 2006; Small et al., 2009). Wang, S. P., Wang, Q., and Feingold, G.: Turbulence, condensation, and liquid water transport in numerically simulated nonprecipitatingstratocumulus clouds, JOURNAL OF THE ATMOSPHERIC SCIENCES, 60, 262–278, https://doi.org/10.1175/1520-0469(2003)060<0262:TCALWT>2.0.CO;2, 2003. Ackerman, A., Kirkpatrick, M., Stevens, D., and Toon, O.: The impact of humidity above stratiform clouds on indirect aerosol climate forcing, NATURE, 432, 1014–1017, https://doi.org/10.1038/nature03174, 2004. Bretherton, C. S., Blossey, P. N., and Uchida, J.: Cloud droplet sedimentation, entrainment efficiency, and subtropical stratocumulus albedo, GEOPHYSICAL RESEARCH LETTERS, 34, https://doi.org/10.1029/2006GL027648, 2007. Hill, A. A., Feingold, G., and Jiang, H.: The Influence of Entrainment and Mixing Assumption on Aerosol-Cloud Interactions in Marine Stratocumulus, JOURNAL OF THE ATMOSPHERIC SCIENCES, 66, 1450–1464, https://doi.org/10.1175/2008JAS2909.1, 2009. Small, J. D., Chuang, P. Y., Feingold, G., and Jiang, H.: Can aerosol decrease cloud lifetime?, GEOPHYSICAL RESEARCH LETTERS, 36, L16 806, https://doi.org/10.1029/2009GL038888, 2009. Xue, H. and Feingold, G.: Large-eddy simulations of trade wind cumuli: Investigation of aerosol indirect effects, JOURNAL OF THE ATMOSPHERIC SCIENCES, 63, 1605–1622, https://doi.org/10.1175/JAS3706.1, 2006. [David Neubauer, Switzerland]	Not applicable. Text has been removed.
18584	34	19	34	20	A reference to the IPCC FAR literature or to Jiang et al. 2006 might be useful: Jiang H Geophys Res Lett (2006) 33:L14806, doi:10.1029/2006GL026024. [Gwenaelle GREMION, Canada]	Not applicable. Text has been completely rewritten.
46458	34	23	34	23	Cite Li et al. (2017) after Fan et al. (2016): Li, Z., D. Rosenfeld, and J. Fan, 2017: Aerosols and their impact on radiation, clouds, precipitation, and severe weather events, Oxford Research Encyclopedias, doi:10.1093/acrefore/9780199389414.013.126. [Zhanqing Li, United States of America]	Not applicable. Text has been completely rewritten.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
12472	34	30	34	33	It should be mentioned that the transition from open-cell stratocumulus to closed-cell stratocumulus occurs on far longer timescales than the transition from closed-cell stratocumlus to open-cell stratocumulus (Feingold et al., 2015). Feingold, G., Koren, I., Yamaguchi, T., and Kazil, J.: On the reversibility of transitions between closed and open cellular convection, Atmos. Chem. Phys., 15, 7351-7367, https://doi.org/10.5194/acp-15-7351-2015, 2015. [David Neubauer, Switzerland]	Not applicable. Text has been removed.
18586	34	36	34	36	The advection of aerosol species and the impact for marine boundary layer is also treated in Dallafior et al. 2016: Dallafior TN J Geophys Res Atmos (2016) 121:49– 66 doi:10.1002/2015JD024070. [Gwenaelle GREMION, Canada]	Not applicable. Text has been completely rewritten.
18914	34	37	34	38	“However, most changes between overcast and broken MSC are observed without any obvious relationship to anthropogenic aerosols, thus not incurring any ERFaci.” Does this sentence imply that incurring ERFaci is only possible due to the anthropogenic aerosols? [Gwenaelle GREMION, Canada]	Not applicable. Text has been removed. But yes, this is by definition what the ERFaci is.
40742	34	38	34	38	Could delete this for brevity [Daniel Murphy, United States of America]	Accepted. This sentence has been deleted.
50142	34	47	34	53	Aerosols can control CGT, by promoting evaporation, so this surmise that CGT is primarily meteorological effects has not been proven. In Rosenfeld et al 2019, they isolated CGT because it was shown to explain much of the variability. But there was no proof that CGT was determined by meteorology. [Joyce Penner, United States of America]	Taken into account. We have modified the text to reflect this.
12474	34	48	34	48	A few studies (Andersen et al., 2017; Christensen et al., 2017; Gryspeerdt et al., 2016) quantified the effect of anthropogenic aerosol on Cf adjustments but confounding factors such as meteorological co-variation can not be excluded. Andersen, H., J. Cermak, J. Fuchs, R. Knutti, and U. Lohmann (2017), Understanding the drivers of marine liquid-water cloud occurrence and properties with global observations using neural networks, Atmos. Chem. Phys., 17(15), 9535–9546, doi:10.5194/acp-17-9535-2017. [David Neubauer, Switzerland]	Taken into account. We are now citing some of these papers in the chapter.
17832	34	50	34	53	The analogy is interesting, but not perfect. While an increase in Nd at constant LWP per definition gives greater cloud reflectivity due to more smaller droplets (Twomey), cloud LWP is determined be several factors. CGT is one factor, and it should be accounted for, but keeping CGT constant does not give a complete account for meteorology, or for LWP-variabitliy. Both precipitation suppression and entrainment enhancement, and other processes affecting LWP may still be at work. CGT-segregation, as suggested by Rosenfeld et al 2019 should not be used to dismiss the body of studies showing weak sensitivity of LWP to Nd. [Frida Bender, Sweden]	Taken into account. The text has now been completely rewritten, partly to reflect this.
40744	34	54	35	15	The discussion here overemphasizes one very new, unverified, and controversial, to put it mildly, study (Rosenfeld et al. 2019). The entire paragraph could probably be deleted for brevity. [Daniel Murphy, United States of America]	Taken into account. The paragraph has been shortened substantially and rewritten.
12476	35	3	35	7	Add reference for Gryspeerdt et al. (2018a). [David Neubauer, Switzerland]	Taken into account. Reference added.
18588	35	7	35	8	The wet scavenging of aerosols in deep convective clouds is extensively treated in Yang et al. 2015: Yang Q J Geophys Res Atmos (2015) 120:8448– 8468 doi:10.1002/2015JD023647 [Gwenaelle GREMION, Canada]	Not applicable. Text has been removed/rewritten.
12478	35	7	35	8	Chen et al. (2014) and Gryspeerdt et al. (2018a) already account for the effect of precipitation and discuss the different behaviour in rain and non-raining regimes. They attribute the negative LWP susceptibility rather to more effective entrainment mixing of dry air at high Nd. [David Neubauer, Switzerland]	Taken into account. We have now extended the discussion about potential negative LWP adjustments and refer to these two papers there.
38668	35	17	35	29	This summary misrepresents the findings in Seifert et al (2015). As well as showing that after SRCE was reached the aerosol effect on clouds was very small, Siefert et al showed that "Realistic radiative cooling, as provided by the interactive radiation using McSI, makes the buffering by the deepening and drying of the layer in response to a suppression of precipitation more effective". They showed that, when this is allowed for ("Interactive radiation"), the total aerosol-cloud effects were even smaller during the transition regime than when SRCE was reached [their Fig. 10; compare leftmost green bars in panels a) and b)]. Dagan et al 2018 admit to a number of limitations in their study and it cannot be considered to invalidate or render inapplicable the Seifert et al (2015) findings. [Nicholas Lewis, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We have revised this summary and made it more balanced, and believe that this concern has been addressed in that process.
6437	35	31	35	32	Replace "from newly developed parameterizations of aerosol effects" with "from new estimates of aerosol effects", since the parameterization itself can also be implemented such that LWP and Cf adjustments are represented. This is for instance done in the CMIP6 model EC-Earth (Fiedler et al., in press, Döscher et al., in prep.). References: Döscher, R. et al.: The community Earth system model EC-Earth for collaborative climate research, in prep. Fiedler, S., Kinne, S., Huang, W. T. K., Räisänen, P., O'Donnell, D., Bellouin, N., Stier, P., Merikanto, J., van Noije, T., Carslaw, K., Makkonen, R., and Lohmann, U.: Anthropogenic aerosol forcing – insights from multi-estimates from aerosol-climate models with reduced complexity, Atmos. Chem. Phys., https://doi.org/10.5194/acp-2018-639, in press. [Stephanie Fiedler, Germany]	Not applicable. Text has been removed.
44810	35	51	36	18	Consider adding discussion on transport/dynamic influences on ice supersaturation (e.g. Petzold et al., Faraday Discuss. 2017). [Astrid Kiendler-Scharr, Germany]	Rejected. We don't view this as central material for the assessment.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
12480	36	4	36	5	Vergara-Temprado et al. (2018) only investigate mixed-phase clouds. The sentence needs to be rephrased to explicitly mention that this is only relates to mixed-phase clouds (but not cirrus (ice) clouds). [David Neubauer, Switzerland]	Accepted. The text has been rewritten to reflect this.
50144	36	4	36	5	This is a blanket statement that is not supported by the underlying paper which only looked at mixed phase clouds and only considered 2 fuels as a source for BC. BC is highly variable in its sources, and thus, many behaviors are possible as INP, which is also mentioned in the introduction of Vergara-Temprado. Moreover, the paper shows that if treated as based on the observations in a model, the impacts are small relative to dust and marine organics. That does not mean that the difference between clouds with and without BC as an INP is small. [Joyce Penner, United States of America]	Accepted. The statement has been rewritten in order to address these concerns.
6439	36	5	36	5	I missed a paragraph on uncertainty due to anthropogenic changes in mineral dust aerosols acting as IN. Relevant works are for instance from Paul Ginoux on the fracion of anthropogenic desert dust and Ben Murray on the ability of soil particles for ice nucleation. [Stephanie Fiedler, Germany]	Rejected. This topic is covered under forcings due to land use change.
13976	36	7	36	16	Evidence of Secondary Ice Production over the Southern Ocean (Huang et al. 2017, QJRMS, DOI:10.1002/qj.3041). Ahn et al. (2017, QJRMS, DOI:10.1002/qj.3011) suggest that there's plenty of mixed phase clouds over the Southern Ocean. Please remember that the Hallett-Mossop process was discovered over the Southern Ocean (Mossop et al. 1970, QJRMS.) [Steven Siems, Australia]	Rejected. None of this contradicts the statement that there is a larger abundance of supercooled liquid in the Southern Ocean than in the NH. Irrespective of this, the section has been revised substantially.
50146	36	9	36	16	You should also note that marine organics have been added as ice nuclei and this improves the agreement of the CAM model with observations of ice water path (Yun and Penner, GRL, 2013). [Joyce Penner, United States of America]	Rejected. This paragraph has been rewritten, but with a focus on anthropogenic impacts on INPs. A model evaluation of INP concentrations is beyond the scope of Ch. 7, but could potentially be covered in Ch. 6.
13984	36	9			There are large inconsistencies in the cloud-top thermodynamic phase of these clouds based on various satellite products. (Huang et al. 2016DOI:10.1175/JCLI-D-15- 0768.1) It certainly isn't all SLW. [Steven Siems, Australia]	Not applicable. Text has been removed/rewritten.
50148	36	22	36	24	"most recent studies have cntinued to show negative changes of LWP". One does not get this feeling reading your summary of recent work (or that from Ch. 5). I would say that this issue is still unresolved. [Joyce Penner, United States of America]	Accepted. The discussion of recent work in FOD could have been more balanced. We have corrected this for the SOD, and the discussion now does support the summary statement.
18590	36	24	36	27	One could mention here that these arguments follow from e.g. Lee et al. 2009, Saleeby et al. 2015: Lee SS J Geophys Res 114:D07204 doi:10.1029/2008JD010513. Saleeby SM J Atmos Sci (2015) 72:1369–1385 https://doi.org/10.1175/JAS-D-14-0153.1 [Gwenaelle GREMION, Canada]	Not applicable. Text has been removed/revised.
50150	36	24	36	27	The Rosenfeld et al (2019) study shows that deeper clouds increase LWP when aerosols are increased (not theopposite as you state here). [Joyce Penner, United States of America]	Rejected. The Rosenfeld study claimed that for a given cloud depth, LWP increases with aerosols, but that this had been masked in observations by the fact that deeper clouds precipitate more and thus remove aerosols more efficiently. This is what was originally stated, however the text has now been substantially revised.
18592	36	29	36	31	There is quite some amount of comprehensive review papers that are worth citing on this topic: above all, the position paper by Rosenfeld et al. 2014, the report by Lohmann 2017 and the review by Storelvmo 2017: Lohmann, U. Curr Clim Change Rep (2017) 3:32. https://doi.org/10.1007/s40641-017-0059-9 Rosenfeld D Rev Geophys (2014) 52:750– 808 doi:10.1002/2013RG000441. Storelvmo T Ann Rev Earth Plan Sci (2017) 45:199-222. [Gwenaelle GREMION, Canada]	Accepted. We now cite two of these reviews.
50152	36	30	36	31	On what basis do you conclude that dee[cloud and ice cloud forcing is positive? The entire discussion that this paragraph refers to does not discuss deep or ice (cirruc, I presume) clouds. Please note that Penner et al. (JGR, 2018) predict negative forcing in cirrus clouds. [Joyce Penner, United States of America]	Taken into account. We have expanded the assessment of aerosol effects on deep convective, mixed-phase and ice clouds, which now on balance support this overall assessment. The Penner et al. (2018) paper is cited in the revised text.
11668	36	33	36	33	The 'Satellite-based evidence' seems a unreasonable section name since the above section (i.e., 7.3.3.2.1) also included many satellite-based studies (e.g., Rosenfeld et al., 2019). [Chuanfeng Zhao, China]	Taken into account. The section has been restructured and subsections renamed.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
44812	36	33			As for related aerosol section: why limit to satellite observations? [Astrid Kiendler-Scharr, Germany]	Rejected. We are assessing studies that are of relevance to GLOBAL aerosol ERFs in Ch. 7 (we state this more explicitly at the beginning of Section 7.3.3 now), so only assess observational evidence with large enough spatial coverage to have relevance in the global mean. That limits us predominantly to satellite observations.
58072	36	35			Did AR5 really introduce a new concept? Didn't it just assess the published literature which introduced this concept? [Nathan Gillett, Canada]	Taken into account. The concept was not new, but the exact framework as it was presented in AR5 was new. We have reworded the sentence to reflect this.
18594	36	41	36	41	Maybe it could be also useful defining LWP, for those who are not familiar with that, similarly to what done for the cloud water path. This is first mentioned in p. 34 ll. 9-10. [Gwenaelle GREMION, Canada]	Accepted. LWP has now been clearly defined.
17834	36	53	37	4	This dismissal of studies using AOD as an indicator of aerosol abundance is quite abrupt. AOD is by no means a perfect proxy for CCN, and neither is AI (Stier 2016, Liu and Li 2014 ACP 14:471-483). But CCN observations remain sparse, and although explaining a limited fraction of the variability in CCN, AOD and AI can be useful for qualitative separations between clean and polluted scenes, and comparisons between global models and satellite observations, where more appropriate proxies are not available. Shifting the focus to the actual key driver of aerosol-cloud interaction Nd, or to other proxies for Nd (like sulfate mass in McCoy et al. 2017) is beneficial. The statistical relation between AOD and Cf is particularly problematic and confounds other relations (e.g. Grandey et al. 2013 and Gryspeerdt et al 2014a (cited) and Engström and Ekman 2010 GRL 37:L18814) But all results from studies of covariation between aerosol and cloud using AOD should not be dismissed (including those by Quaas et al. 2008, 2009a, 2009b cited, Neubauer et al. 2017 who also closely address the influence of humidity on AOD, but also e.g. Wilcox et al 2016 PNAS 113 11794-11799 who show that aerosol absorption can affect cloud development through suppressed boundary layer turbulence, and Bender et al. 2016 J Clim 29 3359-3587 who shows that cloud brightening may be hidden by other processes, not least aerosol absorption above clouds, in a way that models don't capture). [Frida Bender, Sweden]	Taken into account. We have added a few sentences and references to address this issue in the text.
12482	36	53	37	4	Christensen et al. (2017) use AI screened for aerosol swelling and 3D cloud radiative effects in satellite observatons and find a reduction in the combination of RFaci and the LWP adjustment of 52% compared to unscreened AI. Neubauer et al. (2017) use AI computed from dry aerosol in global aerosol-climate models and find a reduction of the combination of RFaci and the LWP adjustment of 74% compared to AI. [David Neubauer, Switzerland]	Taken into account. Both references have been added.
46460	37	1	37	1	The finding of Stir (2016) is very similar to Liu and Li (2014) although using a different method. Thus, the following paper should be cited ahead of Stir: Liu, J., and Z. Li, 2014: Estimation of cloud condensation nuclei concentration from aerosol optical quantities: Influential Factors and Uncertainties, Atmos. Chem. Phys., 14, doi:10.5194/acp-14-471-2014. [Zhanqing Li, United States of America]	Rejected. We already cite two papers to support this statement.
46462	37	1	37	1	Before Gryspeerdt, add the following it is highly pertinent to the theme: Liu and Li (2018a) demonstrated that the enhancement in AOD by the aerosol swelling effect lead to a systematic underestimation of the RFaci depending on aerosol properties and relative humidity when AOD is used as a proxy for CCN. They also found systematic differences in the RFaci estimated from satellite and ground-based measurements for the same clouds observed over the ARM Azores site partially because the satellite retrievals of cloud particle size are only valid near cloud tops whereas the latter is for the cloud column. Such artifacts need to be accounted for in using satellite-based estimates, especially in comparing with ground-based estimates (Liu et al., 2016). To remedy this problem, attempts have made to directly retrieve CCN both from high-resolution satellite imagery data (Rosenfeld et al., 2016), and from ground-based Raman lidar measurements (Lv et al., 2019). Liu, J., Z. Li, and M. Cribb, 2016: Response of marine boundary layer cloud properties to aerosol perturbations associated with meteorological conditions from the 19-month AMF-Azores campaign, J. Atmos. Sci., 73, doi:10.1175/JAS-D-15-0364.1. Lv, M., Z. Wang, Z. Li, T. Luo, R. Ferrare, D. Liu, D. Wu, J. Mao, B. Wan, F. Zhang, and Y. Wang, 2018: Retrieval of cloud condensation nuclei number concentration profiles from lidar extinction and backscatter data , J. Geophys. Res. Atmos., 123, 6082-6098, doi:10.1029/2017JD028102. Liu, J., and Z. Li, 2018, Significant Underestimation in the Optically-based Estimation of the Aerosol First Indirect Effect Induced by the Aerosol Swelling Effect, Geophy. Res. Lett., 45, doi:10.1029/2018GL077679. [Zhanqing Li, United States of America]	Taken into account. This topic was already discussed, and this text has been slightly expanded as per the reviewers suggestion. We are unable to adopt the exact wording, however, partly because of space limitations.
18596	37	6	37	12	Unlike in the other sections, there is no clear definition here of how aerosols are defined. A reader might want to know which measure is used for comparison (AOD, AI, or some other quantity). [Gwenaelle GREMION, Canada]	Accepted. We have now clarified this.

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41766	37	18	37	34	Consider including approaches explicitly putting aerosol effects into the context of meteorological conditions in multivariate statistical/machine learning approaches, e.g. doi:10.5194/acp-17-9535-2017, doi:10.5194/acp-18-16537-2018 [Jan Cermak, Germany]	Taken into account. Thanks for the suggestion, we now cite and discuss this paper.
46466	37	22	37	22	After "statistical relationships", add citations of Tao et al., 2012; Fan et al., 2016; Li et al., 2017). Li, Z., D. Rosenfeld, and J. Fan, 2017: Aerosols and their impact on radiation, clouds, precipitation, and severe weather events, Oxford Research Encyclopedias, doi:10.1093/acrefore/9780199389414.013.126. [Zhanqing Li, United States of America]	Not applicable. Text has been removed/revised.
46464	37	23	37	23	Add citation of Liu and Li (2016) after Grandey et al. (2013), Liu, J., and Z. Li, 2018, Significant Underestimation in the Optically-based Estimation of the Aerosol First Indirect Effect Induced by the Aerosol Swelling Effect, Geophy. Res. Lett., 45, doi:10.1029/2018GL077679. [Zhanqing Li, United States of America]	Accepted. Citation added.
46468	37	23	37	23	After "Marshak, 2015", add the citation of Jeong and Li (2010), Jeong, M.-J., and Z. Li, 2010: Separating real and apparent effects of cloud, humidity, and dynamics on aerosol optical thickness near clouds, J. Geophy. Res. Atmos., 115, doi:10.1029/2009JD013547 [Zhanqing Li, United States of America]	Rejected. We have not added this reference, partly because it was available for assessment for AR5, and partly to avoid excessive citation of the work of some authors.
46470	37	27	37	27	Before "Another solution", add "Liu et al. (2016) found that atmospheric stablity and updraft systematically influence the reltionship. Liu, J., Z. Li, and M. Cribb, 2016: Response of marine boundary layer cloud properties to aerosol perturbations associated with meteorological conditions from the 19-month AMF-Azores campaign, J. Atmos. Sci., 73, doi:10.1175/JAS-D-15-0364.1. [Zhanqing Li, United States of America]	Rejected. We already cite three papers to support this statement.
12484	37	27	37	30	Two more methods used are careful sampling in Christensen et al. (2017) and the use of a neural network in Andersen et al. (2017). For model simulations Neubauer et al. (2017) recommend the use of AI computed from dry aerosol. Andersen, H., J. Cermak, J. Fuchs, R. Knutti, and U. Lohmann (2017), Understanding the drivers of marine liquid-water cloud occurrence and properties with global observations using neural networks, Atmos. Chem. Phys., 17(15), 9535–9546, doi:10.5194/acp-17-9535-2017. [David Neubauer, Switzerland]	Taken into account. Some of these papers have now been assessed.
12524	37	36	37	37	The cited studies are not fully independent as they are based on the same satellite data. [David Neubauer, Switzerland]	Accepted. We have added this to the text.
12526	37	36	37	37	Satellite retrievals of LWP and of N_d have considerable uncertainty e.g. Grosvenor et al. (2018). [David Neubauer, Switzerland]	Taken into account. We are now emphasizing this more in the subsection.
44412	38	1			Chapter 6 pg 48 L9-31 is very similar and or duplicating Chapter 7 pg 38 L11-43. [Matthew Christensen, United Kingdom (of Great Britain and Northern Ireland)]	Thanks for catching this. The text was transferred to Ch. 7 from Ch. 6 before the FOD (were it was originally), but it appears it was never deleted from Ch. 6.
46472	38	7	38	7	after "relationships", add "as critically reviewed by Fan et al. (2016; Li et al., 2017). [Zhanqing Li, United States of America]	Rejected. This is a statement that refers to all the literature that was cited above, so we see no need to cite papers again here.
18598	38	11	38	29	In addition to the mentioned literature, Peters et al. 2011 already noticed that the impact of aerosol precursor emissions by ships is not discernible from the impact of natural variability in cloud formation: Peters K J Geophys Res (2011) 116:D24205, doi:10.1029/2011JD016531. [Gwenaelle GREMION, Canada]	Rejected. We have added several citations for the ship track section, but choose not to include the proposed one since it predates AR5 and as such would have been assessed there.
50154	38	17	38	18	The statement here on ice clouds (a general reference, I presume) based on Christensen et al 2014 is not supported by the referenced paper, which deals with mixed phase clouds in polar regions. [Joyce Penner, United States of America]	Accepted. We have added a qualifier to this statement.
50156	38	22	38	24	Note that Malavelle et al. (2017) does not confirm the Kilauea results from Yuan et al. (2011) [Joyce Penner, United States of America]	Noted. That is also perfectly consistent with the text in FOD on p. 38, l. 22-24.
18600	38	28	38	29	It might be worth noticing that McCoy et al. 2018 analysis is focused on mid-latitudinal cyclones. [Gwenaelle GREMION, Canada]	Rejected. That was already stated.

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46474	38	36	38	36	at the end, add "Notable, among others, are the extensive and systematic investigations into the causality between the drastic changes in atmospheric environment and climate changes in Asia (Li et al., 2016) on various time scales ranging from the diurnal variation (Guo et al., 2016; Lee et al., 2016), weekly cycle (Yang et al., 2016), and decadal trends (Yang and Li, 2014) of severe convective cloud systems under highly different aerosol loading conditions. Guo, J., M. Deng, S. S. Lee, F. Wang, Z. Li, P. Zhai, H. Liu, W. Lv, W. Yao, and X. Li, 2016: Delaying precipitation and lightning by air pollution over the Pearl River Delta. Part I: Observational analyses, J. Geophys. Res. Atmos., 121, 6472-6488, doi:10.1002/2015JD023257 ; Lee, S.-S., J. Guo, and Z. Li, 2016: Delaying precipitation by air pollution over the Pearl River Delta. Part II: Model simulations, J. Geophys. Res. – Atmos., doi/10.1002/2015JD024362. Yang, X., and Z. Li, 2014: Increases in thunderstorm activity and relationships with air pollution in southeast China, J. Geophys. Res. Atmos., 119, 1835–1844, doi:10.1002/2013JD021224. Yang, X., Z. Li, L. Liu, L. Zhou, M. Cribb, and F. Zhang (2016), Distinct weekly cycles of thunderstorms and a potential connection with aerosol type in China, Geophys. Res. Lett., 43, doi:10.1002/2016GL070375. [Zhanqing Li, United States of America]	Rejected. We do not focus on regional studies, but on what we can learn from ship tracks and volcanic emissions into otherwise pristine environments. Asia does not represent such a pristine environment. We further are very limited in space.
18912	38	38	38	53	In the table 7.6 there are empty cells and n/a values, but it is not clear what is the difference in meaning between them. [Gwenaelle GREMION, Canada]	Taken into account. This has now been explicitly stated.
50158	38	42	38	43	You do not discuss cirrus ice clouds [Joyce Penner, United States of America]	Accepted. We have added/expanded text on aerosol effects on cirrus/ice clouds.
44410	39	0			Add Christensen et al. (2017) to Table 7.6 "intrinsic forcing" nomennclature (first row) [Matthew Christensen, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Reference added.
12486	39	19	39	25	Andersen et al. (2017) use an artificial neural network to understand which factors determine CF, LWP, re and cloud optical depth, thereby separting the influence of aerosol and other factors on marine liquid clouds. Andersen, H., J. Cermak, J. Fuchs, R. Knutti, and U. Lohmann (2017), Understanding the drivers of marine liquid-water cloud occurrence and properties with global observations using neural networks, Atmos. Chem. Phys., 17(15), 9535–9546, doi:10.5194/acp-17-9535-2017. [David Neubauer, Switzerland]	Taken into account.Due to space limitations this section has been cut in the FGD and this reference is no longer relevant.
12488	39	19	39	25	Christensen et al. (2017) use AI screened for aerosol swelling and 3D cloud radiative effects in satellite observatons and find a reduction in the CF adjustment of 69% compared to unscreened AI. Neubauer et al. (2017) use AI computed from dry aerosol in global aerosol-climate models and find a reduction of the CF adjustment of 111% compared to AI. [David Neubauer, Switzerland]	Accepted. These papers have now been discussed and cited.
50160	39	28	39	32	The table gives only "intrinsic" values of forcing. It is not clear how increasing the reported values by a factor of 1.3 (130%) leads to the range you give here [Joyce Penner, United States of America]	Taken into account. There are adjustment estimates in the table as well. The range has now been better justified.
58074	39	28		39	As far as I could see, these two paragraphs summarise the assessment of RFaci and ERFaci considering only water clouds, but each paragraph has different ranges for each. Either merge the paragraphs and decide on one assessed range for each, or if there is a difference in the ranges assessed in each paragraph, make this clearer. [Nathan Gillett, Canada]	Accepted. We agree that this was confusing. This has been made clearer in the SOD.
12490	39	34	39	35	Several studies include LWP adjustments implicitly. Only one study assessed global LWP adjustments explicitly. [David Neubauer, Switzerland]	Taken into account. The text has been revised substantially and now reflects this.
50162	39	34	39	37	It is not clear why the 2nd average given here (-0.4) differs from the first average (-0.7) based on the table 7.6, since all of the numbers in Table 7.6 appear to include the LWP adjustmen (they are listed as "intrinsic" values). [Joyce Penner, United States of America]	Taken into account. We agree that the justification for the assessment was confusing, and have corrected this for the SOD.
38670	39	34	39	39	Estimating global-mean ERFaci from satellite retrievals of cloud properties and aerosol optical depth etc. is clearly extremely challenging. I think that more weight should be given to studies of the effects on clouds of localised aerosol emissions from ships and volcanos. These seem to point to a weaker ERFaci than assessed here. The Seifert et al (2015) study, moreover, shows that even during the transitional regime shallow convecting clouds adjust in such a way as to reduce ERFaci to a very low level, contrary to what appears to have been assumed in this assessment (on the basis of Dagan et al 2018). [Nicholas Lewis, United Kingdom (of Great Britain and Northern Ireland)]	Noted. All lines of evidence have their own challenges, and it is difficult to judge which ones are larger. Thus, we take all lines of evidence into account without assigning weights to them.
38672	39	34	39	39	There is also major uncertainty as to what the baseline level of aerosol AOD and CCN was in preindustrial times. Here, there is evidence that CCN may have been much higher than thought in pristine conditions due to the ability of biogenic emissions to seed CCNs even in the absence of SO2 (and hence H2SO4) (Kirkby et al Nature 533.7604 (2016): 521). Gordon et al (2016; https://doi.org/10.1073/pnas.160236011) concluded that: "The mechanism increases CCN concentrations by 20–100% over a large fraction of the preindustrial lower atmosphere, and the magnitude of annual global mean radiative forcing caused by changes of cloud albedo since 1750 is reduced by 0.22 W m-2 (27%) to -0.60 W m-2." The findings of this very relevant study should be properly reflected in the assessed RFaci and ERFaci ranges. [Nicholas Lewis, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. we now cite the Gordon et al. paper, which in fact ends up with a very similar Rfaci estimate to the one assessed, and thus supports our overall assessment.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
50164	39	49	39	50	Is this estimate only for liquid clouds? [Joyce Penner, United States of America]	Rejected. There is no line 49-50 on page 39.
6441	40	10	40	25	Also add estimates from ECHAM6.3 here (See comment on Table 7.5) [Stephanie Fiedler, Germany]	Accepted. ECHAM estimates have been added.
18602	40	13	40	13	I struggled a bit tying to understand the meaning of “bottom-up estimate” in this case, until I found that it was described in 7.3.1 (l.29-39 p. 22). Maybe one could help the reader by referencing this section… [Gwenaelle GREMION, Canada]	Accepted.
18604	40	32	40	33	An extended, although probably incomplete, review on the state-of-the-art and how to improve our understanding of ERFaci is found in Mülmenstädt and Feingold, 2018: Mülmenstädt, J. & Feingold, G. Curr Clim Change Rep (2018) 4: 23. https://doi.org/10.1007/s40641-018-0089-y [Gwenaelle GREMION, Canada]	Rejected. This is a nice paper, but because it doesn't directly relate to any of our assessments we have omitted it to save space.
45552	40	36			This chapter should carefully separate a-priori vs. inverse methods. The study cited here appears to be presenting inverse evidence (e.g. such and so aerosol effect would not produce the correct warming). [Steven Sherwood, Australia]	Accepted. Such separation was our original intention but it was not carried out consistently for the FOD. The section has been rewritten in this respect and does NOT use most of the top-down constraints as lines of evidence, so as to not "double-count".
45590	40	37	40	39	This possibility was discussed in detail by Rosenfeld et al. 2013 (the WCRP special volume) which might be worth citing. Agree with "low confidece". Again however, is this statement effectively based on "inverse method"? How do we know the total ERF_aci is weaker than that inferred for low clouds? [Steven Sherwood, Australia]	Accepted. We agree that this was somewhat speculative, and an inverse method of sorts. We have revised the text to reflect this.
12492	40	41	40	47	Neubauer et al. (2019) find that the reduction in shortwave and longwave ERFaci in the latest version of ECHAM-HAM compared to the previous version is due to different causes for the shortwave part and the longwave part of ERFaci. So at least in this model with explicit aerosol-ice cloud interaction the larger terrestrial spectrum forcing is not necessarily accompanied by a larger solar forcing. Neubauer, D., Ferrachat, S., Siegenthaler-Le Drian, C., Stier, P., Partridge, D. G., Tegen, I., Bey, I., Stanelle, T., Kokkola, H., and Lohmann, U.: The global aerosol-climate model ECHAM6.3-HAM2.3 – Part 2: Cloud evaluation, aerosol radiative forcing and climate sensitivity, Geosci. Model Dev. Discuss., https://doi.org/10.5194/gmd-2018-307, in review, 2019. [David Neubauer, Switzerland]	Noted. However, we hesitate to put too much weight on what is reported based on a single model.
25786	40	53	42	1	It would seem essential to acknowledge that reasoning based on the fact that because Earth temperature has increased, total forcing must be positive is circular. If the physics or observations other than based on change in GMST would countenance a greater negative aerosol forcing and even a positive total forcing, using that reasoning (and forcing obtained thereby) in climate model calculations to get increase in temperature basically uses the results to prove the assumption. Reference;Rodhe, H., Charlson, R.J. and Anderson, T.L. (2000). Avoiding circular logic in climate modeling. Climatic Change 44, 419-422. [Stephen E Schwartz, United States of America]	Taken into account. Philosophically, we agree. But in reality, assessing a total anthropogenic forcing that is negative would be so inconsistent with multiple different lines of evidence that this discussion in practice becomes an academic exercise that we will not delve into in this chapter. However, we refrain from using surface temperature evolution as a line of evidence in the SOD.
14344	40	53			Chung & Soden (2017) Nature Geosci., doi:10.1038/ngeo2988 find that models with larger cloud adjustments to aerosol forcing can better reproduce the observed interhemispheric temperature changes and tropical rain belt shifts over the twentieth century [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. Chung and Soden (2017) does not include quantitative estimates, and thus has been omitted due to space limitations.
12494	41	22	41	26	While the greenhouse gas forcing is relatively well constrained the feedbacks, in particular the cloud feedbacks, are not (sections 7.4). [David Neubauer, Switzerland]	Accepted. We are not using EB constraints in our assessment for the SOD, to avoid circularity in our assessments.
12496	41	43	41	51	How are these results influenced by changes in cloudiness (section 7.4)? In section 7.2.2.3 the evidence for the influence of changes in cloudiness is mentioned. [David Neubauer, Switzerland]	Taken into account. The discussion in 7.2.2.3 has been expanded to discuss effects of changes in cloudiness.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
38674	41	49	41	51	Contrary to what is claimed here, global dimming since the middle of the last century is not (significantly) correlated with global aerosol emissions. Storelvmo et al (2016), cited as the source of this statement, state that correlation between global downwelling solar radiation at the surface and global SO2 emissions (which other anthropogenic aerosol emissions are highly correlated with is very strong, at -0.83 over 1964-1985 and -0.78 over 1986-2010. However, when calculated over their full 1964-2020 analysis period, as it should be, this correlation is insignificant, at -0.21. (I can provide data from their Figure 1 and calculations if required.) That is evidence for little contribution to solar dimming/brightening from anthropogenic aerosols, and implies that weak global dimming in CMIP5 models implies nothing about the aerosol radiative effect. [Nicholas Lewis, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. New research shows that when variability caused by interannual cloud changes is filtered out from this data set, dimming and brightening is in fact highly correlated with aerosol emissions (Julsrud et al., 2019)
58076	41	53		54	The above lines of evidence are based on EBMs constrained with observed temperature change, ocean heat content and /or TCR, itself constrained based on these variables. [Nathan Gillett, Canada]	Accepted. Because of the circularity, we are not using these estimates as lines of evidence for the SOD.
18410	42	6	42	6	Define the terms "ari" and "aci" [Gwenaelle GREMION, Canada]	Rejected. The terms are defined on page 29, lines 15 and 22 respectively.
18408	42	26	42	48	The first sentence of the paragraph (lines 26-28) create an expectation in the reader of a full discussion about the three above arguments. However, only the argument 1 is the one well discussed in the text. The arguments 2 and 3 need more explanation. [Gwenaelle GREMION, Canada]	Noted. This section has been partly rewritten, and hopefully now covers all three lines of argument in similar depth.
12498	42	27	42	31	For ice clouds, recent laboratory measurements show that some soot types can be good INPs (Mahrt et al., 2018). Mahrt, F., Marcolli, C., David, R. O., Grönquist, P., Barthazy Meier, E. J., Lohmann, U., and Kanji, Z. A.: Ice nucleation abilities of soot particles determined with the Horizontal Ice Nucleation Chamber, Atmos. Chem. Phys., 18, 13363-13392, https://doi.org/10.5194/acp-18-13363-2018, 2018. [David Neubauer, Switzerland]	Accepted. We now cite this paper and no longer rule out BC as a potential INP.
12500	42	31	42	33	While there can be compensating effects by anthropogenic CCN and INP in mixed-phase and ice clouds this does not mean that the net effect has to be negligible. [David Neubauer, Switzerland]	Taken into account. The intention was not to state that effects from CCN and INP cancel, but rather that published estimates of the effect of INP perturbation on mixed-phase and ice clouds have produced opposite signs for the associated ERF, so our assessment is that we have too little evidence and very low confidence in the magnitude of this forcing. We have revised the text to make this clearer.
50166	42	37	42	38	It seems from arguments you have given above, the model results are only for liquid water clouds [Joyce Penner, United States of America]	Taken into account. Generally yes, because there is insufficient and conflicting evidence for the ERF associated with mixed-phase and ice clouds. We now state this more explicitly.
38676	42	50	43	27	As Figure 7.10 shows, only model based evidence supports a best estimate for total ERFari+aci more negative than -0.9 W/m2. Most energy balance constraints (the bulk of which are inverse estimates) support a weaker (less -ve) best estimate than -0.9 W/m2, particularly when the fact that Storelvmo et al 2016 does not, contrary to section 7.3.3.3, show any significant correlation between global dimming and SO2 emissions since the middle of the last century is taken into account. [Nicholas Lewis, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. The assessment in Storelvmo et al. (2016) does not use the correlation between dimming and SO2 emissions in any way. Note that inverse estimates are not used as a line of evidence in the SOD, because the same observational constraints are used for ECS/TCR, so using it also for aerosol ERF is circular.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
38678	42	50	43	27	Assigning a much smaller uncertainty range to GCM-based ERFari+aci estimates than to observational constraints is perverse when GCMs cannot physically simulate macro cloud behaviour, let alone aerosol or cloud microphysics. The AR6 assessment should heed the powerful view expressed by Stevens and Fiedler (2017; DOI: 10.1175/JCLI-D-17-0034.1): "We are averse to the idea that climate models, which have gross and well-documented deficiencies in their representation of aerosol–cloud interactions (cf. Boucher et al. 2013), provide a meaningful quantification of forcing uncertainty. 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." The evidence does not support any change in the assessed total ERFari+aci from the -0.9 W/m2 assessed in AR5. [Nicholas Lewis, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. The GCM estimates are entirely consistent with the satellite-based ones. No model is perfect (not even cloud-resolving ones), and satellite retrievals rely on a number of assumptions and are thus uncertain as well. The fact that these lines of evidence are consistent with each other increases confidence in the assessment. However, we have expanded the GCM uncertainty range slightly to reflect the fact that many of them are still missing aerosol effects on deep convective, mixed-phase and ice clouds.
18412	42	51	42	51	"other observational" , it would be nice to have an example of other observational constraints. Otherwise, it is to vague to the reader. [Gwenaelle GREMION, Canada]	Rejected. This is a summary of the studies discussed above.
18414	43	1	43	1	define the RFMIP [Gwenaelle GREMION, Canada]	Taken into account. Now defined on page 40 where it first appears.
14346	43	4			I do not think the first sentence is needed and the second may be slightly simplistic since land use change alters the whole surface energy balance so there can be additional indirect effects on surface temperature involving cloud and water vapour. Irrigation can also affect the surface temperature indirectly by altering the surface energy balance. This section could signpost to a limited discussion in 8.2.2.2.7 and perhaps elsewhere. [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Text has been shortened/rewritten.
15050	43	5	43	5	Satellite studies of aerosols measure the effects from all aerosols combined, not just anthropogenic aerosols and there's no reliable way to tell the difference. The negative ERF that's calculated is used to offset the larger positive effects of CO2, all of which are considered anthropogenic. The over-estimation of the anthropogenic negative effects from aerosols seems to be partially offsetting the over-estimation of the positive effects from CO2 emission and is one of several offsetting errors. [George White, United States of America]	Rejected. The assessment of the aerosol ERF is based on multiple lines of evidence, only one of which is satellite observations. Studies contributing to this line of evidence all consider the fact that not all aerosol particles are anthropogenic.
18416	43	7	43	8	how much, numerically speaking, is the cloud adjustment contribution to the ERFaci? [Gwenaelle GREMION, Canada]	Taken into account: page 41, line 5 of SOD now states this explicitly: -0.2 W/m2.
13416	43	24	43	24	"EWFaci" should be "ERFaci" [Govindasamy Bala, India]	Editorial
33230	43	24	43	24	EWF should be ERF,I believe [Mark Zelinka, United States of America]	Editorial
18418	43	48	43	48	define WMGHGs [Gwenaelle GREMION, Canada]	Rejected. Already defined on page 25, line 13.
53702	44	1	44	34	Please check SRLCC, ch2 [Jan Fuglestvedt, Norway]	Taken into account: SRCCL ch 2 is references.
13418	44	3	44	9	Land use change also changes the rate of evapotranspiration which could alter the water vapor and clouds in the atmosphere (Bala et al. 2008 PNAS; Devaraju et al. PCE 2015). The effects can be categoried as biophysical and biochemical effects. The biophysical effects include albedo change, evapotranspiration change and the roughness length change (which can influence the circulation). In the entire section, I see this discussion is missing. It is worth noting this omission. [Govindasamy Bala, India]	Taken into account: A discussion has been added on the effects on clouds and water vapour.
45556	44	11	44	32	This subsection is not very satisfying as it says nothing about why we expect land use change to affect the global radiation balance and just treats a few model studies as black boxes which is not really an assessment (in contrast the following section begins with a nice physical description for contrails). Clearly albedo is one way, and this was probably well known in previous IPCC reports. It is also expected that irrigation willl increase cloud cover and possible humidity (Boucher et al. 2005, Sherwood et al. 2018) which may explain some results in other studies. Water vapour emimssions seem to have been missed from this discussion particularly the opening paragraph. [Steven Sherwood, Australia]	Taken into account: A discussion has been added on the effects on clouds and water vapour.
13420	44	15	44	16	For what land cover change did the authors obtain a radiative forcing of -0.47 Wm^-2? For the historical land cover change? [Govindasamy Bala, India]	Taken into account: This has been clarified that this refers to the historical period.
18420	44	19	44	20	"small positive adjustment", how much is the "small" contribution? [Gwenaelle GREMION, Canada]	Accepted: This quantity has been added
18422	44	20	44	23	it is not clear the difference between the two studies. Why did they find different results? [Gwenaelle GREMION, Canada]	Taken into account: The point has been added that the disagreement highlights model dependence..

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
42058	44	37	44	37	A few more studies need (explictly) mentioning/assessing here, including Bock and Burkhardt (2016) 10.1002/2016JD025112 and Duda et al 10.5194/acp-19-5313-2019 [Keith Shine, United Kingdom (of Great Britain and Northern Ireland)]	Accepted: These studies have been added and assessed here
50168	44	47	44	48	section 7.3.3 does not mention contrail forcing [Joyce Penner, United States of America]	Accepted - text revised (sentence deleted)
47968	45	6	45	6	'Strong agreement' should be 'high agreement'. IPCC uncertainty language used incorrectly. Please refer to the IPCC guidance note on uncertainty for correct list of terms that can be used: https://wg1.ipcc.ch/SR/documents/ar5_uncertainty-guidance-note.pdf [WGI TSU, France]	Accepted - text revised
36602	45	6	45	8	It might be difficult to do, but it will help the reader if you add a short statement on the reasons for such a model dependence. [Carlos Mechoso, United States of America]	Taken into account. Model dependence is not mentioned in this paragraph. The reasons why ERF is expected to be small but positive are articulated in the previous paragraph.
53704	45	8	45	8	You keep the old number even if traffic inceased by 40%. You mention post AR5 updates withouth giving references, and use these to narrow the range. Would be good if you could explain a bit more and add references.(Given the attention on this sector, it would be useful with some updated and robust assessment of forcing). [Jan Fuglestvedt, Norway]	Taken into account: Further references have been assessed.
18918	45	23	45	23	Dust radiative forcing in the Arctic is calculated as the annual mean top of the atmosphere (TOA) IRF is 0.225 W m-2. High-latitude (>60 N) dust sources contribute about 39% to TOA IRF and have a larger impact (one to two orders of magnitude) on IRF per emitted kilogram of dust than low-latitude sources. Mineral dust deposited on snow accounts for nearly all of the bottom of the atmosphere (BOA) IRF of 0.135 W m-2. More than half of the BOA IRF is caused by dust from high-latitude sources. Kylling, A., Groot Zwaaftink, C. D., Stohl, A., 2018. Mineral dust instantaneous radiative forcing in the Arctic. Geophysical Research Letters 45. doi: 10.1029/2018GL077346. [Gwenaelle GREMION, Canada]	Taken into account. Thank you for making us aware of this study. We have excluded it from the assessment on the basis that (1) it did not separate out dust emissions that result from anthropogenic activities from purely natural sources (which we appreciate is difficult for mineral dust), (2) the evaluation is for IRF and not ERF, and (3) the quantity reported in this paper is really the instantaneous radiative effect rather than the instantaneous radiative forcing (forcing compared to no mineral dust, rather than for a pre-industrial baseline).
13422	45	33	45	33	Change "radiative" to "radiative forcing" [Govindasamy Bala, India]	Accepted
18424	45	36	45	38	the bottom message is not clear. The sentence is a little confuse/not clear [Gwenaelle GREMION, Canada]	Sentence rewritten for clarity - this is to do with ERF for soot on snow
53706	45	39	45	39	this doubling of the RF seems a bit random. Would be good with some more basis for this. [Jan Fuglestvedt, Norway]	Sentence rewritten for clarity - this is to do with ERF for soot on snow. Ref is Bond et al.
15052	45	44	45	47	The dominant solar variability per hemisphere comes from diurnal and seasonal variability. Per Figure 7.4, the average energy flux crossing the equator is tiny relative to the flux entering and leaving each by radiation, so from a radiant perspective, we can consider the 2 hemispheres to be acting independent of each other and subject not only to diurnal and seasonal solar variability, but to the nearly 6% variability in solar forcing between perihelion and aphelion. This is often ignored because its variability happens to be coincident with seasonal variability, albeit in phase in the S hemisphere and out of phase in the N. If the phase of perihelion with respect to the seasons is flipped, no GCM being considered gets close to the right answer whose expected average effect can be readily extracted from ice cores spanning dozens of 22K year cycles as perihelion precesses through the seasons. [George White, United States of America]	Rejected, but thank you for the comment. Solar and orbital forcing on multi-millennial timescales is dealt with in chapter 2 (here we assess climate change since pre-industrial). Additionally, we do not evaluate the effectiveness of climate models (but use them as a line of evidence in appropriate assessments).
29904	45	45			The cycle is not only observed in sunspots, therefore I suggest to change to "solar 11-year activity cycle" [Natalie Krivova, Germany]	Accepted - text revised
29906	45	45			The 205-210 cycle is not really well-confirmed and/or accepted, and is in any case very intermittent. So, this must be re-formulated carefully. The longer cycle is roughly 2400-year long, not 24,000. [Natalie Krivova, Germany]	Accepted - text revised
29908	45	46			Since the current cycle 24 is close to its minimum, "last three cycles" means cycles 22-24. I suppose, here cycle 21-23 are meant instead. Should probably be either "cycles 21-23" or last 4 cycles. [Natalie Krivova, Germany]	Accepted - text revised
24480	45	50			Formula should be difference (or change) in TSI not TSI absolute. [Joanna Haigh, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
29910	45	50			While the statement about the role of the 200-400 band is correct, the references are rather to the opposite, i.e. to the earlier outdated papers, where a much weaker role of this band was obtained. Current estimates of the contribution of the UV band to the TSI variability are by a factor of up to 2-3 higher than in the cited papers. The correct references are: Egorova et al. 2018 (already cited in this chapter later) Yeo, K.~L., Krivova, N.~A., Solanki, S.~K.\2017, Journal of Geophysical Research (Space Physics), 122, 3888 Yeo, K.~L., Ball, W.~T., Krivova, N.~A., et al. 2015, Journal of Geophysical Research (Space Physics), 120, 6055 Ermolli, I., Matthes, K., Dudok de Wit, T., et al. 2013, Atmospheric Chemistry & Physics, 13, 3945 Harder, J.~W., Fontenla, J.~M., Pilewskie, P., et al. 2009, \GRL 36, L07801 Krivova, N.~A., Solanki, S.~K., Floyd, L. 2006, Astron. Astrophys. 452, 631 [Natalie Krivova, Germany]	Taken into account: References have been added.
13424	45	54	46	1	The current sentence is not clear. I suggest to change as "This negative adjustment is due to stratospheric heating from increased absorption by ozone at the short wavelengths and the consequent increased longwave emission to space" [Govindasamy Bala, India]	Taken into account: combined with #58078
58078	45	54			Why does stratospheric heating cause a negative adjustment? [Nathan Gillett, Canada]	Taken into account - short explanation added. Warming of the stratosphere increases the longwave radiation upwards to space, by application of the Stefan-Boltzmann law, cooling the Earth-atmosphere system, and hence is a negative rapid adjustment (from the point of view of TOA downwelling radiation and hence as a contribution to effective radiative forcing). It is the opposite effect to a CO2-induced cooling of the stratosphere, which is a positive adjustment.
29554	46	5	46	9	I suggest a footnote to Table 7.7 to explain on what the ERF for the solar is based on. It is a rough estimate for TSI, not taking into account directly the spectral solar irradiance changes. [Katja Matthes, Germany]	Accepted –note added to the caption of table 7.8.
24482	46	8			ditto [Joanna Haigh, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
58080	46	11		14	Shouldn't the solar forcing be computed for exactly the same time periods as the other forcings? This would be most consistent. The approach described, based on a difference between solar minima, seems to designed to minimse the diagnosed solar forcing. But for example if there are changes in irradiance during solar maxima, this will also have an effect on climate. And if we are interested in understanding present day climate, then the forcing contributions of all the drivers should be diagnosed for the same years on a like-for-like basis. [Nathan Gillett, Canada]	Taken into account. Thank you for the comment. The reason why solar minima are used is articulated in detail in AR5 WG1 CH8, in that they are more stable between solar cycles than any other point of the solar cycle.
24486	46	17	46	19	Comment on why Steinhilber analysis (which gives lower variability) is preferred. [Joanna Haigh, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - in the second order draft the solar forcing discussion has been revised.
29914	46	17	46	19	Irradiance reconstructions since 1850 are still based on sunspot observations (Matthes et al. 2017), not on cosmogenic isotopes [Natalie Krivova, Germany]	Taken into account. In the FGD the sentence states that the present assessment is based on Steinhilber et al. (2012), and is not a description of what is common practice.
24484	46	17			Replace "sunspot number observations" by "observations of solar magnetic features" or similar. The TSI reconstructions use not only sunspot number but also the area covered by sunspots and faculae. [Joanna Haigh, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
29912	46	22	46	34	These two paragraphs are very confusing, unfortunately. 1.What is called here PMOD model (Shapiro et al. 2011) uses 10Be, not 14C. 2. PMIP4 does not use Vieira at al. (2011) or Shapiro et al. (2011). It uses the earlier versions of the models published by Egorova et al. (2018) and Wu et al. (2018). 3. Of the three cited "proxy reconstructions" (line 25), only Lean 2018 is indeed a proxy reconstruction. The two other models are semi-empirical, not proxy models. 4. "Proxies constructed from 14C and 10Be (line 22)": proxies of what? what are these proxies? Should this perhaps be irradiance reocnstructed from 14C and 10Be? 5. "Later recovery" (lines 27/28) or "slow recovery" (33/34): what is meant here? There is no discussion of such a "slow" or "later" recovery in the original paper. The authors could not comment on this to my enquiry either. There might have been some misunderstanding here. Furthermore, a slow recovery from the MM, would rather mean a weaker secular trend, not stronger, would not it? [Natalie Krivova, Germany]	Taken into account. Thank you for this detailed and thorough explanation. In the Second Order Draft the discussion of solar forcing has been revised.
45558	46	22			This makes it sound like you are using paleoclimate models to tell you the solar forcing. I hope it's based on observations! A bit more explanation is needed. [Steven Sherwood, Australia]	Accepted – text revised to elaborate on the data sources that the CMIP6/PMIP4 reconstructions arise from.
45560	46	40	47	10	This section says very little about how we measure volcanic aerosol forcing or how accurate we can expect this to be. [Steven Sherwood, Australia]	Taken into account. Thank you for the comment. We did not evaluate observational evidence in this section, we hope to do so for the Final Draft
14348	46	40			It could be mentioned that quiescent periods of below average explosive volcanic activity contribute a positive radiative forcing (e.g. 1998-2002; Schmidt et al. 2018). [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Accepted – this comment and reference has been included,
13426	46	44	46	46	Volcanic aerosols also absorb terrestrial LW radiation and heat the layer where they reside. This could lead to more LW emission to space which is a rapid adjustment. Is this effect included in the ERF estimate? [Govindasamy Bala, India]	Taken into account. This effect is included in the ERF estimate, as are many other adjustments both in the LW and SW. To reduce ambiguity, "in the shortwave" deleted.
47550	46	44	46	46	This sentence should probably point out that these estimates come from simulations. [Matthew Toohey, Germany]	Accepted - text revised
47552	46	46	46	46	The study is not driven by emissions, the study uses simulations which involve direct sulfur emissions rather than the prescribed aerosol optical properties [Matthew Toohey, Germany]	Accepted – point clarified
41644	46	47	46	47	"found a positive forcing" over what period? And how large compared to the episodic forcing from major eruptions? [Amanda Maycock, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account – we have said "positive rapid adjustment" which offsets the negative forcing, rather than "positive forcing".
27278	47	3	47	6	As noted in my comment to Ch 4, L21, 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, but thank you for pointing our attention towards this. We are only concerned with TOA changes as these drive changes in global mean surface temperature.
47554	47	7	47	8	Sentence starting "To give" is not a complete sentence. [Matthew Toohey, Germany]	Taken into account and rewritten.
47556	47	8	47	8	high confidence seems perhaps overconfident as these scalings are based on two models, and to-date totally unconstrained by any observational analysis. Will there be ay information given on how this confidence level is quantified? [Matthew Toohey, Germany]	Taken into account - agree that "high confidence" based on two studies is not appropriate. Marshall et al. (submitted) provides a third study that improves confidence in this range, which is narrowed due to the addition of the extra study, but we take the reviewer's suggestion that confidence in volcanic ERF is medium rather than high.
53708	47	26	47	26	Re high confidence But still there are only two papers that you build this on? [Jan Fuglestvedt, Norway]	Rejected: The reasons for confidence in these values were discussed in 7.3.2
41652	47	26	47	27	"tentatively assessed" I think the confidence level (medium) captures what you mean by tentatively here, so I suggest just saying it is assessed with medium confidence [Amanda Maycock, United Kingdom (of Great Britain and Northern Ireland)]	accepted - text revised ("tentatively" removed)
33430	47	26	47	28	Earlier, the assessment states, "which counteracts much of the increase in SARF identified by Etminan et al.": the sentence here says "around half": I'd make the two sentences consistent. [Marcus Sarofim, United States of America]	Accepted: The exact value has been added.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
15054	47	32	47	32	Figure 7.11: This shows that the nominal ERF from CO2 since industrialization is about 2 W/m^2, while the nominal effect from doubling asserted earlier is 4 W/m^2. Pre-industrial CO2 was 280 ppm, so based on the same linearity presumed elsewhere, half of doubling would be 420 ppm which is larger than current concentrations. This figure also infers that all of the increase in CO2 since the start of the IR is all anthropogenic. This ignores the delayed temperature dependence of CO2 concentrations clearly seen in the ice cores, that the start of the IR was coincident with the end of the LIA and that some amount of natural warming and the resulting CO2 increases should be expected. [George White, United States of America]	Rejected. CO2 (effective) radiative forcing is approximately logarithmic, not linear, with concentrations. See Myhre et al., 2017, Nature Geoscience: Halfway to doubling of CO2 radiative forcing; AR5; Etminan et al., 2016. The claim that any small (relative to present-day warming compared to 1750) increases in temperature following the Little Ice Age drove changes in atmospheric CO2 concentrations is unsupported by peer-reviewed literature.
44814	47	49	48	5	As main changes originate from CH4, list individual WMGHG separately. [Astrid Kiendler-Scharr, Germany]	Accepted: These changes have been made
25788	47	50	47	50	Table 7.7. Specify that the uncertainties are 5-95% and use square bracket notation. Where other than 5-95, note this. [Stephen E Schwartz, United States of America]	Accepted: These changes have been made
25790	48	1	48	1	in table give CO2 as separate entry (similar to figure 7.11) in view of importance of this gas at present and as reference for other gases. [Stephen E Schwartz, United States of America]	Accepted: This change has been made
37812	48	10			The increase in total anthropogenetic ERF of 8% over AR5 is described in this line as "modest". The adjective used in Table 7.7 is "slight". I would change "slight" to "modest" in Table 7.7, as "modest" is to my mind a more appropriate word to describe 8%. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - we stick with "slight" for now but may revise this in Final Draft
18426	48	51	48	51	the data for the AR6 column is missing in the contrails factor (table) [Gwenaelle GREMION, Canada]	Taken into account. Keep the "Combined contrails and contrail-induced cirrus" row and remove the contrails only.
18428	49	11	49	11	define LBL [Gwenaelle GREMION, Canada]	Rejected. Defined in the beginning of section 7.3.2.
18430	49	13	49	15	The paragraph is not well written. Maybe, something like: "Aerosols have in total contributed an ERF of -1.1 Wm2 (-1.8 to -0.5 Wm2 5-95% confidence range). Which, an -0.9(-1.4 to 0.5)Wm2 contribution from aerosol-cloud interaction and the remaining (-0.2 (-0.4 to 0)Wm2) related to the aerosol-radiation interactions. [Gwenaelle GREMION, Canada]	Rejected. We believe the text to be clearer as it reads now.
53710	49	18	49	18	This is good; and important step to calculate temperature contributions by component and not only RF. [Jan Fuglestvedt, Norway]	Noted. Thank you for the comment.
41654	49	21	49	21	Here and throughout the assessment there needs to be attention paid to the consistency of the use of simple EBM models. Fair, MAGICC and others (see e.g. Box 4.1) are used and the differences between them need careful comparison and explanation. [Amanda Maycock, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Motivated by this comment and others, there has been a grand community effort to compare simple climate models (rcmip.org). We have a cross-chapter box 7.1 that describes this. We do not base our assessments on FaIR now, but use the two-layer Held et al (2010) energy balance model. Results are very similar to those produced by FaIR in the First Order Draft, and in fact Nicholls et al (submitted) shows that there is very little difference in the projections between emulators.
18432	49	22	49	22	Define again the ECS and TCR terms, make the reading easier. [Gwenaelle GREMION, Canada]	Rejected. Terms are defined on page 5 and 6, respectively.
58082	49	23		24	The approach used to derive the TCR and ECS distributions appears to be somewhat arbitrary and is not fully described. Given the importance of this for the results shown in the chapter, this needs to be more clearly described and motivated. Is the approach used based on the published literature? If so, cite the study. [Nathan Gillett, Canada]	Agreed, this section is expanded on and a cross Chapter box added
18434	49	25	49	25	which forcing component was removed? [Gwenaelle GREMION, Canada]	Taken into account. Forcing agents removed one at a time in turn. Text updated to clarify

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39348	49	28			Section 7.3.5.3 P7-49 Insert before line 28: Before attributing warming to anthropogenic causes, it is necessary to establish the accuracy of centennial scale global estimates of temperature change. By comparing 6 global average temperature series from 1880-2012, (Lovejoy 2017) showed that with 90% confidence that the mean temperature change was correct to within ±0.108 oC. The dominant source of error for these centennial scale estimates are the space-time scale reduction factors of each series that are each slightly different. This is a direct consequence of small differences between the nominal space-time resolutions of the gridded data (e.g. one month and 5x5o) and the actual resolutions that depend on the amount of averaging in the space-time interval. The more averaging that is used, the smaller the variability, and small differences between the data sets lead to multiplicative differences in the estimated temperature changes since 1880. Lovejoy, S. (2017). "How accurately do we know the temperature of the surface of the earth? ." Clim. Dyn. [Shaun Lovejoy, Canada]	Rejected, but thank you for the comment. Evaluation of the global mean temperature change since pre-industrial times is the remit of chapter 1. Our forcing assessment provides an alternative line of evidence for attribution of climate change (chapter 3).
41656	49	30	49	32	The present day anthropogenic warming is known quite well (e.g. Haustein et al 2017 doi: 10.1038/s41598-017-14828-5) so can this help to constrain the joint distribution of possible warming due to ERF and response uncertainty? [Amanda Maycock, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We take this into account, but note that using Haustein et al to constrain ERF is a circular argument as their method is similar to ours. However, this plot will be updated using CMIP6 and AR6 data for the SOD. We note that forthcoming papers on "observed" ERF (e.g. Andrews and Forster) do not show a particularly tight constraint on anthropogenic ERF.
33432	49	34	49	36	It would be interesting to include the Hector model (Hartin et al.) in a comparison of simple climate models. [Marcus Sarofim, United States of America]	Accepted. A consistent emulator formulation throughout all of WG1 now appears as Cross Chapter Box 7.1
38680	49	43	49	51	The values for {solar + volcanic} 1750-2017 temperature contribution are inconsistent between figures 7.12 (top) and 7.15. [Nicholas Lewis, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. Thank you for the comment. Unfortunately we do not see the inconsistency. The attributed 1750-2017 solar + volcanic warming in fig 7.12 (top) is difficult to distinguish from zero, as it is in figure 7.15. More prominent major ticks in 7.15 would help interpretation of this figure so has been revised in Second Order Draft. Forcing time series is now passed to Chapter 2.
52036	49	43	49	51	This finding could be confused with a chapter 3 assessment of attributable warming. In reality the two come to very similar findings. Is it worth being more explicit in this regard as presumably coming at the problem from two distinct angles but concluding effectively the same thing is valuable. These will also need to be reconciled when pulling together the SPM presumably? [Peter Thorne, Ireland]	Taken into account. We agree, this change as suggested is now made and more explicitly compared to chapter 3. We have passed our results to chapter 3 to make a full comparison
58086	49	46		48	Observed GSAT change is used twice as a constraint in these calculations, since it is use to constrain estimates of TCR, ECS and ERF, and is then used again as a constraint on the EBM. Probably the fitting procedure assumes that the constraint on GSAT evolution is independent of the priors on TCR, ECS etc. [Nathan Gillett, Canada]	Taken into account. We agree. Only unconstrained results are now shown

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
58084	49	46		51	This unpublished analysis, in which an EBM is fitted to observed warming, to derive estimates of anthropogenic-attributable warming, GHG-attributable warming and aerosol-attributable cooling, falls within the outline of Chapter 3 and is out of scope for Chapter 7. This analysis is not based on multiple published studies, does not consider observational uncertainty or structural uncertainty in the EBM, and readers will be left wondering what are the reasons for any differences between results derived here and results assessed in Chapter 3. I strongly suggest that the analysis in Chapter 7 is restricted to the calculation of temperature-equivalent radiative forcings, using the EBM and assessed ECS, TCR and ERF distributions to convert ERFs into temperature changes, but does not include attempts to attribute observed temperature changes. These estimates can be brought together with estimates of anthropogenic-attributable warming from Chapter 3 based on historical temperature observations in the TS/SPM. Also the authors do not describe what instrumental observations of surface temperature they use for the period 1750 to 1850 in their analysis, which is outside the range of instrumental estimates of GSAT assessed in Chapter 2. Finally, in order to facilitate bringing together estimates of forced temperature changes from Chapter 7 with those from Chapter 3 in the TS/SPM, it would be useful if Chapter 7 could also calculate temperature changes relative to the AR6 agreed quasi-preindustrial base period of 1850-1900. As already noted, instrumental observations of GSAT are not available prior to this. [Nathan Gillett, Canada]	Taken into account. We agree, this change as suggested is now made and more explicitly compared to chapter 3
57888	49	47	49	47	How are the trends calculated? Is the same trend calculation used for OHC and other variables? [Catia Domingues, Australia]	Taken into account and thank you for the comment. In the end we decided to make no revision. "Trends" stated here are not quantitative. The numbers given describe the total warming since pre-industrial as well as the direction of travel. If climate was subjected to GHGs alone it would have experienced 1.7C of warming with an increasing trend in recent years, with aerosols alone contributing -0.6C (relatively stable in recent years). Ocean heat content change is also calculated using this model.
48890	50	8	50	9	The contribution to the global temperature change due to anthropogenic injection of carbon dioxide in the atmosphere, i.e. resulted from combustion of fossil fuels, is approximately 0.02C now. (Smirnov, 2018) [Pekka Sunila, Finland]	Rejected. In Smirnov (2018), the author presented a line by line model he developed to compute the radiative flux emitted by the atmosphere toward the surface. The value of this downward flux is computed for both the contemporary concentration of CO2 and for a doubling of this value. This article has several weaknesses, and among them two are important enough not to refer to this article: (1) the change in downward radiative flux at the surface in response to a doubling of CO2 is very different from that found in the scientific literature, without any mention or explanation being made (2) it has been known since several decades now (e.g. Manabe and Wetherald 1967) that the temperature change in response to an increase in CO2 concentration cannot be estimated from the downward radiative forcing at surface . The radiative forcing at the top of the atmosphere (or at the tropopause) must be considered, and the different feedbacks of the climate system must be taken into account.
37814	50	30	50	31	The Cowtan and Way dataset extends back only to 1850. This could be noted here. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable - this figure has been removed

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
38682	50	46	50	48	The maximum change in volcanic forcing from years in 2000-2005 to subsequent years is shown in Figure 7.14 as very close to -0.2 W/m2. But the figure for global mean stratospheric AOD for v3.0 of the CMIP6 dataset (at ftp://iacftp.ethz.ch/pub_read/luo/CMIP6/StratAerosols_CMIP6_Updates_v3.0.pdf) shows a maximum change in annual mean AOD of ~0.004, which scaled by 18.5 gives a maximum forcing change of -0.074 W/m2. Why the large discrepancy? Figure 7.14 looks wrong to me. AOD values for these years did not change from v3.0 to v4.0. [Nicholas Lewis, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The dashed line in figure 7.14 is for all natural forcing, so includes solar and volcanic components. In 2000, which is a solar maximum, the solar forcing is 0.081 and the volcanic forcing is 0.102 for a total of 0.183. At the following minimum in 2009, the solar forcing is -0.059 and the volcanic forcing is 0.024 for a total of -0.035. The difference of about 0.2 W/m2 between these times is more due to solar forcing than volcanic.
53712	51	15	51	19	Very useful graph. But a natural question to ask: How does this compare to observations… ? (See e.g. Haustein et al.) [Jan Fuglestvedt, Norway]	Taken into account. We combine with #41656; not plotting the Haustein attribution on to this plot as Haustein's analysis uses a similar method to ours. Agree we should cite that study.
37816	51	24	51	29	There are two references here to Earth System. There is also reference to "climate and biogeochemistry feedbacks". This I find confusing. The biogeochemistry that is relevant to the timescales of relevance is part of the climate system as defined by AR5, and the discussed Earth-system feedbacks that occur on longer timescales are all part of the climate-system subset of the Earth System. So I wonder why the references here are not simply to the climate system and climate-system feedbacks. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The definition and the sentences referring to the Earth System, the climate system, the climate feedbacks and the biogeochemical feedbacks have been revised in the whole chapter.
42126	51	24	73	14	Include exploration of how climate feedback changes with response timescale in Section 7.4. Section 7.4 explores how climate feedback may change with background climate state (subsection 7.4.4), and how climate feedback responds to changing patterns of surface warming (subsection 7.4.3). However, there is currently no discussion of how climate feedback evolves over different response timescales. Goodwin, (2018 doi:10.1029/2018EF000889) recently combined a processed-based analysis of climate feedbacks in CMIP5 models with historical observational constraints on surface warming and ocean heat content change to analyse how climate feedback evolves over different response timescales. In Goodwin (2018) prior distributions of climate feedback from many different processes are inserted into an initial efficient-model ensemble. The prior distributions used derive from the different processes evaluated in this chapter: in Goodwin (2018) the Planck response, water vapor-lapse rate feedbacks, cloud feedbacks and surface albedo feedbacks are taken from the analysis by Caldwell et al., 2016, and the surface warming pattern adjustment feedback is taken from Andrews et al., 2015). Goodwin (2018) then performs 10-million simulations with varying combinations of climate feedback due to each process, and evaluates which feedback-combinations agree with historical surface warming and ocean heat content change. See Goodwin (2018 - Table 1 and Figure 2 therein) for the CMIP5+observations constraint on how climate feedback evolves over different response timescales, starting at the Planck feedback at small response timescales and then progressing as more feedbacks become significant. The new climate feedback (y-axis) and response timescale (x-axis) plot of Goodwin (2018 – Figure 2 therein) is far more understandable version of the plots showing radiative imbalance (y-axis) versus temperature change (x-axis) found in the literature, where the changing gradient is linked to changes in climate feedback/sensitivity, for example found in the Knutti et al. (2017) review in Nature Geoscience. The advantage of the Goodwin (2018) method is that it is far more understandable in a pedagogical sense, and also easier to relate to the text, so suitable for the wide audience of IPCC AR chapters. My recommendations are: (1) An explicit equation with a timescale-dependence for alpha (climate feedback) should be included, after the equations in Goodwin (2018), within section 7.4.	Taken into account. Sections 7.4.3 and 7.4.4 explicitly discuss mechanisms of feedback changes in terms of their dependence on either global-mean temperature or the spatial pattern of sea-surface temperature. Goodwin (2018) models the feedback changes in terms of a time-dependence rather than in terms of these physical processes. However, a reference to Goodwin (2018) has been added to section 7.5 where appropriate.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
42128	51	24	73	14	Full references for above comment: Goodwin, P., (2018). On the time evolution of climate sensitivity and future warming, Earth's Future 6, EFT2466, https://doi.org/10.1029/2018EF000889 Caldwell, P. M., Zelinka,M. D., Taylor, K. E., & Marvel, K. (2016). Quantifying the sources of intermodel spread in equilibrium climate sensitivity. Journal of Climate, 29, 513–524. https://doi.org/10.1175/JCLI-D-15-0352.1 Andrews, T., Gregory, J. M., & Webb, M. J. (2015). The dependence of radiative forcing and feedback on evolving patterns of surface temperature change in climate models. Journal of Climate, 28, 1630–1648, doi:10.1175/JCLI-D-14-00545.1 Knutti, R., Rugenstein, M. A. A., & Hergerl, G. C. (2017). Beyond equilibrium climate sensitivity. Nature Geoscience, 10, 727–736, doi:10.1038/ngeo3017 [Philip Goodwin, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Sections 7.4.3 and 7.4.4 explicitly discuss mechanisms of feedback changes in terms of their dependence on either global-mean temperature or the spatial pattern of sea-surface temperature. Goodwin (2018) models the feedback changes in terms of a time-dependence rather than in terms of these physical processes. However, a reference to Goodwin (2018) has been added to section 7.5 where appropriate.
19254	51	27	51	27	Why is there a question mark after "Equation 7.1" ? Is it uncertain whether this equation will be used in subsequent drafts? Or is there some other indication here? A question mark also appears in other places with "Equation 7.1" (Page 51, Line 47). [Gwenaelle GREMION, Canada]	Taken into account. Question mark removed
14350	51	33			It has long been known that climate sensitivity changes over time (e.g. Senior and Mitchell 2000 GRL doi:10.1029/2000GL011373) but perhaps the implications for interpreting the historical record have only recently been recognised [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Text has been modified. References are given in the subsections.
15056	51	36	51	46	It's wrong to reference Hansen's 1984 paper without referencing the late Michael Schlesinger's follow on paper that corrected one of Hansen's errors which was confusing the gain term with feedback term, however; Schlesinger's derivation of the gain equation Hansen had wrong had an even more serious error which assumed non unit open loop gain in one place amplifying W/m^2 into temperature while assuming unit open loop gain in another. This happened when Schlesinger confused the feedback fraction with the feedback factor, where the feedback factor is the feedback fraction times the open loop gain and the two are identical if and only if the open loop gain is unity. The lack of conformance to Bode's simplifying assumptions was missed by both Hansen, Schlesinger and more recently by Gerard Roe in his 2009 paper that was a rehash of Schlesinger's gain equation derivation using different variable names, but repeating the same mistakes. [George White, United States of America]	Not applicable: Citation to Hansen et al. (1984) has been deleted in this context because of shortening of the text by referring to Box 7.1 that explains the energy budget framework in this chapter
15058	51	36	51	46	This plot conclusively demonstrates conformance to the relationships predicted by a simple gray body model. ModelE and likely no other GCM is consistent with the data when its results are presented in this manner. http://www.palisad.com/co2/tp/fig2.png The yellow dots are monthly averages for each 2.5 degree slice of latitude representing the relationship between the surface temperature (Y) and the emissions at TOA (X). The red dots are the monthly averages of the relationship between the surface temperature (Y) and net solar input (X) for the same slices. The larger dots are the averages for each slice over all 3 decades of data. The black line is the prediction of an ideal black body. The green line is the prediction of gray body whose emissivity is 0.62 relative to the surface temperature and planet emissions. The magenta line is the prediction of a surface emissions sensitivity of 1 W/m^2 of surface emissions per W/m^2 of forcing biased up by half the average absorption of surface emissions by the atmosphere. The data conforms quite well to this model and where the two curves intersect is consistent with the global averages otherwise reported in the ISCCP data set. [George White, United States of America]	rejected: We cannot cite unpublished contents
50948	51	44	52	34	Section 7.4.1.1. introduces the Kernel Method, while section 7.4.1.2. starts with stating that two methods are used, not explicitly relating to the kernel method. Please make the text more consistent. [Terje Berntsen, Norway]	taken into account: The text was revised to improve consistency
18814	51	47	51	47	Global mean surface temperature or "surface air" [Gwenaelle GREMION, Canada]	accepted
15060	51	53	51	54	Linearly summing alpha's assumes superposition relative to the relationship between W/m^2 and temperature and this assumes strict linearity which is not the case given the T^4 dependence of W/m^2. In addition, assuming a difference between the ECS and TCR further undermines the assumption of superposition. One final point is that feedback power is linearly proportional to W/m^2 of emissions and not temperature but to the fourth root of temperature. [George White, United States of America]	rejected: The energy budget equation (7.1), derived by the Taylor expansion to the TOA energy balance, has been widely accepted and shown to well approximate the evolution of climate state driven by radiative forcing. The linear decomposition of alpha into components includes an assumption that they are to the first order independent (except for water vapour and lapse rate feedbacks), but the relevance of this assumption was also discussed in the text.

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14352	52	5			could add more detail e.g. in particular to include thermodynamic increases in water vapour and maintenance of a moist adiabatic temperature lapse rate within the Planck response while considering lapse rate changes and relative humidity changes as additional feedbacks [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	noted: This section is a short introduction of the framework, and physical processes associated with individual feedbacks have been explained later in 7.4.2
14354	52	5			An alternative framework is considered by Ceppi & Gregory (2019) Clim. Dyn., doi:10.1007/s00382-019-04825-x, which includes a dependence on the spatial pattern of warming through it's influence on atmospheric stability [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	noted: The effect of warming pattern on the climate feedback does not explicitly appear in the global mean energy budget so not discussed here, but the effect has been fully discussed in 7.4.3.
58088	52	6		8	Some such feedbacks are assed in Chapter 5 - see Figure 5.29. [Nathan Gillett, Canada]	Noted. This section is mainly a short introduction of the framework. Chapter 5 is now cross-referenced in 7.4.2.5
50950	52	7			The sentence describing biogeochemical feedbacks can be misunderstood. These feedbacks lead to changes in aerosols and chemical composition, but are not caused by that. Should also a class of biogeophysical feedbacks be included, e.g. vegetation changes causing albedo changes, changes in evapotranspiration, surface roughness etc.? [Terje Berntsen, Norway]	noted: By definition, feedback occurs via temperature change, which can alter aerosol and chemical compositions and they in turn change the TOA radiation. We have discussed the vegetation change that affects surface albedo feedback in Section 7.4.2.3.
44816	52	10	52	18	Needs some elaboration onconditions under which validity of assumptions is given. What is "small" in this context? [Astrid Kiendler-Scharr, Germany]	taken into account: We have added an explanation about the underlying assumption
57890	52	11	52	11	What is meant by "deeper analysis"? More in-depth analysis? [Catia Domingues, Australia]	Taken into account. Rewritten for clarity.
19256	52	12	52	12	Can you provide some citations or examples of these "recent modeling studies" which are alluded to? This radiative kernel method is going to be central to the rest of the feedbacks section so it seems important to give a good amount of background to the reader here. [Gwenaelle GREMION, Canada]	taken into account: The paragraph was revised in the SOD
18816	52	12	52	16	Down-scaling and revisiting of the sentence is required. [Gwenaelle GREMION, Canada]	taken into account: The paragraph was revised in the SOD
19258	52	16	52	17	Why is the kernel method only valid for small temperature changes? There is a citation provided but since this method is so central to the section perhaps more elaboration - one or two more sentences explaining the validity of the method could be provided. [Gwenaelle GREMION, Canada]	taken into account: We have added an explanation about the underlying assumption
18818	52	18	52	18	"Biogeochemical feedbacks need specific methods ()" sentence incomplete. It will be helpful if method is mentioned with the relevant in-text citation (Gregory et al., 2009 and Heinze et al., 2018) [Gwenaelle GREMION, Canada]	taken into account: The text was revised in the SOD
19342	52	18	52	18	This sentence does not provide much information. Consider elaborating in a sentence or two. [Norman Loeb, United States of America]	taken into account: The text was revised in the SOD
41658	52	18	52	18	cross reference to chapter 5 here? [Amanda Maycock, United Kingdom (of Great Britain and Northern Ireland)]	noted: This section is a short introduction of the framework, and a cross reference to Chapter 5 is mentioned in 7.4.2.5
19260	52	29	52	30	It was a bit unclear to me what you meant by 'latter' and 'former' here. Did you mean 1) the regression slope method and 2) the atmosphere-only SST method? Can this sentence be made clearer, for instance by just stating the method instead of saying 'latter' and 'former'? [Gwenaelle GREMION, Canada]	Not applicable. Section has been rewritten.
18820	52	29	52	30	"latter" and "former" seems to have been interchanged [Gwenaelle GREMION, Canada]	accepted
19262	52	31	52	31	specificity of experimental design' seems like a very general term here. Can you be more specific? Do you mean when model specifics/design are taken into account? Like physics parameterizations? [Gwenaelle GREMION, Canada]	taken into account: The sentences was rephrased in the SOD
38684	52	36	52	39	Since GCMs cannot physically simulate macro cloud behaviour or cloud microphysics, it is not reasonable to place much weight at all on their behaviour when assessing cloud feedbacks. It is very likely that there are biases common to all GCMs, [Nicholas Lewis, United Kingdom (of Great Britain and Northern Ireland)]	rejected: We acknowledge that GCMs contain errors and cannot well represent some processes (e.g., clouds). As stated in the text, the assessment of cloud feedbacks is therefore based not only on GCMs but also on other lines of evidence.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
19264	52	41	52	41	Can past studies be provided here which assumed independence between feedbacks? [Gwenaelle GREMION, Canada]	rejected: This paragraph explains the structure of the section and too much discussion on the feedback co-dependence with citing references disturbs readers. This issue has been discussed in 7.4.1.2 where we described individual feedbacks in CMIP models.
52038	53	1	53	8	In this paragraph is it worth alluding to the work on portability which has been shown for e.g. EC-EARTH not to be guaranteed and may also apply to other models that are run across a family of supercomputers? E.g. https://www.geosci-model-dev-discuss.net/gmd-2019-91/ which very recently appeared. [Peter Thorne, Ireland]	rejected: The suggested issue sounds interesting but does not fit the scope of this section.
50952	53	10	53	30	The quantitative discussion of feedbacks starts with fig 7.16 and text (lines 10-30). These are all based on 4xCO2 experiments (and will be for CMIP6). By construction 4xCO2 does not include C-cycle feedbacks properly, and due to the strong forcing the climate state is strongly perturbed with implications for the feedbacks. There is a need to include a critical discussion of how the set-up of the 4xCO2 experiments affects the interpretation of the results. [Terje Berntsen, Norway]	noted: Primary purpose of 7.4.1.3 is to evaluate climate feedbacks in two generations of CMIP models and the values are not directly used for the synthesis assessment of the feedback in 7.4.2.6. The energy budget framework used in this chapter (see Box 7.1) explains how the climate system responds to the radiative forcing with a prescribed CO2 concentration in the atmosphere, so that the carbon cycle feedback that alters the CO2 concentration is not included (assessed in Chapter 5 instead),
14382	53	11			It is worth considering whether the cloud feedback can be split into the individual feedbacks in an additional panel (e.g. from Table 7.8) [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	rejected: Table 7.8 does not provide numbers, so cannot be shown by a diagram
19266	53	15	53	15	This Planck Feedback value is much higher than what is plotted. The plotted value is only ~-0.8 W/m^2. I see on the figure that it is labeled as '(1/4)' but there is no mention of this in the Figure caption. Furthermore it does not seem appropriate to display the Planck value as a quarter of the real value. This is confusing and misleading. With the Planck Feedback plotted this way it makes it difficult to understand the total feedback becuase the values obviously do not add up. [Gwenaelle GREMION, Canada]	Noted. The label '(1/4)' meant that the value had been multiplied by 0.25 to ease comparison of the Planck feedback with others. The figure has been updated in the FGD, now showing the Planck feedback without the factor.
13428	53	15	53	15	"range" should be changed to "uncertainty"? Similar issue on line 19. [Govindasamy Bala, India]	Rejected. Likely range is what we mean to say here.
41662	53	15	53	16	What is the justification for using 2.5% of mean value for the likely range? [Amanda Maycock, United Kingdom (of Great Britain and Northern Ireland)]	taken into account. The text has been revised in the FGD.
14356	53	18			"counteracting" the Planck feedback (rather than compensating which to me implies cancelling whereas it essentially reduces the feedback by about one third relative to the Planck response). How have the magnitudes and ranges changed since AR4? [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	accepted: We have modified the sentence. The change of WV+LR feedbacks between AR5 and AR6 is mentioned in Section 7.4.2.2.
45562	53	19	53	24	These "likely" statements confuse model spread with probability density. It is a very strong assumption to do this and if so it needs to be stated. AR5 doubled the model spread on the water vapour/lapse rate feedback, for example, to get an assessed pdf. [Steven Sherwood, Australia]	Accepted: We did not intend to assess the 'likely range' solely from the CMIP5/6 models but describe how the feedback terms are different between the two CMIP ensembles. The section has been revised considerably for the FGD.
58090	53	22		23	It is not clear what it means to say that the range of 0.18 - 0.7 W/m^2 is much larger than the mean value of 0.44 W/m^2. [Nathan Gillett, Canada]	taken into account: The text was revised in SOD

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
52040	53	47	53	48	While I agree this is not typically the case there are models - as assessed in chapter 4 - with very large (o.100) ensembles where this would no longer hold true. Is there a role for these in the assessment here and if not why not? [Peter Thorne, Ireland]	noted: Recent large ensembles (e.g., CESM LE) are generated by perturbing initial conditions for better separating internal variability and forced climate response, but they cannot deal with model uncertainty by definition because the feedback calculated with a 4xCO2 experiment should not vary among the ensemble members as far as the model code is identical.
41660	54	4	54	4	for similarities in clustering of response see also Boe (2018) doi: https://doi.org/10.1002/2017GL076829 [Amanda Maycock, United Kingdom (of Great Britain and Northern Ireland)]	This subsection has been entirely rewritten and the sentence you point out has been deleted.
41664	54	6	54	19	Will this be evaluated in CMIP6 models in the SOD? [Amanda Maycock, United Kingdom (of Great Britain and Northern Ireland)]	accepted: We'll evaluate CMIP6 models in SOD
58092	54	9		10	Could this apparent co-dependence of the feedbacks in climate models arise from tuning of the models (climatology and/or climate response)? [Nathan Gillett, Canada]	noted: It is likely that a compensation between the total climate feedback and aerosol cooling arises from model tuning for the 20th century warming (Hourdin et al. 2017 BAMS), but there is no evidence that shows the model tuning causing co-dependence among individual feedbacks.
12502	54	14	54	16	Even without an increase (or change) in low-level clouds the shortwave cloud radiative effect of clouds in the Arctic will change by reduction in Arctic sea ice. Due to the darker ocean surface compared to brigher sea ice, the shortwave cloud radiative effect is enhanced even without cloud changes (Gilgen et al, 2018). Gilgen, A., Huang, W. T. K., Ickes, L., Neubauer, D., and Lohmann, U.: How important are future marine and shipping aerosol emissions in a warming Arctic summer and autumn?, Atmos. Chem. Phys., 18, 10521-10555, https://doi.org/10.5194/acp-18-10521-2018, 2018. [David Neubauer, Switzerland]	rejected: an apparent cloud feedback not due to change in cloud, called the cloud masking effect as has been calculated using radiative kernels, was excluded from the cloud feedback but included in others (e.g. surface albedo feedback).
19268	54	17	54	17	This could be because the CMIP5 model physics representation of these feedbacks was not sufficent. What really would be interesting is whether progress was made with respect to CMIP6. Again it is unfortunate the time gap between CMIP and IPCC. Hopefully if CMIP6 data will be available in future drafts some discussion of whether co-dependencies between feedbacks was improved can be added. [Gwenaelle GREMION, Canada]	noted: We do hope that the full analyses to CMIP6 models can be incorporated in the final assessment
14358	54	17			not very strong? [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Changed to "not strong"
19270	54	32	54	33	It doesn't seem physically consistent to assume both the atmosphere and surface warm by the same amount. This maybe makes more sense if the Planck feedback is added to the Lapse Rate feedback. Am I thinking about this correrctly or are the Lapse Rate and Planck feedback not related in this way? [Gwenaelle GREMION, Canada]	Noted. The temperature feedback is indeed the sum of the Planck and the Lapse Rate feedback. But the decomposition in two feedbacks facilitate the physical understanding of climate response.
38686	54	41	54	43	Why isn't the assessed Planck response -3.22 W/m2/K, in line with the (mean of two kernels) estimate in Vial et al (2013) for linear behaviour models (or -3.20 for all models)? Also, I think a correction needs to be made for the downwards bias in emissivity arising from using abrupt4xCO2 simulation data to estimate Planck feedback; Planck feedback falls with increasing CO2 concentration (Mauritsen et al 2019). [Nicholas Lewis, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The assessed value is based on multiplied line of evidence, not only on numbers of one papers. In any case the value mentioned by the expert is in the likely range of -3.3 to 3.0 W.m-2.°C-1. Concerning the correction for 4xCO2, we do not considered it as we estimate the feedbacks for current climate state. The non linearity of the response is addressed in another section (7.4.3)
41650	54	46	56	24	some of this material is rather review like and could be reduced to focus on what is new since AR5 [Amanda Maycock, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. In the FGD the text has been modified to emphasize assessment of new literature since AR5.
52042	54	48	54	53	This is not quite true. In all the ESMs it isn't quite a pure moist adiabat as shown in Santer et al., 2005, 2008 and, more recently, Mitchell et al., 2013. It is close to a moist adiabat but not quite in the multi-model mean. [Peter Thorne, Ireland]	Accepted. Text has been modified

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41648	54	51	54	52	references appear in different font [Amanda Maycock, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
14360	55	2			"by a balance" - this whole section is excellent by the way! [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Thank you!
27242	55	10	55	11	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 substantiate 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
9306	55	10	55	24	Given the Clausius-Clapeyron relationship, I believe that the key reason for the WV to supply a positive feedback is the observation that the relative humidity is hardly dependent on warming. Now why is that so? On lines 11-12 I read "as the relative humidity is nearly constant", quoting Soden and Held. Soden and Held actually write in their 2006 paper: "it is well known that climate models tend to maintain a fixed tropospheric relative humidity as they warm". In other words, while we empirically know this to be true, we still miss the explanation. I hope you will find a reference allowing to remedy this weakness. [philippe waldteufel, France]	Accepted. More explanation and additional references have been given.
14362	55	22			Ingram (2013) proposed a similar decomposition as mentioned in 7.4.1.1 [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Reference has been included.
18822	55	26	55	33	In this paragraph, some numerical values need to be given for a better understanding rather than "much less", "almost perfect", "small". [Gwenaelle GREMION, Canada]	Accepted. Text has been modified to be more precise.
58094	55	28		29	How can the feedback be defined other than for the whole column? The definition of the feedback includes the surface temperature. [Nathan Gillett, Canada]	Noted. The kernel methods (among other methods) allows to estimate how much the various part of the atmosphere contribute to the change of the flux at the TOA. Of course these changes of the atmosphere are primarily driven by the change in the surface temperature.
18824	55	29	55	30	Sentence framing. It is confusing that inter-model spread can be large or the changes in relative humidity. [Gwenaelle GREMION, Canada]	Accepted. Text has been modified.
14364	55	30			Potentially large tropical relative humidity changes seems to contradict earlier statements that RH changes are small; is there any observational evidence? [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Text has been modified to be more precise.
58096	55	31			Doesn't this argue against treating the LR and WV feedbacks together? [Nathan Gillett, Canada]	Accepted. Text has been modified to be more precise.
27280	56	1	56	24	It would be good to mention the stratospheric water vapor feedback somewhere in this section, based on the recent estimates given in Banerjee et al., 2019 "Stratospheric water vapor: an important climate feedback" (DOI:10.1007/s00382-019-04721-4) [Gabriel Chiodo, Switzerland]	Accepted. A paragraph on stratospheric water vapor feedback has been added.
14366	56	4			Signposting to Section 7.4.2.4 with assessment of the Mauritsen & Stevens (2015) Nature Geoscience doi:10.1038/ngeo2414 study seems appropriate [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Text has been modified.
14368	56	16			There does not seem to be any observational evidence presented. Can this be signposted to Chapter 2 or 8? [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Noted.
52044	56	23	56	24	Given how wrong we would need to be is the use of virtually certain adding unnecessary ambiguity here and should this not instead be stated as a simple unambiguous statement of fact? [Peter Thorne, Ireland]	Accepted. Text has been modified.
28200	56	34	56	34	Refer to Chapter 9 sections discussing observed changes in sea ice area and snow cover. [Chad Thackeray, United States of America]	Accepted. Reference to chapter 9 has been included.
14370	56	36			Reference? "Reduced snow cover on sea ice may contribute as much to albedo feedback as reduced extent of sea ice." [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Reference to recently-published paper (Zhang et al. 2019) added.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
31442	56	43	56	43	"CMIP5 models show large spread in αA..." True. So isn't this a bit in contradiction to the overall assessment at the end of the section that says that there is high confidence in the estimate of the albedo feedback, partly based also on models? Can you say some words on the sources of that spread in models, and why this doesn't induce more uncertainty? [Gerhard Krinner, France]	Taken into account. More precisely, Schneider et al. (2018) calculated the CMIP5 multi-model standard deviation in albedo feedback to be 0.10 W/m2/K, relative to the multi-model mean of 0.40, as communicated later in the subsection. Considering this, we changed the text to "moderate spread". We also clarified that primary sources of model spread are differences in simulated sea-ice loss and differences in modelled snow cover response in boreal forest regions.
58098	56	47		49	Is there really a threshold at 0.5 K warming? Isn't it just the case that the signal to noise ratio gets larger as the warming gets larger? [Nathan Gillett, Canada]	Noted. Yes, this is the case. It is merely that the signal becomes statistically distinguishable from the noise when global delta T exceeds roughly 0.5 K. This passage was removed, however, to reduce text.
18826	57	2	57	2	Unit is missing. [Gwenaelle GREMION, Canada]	Editorial
19272	57	7	57	9	This is alarming. Is there any indication that improvements were made in terms of sea-ice loss with CMIP6 models? [Gwenaelle GREMION, Canada]	noted: Evaluation of sea ice change in the CMIP6 historical runs should be made in 9.3, which is referred to in this section
50954	57	34	57	34	Short-term albedo-vegetation feedbacks will be assessed in SOD (placeholder). On page 7-51, long-term feedbacks are defined as “larger than a few centuries”. Please make sure potential century scale vegetation effects (under 4xCo2) is included here then. [Terje Berntsen, Norway]	Rejected. We have assessed the fast vegetation-albedo feedback as a part of the biophysical feedback in Section 7.4.2.5, but the vegetation-carbon cycle feedback operating at century time scales was not included in 'Long term feedbacks' in Section 7.4.2.6. This is because the feedbacks assessed in this Chapter are those happening to a given increase in the atmospheric CO2 concentration and the carbon cycle feedbacks that modify the CO2 concentration are assessed in Chapter 5.
50956	57	36	57	36	High confidence – when vegetation effects are not yet assessed? [Terje Berntsen, Norway]	Accepted. We have assessed the vegetation feedback as a part of the biophysical feedback in Section 7.4.2.5, and explained why it's sign is assessed as positive with high confidence there.
40746	57	40			Section 7.4.2.4. Somewhere in here there should be a link to section 7.4.3. An important cloud feedback is that marine stratus move in response to slow changes in ocean currents. This is a significant reason for time-dependent changes in the feedback parameter. Reading this excellent section on cloud feedbacks I kept wondering how marine clouds move around, and only saw that later when I got to the next section. [Daniel Murphy, United States of America]	taken into account: We have referred to 7.4.3 when describing how the cloud feedback is assessed in this section.
13438	57	44	62	33	Excellent discussion of the cloud feedbacks. Very clear. [Govindasamy Bala, India]	Noted. Thank you!
14374	57	44			7.4.2.4 may have to assess how confidence in cloud feeback magnitudes have changed/converged in models and whether they explain an increase in overal climate sensitivity in CMIP6 models (e.g. more confidence in positive low altutyde cloud feedback) [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	taken into account: Evaluation of cloud feedbacks in CMIP6 models has been included in 7.4.2.4 of the FGD and the assessment in this section was revised accordingly.
33178	57	49	57	51	poorly phrased sentence with grammatical errors that is hard to follow [Mark Zelinka, United States of America]	Taken into account. Sentence is rewritten for clarity.
14372	57	50			"it has been both an observational and modelling challenge"; remove ", respectively." [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. Section has been rewritten.
19274	58	1	58	1	Was 'CRE' defined yet? I assume this refers to the 'cloud radiative effect' but I don’t see it defined in an immediately previous section. [Gwenaelle GREMION, Canada]	editorial

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
58100	58	3			Why does it follow from the first part of the sentence that the net CRE shows a large negative effect over the eastern part of the oceans? [Nathan Gillett, Canada]	accepted
33232	58	6	58	6	Replace "Albeit" with "While" [Mark Zelinka, United States of America]	Not applicable. Section has been rewritten.
12504	58	6	58	8	But the global mean net CRE can be a tuning target (Hourdin et al., 2017). So this agreement is not surprising and does not mean that the models have a realistic representation of clouds but rather that they are well tuned to present day conditions. [David Neubauer, Switzerland]	We stated here that the overall distribution of CRE in GCMs is similar to satellite observbations, and as you suggest this agreement would have been affected less by tuning than the global-mean CRE.
11664	58	20	58	23	Confused statements: ''was due to a lack of sufficient supercooled cloud liquid droplets that should increase the cloud optical depth.'' The sufficient supercooled cloud liquid droplets are not simulated well by models, thus it is a lack in the models, not in the observations. [Chuanfeng Zhao, China]	Taken into account. Sentence is rewritten for clarity. Now discussed in subsection 7.4.2.4.1
13980	58	25			There is still large uncertainty on the amount of precipitation over the SO from various products (including satellite-based products) and reanalysis (E.g. Behrangi et al. 2014, DOI:10.1175/JCLI-D-13-00679.1 & 2016, doi:10.1002/2015JD024546) Given the direct link between precipitation and latent heat fluxes, there should still be considerable uncertainty in the surface energy budget. There is also considerable uncertainty in the heat fluxes over the SO in various products (as you acknowledge on page 12 line 46.) [Steven Siems, Australia]	noted: Agree with uncertainty in precip and surface heat flux, but errors in the SO clouds mentioned here come mainly from a poor representation of mixed-phase clouds in microphysics scheme, but not directly related to potential errors in surface energy budgets
58102	58	44			Cross-reference chapters 2 and 3 here for observed changes in circulation and their attribution/detection. [Nathan Gillett, Canada]	accepted: We refer to Sections 2.3.1 and 3.3.3 here
38688	59	17	59	23	No account has been taken of Southern Hemisphere Hadley cell (HC) edge latitude extent and shifts in affecting cloud feedback. In 4×CO2-forced runs, models with excessively equatorward climatological HC extents produce stronger SW cloud radiative warming in the lower mid-latitude region and tend to have larger climate sensitivity values than models with more realistic climatological HC extents. Most CMIP5 models have a more equatorward climatological HC extent than observations indicate (Lipat et al 2017; doi: 10.1002/2017GL073151). The effects of this bias should be corrected for in assessing cloud feedback in GCMs. [Nicholas Lewis, United Kingdom (of Great Britain and Northern Ireland)]	taken into account: The issue of SH Hadley cell expansion is related with the extratropical cloud amount feedback assessed on p61 l2-19. The text was revised taking studies such as Lipat et al. (2017) into consideration in SOD.
33180	59	25	59	41	Several recent developments should be noted: Thompson et al (2017) [www.pnas.org/cgi/doi/10.1073/pnas.1620493114] extended FAT to the extratropics, providing a theoretical basis for the positive altitude feedback outside of the tropics as is simulated in all GCMs (Zelinka et al 2016). However, research by Seeley et al (2019a) [10.1029/2018GL080747] and Seeley et la (2019b) [10.1029/2018GL080096] calls into question several of the ingredients of the FAT hypothesis, and leaves open the possibility that high cloud tops need not track isotherms as they rise in altitude with warming. This actually agrees with what GCMs do in practice, where a slight warming of cloud top occurs (Zelinka and Hartmann 2010), though perhaps not for the reasons stated in Zelinka and Hartmann (2010). . [Mark Zelinka, United States of America]	taken into account: We'll cite Thompson et al. paper and also assessed the cloud radiative feedback partly driving jet shifts in middle latitudes (Li et al. 2019). However, after carefully evaluating papers by Seeley et al., we judged that their CRM experiment is highly idealized and not relevant to assess FAT in a realistic condition. A high agreement on FAT among GCM and observations supports this judgement.
12506	59	25	59	41	Hartmann et al. (2018) show that cloud radiative effects have a significant role in the lifetime of anvil cirrus clouds and raise the question whether this has an impact on the FAT mechanism and whether the relevant processes including turbulence and microphysics in anvil clouds can be parameterized adequately in global climate models. Hartmann DL, Gasparini B, Berry SE, Blossey PN. The Life Cycle and Net Radiative Effect of Tropical Anvil Clouds. J Adv Model Earth Sys. 2018;0(0). doi:doi:10.1029/2018MS001484. [David Neubauer, Switzerland]	noted: Hartmann et al. (2018) is a nice study that examined microphysical processes and radiative effect of anvil clouds during the life time in a LES, but they are not directly related with the FAT mechanism (will rather be related with the high-cloud amount feedback) and evaluation of the cloud feedback to surface warming was not made in their paper.
33182	59	34	59	35	The increase in cloud altitude with warming on interannual timescales is also clearly shown in Zelinka and Hartmann (2011) [doi:10.1029/2011JD016459] and Vaillant de Guélis et al (2018) [DOI:10.1038/s41598-018-34943-1]. Also, Marvel et al (2015) [DOI: 10.1175/JCLI-D-14-00734.1] show a positive tropical cloud top altitude trend from ISCCP and PATMOS-x long-term satellite cloud datasets. [Mark Zelinka, United States of America]	We have cited Marvel et al. (2015) (as Zelinka and Harmann was a little old paper) in the assessment of the altitude feedback. Vaillant de Guelis et al. (2018) has been cited in assessing the high cloud amount feedback as they extensively analyzed the amount there.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
43188	59	43	60	14	The paragraph of this assessment needs updated references. There are observational evidences that upper clouds show increase trends and more complex, particularly for high thin clouds: Zelinka and Hartmann (2011), Liu et al. (2017), Su and Jiang (2013), Eastman et al. (2011,2013). Numerical results with high-resolution model support these results by Chen et al. (2016) and review by Satoh et al. (2018). Zelinka, M. D., and D. L. Hartmann (2011): The observed sensitivity of high clouds to mean surface temperature and anomalies in the tropics, J. Geophys. Res., 116, D23103, doi:10.1029/2011JD016459. 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. Su, H. and J. H. Jiang (2013): Tropical clouds and circulation changes during the 2006/07 and 2009/10 El Niños, Journal of Climate, vol. 26, 399-413, Doi: 10.1175/JCLI-D-12-00152.1. Eastman, R., and S. G. Warren, 2013: A 39-yr survey of cloud changes from land stations worldwide 1971-2009: Long-term trends, relation to aerosols, and expansion of the tropical belt. J. Clim., 26, 1286–1303, doi:10.1175/JCLI-D-12-00280.1. Eastman, R., S. G. Warren, and C. J. Hahn, 2011: Variations in cloud cover and cloud types over the Ocean from surface observations, 1954-2008. J. Clim., 24, 5914–5934, doi:10.1175/2011JCLI3972.1. Chen, Y. W., T. Seiki, C. Kodama, M. Satoh, A. T. Noda, and Y. Yamada, 2016: High cloud responses to global warming simulated by two different cloud microphysics schemes implemented in the nonhydrostatic icosahedral atmospheric model (NICAM). J. Clim., 29, 5949–5964, doi:10.1175/JCLI-D-15-0668.1. Satoh, M., A. T. Noda, T. Seiki, Y.-W. Chen, C. Kodama, Y. Yamada, N. Kuba, and Y. Sato, 2018: Toward reduction of the uncertainties in climate sensitivity due to cloud processes using a global non-hydrostatic atmospheric model. Prog. Earth Planet. Sci., 5, 67, doi:10.1186/s40645-018-0226-1. https://doi.org/10.1186/s40645-018-0226-1. [Masaki Satoh, Japan]	taken into account: The text was revised taking suggested references into consideration.
38542	59	49	59	50	Precipitation efficiency (PE) is introduced but it has various definitions. It is better introduce current understanding of PE by Sui et al. (2007). PE is a useful concept. More PE aspects on cloud feedbacks should be expanded in Chapter 7. Sui, C.-H., X. Li, and M.-J. Yang, 2007: On the Definition of Precipitation Efficiency. J. Atmos. Sci., 64, 4506–4513, doi:10.1175/2007JAS2332.1. http://journals.ametsoc.org/doi/abs/10.1175/2007JAS2332.1. and Sui is not preparing a review paper on PE. [Masaki Satoh, Japan]	noted: We have discussed that the anvil cloud feedback greatly depends on microphysical processes including precipitation efficiency in models, but given uncertainty of this feedback we cannot use much more space for this particular cloud regime.
49140	59	54	59	54	Is this "likely" an uncertainty statement? [Yu Kosaka, Japan]	editorial
58104	59	54			Is this 'likely' an application of calibrated uncertainty language? If not, use another word. If it is, describe the underlying quantitative analysis on which it is based. [Nathan Gillett, Canada]	editorial
14376	60	4			The assessment of tropical high cloud feedback could include the "tropical squeeze" effect e.g. Su et al. (2017) Nature Comms https://www.nature.com/articles/ncomms15771 [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	taken into account: The text was revised taking suggested references into consideration.
43186	60	7	60	7	Recent active studies shows that the results by Bony et al. (2016) are not supported by various results. As opposed by the stability iris effect proposed by Bony et al. (2016), cloud microphysics and turbulent mixing affects the high cloud changes (Ohno and Satoh (2018) and Ohno et al. (2019)) and more analysis is being undertaken by the radiative convective equilibirum intercomparison experiments proposed by Wing et al. (2018). Needs changes to the statement. "A thermodynamic mechanism referred to as the 'stability iris effect', independent of convective aggregation, has also been proposed to explain the tendency of many GCMs and CRMs to predict a decrease of the anvil cloud amount with warming (Bony et al., 2016; Cronin and Wing, 2017)." Ohno, T., and M. Satoh, 2018: Roles of Cloud Microphysics on Cloud Responses to Sea Surface Temperatures in Radiative-Convective Equilibrium Experiments Using a High-Resolution Global Nonhydrostatic Model. J. Adv. Model. Earth Syst., doi:10.1029/2018MS001386. http://doi.wiley.com/10.1029/2018MS001386. Ohno, T., M. Satoh, and A. Noda, 2019: Fine vertical resolution Radiative-Convective Equilibrium Experiments: roles of turbulent mixing on the High-Cloud Response to Sea Surface Temperatures. J. Adv. Model. Earth Syst., 0, doi:10.1029/2019MS001704. https://doi.org/10.1029/2019MS001704. Wing, A. A., Reed, K. A., Satoh, M., Stevens, B., Bony, S., Ohno, T. (2018) Radiative-Convective Equilibrium Model Intercomparison Project. Geosci. Model Dev., 11, 793-813, https://doi.org/10.5194/gmd-11-793-2018 [Masaki Satoh, Japan]	taken into account: We acknowledge this area is under active research and we have revised the assessment by taking RCEMIP analyses into consideration. Thanks for the suggestion.
19276	60	12	60	14	Why is there no quantification of the tropical high-cloud amount feedback? Other cloud feedback components have been at least estimated but here we have none? Can some mention of the fact that we don't have estimations of this feedback yet be stated? [Gwenaelle GREMION, Canada]	taken into account: We acknowledge this area is under active research and we have revised the assessment with quantified feedback by taking recent studies.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
36604	60	16	60	17	It may be mentioned here, as it was done for the tropical high clouds (Page 7-59; Lines 51-53) that recent work has revealed that marine boundary layer clouds may amplify modes of interannual to interdecadal climate variability by means of a positive cloud-sea surface temperature (SST) feedback, and that the amplitudes of patterns of coupled atmosphere-ocean variability in GCMs are sensitive to the simulation of marine boundary layer cloud processes. Myers, T. A, C. R. Mechoso, and M. J. DeFlorio, 2017: Importance of positive cloud feedback for tropical Atlantic interhemispheric climate variability. Climate Dynamics. doi:10.1007/s00382-017-3978-1. [Carlos Mechoso, United States of America]	Noted. Nice suggestion, but the issue of cloud radiative effects on modes of variability is outside the scope of Ch. 7.
38690	60	16	60	40	Since there are substantial uncertainties in the pattern of SST warming under CO2 forcing on all timescales relevant for climate change, and the SST change pattern has a first order effect on tropical marine low-low cloud forcing, it is not possible to reasonably conclude that it is extremely unlikely that the resulting feedback is positive (which a very likely range of 0 to +0.5 W/m2/K implies), let alone to do so with high confidence. Note, e.g. that while the majority of CMIP5 models warm faster in the eastern equatorial Pacific Ocean than in the west, GFDL-ESM2M is an exception, and its Walker circulation strengthens in response to warming, with a warming pattern that favours enhanced marine low-clouds. Kohyama et al (2017; DOI: 10.1175/JCLI-D-16-0441.1) show that this ‘’La Niña–like’’ trend simulated by GFDL-ESM2M could be a physically consistent response to warming, [Nicholas Lewis, United Kingdom (of Great Britain and Northern Ireland)]	taken into account: We have linked the assessment with a discussion of the potential impact of SST pattern change on cloud feedback in 7.4.3 and 7.6.2.3.
12508	60	25	60	25	The reference for Qu et al. (2015) is missing [David Neubauer, Switzerland]	Rejected. The reference is present in the reference list.
12510	60	26	60	40	Does the uncertainty range take a possible state dependence (section 7.4.4.) of the tropical marine low-cloud feedback into account? [David Neubauer, Switzerland]	taken into account: We have linked the assessment with a discussion of the potential impact of SST pattern change on cloud feedback in 7.4.3 and 7.6.2.3.
33184	60	38	60	40	Given that the huge amount of progress made on this feedback since AR5, which concluded "Low clouds contribute positive feedback in most models, but that behaviour is not well understood, nor effectively constrained by observations, so we are not confident that it is realistic”, shouldn't a bigger deal be made about this feedback now being very likely positive with high confidence? A reader approaching this topic for the first time would have no sense from reading this paragraph of how substantial of an advance has been made in this area recently. [Mark Zelinka, United States of America]	taken into account: We have clearly stated that a substantial progress has been made for the assessment of low cloud feedback since AR5, and also repeat the arguments in FAQ7.1.
40748	60	40	60	40	I personally think the confidence is not nearly high. Medium? [Daniel Murphy, United States of America]	noted: The confidence level has been determined by considering a tremendous amount of recent works on this feedback, and unless new researches come out against those works that all show the positive low-cloud feedback we'll keep the confidence level in SOD.
13430	60	42	60	54	The CO2-physiological effect due to elevated CO2 also leads to reduced low clouds over land which can contribute to the positive feedback (Cao et al. PNAS 2010). [Govindasamy Bala, India]	noted: The suggested effect has been included in the tropospheric adjustment, assessed as a part of ERF but not the cloud feedback (section 7.3.2)
14378	60	44			Drying over land is also related to the larger warming over land than ocean such that moisture supply from the ocean cannot keep pace with increases in saturation vapour pressure (e.g. Byrne & O'Gorman 2018 PNAS, doi:10.1073/pnas.1722312115) discussed in 8.2.1.2 [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	noted: We have referred to Chapter 8 for saving space and Byrne and O'Gorman (2018) has been cited there.
19278	61	2	61	19	Again there is no quantification of this middle latitude cloud amount feedback. Can some statement be made acknowledging that a quantification cannot be made at this time for such and such reason? [Gwenaelle GREMION, Canada]	taken into account: We have quantified the feedback in SOD.
36606	61	2	61	19	The summary here is not as sharp as for the other clouds. [Carlos Mechoso, United States of America]	taken into account: We have revised the text with quantifying the feedback in SOD.
33186	61	9	61	11	there must be some acknowledgement here that these trends, if real, could still be solely due to internal variability, or at least have a large internal variability component [Mark Zelinka, United States of America]	We have not argued the possible cause of the trends. It is actually not a problem when taking observational estimate of the feedback as it is also based on interannual variability.
58106	61	9		11	Cross-reference Chapter 2 for assessment of observed trends in storm tracks and their observational uncertainties. [Nathan Gillett, Canada]	We have cited relevant sections in Chapters 2 and 4.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
33188	61	11	61	12	I'm not sure what this is referring to. The cited studies clearly show that the GCM-produced cloud radiative anomalies in response to global warming are much larger than can be explained by simple shifts in cloudiness, and that thermodynamic drivers dominate. Grise and Polvani (2014) [DOI: 10.1175/JCLI-D-14-00113.1] show that the models that produce a large annular-type shortwave heating response when the jet shifts poleward are in worse agreement with observations. Moreover, observational studies that have looked at the response of midlatitude clouds to variations in jet latitude on interannual timescales in nature find little change in total cloud cover and hence TOA radiation -- see Tselioudis et al (2016) [10.1002/2016GL068242], Grise and Medeiros (2016) [DOI: 10.1175/JCLI-D-16-0295.1], and Zelinka et al (2018) [DOI: 10.1175/JCLI-D-18-0114.1] [Mark Zelinka, United States of America]	Thanks for the suggestion. We have read references broadly and additionally cited some of them that you suggest. We have assessed that the cloud change associated with the jet shift is small and is not well captured by ESMs.
45564	61	11			This "spurious trend" suggestion needs a reference or some other support [Steven Sherwood, Australia]	noted: Bender et al. (2012) and Eastman and Warren (2013) have been cited
33190	61	17	61	17	I disagree. Growing evidence indicates instead that the dynamical contribution to this feedback (e.g., circulation shifts) is very small and that thermodynamic drivers are likely to be more important. [Mark Zelinka, United States of America]	taken into account, text modified
19280	61	17	61	18	Does this mean that there is little agreement between models on the shift in the jet? How much agreement is there between models on the shfit in the polar jet? [Gwenaelle GREMION, Canada]	noted: Section 3.3.3.3 states 'Models have medium to good performance in reproducing the extratropical jets' whereas the poleward jet shift in nature has been assessed likely (section 2.3.1.3.3)
49142	61	18	61	18	Model-observation mismatch could arise due to internal variability, and does not necessarily indicate "errors". For Hadley cell expansion, recent studies find contributions from internal variability to observed widening (Chapter 3 Section 3.3.3.1). [Yu Kosaka, Japan]	taken into account: We have revised the text by taking further recent studies into consideration.
19282	61	21	61	22	This is an important statement on past arguments. Can some citations be provided? [Gwenaelle GREMION, Canada]	accepted: We now refer to AR5 as the basis of this argument
33192	61	23	61	24	Suggest also mentioning the increase in adiabatic water content, which is also expected to contribute and be pronounced for colder clouds (Betts and Harshvardhan 1987). Indeed, recent work using ground-based measurements by Terai et al (2019) [doi:10.1029/2018JD029359] indicates that both contribute roughly equally in cold clouds in nature. [Mark Zelinka, United States of America]	Thanks for the suggestion. Because of the limited space for the text, we have not explicitly mention the process, but crudely stated 'other processes' where citing Terai et al. (2019).
13432	61	26	61	27	Can the other process or processes that can lead increase in optical depth be mentioned here? [Govindasamy Bala, India]	taken into account: There may be another process, which is now mentioned in SOD
33194	61	28	61	29	Where is this number coming from, and what is the uncertainty on it? What latitude bounds define 'SH high latitudes'? [Mark Zelinka, United States of America]	noted: The value is from Zelinka et al. (2016), for 40-70S
19284	61	29	61	30	If ice were overestimated in models then this would make clouds less optically thick. Wouldn't this underestimate the optical feedback then? The use of 'overestimated' here with a negative feedback intuitively makes me think that you mean it is more negative than it should be. [Gwenaelle GREMION, Canada]	noted: The negative feedback occurs due to a phase change from ice to liquid in a warm climate, so the overestimation of ice will lead to the overestimation of the feedback too
12512	61	29	61	30	In addition, some modelling studies found that the extrattropical cloud optical depth feedback is not important in all extratropical mixed-phase clouds (Bodas-Salcedo, 2018; Lohmann and Neubauer, 2018). Bodas-Salcedo, A.: Cloud condensate and radiative feedbacks at midlatitudes in an aquaplanet, Geophys. Res. Lett., 45, 3635–3643, https://doi.org/10.1002/2018GL077217, 2018. Lohmann, U. and Neubauer, D.: The importance of mixed-phase and ice clouds for climate sensitivity in the global aerosol–climate model ECHAM6-HAM2, Atmos. Chem. Phys., 18, 8807-8828, https://doi.org/10.5194/acp-18-8807-2018, 2018. [David Neubauer, Switzerland]	taken into account: We have been revised the text by taking the suggested references into consideration.
26882	61	32	61	33	"Because of these disagreements between climate models and observational estimates, the extratropical cloud optical depth feedback is assessed to be small positive with medium confidence." This statement appears to be inconsistent with the preceding discussion. The preceding discussion states that cloud optical depth feedback has been assessed negative or close to neutral. [Tomoo Ogura, Japan]	editorial: 'small positive' should read 'small negative'
38692	61	32	61	33	Since models estimate a negative extratropical cloud feedback and observations suggest that is likely an overestimate, shouldn't the assessed value be small negative, not small positive? [Nicholas Lewis, United Kingdom (of Great Britain and Northern Ireland)]	editorial: 'small positive' should read 'small negative'
40750	61	33	6	33	In this sentence it says small positive but Table 7.8 says small negative [Daniel Murphy, United States of America]	editorial
33196	61	33	61	33	Need more information explaining how this feedback is deemed positive rather than weakly negative. [Mark Zelinka, United States of America]	editorial: 'small positive' should read 'small negative'

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14380	61	48			The cloud feedback synthesis can include advances in understanding the role or spatial patterns in warming and stability (Ceppi & Gregory 2019 Clim. Dyn. Doi:10.1007/s00382-019-04825-x; Andrews & Webb, 2017 J. Cim, doi:10.1175/JCLI-D-17-0087.1) or signpost to 7.4.3 [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	taken into account: We have linked the assessment in this section with discussion on influence of SST pattern change to the cloud feedback in 7.4.3.
18828	61	51	61	53	Need references. [Gwenaelle GREMION, Canada]	noted: This is a synthetic argument and references have been cited previously
38696	62	3	62	13	The claimed "broad agreement" in net cloud feedback at interannual variability and decadal to centennial (RCP8.5) climate change found by Colman and Hanson 2017 is misleading and strongly biased. They estimate (Table 1) net cloud feedback from interannual variability to be 1.49x as high as for climate change. Since this is a regression coefficient and the regressor variable is noisy, regression dilution will have biased the estimate towards zero, implying an even larger true ratio. So the observational 0.54 W/m2/K net cloud feedback estimate from interannual variability needs to be divided by in excess of 1.49 to convert it into an estimate of feedback on climate change timescales. that implies a smaller mean value, of less than +0.36 W/m2/K, than the GCM estimate of 0.45 W/m2/K, not a larger value. This should all be brought out in this paragraph. [Nicholas Lewis, United Kingdom (of Great Britain and Northern Ireland)]	noted: There is another reference to be added here (Zhou et al. 2015 GRL), and the synthesis assessment is not solely dependent on the observed correspondence in cloud feedback at interannual and longer time scales.
38698	62	3	62	13	Colman and Hanson 2017 state that although cloud feedbacks at interannual timescales are correlated with those from RCP8.5, and show consistency with the strength of synthetic feedbacks, separate long and short wave components reveal very different, compensating, latitudinal patterns, suggesting the close correlation may be fortuitous. [Nicholas Lewis, United Kingdom (of Great Britain and Northern Ireland)]	noted: Zhou et al. (2015 GRL) also shows, using a different set of model ensembles, the observed correspondence in cloud feedback at interannual and longer time scales, suggesting that the statistics is simply due to fortuitous.
33198	62	4	62	4	Suggest also citing Zhou et al (2015) [doi:10.1002/2015GL066698] who demonstrated that the cloud feedbacks on interannual and long-term timescales are well-correlated across models. [Mark Zelinka, United States of America]	accepted: The reference has been cited in the FGD.
19286	62	5	62	7	This is a really important methodology point being made and an argument that is central to estimation of the cloud feedback. Can some more evidence be provided or studies provided which the reader can refer to? If not can more explaination be provided so that this point is very clear to the reader? [Gwenaelle GREMION, Canada]	noted: There is another reference to be added here (Zhou et al. 2015 GRL), which shows the observed correspondence in cloud feedback at interannual and longer time scales
37818	62	8			Change "data" to "datasets" [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Changed to "For the years 2000–2010, the net cloud feedback calculated using two atmospheric reanalyses (ERA-Interim and MERRA) …"
38694	62	15	62	26	The uncertainty ranges in GCM estimates of cloud feedback are far too narrow. They take no account of the very strong dependence of cloud feedback in GCMs on the pattern of surface warming. All CMIP5 GCMs simulate much the same pattern of surface warming in long term CO2 forced simulations, however they have not correctly simulated historical warming patterns and it may be that the CO2 warming pattern in equilibrium is rather different from that simulated by CMIP5 models. That is an unallowed for source of uncertainty. So is the, partly related, change in Hadley cell extent, which has a major effect on cloud radiative effect in GCMs and may not be correctly simulated by CMIP5 GCMs. [Nicholas Lewis, United Kingdom (of Great Britain and Northern Ireland)]	noted: The GCM spread of the cloud feedback is not reduced in amip+4K (no SST pattern effect), so the assessed range would have not been affected much by the surface warming pattern, which has been fully evaluated in 7.4.3.
12514	62	19	62	21	The middle latitude cloud amount feedback is listed as positive in Table 7.8 whereas here it is considered small positive. [David Neubauer, Switzerland]	editorial: The 'small negative' in the text should read 'small positive'.
38700	62	26	62	26	How, logically, can the very likely range of net cloud feedback be 0 to 1.1 W/m2/K when the uncertainty ranges are symmetrical and the central estimate and likely range are +0.45 [0.1 to 0.8] W/m2/K? It is unsound, against statistical principles and unscientific to use an "expert" assessment to cut off the uncertainty range at zero when there is no physical reason why cloud feedback has to be positive. [Nicholas Lewis, United Kingdom (of Great Britain and Northern Ireland)]	taken into account: the confidence level (high) remains given accumulated studies on the cloud feedback since AR5
13436	62	31	62	33	A schematic figure illustrating the various cloud regimes and the respective sign of feedbacks (corresponding to the last column of Table 7.8 would be very useful [Govindasamy Bala, India]	taken into account. Fig. 7.17 was revised to incorporate further information on cloud feedbacks.
33200	62	32	62	32	"Polar clouds" seems out of place. Every other entry in the table specifies a particular cloud property feedback. Also, why is this feedback not assessed here? [Mark Zelinka, United States of America]	accepted: We have included a short paragraphs on Arctic cloud feedback.
52046	62	39	63	9	Is it worth alluding to how these feedbacks support the linearity underlying the carbon budget concept and explicitly providing a reference back to the section 5 discussion thereof here? [Peter Thorne, Ireland]	Not applicable. Almost all the material previously in section 7.4.2.5.1 has now moved to chapter 6
13440	63	2	63	4	I believe biogeochemical feedbacks also include the changes in atmospheric CO2 (the carbon cycle feedback). Why is this not included? However, I see that the next sentence mentions this. Maybe it be better first state the carbon cycle feedback before discussing aerosol changes. [Govindasamy Bala, India]	Accepted. The CO2 feedback is now mentioned earlier.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
44818	63	3			Add biogenic SOA [Astrid Kiendler-Scharr, Germany]	Not applicable. Almost all the material previously in section 7.4.2.5.1 has now moved to chapter 7
27104	63	16	63	16	This sentence states that quantitative estimates of the magnitude of long-term Earth Systems feedbacks are given in section 7.4.2.5.3, but there is no section 7.4.2.5.3 in the document. I suspect this refers to the two paragraphs on p66 starting at lines 40 and 47 respectively, but these don't state any quantities for the feedbacks. It could also refer to the additional discussion (yet to be added) promised in lines 37 and 38 on p 66? Either way, I think it is important that what is known (quantitatively) for these long-term mechanisms is included here [Chris Satow, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Two things happened here. (1) Section 7.4.2.5.3 was not labelled (p66, lines 40-54). (2) There was some text removed at the very last minute at the end of this section: "An ensemble of such model simulations, with and without prescribed vegetation and ice sheet changes appropriate for the mid-Pliocene [forthcoming PlioMIP synthesis paper; including results from e.g. Chandan and Peltier, 2018] indicates that the vegetation and ice sheet feedbacks combined have a feedback parameter that is likely in the range …… when considering a CO2 increase from 280 to 400 ppmv. A similar approach for the Eocene [forthcoming DeepMIP synthesis paper] indicates a long-term Earth system feedback parameter that is likely in the range …… when considering a CO2 increase from 280 to 1120 ppmv. ". I am not sure why this was removed but it could be added back in once these papers are submitted.
12516	63	22	63	22	Allen et al. (2019) describe an increase in aerosol due to reduced wet removal associated with reduced precipitation over land in a warmer climate. Allen, R. J., Hassan, T., Randles, C. A., Su, H.: Enhanced land-sea warming contrast elevates aerosol pollution in a warmer world. Nature Climate Change 9, 300-203, 2019. https://doi.org/10.1038/s41558-019-0401-4 [David Neubauer, Switzerland]	Not applicable. Almost all the material previously in section 7.4.2.5.1 has now moved to chapter 6
12518	63	22	63	22	Aerosol-mediated (cloud) feedbacks have been discussed in the literature (e.g. Gettelman and Sherwood, 2016). Gettelman A and Sherwood S 2016 Processes responsible for cloud feedback, Curr. Clim. Change Rep.2179–89, DOI 10.1007/s40641-016-0052-8 [David Neubauer, Switzerland]	Not applicable. Almost all the material previously in section 7.4.2.5.1 has now moved to chapter 6
13442	63	23	63	23	Expand DMS. [Govindasamy Bala, India]	Rejected. Defined with first use in section 7.4.2.5
36608	63	23	63	33	In view of the large uncertainties of this feedback it is a little surprising that it has been concluded to be positive. [Carlos Mechoso, United States of America]	Not applicable. Almost all the material previously in section 7.4.2.5.1 has now moved to chapter 6
38702	63	23	63	33	The analysis of Climate-DMS feedback is invalid. It finds that warming is likely to lead to higher marine DMS emissions, but that ocean acidification leads to lower DMS emissions and a resulting increase in SW cloud radiative effect (measured downwards) as cloud albedo reduces. The pH reduction is primarily due to an increase in partial pressure of CO2 in seawater, not to an increase in SST. The resulting change in CRE therefore cannot constitute a climate feedback. Rather, it constitutes a (rapid) adjustment to CO2 and should be included in CO2 ERF. Please see page 9, lines 29-32 of this chapter. The effects of the corresponding increase in DMS with higher SST do however constitute a biogenic climate feedback, which can be expected to be negative. [Nicholas Lewis, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The effect of change in pH is now included in the forcing.
19288	63	33	63	33	Why isn't the uncertainty stated here? It is stated in table 7.11 but not here. The same is true of the dust feedback and stratospheric ozone feedback. [Gwenaelle GREMION, Canada]	Not applicable. Almost all the material previously in section 7.4.2.5.1 has now moved to chapter 6

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
18920	63	35	63	44	Dust-albedo effect was introduced as an Arctic amplification driver in Boy et al. (2019) Boy, M., Thomson, E. S., Acosta Navarro, J.-C., Arnalds, O., Batchvarova, E., Bäck, J., Berninger, F., Bilde, M., Brasseur, Z., Dagsson-Waldhauserova, P., Castarède, D., Dalirian, M., de Leeuw, G., Dragosics, M., Duplissy, E.-M., Duplissy, J., Ekman, A. M. L., Fang, K., Gallet, J.-C., Glasius, M., Gryning, S.-E., Grythe, H., Hansson, H.-C., Hansson, M., Isaksson, E., Iversen, T., Jonsdottir, I., Kasurinen, V., Kirkevåg, A., Korhola, A., Krejci, R., Kristjansson, J. E., Lappalainen, H. K., Lauri, A., Leppäranta, M., Lihavainen, H., Makkonen, R., Massling, A., Meinander, O., Nilsson, E. D., Olafsson, H., Pettersson, J. B. C., Prisle, N. L., Riipinen, I., Roldin, P., Ruppel, M., Salter, M., Sand, M., Seland, Ø., Seppä, H., Skov, H., Soares, J., Stohl, A., Ström, J., Svensson, J., Swietlicki, E., Tabakova, K., Thorsteinsson, T., Virkkula, A., Weyhenmeyer, G. A., Wu, Y., Zieger, P., and Kulmala, M.: Interactions between the atmosphere, cryosphere, and ecosystems at northern high latitudes, Atmos. Chem. Phys., 19, 2015-2061, https://doi.org/10.5194/acp-19-2015-2019, 2019. [Gwenaelle GREMION, Canada]	Not applicable. Almost all the material previously in section 7.4.2.5.1 has now moved to chapter 6
40752	63	35			Should extend to climate dust and sea salt feedbacks. See for example 5.5.2.4 in AR3. It could be short, there is not a lot of new literature on sea salt aerosol feedbacks due to changing wind speeds. [Daniel Murphy, United States of America]	Not applicable. Almost all the material previously in section 7.4.2.5.1 has now moved to chapter 8
41670	63	46	64	1	part of this effect also comes from the influence of ozone on tropical tropopause temperatures and water vapour transport into the stratosphere, which is an uncertain feedback in models. [Amanda Maycock, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. Almost all the material previously in section 7.4.2.5.1 has now moved to chapter 6
41666	63	49	63	50	This was not the first paper that showed a strengthening of the BDC under surface warming. See e.g. Butchart and Scaife (2001) [Amanda Maycock, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. Almost all the material previously in section 7.4.2.5.1 has now moved to chapter 6
41668	63	50	63	51	changes to transport are the dominant contributor with changes in chemical production a secondary effect, see e.g. Meul et al (2014) doi: 10.5194/acp-14-2959-2014 [Amanda Maycock, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. Almost all the material previously in section 7.4.2.5.1 has now moved to chapter 6
38704	64	3	64	10	The recent Sporre et al (Atmos. Chem. Phys., 19, 4763-4782, 2019) study should be cited here. It shows a change of −0.49 W m−2 in aerosol forcing, mainly due to a change in net cloud radiative effect, in equilibrium under a doubling of CO2 in NorESM-Oslo. Of this, about 63% (-0.31 W/m2) was attributable to the rise in surface temperature and the balance (-0.18 W/m2) to the increase in CO2 concentration. Based on the estimated 2.8 K ECS of NorESM, this implies a BVOC climate feedback of -0.11 W/m2/K. This is based on year 2000 initial conditions. Starting from preindustrial conditions, which is standard for feedback and forcing estimation, the effect was 53% stronger, implying a reduction in 2x CO2 ERF of -0.27 W/m2 and a negative BVOC feedback of -0.17 W/m2/K. These effects should be taken into account in this chapter's assessments. [Nicholas Lewis, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. Almost all the material previously in section 7.4.2.5.1 has now moved to chapter 6
52048	64	3	64	10	Chapters 5 and in particular 6 (I think!) both discussed these and should be referred to as that was a more substantive process based assessment than done here? [Peter Thorne, Ireland]	Taken into account. We refer to the relevant sections of chapters 5 and 6, but some discussion is necessary for the impacts on aggregated climate feedbacks.
12520	64	3	64	10	Add Spore et al. (2019). Sporre, M. K., Blichner, S. M., Karset, I. H. H., Makkonen, R., and Berntsen, T. K.: BVOC–aerosol–climate feedbacks investigated using NorESM, Atmos. Chem. Phys., 19, 4763-4782, https://doi.org/10.5194/acp-19-4763-2019, 2019. [David Neubauer, Switzerland]	Not applicable. Almost all the material previously in section 7.4.2.5.1 has now moved to chapter 6
44820	64	3			Refer to Chapter 6 for "Climate-BVOC feedback". [Astrid Kiendler-Scharr, Germany]	Not applicable. Almost all the material previously in section 7.4.2.5.1 has now moved to chapter 6
58108	64	12		15	Climate CH4 and Climate N2O feedbacks are assessed in Chapter 5 - See Figure 5.29. Cross-reference and/or agree which chapter will carry out the primary assessment of these topics. [Nathan Gillett, Canada]	Not applicable. Almost all the material previously in section 7.4.2.5.1 has now moved to chapter 6
37820	64	37			See comment 279. The are references to the Earth system that could instead be to the climate system, in this line and below. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Brought consistency regarding use of "Earth system". Checked for consistency with Box 7.1.
13444	65	8	65	11	"feedback increase"? Do you mean it becomes more positive? [Govindasamy Bala, India]	Accepted. Brought consistency regarding sign of feedback parameter and increase/decrease.
13446	65	30	65	45	Bala et al 2006 in Tellus "Biogeophysical effects of CO2 fertilization on global climate" , using a dynamical vegetation mdoel, showed that there can be a substantial warming (more 0.5 K) resulting from albedo decreases associated with the exapansion of boreal forests in the high latitudes on multi-century timescales. [Govindasamy Bala, India]	Rejected. The Bala paper focusses on the physiological response, and here we are assessing the biophysical response. The reference is also pre-AR5 and older than other references cited in this section.

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53714	66	5	66	6	This sounds rather obvious [Jan Fuglestvedt, Norway]	Taken into account. references added.
27102	66	5	66	39	This section on the long term climate feedbacks and in particular the ice sheets needs to include a section on potential volcanic feedbacks (volcanic CO2 emissions) resulting from removal of the ice sheets. The idea is that removal of ice from the crust both increases magmatic production rates and the number of eruptions. Although this is not well quantified yet, several papers have recognised the potential of this mechanism. For example: Swindles et al., 2018 (Geology 46 p 47-50) and Schmidt et al., 2013 (Journal of Geophysical Research, 118, p. 3366–3379), Watt et al., 2013 (Earth Science Reviews 122 p77-102). The mechanism may be a significant climate feedback on long timescales. [Chris Satow, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. CO2 doesn't change in our concentration-driven framework.
19290	66	7	66	7	What is 'P-E'? I do not see it defined before this point in Chapter 7. [Gwenaelle GREMION, Canada]	Accepted. sentence reworked.
38706	66	21	66	35	In equilibrium the feedbacks from ice sheets being smaller need not necessarily be positive. Increased precipitation may lead to a continuing higher level of freshwater fluxes and/or there may be a permanent change in ocean circulation from that at present that reduces GMST. It is also very doubtful that a doubling of PI CO2 concentration, which is what is involved for determining feedbacks relevant to ECS, would cause much of a reduction in the main East Antarctic icesheet even on multi-millennial timescales. [Nicholas Lewis, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. Changes in precipitation are taken into account in these simulations. And although I agree that a doubling of CO2 is unlikely to melt the entire EAIS, here we are assessing the feedback parameter, not ECS.
31980	66	40			sea ice is key to climate sensitivity at high latitudes through its direct albedo effect and its indirect effect on ocean and atmosphere heat transport (Ferreira et al 2018). [Marie-France Loutre, Switzerland]	Rejected. Sea-ice is a short term feedback that is discussed already in section 7.4.2.3
18830	66	50	66	50	Double bracket. Lunt et al., 2009)). [Gwenaelle GREMION, Canada]	Editorial
39282	67	16	67	21	It is not clear from the table caption where the values in the "Assessment" column come from (i.e., which studies etc.) Are they all in the text? Citations needed here. [Daniel Ibarra, United States of America]	Taken into account. Provenance of feedabck from other chapters is now clear.
38708	67	27	67	31	As per earlier comments of mine, net cloud feedback cannot reasonably be assessed as positive with high confidence. Moreover the effects of CO2-driven DMS reductions are mischaracterised as a likely 0.12-0.16 W/m2/K positive feedback, whereas they should be treated as a positive CO2 forcing adjustment, with the SST warming driven increase in DMS producing a likely negative climate feedback. Additionally, there is evidence for a negative BVOC feedback of -0.17 W/m2/K, so total biogenic feedbacks may be ~0.3 W/m2/K more negative than assumed in this assessment. Accordingly, I consider that the "very likely more positive than" value of -1.95 W/m2/K with high confidence given in this paragraph should be changed to -2.25 W/m2/K with medium confidence. [Nicholas Lewis, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The assessed confidence and likely range of the net cloud feedback has been carefully considered for the FGD, and given increased level of confidence in the assessments of individual cloud feedbacks, we have assigned high confidence to the net cloud feedback assessment.
40754	68	8			The introductory paragraph on pattern effect should mention that this is a major reason why short-term alpha is different than long-term alpha (e.g. Andrews, 2005 J Climate, 1630). [Daniel Murphy, United States of America]	Taken into account. This text has been modified to provide a more general introduction along these lines
19098	68	8			This section analyses how SST warming patterns influence radiative feedback changes. The whole section is too fragmented: it aims to be specifically focused on warming patterns, but it results in being somehow confusing and repetitive. Authors collected a number of referenced facts being far to give a proper structure to the section. I warmly suggest to put more effort in describing the main scientific problem and being more concise and precise in summarising main findings. I also would try to link SST equatorial Pacific trends to Walker circulation changes and La-Niña like condition. See specific comments to lines. [Gwenaelle GREMION, Canada]	Taken into account. This text has been modified to address these points.
19100	68	10	68	13	Authors briefly set up an tentative introduction in which they aim to emphasise the gap in the knowledge without really taking care of stating the scientific problem. I personally understand that there is something missing in the knowledge, but I cannot figure out what: too less information. I suggest to expand the lines describing the problem here. Furthermore, rather than being too much generic in saying "variety of explanations have been proposed", I would list directly what are the explanations given in the AR5… since you are talking about SST and sea-ice first, and later about cloud feedback. This would help you to structure the section. [Gwenaelle GREMION, Canada]	Taken into account. This text has been modified to provide a more general introduction along these lines.
19102	68	14	68	15	On "role of evolving SST patterns". It took me a while personally speaking to figure out later what you were talk about. Later on you always mention "warming patterns" without giving exact information of what are they in the beginning, confusing me. If you are referring throughout the section to SST warming patterns only, then be clear since the beginning: at Ln 14 - 15 I suggest to rephrase as "role of SST warming patterns", as well as in the title of the section. [Gwenaelle GREMION, Canada]	Taken into account. This text has been modified to clarify that we are referring to SST patterns.
19104	68	18	68	19	This sentence is not clear to me. Please consider to rephrase to avoid misunderstanding. [Gwenaelle GREMION, Canada]	Taken into account. Sentence rephrased.
19108	68	19	68	23	Please consider to revise this paragraph in order to clarify what are you talking about. [Gwenaelle GREMION, Canada]	Taken into account. Paragraph revised to clarify what we are talking about.

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19106	68	19			it is not clear what is the warming pattern you are talking about. Are you here referring to persistent temperature anomalies in the same location of the Earth? Do you mean SST only? I think that the jump between the preliminary introduction at Ln 10 - 13 and Ln 18 - 19 is too abrupt. [Gwenaelle GREMION, Canada]	Taken into account. This text has been modified to clarify that we are referring to SST patterns.
19110	68	25	68	27	Are thought by whom? Please provide reference here. [Gwenaelle GREMION, Canada]	Taken into account. Sentence rephrased and references added.
19112	68	31	68	40	This paragraph is a repetition with a little expansion of Ln 27 - 31. Consider to rephrase. [Gwenaelle GREMION, Canada]	Noted. Not clear that it is a repetition, so text unchanged.
36610	68				Section 7.4.3 seems to be well-written and highly informative. A possible caveat is that GCM biases can influence the conclusions to a higher degree than acknowledged. [Carlos Mechoso, United States of America]	Accepted. In the FGD the text has been modified to better acknowledge the role of GCMs.
13434	69	7	69	15	Figure 7.19a: the labels are too small. [Govindasamy Bala, India]	Accepted. Figure revised
13448	69	8	69	11	Fig. 7.19b. Is this the radiative response per unit local surface warming? It is not clear from the caption how much warming is imposed at each ocean grid point. What period of historical warming? [Govindasamy Bala, India]	Taken into account. This figure has been substantially modified and no longer shows radiative response. Time periods are clarified.
38710	69	20	69	45	There is no logic in considering, as is done here, linear SST trends over 1900-2017 (Fig. 7.19(c)). It looks like a case of cherry picking. Changes over the full HadISST1 record, from 1870 on, should be shown. The AMIP simulations that have been used to assess pattern effects over the historical period started in 1870 or 1871, and most historical period energy budget and simple model climate sensitivity estimates used analysis periods starting in or before 1870. Moreover, the influence of Atlantic multidecadal variability / the AMO was similar in the 1870s and in the 21st century to date, largely cancelling out its influence when using 1870 as a start date, while it was approaching its opposite extreme in the early1900s. Plotting instead the change in HadISST1 from 1870-82 to 2007-16, the main analysis periods (ignoring 1869) used in the Lewis and Curry 2018 energy budget study, the warming pattern is different, with no lack of warming in the equatorial eastern and central Pacific. [Nicholas Lewis, United Kingdom (of Great Britain and Northern Ireland)]	Noted. 1900-2017 was chosen for consistency with numerous publications (e.g., Coats and Karnauska 2017) who consider equatorial Pacific Ocean temperature gradients and for consistency with other chapters. Note also that our reference period used for energy budget calculations is 1850-1900. Most importantly, this figure only serves to illustrate the qualitative differences between the observed and projected warming patterns, for which the main features are robust to the choice of year (1870 vs 1900). The text has been modified to clarify.
38712	69	20	69	45	Both for changes from the 1870-82 mean to the 2007-16 mean, and for the 1870-2016 centennial linear trends, SST warming per HadISST1 was measurably lower in the deep tropical western Pacific (15S-15N, 90E-195E) than overall mean tropical (30S-30N) SST warming. While changes since 1980 have the opposite relationship, the observed SST warming pattern over the full historical record is very different from the post-1980 pattern. Studies based on changes over the last few decades (or post 1900 changes) have little relevance to historical period climate sensitivity estimates. [Nicholas Lewis, United Kingdom (of Great Britain and Northern Ireland)]	As noted, this figure only serves to illustrate the qualitative differences between the observed and projected warming patterns, for which the main features are robust to the choice of year (1870 vs 1900). Quantification of the pattern effect relies on specific studies that compare radiative feedbacks over under transient and long-term warming.
52052	69	20	69	45	This discussion leaves open why the SSTs have been so in the 20th/early 21st Centuries. Is it down to forcing, variability or both? It seems that this is an elephant in the room issue in this discussion as presently written and needs to be addressed explicitly somehow? Maybe it would help to bring th discussion that precedes figure 7.20 up to here? [Peter Thorne, Ireland]	Taken into account. The sections have been reordered to discuss SST patterns earlier in the chapter, and the text has been revised to make clear that it is not currently known whether variability or forcing has caused the observed SST patterns.
19250	69	20			This long paragraph starting with observed SSTs in the Pacific should be preceded by an explanation of the global importance of Pacific SSTs. The preceding paragraphy makes the importance of tropical convective systems clear, but does not explain why we should care about Pacific systems in particular. [Gwenaelle GREMION, Canada]	Taken into account. Section revised to clarify importance of Pacific Ocean SSTs.
38716	69	21	69	24	It is not clear that the stronger post-1980 equatorial Pacific temperature gradient has to any material extent been driven by aerosol forcing, which has changed little overall since 1980. I suggest inserting "possibly" before "aerosol forcing". [Nicholas Lewis, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. This text has been revised.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
41672	69	21	69	24	Does this really constitute a "feedback" in the traditional sense of having a role in determining the response to an external forcing. My interpretation would be that in the observed record (and in models too) there have been multi-decadal fluctuations in SST patterns due to internal variability (i.e. largely uncoupled from a response to evolving forcing) and these have a signature in the TOA energy balance for the physical reasons explained in this paragraph (and as diagnosed from AMIP SST simulations). So those fluctuations limit the ability to infer a true climate feedback parameter from the observed record, but in this context does it actually have anything to do with the overall climate response to forcing which is the thing we are primarily concerned with? Calling this a feedback suggests it does, but to me it seems more like a diagnostic issue rather than a physical issue. It appears distinct from systematic changes in radiative feedbacks under idealised forcing scenarios like 4xCO2 or 1%peryear CO2. If you think it does on what time scale would those fluctuations be relevant for climate response? I think a more careful discussion of the distinction between climate fluctuations that impact on the TOA energy balance in a manner decoupled from GMST and processes that actually influence how the climate system responds to an imposed forcing would benefit readers in understanding how the theoretical advances since AR5 relate to actual climate system response. [Amanda Maycock, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. There are multiple ways this topic could be framed, as seen in the somewhat messy literature on this. We've chosen to frame it in terms of dN being a function of SST patterns, which can evolve over time under forcing or with internal variability. We prefer this framing because it is not clear what has drive the observed SST patterns, so both effects must be accounted for at once. But to help clarify our meaning, we're now careful not to refer to this as "time varying feedbacks" and to, instead, explicitly link dN to changing SST patterns while noting that multiple things could be influencing those patterns.
41674	69	21	69	24	The role of Asian aerosol emissions in driving the recent Pacific SST trends is presented here without an assessment statement but since only one study points to this it should probably be assessed with low confidence. The role of internal variability has more lines of evidence to support. [Amanda Maycock, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. This text has been revised and merged with section 7.4.4. Uncertainty qualifiers have been added.
58110	69	21		27	Uncertainty qualifiers should be added to these sentences assessing the causes of changes in Pacific SST gradient and Southern Ocean SSTs. Also, reference Chapter 3 discussion of IPO changes (3.7.6). Further the statement that cooling of Southern Ocean SSTs since 1980 was caused in part by ozone depletion does not seem to be a conclusion of Section 9.2, which is cited here, and it also seems largely at odds with the results of Swart et al. (2018; https://www.nature.com/articles/s41561-018-0226-1), Figure 3c. The text should be revised to better reflect the assessment of Chapter 9 and the published literature. [Nathan Gillett, Canada]	Taken into account. This text has been revised and merged with section 7.4.4. Uncertainty qualifiers have been added along with references to Section 3.7.6.
19116	69	22	69	24	I would try to link the dynamics of east-west Pacific temperature changes with Walker circulation changes and a more persistent La-Niña like state here. Try to link SST warming patterns with tropical atmospheric circulation changes and provide crossref. within IPCC AR6 FOD. Here some suggested papers: Sohn et al., 2013: Observational evidences of Walker circulation change over the last 30 years contrasting with GCM results. Sohn et al., 2016: The role of the dry static stability for the recent change in the Pacific Walker circulation. Coats and Karnauskas 2017: Are Simulated and Observed Twentieth Century Tropical Pacific Sea Surface Temperature Trends Significant Relative to Internal Variability. McGregor et al., 2014: Recent Walker circulation strengthening and Pacific cooling amplified by Atlantic warming. Ueda et al., 2015: Combined effects of recent Pacific cooling and Indian Ocean warming on the Asian monsoon. [Gwenaelle GREMION, Canada]	Taken into account. The section discussing SST patterns now occurs prior to this feedback discussion. That text has been modified and references added.
19114	69	22			It is not clear what gradient. Be more precise… I would rephrase as “much of the zonal asymmetry of equatorial pacific temperatures” or “east-west equatorial Pacific temperature gradient”. [Gwenaelle GREMION, Canada]	Accepted - text revised

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
38714	69	32	69	36	There is a fundamental misunderstanding here. Changes in TOA radiation arsing from unforced changes in SST patterns with zero change in GMST do not constitute a climate feedback. Rather, they constitute internal variability driven unforced changes in net outgoing radiation. To quote Bjorn Stevens (2017), the radiative response can be viewed "as $R = -\lambda T + \mu_R$; where the second term, has expectation zero, and expresses the decoupling between surface temperature and radiative response which one expects as a result of internal variability". What the cited studies show is not that climate feedback has been unusually negative over recent decades, but that internal variability (albeit some of which may have been caused by volcanic eruptions) has caused outgoing radiation to be unusually high in relation to GMST. Only if the particular pattern of SST changes post 1980 was primarily CO2-forced would it be affect actual climate sensitivity, notwithstanding that it would affect energy budget estimates of sensitivity if carried out over the post 1980 period. [Nicholas Lewis, United Kingdom (of Great Britain and Northern Ireland)]	There are multiple ways this topic could be framed, as seen in the somewhat messy literature on this. We've chosen to frame it in terms of TOA radiation being a function of SST patterns, which can evolve over time under forcing or with internal variability. We perfer this framing because it is not clear what has drive the observed SST patterns, so both effects must be accounted for at once. But to help clarify our meaning, we're now careful not to refer to this as "time varying feedbacks" and to, instead, explcitly link TOA radiation to changing SST patterns while noting that multiple things could be influencing those patterns. The cited studies show that the pattern of warming over the historical period (or under transient coupled simulations) produced an anomalous TOA radiation response per degree of global surface warming; thus, the feedbacks calculated from this period appear more-negative than the feedbacks under the projected pattern of equilibrium warming. The same answer is achieve whether this effect is accounted for by subtracting off the anomalous TOA radiation from this period or by adjusting the feedback (as
38718	69	36	69	39	This finding conflates variation over time in feedback under CO2-forced warming in AOGCMs, which is fairly well explored across the CMIP5 ensemble, with a possible difference between the radiative response over the historical period and what would be expected over a similar period under CO2-forced warming. The former leads, for the median CMIP5 model, to only a minor (10%) difference between ECS estimated from forcing changes with a time-profile corresponding to best estimates of total forcing evolution over the historical period and ECS estimated from years21-150 of abrupt4xCO2 simulation data, and has almost no relevance to warming up to at least 2200. It would be much more useful to compare feedback estimated over the historical period with that estimated over, say, years 1-100 of 1pctCO2 simulations (which broadly corresponds to the time-profile of historical ERF evolution: Armour 2017). [Nicholas Lewis, United Kingdom (of Great Britain and Northern Ireland)]	The pattern effect as quantified here accounts for the pattern of transient warming being different from that projected for long-term warming under CO2 doubling or quadrupling (which defines ECS). Multiple factors contribute to setting the pattern of transient warming (transient response to greenhouse gas forcings, internal variability, and aerosol forcing), and thus all are accounted for in the pattern effect. As described in the text, all of these factors are thought to be transient in nature based on process understanding and comparison to the paleoclimate record of the pattern of equilibrium warming.
33202	69	39	69	40	Suggest avoiding the phrase "more positive" since the net overall feedback is negative on both timescales. Perhaps use "That is, the net negative radiative feedback within GCMs is weaker (higher ECS) for the long-term pattern of warming under CO2 forcing than it is over the historical record." [Mark Zelinka, United States of America]	Taken into account. Section has been revised

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
38720	69	40	69	43	On the contrary, HadISST1 and HadISST2 both show that SST warming over 1870-82 to respectively 1997-2013 and 1997-2010 has not been relatively large in regions of tropical ascent (15S-15N, 90E-195E or 45E-195E) compared to over the tropics (30S-30N) as a whole. The same applies to linear trends over 1871-2010. Please get your facts right. I can supply calculations of these changes if you wish. [Nicholas Lewis, United Kingdom (of Great Britain and Northern Ireland)]	As noted, 1900-2017 was chosen for consistency with numerous publications (e.g., Coats and Karnauska 2017) who consider equatorial Pacific Ocean temperature gradients and for consistency with other chapters. Note also that our reference period used for energy budget calculations is 1850-1900. Most importantly, this figure and discussion only serves to illustrate the qualitative differences between the observed and projected warming patterns, for which the main features are robust to the choice of year for starting point. The text has been modified to clarify.
19252	69	40			What precisely is the difference between "long-term pattern of warming under CO2 forcing" and "the historical record"? Which of these options refers to the abrupt4xCO2 scenario in the previous sentance, or the atmosphere-only vs. coupled GCMs from the sentances before? [Gwenaelle GREMION, Canada]	Taken into account. Combined with #33202
38722	69	43	69	45	At least in relation to tropical descent regions, this is model and period dependent, and untrue as written. E.g., in CESM1-CAM5, over years 87-156 of its 1pctCO2 simulation (which is a good analogue for future warming during the 21st century) warming in the Indo-Pacific warm pool (45E-195E, 15S-15N) is a few percent higher than in the tropics (30S-30N) as a whole, and it is only a few percent lower than over the wider ice-free ocean (50S-50N or 60S-60N). Have you any evidence that CESM1-CAM5 is an outlier in this respect? [Nicholas Lewis, United Kingdom (of Great Britain and Northern Ireland)]	This text has been removed and replaced with a discussion in Section 7.4.4.3. The text has been modified to clarify that the relevant comparison is between the transient pattern of warming (historical using AMIP simulations or anologs for historical in coupled GCMs) and the long-term pattern of warming as estimated from abrupt4xCO2 simulations. There the majority of models show more warming in tropical descent regions and at higher latitudes on long timescales.
18436	69	47	69	53	These lines seem to refer to the same study and results as lines 33-38. If so, I suggest keeping this (latter) explanation, as it is more clearly written. [Gwenaelle GREMION, Canada]	Taken into account. Text revised to consolidate and avoid this repetition.
38724	69	47	69	53	The findings in Andrews et al 2018 that are cited here are critically dependent on the observational SST and sea-ice dataset used. They are based on the AMIP II dataset, which switches SST datasets in 1981 and uses the old HadISST1 sea-ice estimates. Andrews et al 2018 tested, in two of their GCMs, the effect of using instead the more recent HadISST2 SST and sea-ice dataset. The resulting historical radiative feedback was far less negative than in their main AMIP II SST/sea-ice dataset based results; for one of the two models it was the same, within 1%, as that under abrupt4xCO2. These facts should be brought out in this paragraph to avoid it being misleading. Moreover, the final sentence should be amended to read "These feedback changes imply that the value of ECS may or may not be substantially larger than that inferred from the historical record". [Nicholas Lewis, United Kingdom (of Great Britain and Northern Ireland)]	This is indeed a major caveat to the Andrews et al. results. The text has been revised to better reflect this uncertainty and to increase the uncertainty in the assessed value of alpha'.
38726	69	47	69	53	The contrary findings in Lewis and Curry (2018; section 7a) should also be mentioned here. Its Figure 5 shows a largely linear dependence on the surface temperature anomaly of changes in net outgoing radiation in observations (estimated from forcing changes) and in a GCM. If there is an intention of showing a balanced picture of the available evidence, including that from observations, a version of Lewis and Curry Figure 5b (the relationship in observations) should be shown in Figure 20 alongside the panels showing results from Andrews et al (2018). [Nicholas Lewis, United Kingdom (of Great Britain and Northern Ireland)]	As discussed in Andrews et al. (2018), the HadGEM AMIP simulation used by Lewis & Curry (2018) had an error and the updated simulation shows a pattern effect that is similar to that in 7 other AGCMs.
38728	70	1	70	5	Lewis and Curry (2018) gave a detailed analysis showing that the Armour (2017) estimate of the difference in ECS estimated from 1pctCO2 (over years 1-100) and abrupt4xCO2 (over years 21-150) CMIP5 models simulation data was greatly excessive. Ignoring these Lewis and Curry 2018 findings is unjustifiable. When properly estimated the difference was just under 10%, compared to Armour's 26% estimate. Note also that ECS as defined in Armour 2017 and Lewis and Curry 2018 is estiamted from regrression over years 21-150 of abrupt4xCO2, which gives higher estiamtes than regressing over years 1-150 as per the definition of estiamted ECS in AOGCMs adopted in this chapter. [Nicholas Lewis, United Kingdom (of Great Britain and Northern Ireland)]	The text has been revised to discuss and show the CMIP5 results of Lewis and Curry 2018 alongside those of Armour 2017. The text has been revised to clarify that that we are using regression over 1-150 of abrupt 4xCO2 here, rather than 21-150 as in those studies.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
38730	70	2	70	5	Please make clear here that is plotted in the figure involved (which I deduce is 7.20 d, not 7.19 c as stated) differs from that estimated in Armour 2017. Armour 2017 estimated Equilibrium feedback by linear regression over years 21-150 of abrupt4xCO2 simulations (see his Errata), not over years 1-150 as stated in the caption to Figure 7.20. Alternatively, that caption is wrong, in which case make clear that the equilibrium feedback plotted differs from that per the Chapter 7 definition of estimated ECS in AOGCMs. [Nicholas Lewis, United Kingdom (of Great Britain and Northern Ireland)]	Text and figure caption revised to clarify that feedbacks and ECS are estimated using regression over years 1-150 here, not 21-150.
38732	70	5	70	8	The second sentence appears to be untrue in relation to CESM1-CAM5, judging by how the relationship in that model between SST warming in the Indo-Pacific warm pool and elsewhere in the tropics (and in the ice-free ocean 60S-60N) compares between its 1pctCO2 and abrupt4xCO2 simulations and in the HadISST1 and HadISST2 observational datasets. Moreover, estimates of feedback using a Green's function approach (Zhou et al 2017) imply that the first sentence is incorrect in relation to CAM5.3. If there is valid evidence that the claims in these two sentences are nevertheless true for CMIP5 models as a whole, please cite it. Andrews 2012 only provides evidence in relation to use of the AMIP II dataset. [Nicholas Lewis, United Kingdom (of Great Britain and Northern Ireland)]	The analysis of Andrews et al. (2018) includes CAM5 and shows it to have similar feedback variations to the other models used, with a more negative feedback at the end of the 20th century than inferred from CO2 doubling experiments (ECS of 4.2K with a slab ocean). This is consistent with the results of Zhou et al. (2017) who also showed that the Green's function accurately captured cloud feedback variations in an AMIP simulation with that model. Dong et al. (2019) also used the CAM5 Green's functions and found variations quite similar to CAM4 and the Andrews result with CAM5 forced with AMIP SSTs.
33204	70	8	70	12	In Zhou et al (2016) we found that this result extends to the piControl runs. Namely, the observed trend in ascent-region versus tropical mean SST was on the extreme tail of the distrubution of all possible 30-year periods in all available piControl runs. This could indicate that the observed pattern is partly forced or that that GCMs may lack sufficient internal variaibility that can generate gradients in warming of sufficiaent magnitude. My suggestion is to note that not only could the forcing and/or forced reponse be biased in models, but that they also may have insufficient internal variability in this regard. [Mark Zelinka, United States of America]	Taken into account. The text has been modified to note this.
49144	70	10	70	12	In support of this notion, Chapter 3 assesses that CMIP5 models tend to underrepresent the magnitude of Pacific decadal variability. [Yu Kosaka, Japan]	Taken into account. Reference to Chapter 3 added.
38734	70	12	70	15	GCM simulations using prescribed historical warming patterns, even if based on a modern SST and sea-ice dataset, only provide a more realistic representation of historical feedbacks (or, more accurately, of the historical radiative response) in the GCMs involved. There is no reason to believe that they provide an accurate estimate of the range or central value of historical feedbacks in the real climate system. The feedbacks in GCM historical AMIP experiments largely reflect the underlying sensitivity of the GCM involved. [Nicholas Lewis, United Kingdom (of Great Britain and Northern Ireland)]	The text has been revised and is consistent with this view. The assessed magnitude and uncertainty of the pattern effect (alpha') is not derived solely from the GCMs but from a combination of coupled model simulations, AGCM simulations, process understanding, and paleoclimate evidence for long-term warming patterns. There is a large range of uncertainty for various reasons that are discussed.
13456	70	26	70	26	"over the" is repeated [Govindasamy Bala, India]	Editorial
13458	70	32	70	32	The 1% per annuam experiment - I believe the increase is only upto doubling of co2 and no increase beyond. This can be mentioned in the caption for clarity. [Govindasamy Bala, India]	Rejected. The 1%/yr CO2 increase simulations to go 4xCO2 above pre-industrial values (around year 140).
18438	70	42	70	44	Radiative feedbacks and albedo are affected by cloud cover and snow cover as well as SST and sea ice concentrations [Gwenaelle GREMION, Canada]	Taken into account. This sentence refers to changes in radiative feedbacks, which have been linked to evolving SST patterns. As discussed, these feedback changes occur primarily due to changing clouds and lapse-rate feedbacks. Text modified to clarify that we are referring to changes in feedbacks here.
52054	70	44	70	47	I would urge a degree of caution here because its unclear which SST record is used as the boundary condition. Given the emergence of the ship-buoy issue (see chapter 2) depending upon the version used this may or may not be fully accounted for. So, while the sea-ice is undoubtedly much better there could still be ambiguity in the SST in the modern period arising from whether the used SST forcing adequately accounts for the modern data issues or not. [Peter Thorne, Ireland]	Taken into account. The text has been revised to emphasize uncertainty in SST and sea-ice concentration datasets.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
19118	70	50	71	4	Are you talking about the relative contribution of SST warming pattern to clouds as overall contribution to change in radiative feedbacks? I am bit lost. Perhaps you might want to remark differences between paragraphs within the same section by formatting the text with a proper use of hard return. Anyway, I would also expand this paragraph on clouds response to SST warming patterns here. [Gwenaelle GREMION, Canada]	Taken into account. Text modified to clarify.
18440	70	51	70	52	How long will it take for the observed pattern of warming to reach equilibrium? This would help readers see the relative importance of transient vs. equilibrium warming patterns. [Gwenaelle GREMION, Canada]	Taken into account. Text modified to give sense of the timescale to reach equilibrium, highlighting how far away we are given only a century of warming.
38736	70	51	70	52	Make clear here that it is the pattern of warming observed over the full historical period that is being referred to, not that over the last few decades (historical period climate sensitivity estimates normally use observed changes from some point in the 3rd quarter of the 19th century).. [Nicholas Lewis, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Text modified to clarify this.
38738	70	52	71	1	It is simply not true that warming in the eastern equatorial Pacific ocean is largely absent over the historical record. Taking that area as 225E-285E, 15S-15N, per HadISST1 the mean SST increased by 0.50 K from 1870-1882 (the start of the record to the last year before heavy volcanism) to 1998-2017 (the most recent 20 years). That warming is close to the SST increase of 0.54 K for the entire tropics 30S-30N, and exceeds the increase of 0.49 K in the West Pacific warm pool (90E-195E, 15S-15N). The corresponding SST increases per HadISST2 (ending in 2010, as its record has not been updated since then) are: equatorial east Pacific 0.43 K, all tropics 30S-30N 0.49 K, west Pacific warm pool 0.44 K. [Nicholas Lewis, United Kingdom (of Great Britain and Northern Ireland)]	The relevant comparison here is warming in these regions relative to the rest of the global oceans. Indeed, warming in these regions over the historical record has been relatively less than projected for the future. The text has been modified to make clear that these are relative comparisons that are being made.
38740	70	52	71	2	I disagree that there is high agreement across paleoclimate proxies that warming in the east Pacific ocean will eventually exceed that in the west Pacific ocean. The best studied and understood paleoclimate period is the last glacial maximum (LGM). The Annan and Hargeaves 2013 (doi:10.5194/cp-9-367-2013) new global reconstruction, using proxy data, of temperature changes from the LGM shows much the same average SST increase to preindustrial in the west Pacific as in the east Pacific. Medium confidence in greater eventual warming in the east than the west Pacific seems unsupportable. [Nicholas Lewis, United Kingdom (of Great Britain and Northern Ireland)]	The best analog for future warming is the Pliocene, which indeed shows more warming in the eastern tropical Pacific than in the western tropical Pacific (see Burls and Fedorov 2017, and Tierney et al. 2019a). However, an updated reconstruction for the LGM (Tierney et al. 2019b) shows similar patterns. We have rearranged the chapter to assess the proxy record of tropical SST changes in support of the likelihood statements here.
33206	71	1	71	3	I can see how we would expect the rate of warming to be larger at some time in the future in these regions, but can we know a priori that the total ultimate amount of warming relative to the control state will be larger in these regions? [Mark Zelinka, United States of America]	Taken into account. We are basing these confidence statements on modelling studies and the paleoclimate record. To avoid the sense that we are asserting anything a priori, we have rearranged the chapter to discuss this evidence for warming patterns prior to this location where we use them to infer anything about feedback changes.
18442	71	3	71	4	The word "thus" implies that the previously-discussed warming patterns (eastern Pacific SSTs, western Pacific SSTs, southern ocean SSTs) lead to the conclusion that "radiative feedbacks will become more positive". Are the effects of changes on land, in the Atlantic, etc, completely negligible? [Gwenaelle GREMION, Canada]	Taken into account. The cited literature points to SSTs in the tropical Pacific Ocean and Southern Ocean as the dominant causes of feedback changes. As discussed in the text, the reason is that these are the regions where ocean dynamics slow surface warming, and where observed changes look substantially different from those projected in the future. The pattern of land warming will of course change in the future, but it is these ocean regions that set the timescale. Text in this section modified to clarify.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
38742	71	3	71	4	Replace "in the future" by "eventually". In the future suggests that there is confidence that radiative feedbacks will from now on be more positive than over the historical period, but there is evidence in at least one model that a ‘‘La Niña–like’’ trend, which tends to produce a strong radiative response, may be a forced response to increasing CO2.Kohyama et al (2017; DOI: 10.1175/JCLI-D-16-0441.1) show that in GFDL-ESM2M a La Nina like state persists up to the end of the 300 year long abrupt4xCO2 simulation, and that this model has more realistic ENSO representation than most CMIP5 models. [Nicholas Lewis, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The text has been modified to reflect this.
38744	71	3	71	4	As the evidence is largely model-based, is not supported by LGM proxy evidence as regards tropical Pacific warming, and it is not certain from observational evidence that warming in the Southern ocean (which is relatively small) will produce a substantially less strong radiative response than average, I do not think it is justifiable to claim high confidence that radiative feedbacks will become less negative (correct use of English: not "more positive"; something negative can't become "more" positive since it is not positive to start with). Low confidence is more appropriate in view of the evidence, particularly as regards regional feedback strength, being very largely GCM -based, and the fact that the west Pacific warm pool has not in fact warmed more than the tropics as a whole over the full historical period. [Nicholas Lewis, United Kingdom (of Great Britain and Northern Ireland)]	These confidence statements on based on modelling studies, observations, and the paleoclimate record. We have rearranged the chapter to discuss this evidence for long-term warming patterns prior to this. Evidence for the radiative response to warming patterns comes from modelling studies as well as satellite observations (Zhou et al. 2016; Loeb et al. 2018; 2020; Fueglistaler et al. 2019; and others). Text in this section has been modified to clarify the link between this evidence and the confidence statements. Language modified to adopt 'less negative' throughout.
38746	71	4	71	9	The results in Andrews et al 2018 should not be used to give a range not only because they rely solely based on results from 6 GCMs but because they rely solely on a single SST and sea-ice dataset, and it is known that results using the more modern, improved, HadISST2 SST and sea-ice dataset are radically different in both models in which they have been tested. [Nicholas Lewis, United Kingdom (of Great Britain and Northern Ireland)]	As noted, be now assess a magnitude and uncertainty on the pattern effect that is informed by Andrews et al. (2018) as well as a range of other lines of evidence. We also devote more text to discussing the caveats of the Andrews et al. estimate including uncertainties coming from historical SST/SIC datasets used.
38748	71	12	71	18	Please make clear what variables, if any, "climate (mean) state" refers to other than to (climatological) global mean surface temperature. It appears that all the evidence cited shows the joint effect of increased CO2 concentration and increased surface temperature. [Nicholas Lewis, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Have modified the introduction to this section to highlight that state dependence can apply to just global-mean temperature, or to other aspects of state, such as paleoclimate configurations. I feel that the difference between apparent state dependence due to non-linearity in forcings, versus true state-dpeendece due to non-linearity in feedbacks, is already clarified in the text.
19122	71	12			Consider to rewrite the several parts of this section (especially 7.4.4.1). It is too fragmented. Main messages are not clear at all. Put more effort in describing processes and expand where needed. [Gwenaelle GREMION, Canada]	Accepted. All sections have had substantial editing, and restructuring. Hopefully this is clearer now.
42430	71	12			Page 7-71, starting on line12 secton. We suggest to reference the paper on the database of long-term surface temperature for the norther hemisphere from geothermal data published last week: Francisco José Cuesta-Valero, Almudena García-García, Hugo Beltrami, Eduardo Zorita, and Fernando Jaume-Santero (2018) Long-term Surface Temperature (LoST) Database as a complement for GCM preindustrial simulations, Clim. Past, 15, 1099-1111, 2019 https://doi.org/10.5194/cp-15-1099-2019. The paper assembled a new gridded database of long-term ground surface temperatures (LoST database) obtained from geothermal data over North America, for use as a potential reference for the evaluation of GCM preindustrial simulations.. [Hugo Beltrami, Canada]	Rejected. Not relevant here, but I forwarded to Chapter 2 Lead Author for their information.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
9970	71	14	71	18	Authors should consider that there is a general framework for studying climate response to forcings based on statistical mechanics. This explains clearly where state dependence comes from; see Ragone et al. Clim. Dyn. 166, 1036-1064 (2017); Lucarini et al. J. Stat. Phys. 166, 1036 (2017) [Valerio Lucarini, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. I read this paper. Although interesting, it is not relevant here to this discussion of state-dependence on mean climate state. Instead, it discusses state-dependence arising from inertia in the climate system; whereas here we are discussing the equilibrium response on long timescales.
18444	71	14			Grammar: strength -> strengths [Gwenaelle GREMION, Canada]	Editorial
19124	71	15	71	16	Here you need to add references. [Gwenaelle GREMION, Canada]	Noted. There is a reference here to Box 7.1 where the assumption of constant alpha is discussed.
56140	71	20	71	28	Make sure that these statements on SH climate are consistent with the conclusions in WMO 2018; or provide the respective updates [Rolf Müller, Germany]	Rejected. I assume that the wrong page/chapter number has been given, because this comment does not make sense here.
18446	71	22			The use of "so-called" and quotation marks are unnecessarily doubtful and informal. "State-dependence" is a technical term used in many fields, and is appropriate here as is. [Gwenaelle GREMION, Canada]	Accepted. Modified as suggested.
13450	71	27	71	27	It would be meaningful if "climate system" is changed to "climate response" or "climate feedback" [Govindasamy Bala, India]	Accepted - text revised
8616	71	27	71	29	An extensive investigation of the state dependence of the climate response with an EMIC (PlaSim) is carried on in Boschi et al. 2013, where an hysteresis cycle is described for the trajectories of the climate system as a function of entropy and emission temperature. Boschi R, Lucarini V, Pascale S (2013) Bistability of the climate around the habitable zone: A thermodynamic investigation. Icarus 226:1724–1742. doi: 10.1016/j.icarus.2013.03.017 [Valerio Lembo, Germany]	Rejected. I read this paper. Although interesting, I feel that the possibility of transitions between states and bistability has already been covered by reference to the more recent work of Popp, Steffen, and von der Heydt and Ashwin.
12522	71	40	71	40	Add Schneider et al. (2019). Schneider, T. et al. Nature Geosci. https://doi.org/10.1038/s41561-019-0310-1 (2019). [David Neubauer, Switzerland]	Accepted. Added to section on changes in state.
19126	71	40			The whole section is poorly written. Main messages are not clear at all. [Gwenaelle GREMION, Canada]	Rejected. We have proof-read this section carefully, as with all sections. However, I note that for the zero-order draft, one of the comments on this section was "this is a very well written readable important section"
18448	71	42			Passive tense; try "Several studies since AR5 have used models to explore state-dependency" [Gwenaelle GREMION, Canada]	Editorial
38750	71	43	71	43	It could be noted that Jonko et al 2013 used a low resolution version of the pre-CMIP5 model CCSM3, so less weight should arguably be given to its results. [Nicholas Lewis, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Noted in text that the models are of varying complexity.
19128	71	44	71	53	These sentences are not clear and too fragmented. I would rephrase here as: “Generally, temperature responds non-linearly when multiple simulations are carried out across successive CO2 doublings (add ref. here). The non-linear behavior of temperature is partly due to forcing itself (i.e. it increases more rapidly than expected) and partly due to state-dependence in feedbacks. Although not every modelling groups partitioned the non-linear temperature response into forcings versus feedbacks, there is an overall agreement among GCMs on a direct relationship between feedback strength (….) and temperature from preindustrial values (…). The relationship holds because the water vapour (…) and cloud (….) feedbacks become stronger as the climate warms (….)”. [Gwenaelle GREMION, Canada]	Accepted. Re-phrased this paragraph somewhat.
18450	71	48			"Not all modelling studies have partitioned..." is a weak topic sentence for the paragraph; I suggest re-ordering and starting the paragraph with, "There is general agreement amongst GCMs that..." [Gwenaelle GREMION, Canada]	Taken into account - sentence revised

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
38752	71	49	71	51	Figure 1 of Meraner et al 2013 shows only minor differences in feedback strength until the GMST rise from preindustrial exceeds about 6 K. The much reduced feedback strength shown in Figure 7.21 at warming of ~6.7 K reflects dominance of the regression by points clustered around the 6.7 K 4x CO2 equilibrium warming. Nor do CMIP5 models, assuming that on average the CO2 concentration - forcing relationships produced by their radiation codes are unbiased (relative to the Etminan et al 2016 estimate), show any obvious change in feedback strength up to at least 4 to 5 K. Consistent with this, Caballero and Huber 2013 found that feedback strength was pretty constant up to 22 C or so and then weakened. Therefore the statement in these lines should be qualified to apply only to increases from preindustrial exceeding these sorts of levels. [Nicholas Lewis, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. I am not sure I really agree here. Meraner shows an increase in feedback parameter from 2x to 4x of -1.65 to -1.35 Wm-2K-1. This is reflected in our Figure 7.21. The reproduction of Figure 7.21 was very poor in the FOD so this may not have been clear. There was no regression line shown in Figure 7.21. All studies cited show increases in feedback parameter (magnitude of alpha decreasing) from ~ 2x to 4x CO2.
15062	71	52	71	52	Cloud albedo feedback only matters when the surface is not covered by ice and snow. To the extent that ice is melting, the cloud albedo affect increases with increasing temperature. Clouds mitigate about 2/3 of the albedo effect of decreased surface reflection when snow and ice melts, as clouds would have reflected that energy anyway. Note that clouds also mitigate 2/3 of the effects of incremental CO2, since across the 2/3 of the planet covered by clouds, the water in clouds is already absorbing most of what would be incrementally absorbed by higher GHG concentrations. [George White, United States of America]	Rejected. The cloud processes discussed are already included in all the model estimates assessed here.
19132	71	53	72	7	I strongly recommend to rephrase this paragraph. It is totally confusing. [Gwenaelle GREMION, Canada]	Taken into account. We believe it is clear in context of the paragraph so no changes made
19130	71	53			“These changes”. Not clear what changes are you talking about. [Gwenaelle GREMION, Canada]	Taken into account. We believe it is clear in context of the paragraph so no changes made
13452	72	1	71	3	Why does the planetary emissivity decrease with warming? [Govindasamy Bala, India]	Accepted. Added a brief explanation (full explanation is in the paper).
18452	72	3		5	Comment on spatial patterns might belong in section 7.4.3 [Gwenaelle GREMION, Canada]	Taken into account. This sentence refers to AMOC and is appropriate here.
38754	72	5	72	7	Incorrect use of English: it should say "feedback strength may start increasing". "Strength" is a measure of absolute magnitude; strength is never negative. So a more highly negative feedback is a stronger feedback, not a weaker feedback. This error occurs in many places in this Chapter. So the wording used often means exactly the opposite of what is intended. [Nicholas Lewis, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Throughout the chapter and this section in particular we are now clearer in terms of language regarding changes in the feedback parameter.
18454	72	5		7	Are the CO2>4000ppmv results important enough to include here? They refer to uncertain effects in a very far/unlikely extreme-CO2 future. [Gwenaelle GREMION, Canada]	Rejected. I think that they are important, because otherwise the implication may be that the feedbacks continue increasing indefinitely. The non-linearity at very high CO2 is important for interpretation of the paleo record.
18456	72	9		18	As I understand this paragraph, the EMIC results serve to show that non-linearities emerge from processes, like water vapor, that are rarely represented in EMICs, and that non-linearities can take a long time to emerge in simulations. It is not clear to me that this point merits a full paragraph. [Gwenaelle GREMION, Canada]	Accepted. Have cut the paragraph slightly, but it is important for pointing out a "knowledge gap" that results from short integrations.
19134	72	20	72	21	add “mean state” here. [Gwenaelle GREMION, Canada]	Taken into account. No clear justification why change required.
53716	72	20	72	23	Check what ch4 says about this. [Jan Fuglestvedt, Norway]	Taken into account. We did add a reference to Chapter 4 Section 4.7.3 but this has since been removed as no longer as relevant in latest draft.
18458	72	20		23	The possibility of a fundamental change in state should perhaps be mentioned earlier; although this possibility is associated with significant uncertainty, if a fundamental change of state did appear, it would overturn most or all of the preceding results. [Gwenaelle GREMION, Canada]	Rejected. I think that this is sufficiently uncertain that it should not appear too early.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
38756	72	25	72	29	There is little modelling evidence that feedback becomes less negative for increases of up to about 5 K above preindustrial. If that occurred it would be evident from abrupt4xCO2 simulation data. Such data can, for all or almost all CMIP5 models, be fitted very accurately using a 2-box model with deep ocean efficacy but no feedback temperature dependence. The statement in this paragraph should be qualified so as to apply only when temperature rises exceed 5 K and/or it indicated that any effect below 5 K is minor and can only be assigned a low confidence. [Nicholas Lewis, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. I would argue the opposite. Added that the non-linearity in feedbacks is not only evident at very high CO2 concentrations, being apparent in the difference in temperature response to a 2×CO2 forcing compared with a 4×CO2 forcing (Mauritsen et al., 2019; Rugenstein, 2019b)
18460	72	29			Clear concluding paragraph; last phrase could be shortened to, "there is insufficient evidence to quantify the associated non-linear feedbacks" [Gwenaelle GREMION, Canada]	Taken into account. Text revised to simplify (not exactly how reviewer suggests).
19136	72	39	72	40	Rephrase and expand here. “on the assumed ice-sheet forcing, which is not well known” as “on the assumed ice-sheet forcing, which is largely uncertain because … (expand here), and on the orbital forcing, which is … (expand here)”. [Gwenaelle GREMION, Canada]	Accepted. Added "due to a relative lack of observations of ice sheet extent and distribution prior to the Last Glacial Maximum, 21,000 years ago. and added "Additionally, if the analysis excludes time periods where the temperature and CO2 data are not well correlated, which occurs in general at times when sea level is falling and obliquity is decreasing, the state-dependence reduces (Köhler et al., 2018)"
13684	72	40	72	40	The half-sentence "and the orbital forcing (Köhler et al., 2018)" describes the content of that paper inaccuratly. I suggest to change to: "The highly resolved data of the 800,000 years covered by ice core CO2 revealed that during times of decreasing obliquity (periods of land ice sheet growth and sea level fall) the multimillennial component of reconstructed temperature change diverges from CO2. Intervals of strong temperature-CO2 divergence should not be considered for the the estimation of ECS, because in the future sea level is expected to rise, not to fall. If the analysed data are restricted accordingly the state-dependency is reduced leading to smaller ECS (Köhler et al., 2018)." [Peter Köhler, Germany]	Accepted. Added "Additionally, if the analysis excludes time periods where the temperature and CO2 data are not well correlated, which occurs in general at times when sea level is falling and obliquity is decreasing, the state-dependence reduces (Köhler et al., 2018)"
19138	72	40	72	42	Consider to clarify here. [Gwenaelle GREMION, Canada]	Accepted. Tried to clarify.
13454	72	50	72	50	Any idea why it is mostly shortwave cloud feedback that changes? Any fundamental physics here? [Govindasamy Bala, India]	Takne into account. Some of this section has been mad more concise so no longer relevant.
18462	72				Section 7.5 is significantly more challenging and time-consuming to read than section 7.4. This could be improved by editing the section to ensure that ideas are presented and applied in a linear manner within each paragraph. At present, the section contains many long, complex sentences, and many constructs such as "the metrics assessed above" that require readers to move their attention continuously back and forth through the paragraph. Moreover, the section is densely packed with acronyms and mathematical symbols. While all of these are used consistently, and none are individually objectionable, the net effect is an increase the number of concepts that readers must hold in memory in order to understand the section, and further decrease its readability. [Gwenaelle GREMION, Canada]	The section structure has been heavily revised to make it shorter and more readable
53016	73	19	95	29	In this section climate sensitivity in global models is based on simplistic sensitivity analyses, i.e., using local or one-factor-at-a-time (OAT) analyses that assume model linearity and additivity. However, the assumption of linearity and additivity in climate models may be questionable. Global sensitivity analysis (GSA) is a more robust approach for sensitivity analysis that can be used to evaluate how the uncertainty in the outputs of a system model, e.g., a climate model, can be assigned to different uncertainties in the model inputs. Thus, in my opinion, a discussion regarding the potential use of GSA in the assessment of climate sensitivity should be included in this section. The following references can be useful: •Saltelli, A.; Tarantola, S.; Campolongo, F.; Ratto, M. Sensitivity Analysis in Practice. A Guide to Assessing Scientific Models; John Wiley & Sons Publishers: New York, 2004.https://onlinelibrary.wiley.com/doi/book/10.1002/0470870958 •A. Saltelli, S. Tarantola & K. P.-S. Chan (1999) A Quantitative Model-Independent Method for Global Sensitivity Analysis of Model Output, Technometrics, 41:1, 39-56, DOI: 10.1080/00401706.1999.10485594. https://www.tandfonline.com/doi/abs/10.1080/00401706.1999.10485594 •Anderson, B. , Borgonovo, E. , Galeotti, M. and Roson, R. (2014), Uncertainty in Climate Change Modeling: Can Global Sensitivity Analysis Be of Help?. Risk Analysis, 34: 271-293. doi:10.1111/risa.12117. https://onlinelibrary.wiley.com/doi/abs/10.1111/risa.12117 [Guerra Omar, United States of America]	Not applicable. The global sensitivity analysis approach is an interesting area of research, but due to space limitations we are not able to describe it in this assessment.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
42122	73	19	95	29	Section 7.5 p73 to p95 Estimates of ECS from observations and palaeoclimate constraints Two recent studies that estimate ECS from different methods are ignored: Goodwin et al. (2018, doi:10.1038/s41561-017-0054-8) in Nature Geoscience and Goodwin (2018, doi:10.1029/2018EF000889) in Earth's Future. Goodwin et al. (2018) combines palaeoclimate evidence with historical observations to constrain ECS to 2.6 K (with a 95 % range from 2.0 to 4.3 K). Goodwin (2018) combines CMIP5 climate feedback analysis with observational constraints (essentially extracting the observation-consistent combinations from CMIP5 process-based analysis of climate feedback strengths) to find ECS = 2.9 K (95 % range from 1.9 to 4.6 K) for feedbacks operating on a 100-year response timescale. Goodwin et al. (2018) starts with a paleoclimate-constrained prior distribution for ECS and then uses additional information from historic observations to generate a posterior distribution applicable to the present day climate change. The palaeo-based prior distribution is used to generate a very large ensemble of efficient model simulations, and forced with historic radiative forcing (adopting the AR5 distributions for uncertainty in radiative forcing). These simulations are then checked against historical observational constraints from reconstructions of both surface warming and ocean heat uptake. The posterior ECS distribution for the present day is analysed from the simulations that agree with both the palaeo-climate and historical climate observations. This palaeo+historical observational estimate of ECS gives (Goodwin et al., 2018): ECS =2.0 to 4.3 K at 95% uncertainty, with a best estimate of ECS = 2.6 K. This method of combining palaeo and historical observational constraints to estimate ECS is not currently covered in the chapter, and so including this study would improve the chapter's evaluation of ECS. This could be included within sub-section 7.5.3 'Estimates based on the historical temperature record' Goodwin (2018) uses another method not covered by studies currently cited in Chapter 7, adopting time-varying climate feedback due to the different response timescales of individual climate feedback processes. Goodwin (2018) uses prior information from the analysis of climate feedback strengths from individual processes in CMIP5 models. Prior distributions representing the strengths of the Planck feedback, water vapor-lapse rate feedback, cloud feedback and surface albedo feedback are adopted from the analysis of CMIP5 model ensemble by Caldwell et al., (2016), and a prior distribution for the surface warming pattern adjustment feedback is adopted from analysis of CMIP5 models by Andrews et al. (2015). Goodwin forces an efficient climate model with these prior feedback-strength distributions from the CMIP5 ensemble, allowing each feedback to respond via its own timescale depending on the process. The simulations produced are compared to historic	Taken into account. References to Goodwin (2016) and Goodwin et al (2018) have been added and discussed in Section 7.5.2.2.
42124	73	19	95	29	Full references for above comment: Goodwin. P., A. Katavouta, V.M. Roussenov, G.L. Foster, E.J. Rohling and R.G. Williams, (2018a) Pathways to 1.5 and 2 °C warming based on observational and geological constraints, Nature Geoscience 11, 102-107, doi:10.1038/s41561-017-0054-8. Goodwin, P., (2018). On the time evolution of climate sensitivity and future warming, Earth's Future 6, EFT2466, doi:10.1029/2018EF000889 Caldwell, P. M., Zelinka,M. D., Taylor, K. E., & Marvel, K. (2016). Quantifying the sources of intermodel spread in equilibrium climate sensitivity. Journal of Climate, 29, 513–524. https://doi.org/10.1175/JCLI-D-15-0352.1 Andrews, T., Gregory, J. M., & Webb, M. J. (2015). The dependence of radiative forcing and feedback on evolving patterns of surface temperature change in climate models. Journal of Climate, 28, 1630–1648, doi:10.1175/JCLI-D-14-00545.1 [Philip Goodwin, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. References to Goodwin (2016) and Goodwin et al (2018) have been added and discussed in Section 7.5.2.2.
9972	73	21	73	22	A comprehensive theort of ECS and TCR exists - Ragone et al. Clim. Dyn. 166, 1036-1064 (2017); Lucarini et al. J. Stat. Phys. 166, 1036 (2017) [Valerio Lucarini, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The nonequilibrium statistical mechanics view of ECS and TCR is an interesting area of research, but due to space limitations we are not able to describe it in this assessment.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
15064	73	21	73	50	There's no need to estimate the ECS when it can be inferred from unambiguous measurements with less than 10% uncertainty based on Conservation of Energy constraints alone. The 240 W/m^2 of average incident solar energy results in 390 W/m^2 of average surface emissions, or about 1.62 W/m^2 of average surface emissions per W/m^2 of forcing. Starting from 390 W/m^2, an increase of 1.62 W/m^2 results in a temperature increase of 0.3C, corresponding to a sensitivity factor (1/alpha) of 0.3C per W/m^2 limited only by the accuracy of the average surface temperature and the net incident solar power. Independent of any uncertainty, the COE based upper bound emission sensitivity of 2 W/m^2 of surface emissions per W/m^2 is below the estimated emissions sensitivity required to support the presumed lower bound sensitivity of about 0.4C per W/m^2. [George White, United States of America]	Noted. This comment confuses mean state with perturbation, so is not relevant to the problem being discussed. Climate models also show fluxes he quotes and has different sensitivities. Emission at the surface is a poor proxy for energy budget changes and estimates of surface temperature change. There is no reliable published literature to support the reviewers point
15066	73	21	73	50	The measured mean emissions sensitivity of 1.62 W/m^2 of surface emissions per W/m^2 of forcing +/- <10% is so close to the golden ratio of 1.618023, it may not be a coincidence considering how this ratio frequently emerges in the steady state solutions of chaotically self organized systems. One contributing factor as to why this arises in other systems is as the consequence of a chaotically varying, yet otherwise unconstrained variable where this ratio appears in the its steady state value relative to its possible limits. For the climate system, this unconstrained variable is the fraction of surface emissions absorbed by the atmosphere where a steady state can be established for any amount of absorption between 0 and 100%. If g is the golden ratio, the steady state absorption becomes 2*(1 – 1/g) leading to an effective emissivity of 1/g. Clouds vary absorption and a radiant balance can be established for any fraction of the surface covered by clouds, yet the average fraction of the planet covered by clouds is relatively constant. Repeatable tests of satellite data confirm that the emissivity of the planet relative to the surface temperature is 1/1.62 = 0.62 as expected based on an emissions sensitivity of 1.62 W/m^2 per W/m^2 of forcing. This ratio is mostly independent of the surface temperature and/or solar input, moreover; it's the most tightly regulated ratio of any pair of climate variables. From a theoretical perspective, the sensitivity of a gray body is given exactly as 1/(4*e*o*T^3), where e is the effective emissivity, o is the SB constant and for T=288K and e=0.62, this also results in a sensitivity of about 0.3C per W/m^2 which is right in the middle of the range predicted by skeptics. The implication of average absorption being constant means that increasing CO2 would not increase the average absorption, but instead, clouds will decrease in order to offset any static increase in absorption by CO2 and what little warming does arise is from a reduction in albedo. Interestingly enough, if e is the effective emissivity representing the attentuation of surface emissions as it results in planet emissions, the steady state gain, g, becomes the ratio of surface emissions to planet emissions and the output path is quantified by g = 1/e. If e is the same in both directions through the atmosphere and the gain is calculated from space to the surface as g = e + e^2 + e^3 + …, the result is that g = e/(1 – e). Setting the two gain functions equal to each other, we get 1/e = e/(1 – e) whose only possible solution is when e = 1/G, where G is the golden ratio of 1.618… [George White, United States of America]	Noted. This comment confuses mean state with perturbation, so is not relevant to the problem being discussed. Climate models also show fluxes he quotes and has different sensitivities. Emission at the surface is a poor proxy for energy budget changes and estimates of surface temperature change. There is no reliable published literature to support the reviewers point
15068	73	21	73	59	This plot supports the gray body behavior of the planet's emissions, relative to its surface temperature: http://www.palisad.com/co2/tp/fig1.png The plot is of the surface temperature vs. the emissions at TOA. The green line is the prediction based on a gray body whose emissivity is 0.62, the slope of which is the sensitivity factor of about 0.3C per W/m^2. The blue line illustrates how the sensitivity factor presumed in this report aligns with the actual data. The reason it passes through zero when centered on the current surface state, rather than being tangent to the actual response, is a consequence of the assumption of approximate linearity between T and W/m^2 which ignores the 1/(4*T^3) dependence on the derivative of this relationship (the sensitivity). [George White, United States of America]	Noted. This comment confuses mean state with perturbation, so is not relevant to the problem being discussed. Climate models also show fluxes he quotes and has different sensitivities. Emission at the surface is a poor proxy for energy budget changes and estimates of surface temperature change. There is no reliable published literature to support the reviewers point

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
15070	73	21	73	59	The effective emissivity can also be calculated from the bottom up. From the ISCCP data, the surface is about 2/3 covered by clouds whose average emissivity is 0.72 and HITRAN based simulations tell us that the average fraction of surface emissions absorbed by GHG's in the cloud free sky is about 64%. Since the size of the average cloud water droplet is on the order of the wavelength of the relevant LWIR, absorption by the liquid water in clouds is not 100% and estimated to be about 90% and is the least certain of the constituent components. By weighting atmospheric absorption separately for cloudy and clear skies and considering that half of what's absorbed ultimately reaches space, the effective emissivity can be calculated by first calculating the fraction of surface emissions absorbed by the atmosphere and clouds, A = (1-p)*As + p*((1-ec)*As + ec*Ac), As is the average fraction of absorption of surface emissions by GHG's, Ac is the fraction of absorption by water in clouds, p is the cloud fraction, ec is the cloud emissivity, where both p and ec are chaotically dynamic. The average result is A = (1-.66)*.64 + (0.66)*((1-.72)*.64 + .72*.9) = .763 Next, calculate the effective emissivity based on the geometry requiring that half of what's absorbed by the atmosphere will ultimately be emitted into space. E = 1 - A/2, E = 1 - .763/2 = 0.62 Note that if As or Ac is higher then stated, then A is higher and more than half of what was absorbed by the atmosphere must be emitted into space in the steady state, meaning that less than half is returned to the surface. To support a larger fraction returned to the surface requires a reduction in A which increases the size of the LWIR transparent window. [George White, United States of America]	Noted. This is not a valid estimate of sensitivity as no perturbation is introduced. The approach is not supported in the literature
18494	73	21			Does ECS has regional or seasonal counterparts? It is mentioned that the warming is not uniform in space and some parts of globe may warm higher than others. Similarly, the warming could also vary across time and some seasons may show large warming than others. From the policy-making perspective, this kind of information could also be very useful. Though the ECS is defined for global annual means, is there any other way or is there a metric that is defined to take care of the regional and temporal variations in response of surface temperature to CO2 doubling or quadrupling? [Gwenaelle GREMION, Canada]	ECS is defined in terms of global and annual mean surface temperature change. Thus, it does not include all information about seasonal and regional changes. However, as discussed in our new introduction (to which this section has been moved), there are strong correlations between ECS and regional warming, making it an extremely useful metric.
18464	73	22		24	Suggest rephrasing sentence to "This section assesses the constraints placed on the ECR and TCR by radiative forcing [data], climate feedbacks, [etc]." [Gwenaelle GREMION, Canada]	Editorial
55736	73	31	73	39	Given the well-documented change in feedback parameter (α), i.e., the slope of N vs. T, with time in the abrupt-4xCO2 simulations, a better assessment of the methods for calculating ECS from models, and the associated uncertainties, is needed. For instance, would using years 51-150 instead of 1-150 give a more accurate estimate of the true model ECS? [Larry Horowitz, United States of America]	Taken into account. A discussion of this has been added to Section 7.4.1.2.
18466	73	33			Suggest rephrasing "and extrapolating to equilibrium the regression..." to "regressing the global mean TOA radiation against global mean near-surface air temperature and extrapolating to equilibrium" [Gwenaelle GREMION, Canada]	Editorial
53032	73	35	73	36	Please indicate how accurate this " an approximation to the true ECS that would be reached if the model was run to equilibrium" approximation has been found to be. [Steven Smith, United States of America]	Taken into account. A discussion of this has been added to Section 7.4.1.2.
38758	73	36	73	38	This sentence is wrong, even assuming that the ECS referred to is the "conventionally" diagnosed ECS referred to in line 31 that incorrectly embodies a purely logarithmic CO2 forcing- concentration relationship and ignores variation of alpha with time. The y-intercept of a linear regression line fit to years 1 to 150 of abrupt4xCO2 simulation data is not a measure of model-specific ERF. It actually has no physical interpretation. If alpha were constant over years 1-150 then it would be interpretable as an ERF, but (save in a few models) alpha clearly changes during abrupt4xCO2 simulations. The y-intercept of regression over years 2-10 or 2-20 (i.e., excluding years affected by fast adjustments or a later change in feedback strength) does give a meaningful model-specific estimate of ERF, but that estimate is typically significantly larger than the y-intercept from regression over years 1-150 (and smaller than the land-warming-adjusted fixed-SST derived ERF). [Nicholas Lewis, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The sentence this refers to is valid for a model run to equilibrium under CO2 doubling. The issues brought up have to do with how ECS and ERF are estimated from 4xCO2 simulations. Text revised to discuss the caveats to this.
19140	73	36			What do you mean for "methodological limitations"? [Gwenaelle GREMION, Canada]	Taken into account. Text revised to clarify.
18496	73	38	73	39	Total cloud feedbacks or Longwave or Shortwave? Maybe useful to mention here for more clarity [Gwenaelle GREMION, Canada]	Accepted: The major component responsible for the ECS spread is shortwave cloud feedbacks. This has now been clarified.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
38760	73	38	73	39	It is not clear that the contribution of cloud feedbacks to variations in alpha accounts for most of the spread in ECS in CMIP5 GCMs. That spread is approximately 2.2:1. According to Chung and Soden (2015; doi:10.1088/1748-9326/10/7/074004), the CMIP5 model spread in 2x CO2 ERF is approximately 1.8:1, which is not very much lower than the spread in ECS, implying a smaller role for cloud feedback variation (unless they strongly co-vary in compensating directions). Supporting this contention, Vial et al (2013) estimates an inter-model standard deviation in cloud feedback of no more than 0.28x (0.45 / 1.6) model mean alpha. [Nicholas Lewis, United Kingdom (of Great Britain and Northern Ireland)]	noted: We agree that the spread in ERF is not negligible, but the contribution to the ECS spread is smaller than the contribution by the spread in alpha (alpha_cloud in particular). Vial et al. (2013; Clim Dyn, doi:10.1007/s00382-013-1725-9) estimated that 70% of the CMIP5 model spread in ECS is explained by the spread in cloud feedback.
18490	73	39	73	39	It could be useful to mention the factors on which ECS depends/could depend in climate models together at one place (e.g. convection, stratospheric chemistry, internal natural variability etc.). The researchers/reader can then easily identify where to look if they want to understand the variations of ECS amongst models. [Gwenaelle GREMION, Canada]	noted: This is a so big issue that we could not explain with a short paragraph here, and instead tried to do in several sections of the chapter (ERF in 7.3.2.1, feedback in 7.4.2, and ECS in 7.5.5).
18468	73	42			A 1% increase per year is perfectly exponential, not "approximately linear"; there's a 10% difference in the increase over the 20-year period mentioned in the next sentance [Gwenaelle GREMION, Canada]	This text states that a 1%/yr increase in CO2 causes a nearly-linear increase in ERF. Text modified to clarify that this is because of the approximately logarithmic dependence of ERF on CO2 concentration.
15072	73	43	73	46	Since 70% of the surface whose temperature we care about is the top layer of the ocean, ocean heat uptake is the most direct cause of surface temperature increases and isn't the cause of a reduction in that increase. The relationship between stored energy and temperature is exactly linear (i.e. 1 calorie into 1 gram water -> 1C) and only the well mixed water in the top of the oceans matters relative to the energy stored by the planet manifesting its temperature thus radiating that stored energy away. The temperatures of the deep ocean, even under the tropics, are dictated by the temperature density profile of water, an endless supply of cold, dense water originating from the poles during winter and water that's deep enough based on its thermal conductivity to insulate deep cold water from warm surface water. The cold water sinks to depth at the polar ends of the thermohaline circulation, hydraulically pushing water up at the equator replacing that which was evaporated and transported to the poles. Per Fourier's Law, the observed linear temperature profile of the thermocline is consistent with it acting as a layer of insulation between cold deep water and warm surface water, even at the equator. [George White, United States of America]	Noted. This comment is a statement unsupported in the literature and does not seem to require a response
19142	73	44			You need a ref. here: "TCR is always smaller than ECS because ocean heat uptake acts to reduce surface warming". [Gwenaelle GREMION, Canada]	Rejected. This statement follows directly from the definitions of TCR and ECS so no reference is given.
25792	73	47	73	47	One would not expect a linear relation between ECS and TCS. One would expect a linear relation in their inverses: 1/TCS = 1/ECS + kappa/F2x, where kappa describes the rate of heat flow from mixed layer ocean to deep ocean, F_heat = kappa * DeltaT, where DeltaT is the increase in temp of mixed layer relative to preindustrial. [Stephen E Schwartz, United States of America]	Taken into account. This text has been modified to note the nonlinearity.
19144	73	50	73	51	The section is somehow not clear. I suggest to rephrase as: "The TCR has been diagnosed in GCMs from transient simulations in which the CO2 concentration increases at 1% yr-1 (the 1%CO2, with an approximately linear increase in ERF over time). It is defined as the average over a 20-year period, centred at the time of atmospheric CO2 doubling, which occurs at year 70 (Box 7.1). TCR is always smaller than ECS given that the ocean heat uptake counteracts the surface warming, but it is strongly correlated with ECS across CMIP5 models (r =0.8, Armour, 2017; Grose et al., 2018), because both TCR and ECS are inherently related measures of climate response to forcing (both are in fact dependent on ΔF2Å~CO2 and α). The approximately linear relationship between TCR and ECS may become eventually nonlinear for values of ECS higher than those spanned by CMIP5 models (Hansen et al., 1985; Knutti et al., 2005; Millar et al., 2015) because to ocean heat uptake plays a central role in setting the warming rate when the radiative response to warming is too weak". [Gwenaelle GREMION, Canada]	Taken into account. This text has been modified for clarity.
53718	73	53	74	33	This is very important but could be made more visible, and also clearer. To scientists it is obvious why we focus so much on this, but still it would be usful to explain in more plain language the importance of having good estimates of ECS and TCR and their use. (For many policymakers scenarios and GWPs are the main elements their use. And GWP does not contain the ECS or TCR). Some more bridging to the applictaions would be useful; Carbon budgets, Simple climate models for scenarios, some emission metrics (GTP), but also as a communication tool for how well we understand the responses to emissions. [Jan Fuglestvedt, Norway]	Taken into account. Good points. This section has been revised and moved to the chapter introduction where it will help to frame the chapter's emphasis on these metrics.
13460	74	3	74	7	A referecne to "pattern scaling" section discussed in chapter 4 should be made here. [Govindasamy Bala, India]	Text modified to reference Chapter 4.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
25794	74	11	74	11	Schwartz (2018) points out that transient climate sensitivity (which is similar to TCR in definition and magnitude, but he argues mpre pertinent to temperature response) is more relevant to change in global temperature change on the multidecadal to multi century time scale than ECS because of the long time constant associated with ECS. See Fig A1 of that paper. Also Held (2010) has made the point that because of the long time constant associated with ECS, a reversal of forcing would lead to a reversal of temperature change, again supporting the relevance of transient climate sensitivity, which governs that response. Schwartz, S. E. (2018). Unrealized global temperature increase: Implications of current uncertainties. J. Geophys. Res. Atmospheres, 123, 3462–3482. https://doi.org/10.1002/ 2017JD028121 [Stephen E Schwartz, United States of America]	Rejected. We do not use the concept of transient climate sensitivity here, though, electing to only discuss ECS and TCR as warming metrics. Schwartz (2018) has been cited elsewhere in the chapter.
25796	74	12	74	12	Within the energy balance model one expects forcings to be fungible and responses to be proportional to forcings with same proportionality coefficient. Given the present uncertainty in sensitivity, the slight differences that might attach to different forcing agents is quite secondary. [Stephen E Schwartz, United States of America]	Agreed. But this text is meant to note that while values of TCR and ECS are in K, they shouldn't be confused with warming projections under realistic emissions scenarios. They do, however, correlate with the warming under realistic scenarios. Text modified to clarify.
19146	74	13	74	15	I would expand why TCR is more policy-relevant than ECS here. [Gwenaelle GREMION, Canada]	Not applicable. This text has been moved to the introduction and modified. Note that it is argued that ECS may be more policy relevant than TCR because it is more strongly correlated with future warming.
19148	74	16	74	17	I suggest to delete the sentence: “However, recent results suggest that the situation is more complicated (Gregory et al., 2015; Grose et al., 2018)”. It seems totally out of context to me. It is not clear at what kind of complication authors might want to refer to. [Gwenaelle GREMION, Canada]	Taken into account. Complexity explained in the rest of the paragraph - no changes made.
25798	74	16	74	25	Is the correlation reported by Grose across cmip models? If so, it would seem a very week reed to lean on. [Stephen E Schwartz, United States of America]	Taken into account. Yes, the Grose et al. paper finds this correlation across CMIP5 models. Text has been moved to the introduction and modified to note caution here, but also to discuss physical mechanisms thought to be responsible.
19150	74	17	74	26	Please clarify these statements. It is unclear what it the point here. This paragraph is not adding the relevant information that the section is supposed to provide: why ECS, TCR are relevant for projections and policy. Be more focus on main messages in this section. [Gwenaelle GREMION, Canada]	Not applicable. This section has been moved to the introduction and the text has been modified.
58112	74	21		25	Is the result more apparent in scenarios in which the radiative forcing stabilises? If so, that could be part of the explanation. [Nathan Gillett, Canada]	According to Grose et al., the effect is actually less apparent in scenarios where forcing stabilizes. Note that this text has been modified and moved.
38762	74	22	74	25	An obvious (partial, at least) explanation is different from that suggested here. Warming up to 2105 will largely reflect TCR x the forcing change up to 2015. Warming post 2015 arising from forcing up to 2015 will eventually reflect approximately the excess of ECS over TCR. Suppose 50% of that "committed" warming occurs by 2100, and that the forcing change over 2015-2100 is the same as that over 1850-2015, say deltaF. In that case warming over 2015-2100 will equal { 0.5 * (ECS - TCR) * deltaF + TCR * deltaF } = 0.5 ECS + 0.5 TCR, so this explanation on its own would account for the 2015 - 2100 warming having broadly equal correlation with TCR and ECS. [Nicholas Lewis, United Kingdom (of Great Britain and Northern Ireland)]	Interesting idea. Though we must stick with assessing the published literature. This text has been modified and moved to the introduction.
49146	74	27	74	33	This has been mentioned in Box 7.1 and thus can be omitted or shortened [Yu Kosaka, Japan]	Taken into account. This text has been moved to the introduction and modified to avoid duplication of concepts.
9606	74	30	74	31	Perhaps instead of "TCRE compares”, rather say “TCRE relates cumulative CO2 emissions with global mean warming", to be consistent with Chapter 5. [Katarzyna (Kasia) Tokarska, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Text modified.
18472	74	42		44	Suggest rephrasing "such a response is approximated in..." to "This response is quantified in climate models through abrupt4xCO2 simulations. The response of N against T is linearly regressed and extrapolated to N=0." [Gwenaelle GREMION, Canada]	Editorial

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
18476	74	42		50	This section is difficult to read and, perhaps, unnecessary. The content from "linear regression of N against T is applied" to "the raw ECS range" is a lengthy verbal description of mathematical data processing. The section requires readers to visualize many mathematical operations (linear regression, scaling with logarithms, errors in regression, etc). This information (1) might be better conveyed by carefully laid out equations and variables, and (2) could be given concisely as a reference, rather than being laid out here, so that readers can focus on results more than methods. [Gwenaelle GREMION, Canada]	taken into account: The beginning paragraph was simplified as it duplicates Box 7.1. Also we have avoided some mathematical terms in the section and rewrite in a plain language.
18470	74	42			I am not sure what the verb "realizes" means with respect to the subject "steady global-mean near-surface air temperature response to a time-invariant perturbation in the CO2 radiative forces". [Gwenaelle GREMION, Canada]	Taken into account. Text revised to "is realised", meaning temperature eventually reaches equilibrium over a long time period
38764	74	43	74	46	A known false assumption, of a purely logarithmic CO2 forcing- concentration relationship is made here, which is inconsistent with the treatment of radiative forcing sections of this chapter. There is no justification for such sloppiness. The relationship given in Etminan et al 2016 should be used instead, and all ECS and ERF estimates derived from abrupt4xCO2 simulations divided by 2.092 (as in Lewis and Curry 2018), not by 2. [Nicholas Lewis, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. The sentence has been removed in SOD. Assessment of the instantaneous radiative forcing (section 7.3.2.1) does not use such a simple logarithmic relationship and actually adopted the method by Etminan et al. (2016).
18474	74	43			What are deltaN and deltaT? [Gwenaelle GREMION, Canada]	Taken into account - covered in section 7.4.1.1
25800	74	46	74	46	Should probaby specify that it is the F2x pertinent to that model, not the best estimate F2x [Stephen E Schwartz, United States of America]	Rejected. We have actually referred the best estimate in 7.3 to as F2x here.
14384	74	48			Remove "be" [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Accepted.
38766	75	1	75	17	It should be made clear that this method is only slightly less dependent on GCMs. Moreover, feedbacks in GCMs depend critically on SST warming patterns, which are very similar in most GCMs but differ between GCMs and observations. Further, the strong dependence between feedback strength and F_2xCO2 in GCMs combined with the substantial disagreement in many models of their F_2xCO2 with LBL-derived estimates suggests aliasing of F_2xCO2 errors with feedback errors in GCMs (which may well, as suggested later on page 75, be related to cloud adjustments and feedbacks), making GCM-based estimates of feedbacks even more uncertain, and likely biased. [Nicholas Lewis, United Kingdom (of Great Britain and Northern Ireland)]	rejected: As we explained in 7.4.2, the process-based evaluation of feedbacks has been made by referring to observations, theory, and process resolving models (LES, CRM etc), some of which are supported by GCMs. Indeed, Table 7.11 summarizes the assessed ranges of individual feedbacks in this chapter, each of which is different from direct calculation from CMIP5/6 models. Likewise, the assessment of F_2xCO2 in 7.3.2 is based on LBL and an estimate of stratospheric/tropospheric adjustment and does not much reply on F_2xCO2 as calculated by the regression to GCM 4xCO2 runs. The SST pattern effect is separately considered in 7.5.3 and it does not seriously affect the process-based assessment here.
18478	75	1		16	This paragraph is highly mathematical and not obviously necessary. The section might be clearer if it was replaced with a concise statement such as, "An alternative approach to assess the ECS range, less dependent on GCMs, is to estimate F2xCO2 and alpha separately from independent lines of evidence. This approach is laid out in [reference]. It has the advantages of [x], the disadvantages of [y], and results [z]." [Gwenaelle GREMION, Canada]	taken into account: Good suggestion. This section gives one line of evidence for ECS assessment (i.e., from process understanding of the forcing and feedbacks), so is necessary for the synthesis assessment in 7.5.5. But the text was simplified and revised by eliminating mathematical explanation to increase readability.
19152	75	3	74	15	I suggest to make it clear what is IRF and LBL. [Gwenaelle GREMION, Canada]	Taken into account. IRF defined in box 7.1, LBL defined in section 7.3.2.
58114	75	8			Should 'slight' be 'slightly more'? [Nathan Gillett, Canada]	editorial: reworded
14386	75	27			"the same" [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Accepted.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
38768	75	35	75	39	a) TCR does not only reflect the fast (~years 1-20) response in abrupt4xCO2 simulations. Forcing increments during the first 50 of the 70 year TCR period also reflect post-20 year responses; and b) the correlation between alpha up to year 20 and alpha over years 21-150 is low in CMIP5 GCMs and is positive not negative: +0.23 for the set of 31 CMIP5 models studied in section S4 of Lewis & Curry 2018 (using regression over years 2-20 and 21-150 employing Deming regression over years 21-150 to avoid bias from regression dilution caused by internal variability in temperature). [Nicholas Lewis, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable: We did not discuss TCR here.
14388	75	35			low cloud changes are also involved in determining a negative relationship between ECS and hydrological sensitivity e.g. Watanabe et al. (2018) Nature Clim., doi:10.1038/s41558-018-0272-0 (8.2.1.1.1) [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Thanks for the suggestion. The reference has been cited.
39434	75	39	75	43	The process-based assessed ranges for ECS are unreasonably small, and the assigned confidence far too high, in view of all the uncertainties associated with dependence both on GCM parameterizations and on GCM warming patterns. It has been found that ECS can be varied over nearly a 2:1 range in a state of the art GCM simply by altering a single parameterization, without any clear way to distinguish from observations that any version behaved more realistically (Zhao et al 2016; DOI: 10.1175/JCLI-D-15-0191.1). That by itself implies that the 2.3 - 4.1 C likely range is far too narrow. This study should be cited and its findings and their implications pointed out. [Nicholas Lewis, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Zhao et al. (2015) (reviewer gave incorrect publication year) is a nice study examining dependence of climate feedback on different treatment of precipitation efficiency in cumulus clouds. This process uncertainty is relevant to the high cloud amount feedback and the paper has been cited in 7.4.2.4. As Zhao et al. acknowledge, their approach of using Cess sensitivity parameter cannot be interpreted at its face value for estimates of their models' ECS (e.g. absence of surface albedo feedback). Yet, the maximum and minimum in alpha among their three experiments was 0.88 W/m^2/K, which is still within the 90% range of our assessed net climate feedback (Table 7.11).
58116	75	41			Is this 'likely' range the 17-83% range of the calculated distribution? i.e. the authors are not adding to the uncertainty estimate for unaccounted for sources of uncertainty? Explain and justify the approach. [Nathan Gillett, Canada]	noted: Yes, the likely range was calculated from the calculated probability distribution of ECS based on the ranges in ERF and alpha assessed in 7.3.2.1. and 7.4.2.6. Unaccounted source of uncertainty has already been included in the ranges of forcing and feedback.
45566	75				It is not clear from the wording that the ECS given here is meant to be a preliminary calculation based on GCMs rather than a final judgment (ditto for later calculations based on different types of evdence). Also the text is a very roundabout way of saying (I think) that you will inflate the GCM range by 30%, justified by eliminating the anticorrelation between forcing and feedback. I would not have identified this as the main problem with the GCM range, but rather, the possibility of missing feedbacks. So this discussion may need nuancing, it seems like a lot of discussion is being given to the forcing-feedabck independence issue which may turn out to be a red herring (it is probably just because when models are out of equilibrium, there are e.g. enhanced land-ocean contrasts which provoke cloud responses that go away when a new equilibrium is realised). Finally, given that this result here seems to be based on GCMs, should this discussion be combined with the GCM discussion that comes back later in Section 7.5.5? [Steven Sherwood, Australia]	taken into account: The ECS assessment in this section is based on process evaluations for ERF (section 7.3.2.1) and individual feedbacks (section 7.4.2) which are only partly based on GCMs. This is only one of multiple lines of evidence for the synthesis assessment in 7.5.5. The text has been revised and clarified with respect to the different lines of evidence that inform the assessment of ECS.
25802	76	8	77	11	I call attn to another paper that infers ECS (and transient climate sensitivity) from observed increae in GMST and range of total forcing. Possible language Based on AR5 5-95% confidence range of aerosol and total forcing Schwartz (2018), using a two-compartment energy balance model together with observed increase in GMST over the instrumental record, inferred transient climate sensitivity 1.30 (1.01, 2.04) K and equilibriium climate sensitivity 1.69 (1.22, 3.18) K, where the ranges correspond to the 5-95% limits of the forcings. Schwartz, S. E. (2018). Unrealized global temperature increase: Implications of current uncertainties. J. Geophys. Res. Atmospheres, 123, 3462–3482. https://doi.org/10.1002/ 2017JD028121 [Stephen E Schwartz, United States of America]	noted: nice suggestion, but the reference does not fit the scope of this section which is based on process-based assessment of ECS and TCR but not based on historical observations.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
45568	76	8			It is not clear why a simplified EBM is being used here. The forgoing ECS which is being transferred was obtained from coupled GCMs. Why not then use the same GCMs to obtain TCR? What is the advantage, given you're already 'trusting' the GCMs, to then use a reduced model? You could inflate similarly as before. [Steven Sherwood, Australia]	noted: In this section TCR is transferred from ECS assessed using process-based ERF and feedbacks (sections 7.3.2 and 7.4.6), and we did not use ECSs from GCMs as if they are 'trusted'. Probably the confusion arose from including ranges from CMIP models for comparison with the process-based estimates. We have simplified the figure and eliminate mentioning ECSs from GCMs, which is separately discussed in 7.5.5.
18480	76	10		11	This is the clearest statement of ECS estimation in the paper: ECS = -dF2xCO2/alpha. This statement might better belong in section 7.5.2.1, where that estimation is discussed in detail. [Gwenaelle GREMION, Canada]	noted: A similar statement was given in 7.5.2.1 (p.74 L.46).
25804	76	15	76	15	Add references: Schneider and Thompson (1981), historically important; Boucher and Reddy (2008); Schwartz (2013, 2018). Boucher O, Reddy MS (2008) Climate trade-off between black carbon and carbon dioxide emissions. Energy Policy 36:193–200. doi:10.1016/j.enpol.2007.08.039 Schneider, S. H., and S. L. Thompson (1981), Atmospheric CO2 and climate: Importance of the transient response, J Geophys Res, 86, 3135-3147, doi:10.1029/JC086iC04p03135. Schwartz, S. E. 2012. Determination of Earth's transient and equilibrium climate sensitivities from observations over the twentieth century: Strong dependence on assumed forcing. Surveys in Geophys. 33, 745-777, doi:710.1007/s10712-10012-19180-10714. Schwartz, S. E. (2018). Unrealized global temperature increase: Implications of current uncertainties. J. Geophys. Res. Atmospheres, 123, 3462–3482. https://doi.org/10.1002/ 2017JD028121 [Stephen E Schwartz, United States of America]	rejected: The suggested references do not fit the scope of this section which specifically aims at transferring the ECS estimate from process-based understanding to TCR.
39350	76	27			7.5.2.2 P7-76 line 27 The two layer models already involve numerous parameters, yet are poor approximations to the earth system that instead has a hierarchy of energy storage mechanisms. Models of this hierarchy involving only two parameters are possible by exploiting the scaling (power law) symmetry of the climate response (van Hateren 2013), (Rypdal 2012), (Hébert, Lovejoy et al. 2019), (Lovejoy 2019). (Hébert, Lovejoy et al. 2019), was able to use such a scaling model to estimate the most likely ECS 2.1 oC in the range 1.8 - 2.6 (17-83% confidence) and the most likely TCR 1.6 oC in the range 1.5 – 1.8 (17-83% confidence) as well as making climate projections to 2050 and 2100, (Lovejoy 2019). These models may also be justified by the fractional energy balance equation in which the energy storage is modelled by a scaling (fractional) derivative (Lovejoy, Varotsos et al. 2018), (Lovejoy 2019). Hébert, R., et al. (2019). "An Observation-based Scaling Model for Climate Sensitivity Estimates and Global Projections to 2100." Climate Dynamics (under revision). Lovejoy, S. (2019). Weather, Macroweather and Climate: our random yet predictable atmosphere. New York, N.Y. USA Oxford U. Press. Lovejoy, S., et al. (2018). "Atmospheric Scaling and Climate Variability Across Scales " Earth and Space Science submitted, Sept. Rypdal, K. (2012). "Global temperature response to radiative forcing: Solar cycle versus volcanic eruptions." J. Geophys. Res. 117: D06115. van Hateren, J. H. (2013). "A fractal climate response function can simulate global average temperature trends of the modern era and the past millennium." Clim. Dyn. 40: 2651. [Shaun Lovejoy, Canada]	noted: We will check the suggested recent references when they become available. However, we think that the two-layer EBM is a good physically based approximation of GCMs (and hence nature) as demonstrated by Geoffroy et al. (2012, 2013a, 2013b). Other studies, e.g. Rypdal (2012) also used a similar two-layer model with a complex transforms in frequency domain, but fitting parameters with individual GCMs done by Geoffroy et al. gives a better reproduction of temperature evolution in GCMs.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
53034	76	28	76	29	Clarify in this sentence the term "surface temperature,". Is this sea-surface temperature (I assume), or near-surface air temperature (e.g. definitions previously clarified) [Steven Smith, United States of America]	We have used the term 'surface temperature' interchangiably between SAT and SST here given that their difference has only very minor contribution to the global-mean surface temperature change (Chapter 2).
14390	76	38			"...the larger the fast response occuring in a GCM, the greater the..."" [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Text revised.
25806	76	44	77	5	Suggest that this EBM be more explicitly developed here. Essential is an equation relating TCS and ECS. I refer you to such a more explicit development in Appendix A of Schwartz (2018). I would urge you to develop equations in terms of sensitivities in systematic units, K / W m-2, and then having done that, at the end translate into CO2 doubling units via F_2x. It's much cleaner. It is not necessary to rely on GCMs to obtain εγ . This can be obtained observationally from net flux of heat into the deep ocean and the increase in GMST over the instrumental record. The quantity εγ at line 2 on page 77 has units, W m-2 K-1, which should be given. As noted it is hard to decouple εγ; therefore just use a single quantity, commonly given as kappa and don't even bother to introduce εγ . The argument suffers further from the identification of TCR (response at 70 years to 1% yr-1 increase of CO2) with Transient Climate Sensitivity, TCS, which is equal to TCR + the further unrealized response at year 70, as described in that appendix, but the resultant error is small. I suggest a box to spell this all out, along the lines of the appendix of Schwartz (2018), which I think would be helpful to the reader. I would be happy to adapt that appendix to a box for the SOD. Schwartz, S. E. (2018). Unrealized global temperature increase: Implications of current uncertainties. J. Geophys. Res. Atmospheres, 123, 3462–3482. https://doi.org/10.1002/ 2017JD028121 [Stephen E Schwartz, United States of America]	noted: Thank you for the comment. However, there are many suggestions for revisions in which writing mathematical details should be avoided for non-experts. The two-layer EBM is one of emulators widely adopted in the Report (explained in Boxes 1.5 and 4.2), so that we refer to those Boxes instead of going into details of the EBM.
25808	76	44	77	5	Panel b of figure 7-23 almost illustrates the difference between TCR and TCS. TCR, the value of DeltaT at year 70 is ever so slightly less than the value of this quantity at, say year 100, when the lag due to the short time-constant transient has died out. This might usefully be noted in the text. It is a subtle point but important from a definitional perspective. [Stephen E Schwartz, United States of America]	noted: In this illustration, the equilibration starts at year 70 when the CO2 ramp-up ends and is stabilized to a doubled value. Therefore, the ocean heat uptake is not yet saturated at year 100 when the response is still far from ECS.
25810	76	44	77	5	Referring to table 7.13. I note that alpha is given here as a positive quanttity, whereas it is defined at page 7-9, line 19 as a negative quantity. I recommend the positive quantity throughout. [Stephen E Schwartz, United States of America]	taken into account: the total alpha has been defined as to be negative (Box 7.1 and Table 7.11) so that positive value means a positive feedback. The sign in Table 7.13 was reversed.
38770	76	48	76	48	Surely this equation for TCR is wrong? It is missing a term for heat uptake by the ocean surface layer, which is quite significant in 1pctCO2 simulations; gamma only deals with deep ocean heat uptake. Geoffroy et al (2013a) (part II) estimated a CMIP5 mean ocean surface heat capacity of 8.2 W-yr m-2 K-1, any by year 70 GMST is rising at 0.3 to 0.35 K/ decade in a typical CMIP5 model, implying a surface layer heat uptake of 0.25 - 0.3 W/m2 - far from negligible. [Nicholas Lewis, United Kingdom (of Great Britain and Northern Ireland)]	rejected: gamma is the heat exchange coefficient between the surface and deep layers, so the expression for TCR deals with the effect of heat uptake. Heat absorbed by the surface layer should not be called uptake as it is represented by the surface warming due either to forcing or climate feedbacks.
25812	76	49	76	49	damping, not dampening [Stephen E Schwartz, United States of America]	Accepted. Text revised
36612	76				Section 7.5.2.2. I am not quite clear on why the authors mention work with a two layer EBM. Perhaps you could dedicate a few words to explain why this in a useful exercise. [Carlos Mechoso, United States of America]	taken into account; the sentence was revised in SOD so that readers understand why taking a two-layer EBM can be a good emulator for the present purpose.
49148	77	2	77	2	Please give the unit for εγ. [Yu Kosaka, Japan]	Accepted. W m-2 °C-1 units added.
53036	77	7	77	10	Clarify here and in other sections if this is an estimated due solely to the methods described (e.g., two-layer EBM),, or if this is the aggregate TCR estimate for the chapter. [Steven Smith, United States of America]	taken into account: We have clarified that the estimated values in this section are based on a specific line of evidence and will be combined with estimates from other lines of evidence later in 7.5.7.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
38772	77	7	77	10	I am unable to reconcile the TCR estimate to the estimates of the various parameters given. Please ensure that in the SOD it is stated exactly how and using what parameter values the TCR estimates have been calculated. You should also make very clear that this method of deriving TCR from ECS is substantially dependent on estimates from CMIP5 models of C, C_0, ε and γ. [Nicholas Lewis, United Kingdom (of Great Britain and Northern Ireland)]	noted: We may improve clarity in SOD, but have explained explicitly that C and C_0 were fixed at the multi model mean values whereas uncertainty in epsilon*gamma was included in the assessment of TCR
25814	77	10	77	10	Suggest give 5-95% range also. [Stephen E Schwartz, United States of America]	Accepted. 5-95% range given in FGD.
9608	77	28	77	35	Should ECS derived from historical conditions be referred to as the 'effective climate sensitivity', since it is not assessed at equilibrium, and it is not exactly the same as ECS? It may be good to clarify this difference. [Katarzyna (Kasia) Tokarska, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Thanks for the suggestion. We now introduce this concept in the introduction and use it here.
49150	77	38	82	7	The use of "likely" to indicate "median" may be inconsistent with other parts of the report. [Yu Kosaka, Japan]	Taken into account. Language throughout this section has been checked for consistency with the rest of the report.
45572	77	38			It appears that the historical-warming based ECS and TCR estimates given in this report are derived by the authors using a methodology that is not clear. It may be repeating that of previous studies such as Otto et al. but this is not explicitly stated. [Steven Sherwood, Australia]	Taken into account. Text modified to clarify that the methods of Otto et al. (2913) were used for this calculation.
18770	77	47	77	49	This statement could be improved by "a conceptual model typically based on the global-mean energy budget" . The current statement could lead the non-expert to think in a numerical simulation. [Gwenaelle GREMION, Canada]	Taken into account. Text modified.
18768	77	49	77	49	The reference Hegerl and Zweirs (2011) seems out of place here. The the main topic of this article consists in detection and atribution of climate change using model simulations, which is not energy budget models. [Gwenaelle GREMION, Canada]	Taken into account. Reference removed.
38774	77	49	77	53	This claim is false. Lewis and Curry 2018 (LC18) gives a global-mean energy budget estimate that does account a) for the dependence of feedbacks on the spatial pattern of surface warming (as reflected in the lower feedback in CMIP5 models over years 21-150 of abrupt4xCO2 simulations compared to 1pctCO2 simulations , which it shows are a good analogue for historical simulations) and b) for incomplete global coverage of some GMST records. The resulting central ECS estimate is 1.76 K. No study has validly shown that this estimate is biased low by either of the factors mentioned. Although the globally-complete GMST index used in LC8 blends SST over ocean with air temperature over land, it shows that observational / reanalysis (as opposed to free running GCM) evidence indicates that this causes only a very minor bias, of a few percent at most. That is less than the excess in CMIP5 models of ECS estimated over years 21-150 of abrupt4xCO2 simulations (as used to estimate ECS in LC18) over that estimated over years 1-150 (as used to estimate ECS in this Chapter). LC18 also shows that there is no evidence of any bias in sensitivity estimation as a result of historical warming patterns not matching those simulated by AOGCMs, nor as a result of compositive historical forcing differing from CO2-only forcing. The findings of Lewis and Curry 2018 in all these respects should be covered. [Nicholas Lewis, United Kingdom (of Great Britain and Northern Ireland)]	We believe this statement is an accurate reflection of the assessed literature. There are differences in estimates of the magnitude of these two effects across studies, but all that we know of (including Lewis and Curry 2018) demonstrate that ECS inferred from historical energy budget constraints is biased low for these two reasons. Note that the text below this has been revised to discuss the results of Lewis and Curry 2018 alongside those of Armour 2017 and to assess an overall value of alpha' that spans values near zero.
9610	78	4	78	5	This is referring to the effective climate sensitivity, since these are non-equilibrium conditions. I think this distinction is important and should be mentioned here. [Katarzyna (Kasia) Tokarska, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Effective climate sensitivity concept added and used throughout.
14392	78	4			I wonder if the historical estimates of ΔT, ΔF, ΔN should be labelled (e.g. subscript hist?) to make it clear these are present day values? [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. We appreciate the suggestion. However, these variables are meant to be used generally here. We give specific values for them based on multiple different observational periods and believe it would be cumbersome and confusing to carry around different subscripts for each use, so we have left it as is.
45570	78	5	78	46	I think you mean, studies making similar assumptions or system models have obtained similar results? [Steven Sherwood, Australia]	Taken into account. Yes. The text has been modified to reflect this.
38776	78	7	78	9	No such assumption is required to infer TCR from the historical record. The assumption required is simply that the evolution of forcing over the historical record, or some portion of it starting before the rapid post 1950s increase in forcing, approximates (in its effects on GMST) a 70 year long linear forcing ramp (Otto et al 2013). [Nicholas Lewis, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The text was revised.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
25816	78	15	78	21	Given the large uncertainty in forcing over the historical record, it seems to me that any concern over the change in alpha with changing global mean surface temperature over this record must be viewed as secondary. [Stephen E Schwartz, United States of America]	Rejected. As described in this section, it is indeed secondary for the lower bound of ECS, but not for the median value or upper bound so it must be included.
14394	78	22			See also formulation by Ceppi & Gregory (2019) Clim. Dyn. [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Reference added.
19302	78	24	78	24	"Allowing" is not an accurate word here. "Considering the variations of feedbacks with the spatial pattern..." would be better. [Gwenaelle GREMION, Canada]	Taken into account, but no changes made as this paragraph refers to conceptual energy balance models, where the context is models allow (or permit, if you prefer) different levels of complexity
38778	78	27	78	30	This method is invalid and should be abandoned. First, it makes no allowance for the relationship in GCMs between α and α' (Armour 2017; Lewis and Curry 2018). Secondly, the value of α' derived in Andrews 2018 is unreliable and likely greatly biased. That is primarily because it is based solely on the AMIP II SST and sea ice dataset, and results using other datasets are very different (although there are other issues as well). As Andrews et al 2018's supplementary information shows, when using instead the more modern, improved HadISST2 SST and sea-ice dataset, in the two models tested the value of α' was 0.6 W/m2/K smaller. Applying this adjustment to their model-mean estimate of α' would reduce it to a negligible level (< 0.1 W/m2/K). Also, my own results using a Green's function approach (Zhou et al 2013) show that estimated feedback in CAM5.3 is the same when driven over the historical period by any of HadISST1, or HadISST2, or the historical simulation warming pattern in CESM1-CAM5, or the years 1-70 warming pattern in the CESM1-CAM5 1pctCO2 simulation. [Nicholas Lewis, United Kingdom (of Great Britain and Northern Ireland)]	We now assess a value of alpha' that considers multiple lines of evidence, including Andrews et al., Armour, Lewis & Curry, and Dong et al. for coupled CMIP6 models, and taking into account observational evidence of the link between surface warming patterns and TOA radiation as well as proxy evidence of long-term warming patterns. The value of alpha' is uncertain for a number of reasons and thus it spans a wide range of values, including values near zero.
18772	78	37	78	37	The reference "Richardson et al. (2018a)" is out of place here. This paper analyzes the role of the different definitions of global mean surface temperature in CRU data and CMIP5 simulations on the quantification of the amount of carbon that can be emitted to meet the temperature targets of the Paris Agreement. [Gwenaelle GREMION, Canada]	Taken into account. Reference removed.
18774	78	38	78	38	The references Forster (2016) and Knutti et al. (2017) are too broad for supporting the claim "improved estimates of radiative forcing". References specifically addressing this claim should be provided (maybe from the references in Forster or Knutti et al.). [Gwenaelle GREMION, Canada]	Taken into account. This text has been modified.
39352	78	46			7.5.3.1 P7-78 line 46 (end) Add: , (Hébert, Lovejoy et al. 2019), most likely ECS 2.1 oC in the range 1.8 - 2.6 (17-83% confidence) and the most likely TCR 1.6 oC in the range 1.5 – 1.8 (17-83% confidence). Hébert, R., et al. (2019). "An Observation-based Scaling Model for Climate Sensitivity Estimates and Global Projections to 2100." Climate Dynamics (under revision). [Shaun Lovejoy, Canada]	Noted. Please email a copy of the submitted manuscript to the Chapter 7 authors so that it can be read and included.
57892	78	48	78	49	The Argo profiling floats have only achieved its near-global coverage target in November 2007. See Riser et al. What about consistent with SROCC chapters 1 and 5? Also, Johnson et al. 2019 (BAMS, submitted) show that largest differences between observational GOHC estimates occur prior to 2006. [Catia Domingues, Australia]	We now use the period 2006-2018 and have ensured consistency with Section 7.2.
38780	78	48	78	50	The 2002 data is far too early for accurate ocean heat content measurements. The 2002-2005 period was affected by lack of adequate ARGO coverage and uncertainty arising from the switch to ARGO, certainly sub-700 m depth. ARGO deeper floats were substantially short of adequate coverage before 2006, or possibly 2005. Hence NOAA does not give annual estimates of 0-2000 m OHC before 2005., and with a much higher uncertainty range for 2005 than for 2006. The 2002-2018 period is therefore a poor choice for deriving an energy budget ECS estimate. The 2006-2018 period would be much more appropriate. [Nicholas Lewis, United Kingdom (of Great Britain and Northern Ireland)]	We now use the period 2006-2018 and have ensured consistency with Section 7.2.
14396	78	50			check consistency with 7.2 which I think states 0.71 Wm-2 for 2005-2015 not 0.75 Wm-2 from 2002-2018 which I guess is an update. [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Revised for consistency with 7.2.
18776	78	52	78	52	The reference "Lewis et al. (2018)" must be written as "Lewis and Curry (2018)". Both authors are repeated in the list of references. [Gwenaelle GREMION, Canada]	Accepted. Thank you for pointing out this.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
38782	78	52	78	54	The GCM simulation evidence cited in Lewis and Curry 2015 and Lewis and Curry 2018 suggested a global energy imbalance of 0.2 W/m2 over 1850-1900, not 0.1 W/m2. The 0.1 W/m2 value used in those studies was after scaling down the GCM estimates by 0.6 to reflect the much lower value of ECS derived in those studies than in the relevant GCMs, which reason does not apply to this chapter's estimates. Also, the Zanna et al 2019 study was initialised at zero ocean heat uptake in 1850, so its 1850-1900 mean ocean heat uptake estimate is only an anomaly relative to ocean heat uptake in 1850 and needs to be adjusted for 1850 heat uptake, which was positive per long term GCM simulations. So the evidence cited in fact supports a higher ocean heat uptake than 0.1 W/m2 over 1850-1900 than 0.1 W/m2. [Nicholas Lewis, United Kingdom (of Great Britain and Northern Ireland)]	We now use a value 0.2 W/m2 as suggested and choose an uncertainty range of +/- 0.2 W/m2 to span these and other studies such as Gebbie and Huybers (2019).
18784	78	54	78	54	Please, note that Zanna et al. (2019) does not employ any general circulation model for the reconstruction. Authors employ the results of Ref. 12 (which are based on an ocean general circulation model) to obtain their results of ocean heat content, which means that Ref. 12 in Zanna et al. should also be cited. Therefore, the expression "GCMs forced by observed SSTs" is quite inaccurate, since there is only one OGCM involved in the process, and Zanna et al. did not forced that OGCM with SSTs, but a Green function estimated from a data assimilation experiment using the aforementioned OGCM. [Gwenaelle GREMION, Canada]	Taken into account. The text has been modified to clarify this.
25818	79	1	79	12	The uncertainties are given for forcing, TCR, ECS independently, but they are not independent; the entanglement issue; a low forcing implies high sensitivity, and vice versa. This needs to be stated. [Stephen E Schwartz, United States of America]	Taken into account. ECS and TCR are explicitly correlated with forcing via equation ECS = $\Delta F_{2\times CO2}\, \Delta T/(\Delta F - \Delta N)$ and the fact that the largest fractional uncertainty is in ΔF. Text added to note this.
9612	79	6	79	7	since ERF includes CO2, is this "cross-correlation" accounted for? [Katarzyna (Kasia) Tokarska, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Yes, it is. Text added to note this.
18750	79	14	79	28	It is indicated that HadCRUT4 trends for surface air temperature (SAT) are 16% smaller than a certain global SAT trend estimate. But it is not indicated the source of such global SAT trend. Are the authors comparing against simulated global SAT? Are they comparing against other observational products? Are they comparing against reanalysis products? A different analysis of the HadCRUT4 itself? Please, make this paragraph clearer. [Gwenaelle GREMION, Canada]	Taken into account. This paragraph has been reworked with the latest data from Chapter 2 and properly cited
18752	79	14	79	28	The text says that the HadCRUT4 SAT trends differ in comparison with "other lines of evidence". Such alternative lines of evidence should be cited and in this paragraph. Additionally, those alternative "lines of evidence" have a better spatial coverage than HadCRUT4 for SAT indicators. Which are those indicators? Which are the sources suggesting those discrepancies? More detailed information is needed in this paragraph, and more references are also desirable here. [Gwenaelle GREMION, Canada]	Taken into account. This paragraph has been reworked with the latest data from Chapter 2 and properly cited
37822	79	14	79	28	This paragraph needs reconsidering. Atmospheric reanalyses provide estimates of surface air temperature trends over the past 40 years that are not very different from the blended SST/SAT trends from more traditional gridded datasets. Updating figures published by Simmons et al. (2017. doi: 10.1002/qj.2949), reanalysis trends for 1979-2018 are 0.182ºC/decade for GSAT from ERA5, 0.183ºC/decade for GSAT from ERA-Interim, and 0.178ºC/decade for GSAT from JRA-55, while GMST from HadCRUT4 gives 0.176ºC/decade. This suggest only a 5% underestimate for HadCRUT4. We have no evidence to suggest that the reanalyses underestimate the rate of increase in GSAT by as much as 10%. The modelling used by Richardson et al. may have overestimated the 16%, HadCRUT4 may be subject to other errors that compensate the effect discussed by Richardson et al., or perhaps the past forty years are not representative of the period Richardson et al. considered. Either way, a more cautious wording could be sought for this paragraph. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We agree, This paragraph has been reworked with the latest data from Chapter 2 and properly cited
15074	79	16	70	16	There are many reconstructions that show significantly different results from HadCRUT4. For example, UAH, which seems to be closer to the ground truth than either HadCRUT4 or GISStemp. At the very least, the significant uncertainty across the many reconstructions should be reflected as decreasing the certainty in the reported result. Ignoring repeatable science just because it conflicts with the desired policy goals is not legitimate science. [George White, United States of America]	Rejected. This section uses surface warming since the 1800s to constrain climate sensitivity. UAH provides estimates of lower tropospheric warming, not surface warming, and does not have a record of sufficient length to be used here.
18786	79	18	79	20	This claim is incomplete. It says that trends in HadCRUT4 data are 16% smaller than air temperature warming, but how is such warming estimated? From meteorological observations? From simulations? From reanalysis products? Additionally, both references supporting the claim are inaccurate. Both articles assess the effect of the different definitions of surface temperature in CMIP5 simulations and in CRU data, but they do not derive sensitivity estimates from HadCRUT4 data, and Richardson et al. (2018a) is totally out of place here and it should not be cited in this context. [Gwenaelle GREMION, Canada]	Taken into account. We agree, This paragraph has been reworked with the latest data from Chapter 2 and properly cited

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
18788	79	24	79	26	I could not check this number (16%) in the provided reference [Gwenaelle GREMION, Canada]	Taken into account. This paragraph has been reworked with the latest data from Chapter 2 and properly cited
38784	79	26	79	28	The decision to use global near-surface air temperature as the standard measure of global warming conflicts with previous IPCC usage. As the 2018 IPCC SR1.5 report says (section 1.2.1.1): "The IPCC has traditionally defined changes in observed GMST as a weighted average of near-surface air temperature (SAT) changes over land and sea surface temperature (SST) changes over the oceans". SR1.5 uses that definition. It should be changed back here, for all purposes, to a blend of air temperature over land and SST over ocean. Air temperature over the ocean is much less relevant for almost all purposes. And near-surface air temperature, unlike SST, is not a prognostic variable in GCMs. [Nicholas Lewis, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. The choice to use SAT rather than a blend of SAT and SST was made at a higher level than this chapter, and is clarified in Chapter 1. Among its advantages are that estimates of ECS and TCR can be linked directly to model projections of future warming.
9614	79	48	79	52	I think this is a very important point that should be mentioned at the beginning of the previous section (historical effective climate sensitivity), explaining what difference it makes if the conditions are non-equilibrium [Katarzyna (Kasia) Tokarska, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The text on historical effective climate sensitivity has a discussion of this. The section has been revised, so hopefully it flows better now.
14398	79	49			"degrede" --> "degree" [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
38786	79	51	80	1	Lewis & Curry 2018 showed that the Armour 2017 estimates of the difference between CMIP5 model ECS estimated under abrupt4xCO2 and under 1pctCO2 were substantially biased, and calculated corrected versions for a larger set of CMIP5 31 models. You should cite Lewis & Curry 2018 here and use a values consistent with its findings. It found a 9.5% excess of ECS estimated from regression over years 21-150 of abrupt4xCO2 over that estimated from 1pctCO2. Using instead the model ECS definition adopted here of that from regression over years 1-150 of abrupt4xCO2, but still using the more accurate Etminan et al 2016 CO2 forcing-concentration relationship, the Lewis & Curry 2018 excess falls to 5% (I am the lead author and have recalculated the Lewis & Curry 2018 Table S2 values on this basis). [Nicholas Lewis, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. This section has been expanded to include values from Lewis and Curry (2018) in the text and figures and to consider these values in the overall assessed range of alpha'.
38788	79	51	80	1	It is mathematically and physically invalid to compute α' by differencing α from regression of abrupt4xCO2 data and that from energy balance estimates based on 1pctCO2 simulation data (or historical simulation or observational data) and to use that to derive any bias in ECS estimated from energy balance estimates relative to that from regression of abrupt4xCO2 data up to year 150. That is because they involve conceptually different y-intercepts. The y-intercept from Gregory-plot linear regression over years 1-150 of abrupt4xCO2 has no physical meaning. It is not a valid estimate of ERF unless α is constant over that period (i.e. the true N vs T relationship is linear), and in most AOGCMs α is not constant over years 1-150. When α' for CMIP5 models is calculated on a consistent basis in CMIP5 models using their own F_2xCO2 estimated from regression over years 2-10 of abrupt4xCO2 to derive α from 1pctCO2 data and also to convert ECS from regression over years 1-150 of abrupt4xCO2 into α, the ensemble mean α' is just under +0.1 W/m2/K. I can supply detailed calculations. [Nicholas Lewis, United Kingdom (of Great Britain and Northern Ireland)]	We have taken care to estimate alpha' in a consistent way (by using alpha defined as -F_2xCO2/ECS rather than from regression over years 1-150 of abrupt 4xCO2 simulation) wherever possible, including from Armour (2017), Lewis & Curry (2018) and Dong et al. (2020). This reduces the value of alpha' from the Andrews et al. simulations by less than 0.1 W/m2/K. This difference falls far within the range of uncertainty on the value of alpha' used here. We appreciate the calculations you have provided for this.
38790	79	51	80	1	Modelled values of ECS under abrupt4xCO2, defined by regression over years 1-150 as in this Chapter, are in fact only 5% higher on average in CMIP5 models than those under 1pctCO2 (per column 3 of Lewis & Curry 2018 Table S2), not 15% higher. The range for the 31 models in that table is -11% to +29%. (All these ECS values are calculated using the Etminan 2016 CO2 forcing-concentration relationship, there being no reason to think that CMIP5 models on average embody an incorrect relationship, such as a purely logarithmic one.) [Nicholas Lewis, United Kingdom (of Great Britain and Northern Ireland)]	We appreciate the calculations you have provided for this and include these updated values from Lewis & Curry (2018).
38792	80	7	80	11	The findings in Andrews et al 2018 are critically dependent on the observational SST and sea-ice dataset used. They are based on the AMIP II dataset, which switches SST datasets in 1981 and uses the old HadISST1 sea-ice estimates. Andrews et al 2018 tested, in two of their GCMs, the effect of using instead the more recent, improved, HadISST2 SST and sea-ice dataset. The resulting historical radiative feedback was far less negative than in their main AMIP II SST/sea-ice dataset based results; for one of the two models it was the same, within 1%, as that under abrupt4xCO2. The +0.6 W/m2/K α' estimate from this study is therefore very likely unrealistic should be disregarded. [Nicholas Lewis, United Kingdom (of Great Britain and Northern Ireland)]	As noted above, we now use multiple lines of evidence to assess a range for alpha', of which Andrews et al. (2018) is only one of them. We also devote more text to discussing the caveats in these estimates including uncertainties coming from historical SST/SIC datasets used.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
38794	80	12	80	16	Over years 1-100 of CMIP5 1pctCO2 simulations, SST warming in the Indo-west Pacific warm pool was very similar to that elsewhere (taking CESM1-CAM5 as a representative model). It was also very similar in historical observations (e.g., linear trend over 1870-2013 or 1871-2010) per HadISST1 and HadISST2; only in the composite AMIP II dataset was SST warming in the Indo-west Pacific warm pool greater.. [Nicholas Lewis, United Kingdom (of Great Britain and Northern Ireland)]	This sentence refers to observations, not model simulations. In any case, it is the warming in the warm pool relative to the rest of the world oceans that matters, not the absolute warming in that region. Note that the text has been revised to discuss uncertainty in the pattern effect from uncertainty in SST datasets based on other comments.
38796	80	16	80	19	The AMIP piForcing method referred to also assumes that the record of observed changes not only in temperature but also in sea ice is accurate. That does not appear to be the case for the AMIP II dataset used in Andrews et al 2018. [Nicholas Lewis, United Kingdom (of Great Britain and Northern Ireland)]	The text has been revised to discuss SST and sea ice concentration uncertainties over the historical record as a major uncertainty for estimates of the historical value of alpha', and the assessed range of alpha' has been expanded to account for these uncertainties.
9616	80	21	80	22	I found this section unclear, does it refer to historical ECS after an adjustment? perhaps it may be good to refer to it as the effective ECS? [Katarzyna (Kasia) Tokarska, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The text has been revised to adopt the suggestion of referring to 'effective climate sensitivity' separately from ECS to make this more clear.
14400	80	24			Can the very high estimates of ECS can be discounted on physical grounds? [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Indeed, other lines of evidence for ECS presented in the chapter are used to constrain the high end of ECS. This is simply saying that historical energy budget constraints alone are insufficient to do so.
15076	80	39	80	42	It's incorrect to claim that the ECS is likely higher based on model results, as the models have been consistently wrong since climate modeling began and they don't seem to be getting any better, especially with regard to hindcasting. It's far more likely that the historic data is more correct than what the models are predicting and the current approach of adjusting historic data to fit the models is unconditionally wrong. Interestingly enough, the best hindcasting model is the Russian one which also predicts the lowest ECS and is often ignored as an outlier. The conclusions in this report rely far too much on complicated models with an excessive amount of knobs and dials tweaking their behavior and whose veracity is impossible to ascertain. The climate system has far too many unknowns to be modeling it from the bottom up as a collection of tiny, chaotically coupled pieces. A top down model based on the laws of physics which considers the Earth a single entity whose observable macroscopic behavior conforms to those laws and that represents the net result of the chaos is a far better approach. [George White, United States of America]	This evidence comes from the cited literature. The reviewers comment is not supported by this literature
15078	80	39	80	42	As an illustration of how bad the models are, consider the GISS ModelE. Its code base is a jumble of untestable, unmaintainable spaghetti Fortran, written in a 1960's dialect of the language. The code has thousands of baked in floating point constants in the most critical code related to predicting energy fluxes (RADIATION.F), most of which are undocumented. It's currently being 'upgraded' to a 1990's version of Fortran by replacing the goto spaghetti with do loops and other less ancient constructs. Based on my review of ModelE, I expect it to be irreconcilably wrong and if someone working for me produced code that sloppy, they wouldn't last very long. The fact that trillions of dollars of otherwise counterproductive policy depends on ModelE being correct is abhorrent. Many of the other models have their origin in ModelE, either conceptually, directly or are tuned to match, which only makes things worse by providing false confirmation masquerading as independent analysis. [George White, United States of America]	This comment is a statement unsupported in the literature and does not seem to require a response
8618	80	53	80	53	I am not sure if it fits here, but an ECS and a TCR estimates have also been given using the Ruelle Response Theory and two ensembles of an atmospheric-only EMIC under abrupt2xCO2 and 1pctCO2 scenarios, respectively (Ragone et al., 2016). Ragone F, Lucarini V, Lunkeit F (2016) A new framework for climate sensitivity and prediction: a modelling perspective. Clim Dyn 46:1459–1471. doi: 10.1007/s00382-015-2657-3 [Valerio Lembo, Germany]	Taken into account. We attempt to include these papers where appropriate.
40756	81	1	81	22	Could shorten for brevity. [Daniel Murphy, United States of America]	Taken into account. This text has been revised for brevity.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
53038	81	1	81	54	This section needs to note that SCMs can produce biased results if they don't account for the faster response time of historical forcing agents that are concentrated either over land or in the northern hemisphere (such as anthropogenic aerosols). Shindell (2014;DOI: 10.1038/NCLIMATE2136), and see also discussion by Meinshausen et al., 2011, doi:10.5194/acp-11-1417-2011) and also (for BC), Sand et al. (2015; 10.1175/JCLI-D-14-00050.1) and Yang et al. (2019; 10.5194/acp-19-2405-2019). [Steven Smith, United States of America]	Rejected. Perhaps we have missed it, but it is not clear that this point is made in these references, which appear to mostly discuss the role of aerosol or BC forcings outside of the context of SCMs.
18790	81	7	81	8	“Forest (2002)” should be replaced by “Forest et al. (2002)” [Gwenaelle GREMION, Canada]	Accepted. Thank you for pointing this out.
38798	81	7	81	9	Neither my 2013 and 2015 papers, which unlike almost all the others cited use scientifically more appropriate objective Bayesian methods, would appear to be relevant here. Was an objective method used to select which studies to cite? If not, what criteria was used? Lewis, Nicholas. "An objective Bayesian improved approach for applying optimal fingerprint techniques to estimate climate sensitivity." Journal of Climate 26.19 (2013): 7414-7429 uses almost the same model as Libardoni and Forest 2011, and is much more highly cited than that paper. And my 2015 paper (Lewis, Nicholas. "Implications of recent multimodel attribution studies for climate sensitivity." Climate dynamics 46.5-6 (2016): 1387-1396), which captures only global mean warming, is subsequent to AR5, unlike all the papers cited here. [Nicholas Lewis, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. A reference to Lewis (2013) has been added here.
18794	81	8	81	8	The list of references here is quite large, but some articles are unnecessary. Forest et al. (2006) and Forest et al. (2008) seem to use the same model, so one of the articles can be removed from the list. In a similar way, Libardoni and Forest (2011) uses the same method and model than Forest et al. (2008), so one of these articles can be removed too. [Gwenaelle GREMION, Canada]	Taken into account. The reference list has been shortened to focus on post-AR5 publications.
38800	81	20	81	22	It should be pointed out that the noninformative priors used in objective Bayesian studies, in particular Lewis 2013 and Lewis 2015, have very little impact on the results. Noninformative priors are mathematical weight functions, not probabilistic representations of existing knowledge, and are formulated in such as way to allow even weak data to dominate the results. [Nicholas Lewis, United Kingdom (of Great Britain and Northern Ireland)]	Noted with a reference to Lewis (2013).
54830	81	21			large impact' is a bit over the top I think it does have a visible impact particularly on the upper tail [Gabriele Hegerl, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. This text has been modified.
25820	81	33	81	33	Add: Schwartz (2018) showed that the historical record of global mean surface temperature is consistent with a range of total forcing that corresponds to the AR5 5-95% confidence limits of aerosol forcing -1.88 to -0.09 W m-2, but requiring differing transient climate sensitivity, 2.04 to 1.01 ˚C, respectively, corresponding to equilibrium sensitivity 3.2 to 1.2.˚C. (These equilibrium sensitivity values were calculated with global heating rate taken as 0.51 W m-2 and F_2x of CO2 taken as 3.7 ˚C and would have to be adjusted slightly for heating rate taken as 0.42 W m-2, reported here.) Schwartz, S. E. (2018). Unrealized global temperature increase: Implications of current uncertainties. J. Geophys. Res. Atmospheres, 123, 3462–3482. https://doi.org/10.1002/ 2017JD028121 [Stephen E Schwartz, United States of America]	Taken into account. Reference added.
18796	81	51	81	51	Richardson et al. (2018a) is out of place here. This is the third time that this article is out of context (see comments 17 and 24). The paper does not add any information for the topic discussed in this chapter and should not be cited. [Gwenaelle GREMION, Canada]	Taken into account. We agree, reference deleted
45574	82	5			You mean, infered in the EBM analysis in the previous section? The Skeie and Johansson studies are also from the historical wamring. [Steven Sherwood, Australia]	Taken into account. The text has been revised added to clarify this.
54828	82	8			At this point I would have expected to read about the quite extensive litereature on estimating TCR from attribution; there are some Stott papers; also Schurer et al 2018 J Climate which is a very recent and quite complete attempt (involving me hence self serving) but apart from self serving, arguably attributing using the full single forcing runs is more reliable to estimate the TCR than simple dynamical models or box models; as the feedbacks dont have to stay constant but instead change the way they change in the model used for the fingerprints. which is one step less outrageous than assuming them constant (and the only source of info we have about changing feedbacks anyway) [Gabriele Hegerl, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. Schurer et al (2018) and related papers are assessed in Section 7.5.4.3 since they are not based on the SCMs described here.
52056	82	12	82	23	In this paragraph there is repeated reference to observational accuracy but accuracy is a deprecated term in metrology (measurement science) because it presupposes the true state is known / knowable which, in some limit, it never shall be. Instead in metrology we talk about uncertainty and perhaps fidelity. Also, I think here what you really mean is that there is not the required level of consistency / comparability between disparate variables required to perform the closure. I think being more technically correct in the wording here would help to clarify the issues to the reader. This may also apply to other areas of the chapter and this was just the first time I picked it up. [Peter Thorne, Ireland]	Taken into account. Language checked throughout.
18754	82	16	82	16	The acronym GOHC should be replaced by “global ocean heat content”. [Gwenaelle GREMION, Canada]	Accepted
18798	82	39	82	39	Lutsko and Takahashi (2018) clearly suggests that the regression coefficients for the total cloud-sky flux are found to be strongly related to ECS, even considering it a potential emergent constraint for ECS. Therefore, the claim “When tested with GCMs, regression-based feedback have been found to be weakly correlated with values of ECS” is highly inaccurate and should be modified to include the exception of total cloud-sky flux. [Gwenaelle GREMION, Canada]	Taken into account. The text has been modified to clarify.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
33208	82	42	82	42	The reference to Zhou et al (2015) is a little misleading: they did indeed find a good across-model correlation between *global mean* cloud feedbacks on inter-annual and long-term timescales. It is not a correlation just for subtropical low clouds, although these cloud types play an important role in driving this correlation. [Mark Zelinka, United States of America]	Taken into account. The text has been modified to clarify.
18800	82	53	82	53	Knutti et al. (2017) is a review article. It would be more useful to cite the specific references that study the climate response to volcanic eruptions. [Gwenaelle GREMION, Canada]	Rejected. We have strived to cite the new literature published since AR5. For literature prior to AR5, we feel that it is acceptable to cite review papers or AR5 itself.
18758	83	9	83	9	Knutti et al. (2017) is a review article. Although review articles can and must be cited, in this case the article is provided as a reference for a specific claim about ECS estimates from internal climate variability. The problem is that Knutti et al. (2017) is a review of ECS estimates, not just for ECS estimates from internal climate variability. Therefore, additional references explaining the methodology for estimating ECS using internal variability must be provided beyond Knutti et al. (2017). E.g., the references in that review article supporting this specific claim about ECS estimates. [Gwenaelle GREMION, Canada]	Rejected. As noted above, we want to cite the new literature published since AR5. For literature prior to AR5, we feel that it is acceptable to cite review papers and AR5.
9974	83	11	83	14	Please add words of caution on the use of emergent constraints or related approaches. Fundamental theoretical issues in associating using such simplified approaches the variability and the sensitivity are explained in Lucarini et al. J. Stat. Phys. 166, 1036 (2017) and in A. Gritsun and V. Lucarini, Physica D 349, 62 (2017), Lucarini J. Stat. Phys. 173, 1698-1721(2018) This boils down to the fact that the climate is not an equilibrium statistical mechanical system. [Valerio Lucarini, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Discussion of emergent constraints from variability has been moved to a section below where emergent constraints in general are assessed. Caveats added.
18802	83	13	83	14	Both Knutti et al. (2017) and Knutti and Hegerl (2008) are review articles. Why not to cite the relevant references for the claim too? It would be more useful to the reader. [Gwenaelle GREMION, Canada]	Rejected. We have strived to cite the new literature published since AR5. For literature prior to AR5, we feel that it is acceptable to cite review papers or AR5 itself.
18756	83	14	83	14	The reference (Cox et al. 2018a) should be written as “Cox et al. (2018a)”. [Gwenaelle GREMION, Canada]	Editorial
48832	83	29	83	30	Based on the total absorption coefficient, which is a sum of those due to CO2 molecules and other atmospheric components, one can evaluate an additional radiative flux to the Earth’s surface due to a change of the atmospheric carbon dioxide concentration, and the corresponding analysis shows that contemporary injection of carbon dioxide in the atmosphere as a result of combustion of fossil fuels is not important for the greenhouse effect. (Smirnov, 2018) [Pekka Sunila, Finland]	Rejected. We presume the reviewer means https://doi.org/10.1088/1361-6463/aabac6. Examining the paper it does not make a valid estimate of ECS, it ignores shortwave effects and feedbacks and missing out key processes, We choose to ignore the paper as many other papers are uncited that do not pertain to the definition of climate sensitivity used in this report.
38802	83	33	83	44	These assessments are in my view substantially biased upwards and quite wrong. The evidence used to adjust assessed sensitivity values up is extremely weak and very non-robust. For example, the α' value of +0.2 W/m2/K and range derived from Armour (2017) was shown in Lewis & Curry 2108 to be hugely exaggerated, a fact not mentioned in this Chapter. And the α' value of +0.6 W/m2/K and range derived from Andrews et al 2018 are critically dependent on the particular SST and sea ice dataset used, with use of a more recent, improved dataset (HadISST2) being shown to reduce the α' value by 0.6 W/m2/K in the models tested. The current draft of this chapter IMO falls far short of the standards of caution in critically appraising studies and weighing the reliability of evidence from them established in the corresponding chapter in AR5 WG1. In my view, it does not represent a properly balanced critical and objective assessment of the available evidence. [Nicholas Lewis, United Kingdom (of Great Britain and Northern Ireland)]	As noted in other responses, several changes have been made to this section to better discuss the uncertainties in our estimates of feedback changes. In particular, an overall value of alpha' is assessed based on multiple lines of evidence and spans a wide range values (including near zero) commensurate with the uncertainties. Values from Lewis & Curry (2018) are now presented in the text and figures.
58118	83	39		41	This assessment that estimates based on climate variability provide medium confidence that ECS is unlikely greater than 5C seems to be at odds with the discussion on lines 9-28 which call into question the results of studies constraining ECS based on variability. This is one of the pieces of evidence which is used to derive a new overall lower upper limit on ECS, so this is important. [Nathan Gillett, Canada]	Taken into account. This statement has been revised. The discussion of emergent constraints has also been moved from the variability section to the section on emergent constraints (7.5.4.1) where their ability to constrain the upper limit on ECS is assessed.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
8666	83	40	93	1	I am not very happy about the emergent constraints being used as a probabilistic estimate, as a biased ensemble (wrong data, or wrong boundary conditions, or wrong forcing, or all models wrong in the same way) will produce an erroneous result. Table 7.12 states "spread from an emergent constraint". I quite like that as it does not pretend to be something it is not. So, it is probably just a case of being careful with the phrasing so as to not give the wrong impression. [Julia Hargreaves, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Indeed, the emergent constraint line of evidence is based on a heterogenous set of studies, and some of them are prone to issues of models, forcings or boundary conditions in different ways. The uncertainties in the assessment are therefore inflated in order to reflect this. Text was added to the Emergent constraint and Synthesis sections to reflect this.
18760	83	49	83	52	Here, the text says that paleoclimate estimates of ECS and long-term Earth's sensitivity are independent of model simulations. But just the line before, the text indicates that the estimates considered here comes from paleoclimate data and simulations. The text, therefore, must be changed here, maybe indicating that sensitivity estimates from paleoclimate data and paleosimulations are independent of historical simulations (and their tuning practices), as well as of idealized experiments such as abrupt changes in carbon dioxide concentrations. [Gwenaelle GREMION, Canada]	Accepted. Paleo models (i.e. emergent constraints) has now been removed from this section. Text adjusted accordingly.
18762	84	5	84	7	Several lines of evidence in this chapter support the concept of a climate sensitivity changing with several factors, as it is indicated here. But this is only an issue for estimating the ECS during the Industrial period. Studying the past evolution of the climate sensitivity may be interesting for constraining the possible evolution of ECS in the 21st century, since the reference state is going to change globally during this century. The text can reflect these range of nuances, and even propose the question: can paleoclimate records/simulations be used to estimate or constrain α'? [Gwenaelle GREMION, Canada]	Accepted. Changes emphasis in intro somewhat, highlighting that state-dependence of feedbacks needs to be accounted for, rather than just listing state-dependence as an uncertainty.
19224	84	14	84	15	it would be nice to specify what the currently best estimates are, still >1-7°C? [Baerbel Hoenisch, United States of America]	Rejected. Here we are discussing the level of confidence, not the values themselves. The AR5 values are in the previous paragraph, and the new AR6 values are given in the summary at the end of this section. I think it would be too early to reveal them here.
13680	84	17	84	17	Add a reference: Stap et al., (2019). Full citation': Stap, L. B., Köhler, P., and Lohmann, G.: 2019. Including the efficacy of land ice changes in deriving climate sensitivity from paleodata, Earth Syst. Dynam, 10, 333-345, https://doi.org/10.5194/esd-10-333-2019. Comment: This paper is also added in the Table 7.12, and should therefore be contained in the text as well, I make a suggestion how this can be done in the next comment). [Peter Köhler, Germany]	Accepted. Added to table and text.
52058	84	20			It feels incorrect to refer to chapter 4 here when it is chapter 3 that has undertaken the substantive assessment of improvements in model performance. I would suggest instead referencing the synthesis of model performance section of chapter 3 here. [Peter Thorne, Ireland]	Not applicable. Sentence removed.
18744	84	24	85	13	There is an spurious interrogation sign when citing Equation 7.1. Is this a compilation error? If not, please remove it from the text. [Gwenaelle GREMION, Canada]	Editorial
18746	84	28	85	37	More details should be provided for the different periods of study here. For example, the number of years before present and the climate conditions of each period (e.g., CO2 levels). This comment is especially relevant for the KM5c period, which is not identified with any temporal window. [Gwenaelle GREMION, Canada]	Accepted. LGM and MPWP are defined when frst used, but KM5c is not. Originally added "3.204–3.207 million years ago" for KM5c, but this was removed in final version due to lack of space.
31978	84	28			The extended time scales of feedback processes available through the paleorecord allowed Fischer et al (2018) to present a refined assessment of the long-term Earth System Sensitivity of up to two times larger than the Charney Sensitivity derived from climate models run to 2100 . [Marie-France Loutre, Switzerland]	Taken into account. This was added in the SOD, but removed in the final version due to lack of space.
52060	84	38			I'm not sure that I agree with the statement that orbital forcing was broadly equivalent. It may be in the global mean annual mean but was likely substantively different in e.g. seasonality in important ways as you go on to note later. Some nuancing of this text feels warranted to avoid a reader making an unwarranted inference. [Peter Thorne, Ireland]	Rejected. For the LGM, even the latitudinal and seasonal forcing is very small. However, I have added "at that time" earlier in the sentence to clariy this further.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
38804	84	53	85	1	To be valid estimates of ECS, paleo energy budget studies need to also allow for changes in non-CO2 greenhouse gases, in aerosol/dust forcing, and in sea level (Köhler, Peter, et al. "What caused Earth's temperature variations during the last 800,000 years? Data-based evidence on radiative forcing and constraints on climate sensitivity." Quaternary Science Reviews 29.1-2 (2010): 129-145.) That point should be made. Not allowing for these factors as well as for changes in CO2, land ice sheets and vegetation will typically lead to an upwards bias in paleo ECS estimates. For the LGM-Holocene transition, that bias would be 49% based on the central estimates in Kohler et al 2010 [Nicholas Lewis, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. Our definition of ECS actually includes these processes as feedbacks, and ECS == S[CO2,LI] in the Rohling et al nomenclature. However, we have now made this clearer, so thank you for this comment.
18806	85	10	85	10	The year in the reference "Heinemann et al." is wrong. The correct year is 2009. [Gwenaelle GREMION, Canada]	Not applicable - no longer referenced
18764	85	13	85	13	Equation 7.1 does not have a denominator. I think authors mean the difference between F and N, so this claim should be modified. [Gwenaelle GREMION, Canada]	Accepted. Modified to state that ΔT and ΔF are close to zero.
13682	85	14	85	14	I suggest to add a paragraph similar to the following text: "There have been substantial improvement in the understanding how climate sensitivity can be derived from paleo data since AR5. For example, based on identical data sets of changes in global mean temperature and radiative forcing over the period covered by ice core data (the last 800,000 years) the mean estimate of ECS based on S[GHG,LI,AE,VG] in the nomenclature of Rohling et al. (2012) has been 2.5°C when ignoring state-dependency (Rohling et al. 2012), but rises to 3.7°C (warmer half of the data, Köhler et al., 2015) or 6.4°C (full interglacials, Köhler et al., 2017) when state-dependency is considered. ECS expresses a relation between temperature change and CO2 change. Therefore, periods in which changes in CO2 and temperature diverge, which has been detected to occur on multi-millennial timescale when land ice growths, should be neglected, reducing the ECS for full interglacials from stated 6.4°C to 3.8°C (Köhler et al., 2018). If additionally different efficacies of radiative forcing are considered (Hansen et al., 2005, Stap et al., 2019), ECS for the same conditions rises to 5.8°C (Table 7.12). These changes in the quantification of ECS are based on an improvement of the interpretation, and not on changes in the underlying data." New references: - Hansen et al. (2005) is already included in the list of references - Stap et al., (2019). Full citation': Stap, L. B., Köhler, P., and Lohmann, G.: 2019. Including the efficacy of land ice changes in deriving climate sensitivity from paleodata, Earth Syst. Dynam. 10, 333-345, https://doi.org/10.5194/esd-10-333-2019. [Peter Köhler, Germany]	Taken into account. We do now include the Stap paper and discuss uncertainties in ice sheet forcing. However, our definiton of climate sensitivity only includes ice sheets as a forcing, so some of the other comments in this box are no longer relevant.
8664	85	40	93	1	7.5.4.2 Paleoclimate data constraints on model simulations 7.5.6 Emergent constraints on ECS I think it would be good if the authors of these sections got together and hammered out something that is consistent. I feel there might be a good version of the truth somewhere between the two.. [Julia Hargreaves, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. This section has been moved.
44562	85	46	85	46	The main discussion on emergent constraints is now in section 1.4.5.2 (moved from Chapter 3). [Bjorn Samset, Norway]	Accepted. Revision made.
8662	85	47	85	48	"The analysis can be carried out with an ensemble of parameter-perturbed instances of a single model, or with an ensemble of different models." Best remove the mention of PPEs here. In AR5, PPEs were already shown to be unreliable. This means we know the whole PPE is biased. An emergent constraint from such an ensemble is expected to be incorrect. [Julia Hargreaves, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. This section has been moved.
18808	85	51	85	52	It seems that Harrison et al. (2014) did not estimate the used values of climate sensitivity, but they used results from the PALEOSENS project. Thus, this should be stated in the text, and Harrison et al. removed. [Gwenaelle GREMION, Canada]	Not applicable. This section has been moved.
18810	85	51	85	52	Harrison et al. (2015) is a review article. It would be useful to cite the relevant references in the article, not just the review article. [Gwenaelle GREMION, Canada]	Not applicable. This section has been moved.
18766	85	53	85	53	PMIP3 is not defined in the entire chapter. In general, the myriad of acronyms used in this chapter should be carefully checked to be sure that all of them are correctly defined. [Gwenaelle GREMION, Canada]	Accepted. Acronym defined.
18748	86	6	86	6	I think the acronym ESS has not been defined on the text. [Gwenaelle GREMION, Canada]	Accepted. Removed ESS.
13686	86	7	86	8	Add reference "Stap et al. (2019)", Full reference: Stap, L. B., Köhler, P., and Lohmann, G.: 2019. Including the efficacy of land ice changes in deriving climate sensitivity from paleodata, Earth Syst. Dynam. 10, 333-345, https://doi.org/10.5194/esd-10-333-2019 [Peter Köhler, Germany]	Accepted. Reference added.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
19226	86	12	86	12	I find the primary focus on boron isotopes as a proxy for CO2 somewhat offensive to all the other paleo-CO2 experts. Boron isotopes have issues as well, and this report (here and in other chapters) should not show them as the only viable alternative to ice core measurements. This report will likely be viewed by funding agencies and it would be more than unfortunate if funding for other proxy estimates would decline in response to this report. We need evidence from different proxies, and can't just put all eggs in one basket. [Baerbel Hoenisch, United States of America]	Accepted. Put an "e.g." and a "for example" in this sentence to make it clear that there are other proxies; hover, in table 7.7 it is clearly the boron isotopes that are making the biggest contribution to advances in ECS since AR5. Added some other references to non-boron proxies, e.g. Super et al.
38808	86	12	87	6	Table 7.12: Martinez-Boti 2015's regression results imply a lower ECS estimate than 3.7°C for the Pliocene. Martinez-Boti 2015's climate sensitivity estimates ignore non-CO2 + land ice forcings. In palaeodata of the last 0:8 Myr, the equilibrium climate sensitivity considering all feedbacks was only about two thirds of ECS[CO2,LI] (PALAEOSENS-Project Members, 2012, per Kohler et al 2015). Non-CO2 greenhouse gas forcing accounts for much of the difference, and the ratio is likely to be similar in the Piocene. Correcting for these omitted forcings reduces the Martinez-Boti estimate to 2.4 K. This should be shown in Table 7.12 with a note that the [CO2,LI] sensitivity has been converted to ECS using this ratio and a doubled CO2 forcing of 3.7 Wm-2 (which matches the 3.7 Wm-2 M-Boti used when calculating F[CO2]). [Nicholas Lewis, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. Our definition of ECS actually includes these processes as feedbacks, and ECS == S[CO2,LI] in the Rohling et al nomenclature. However, we have now made this clearer (specifically, in the section on LGM estimates of ECS), so thank you for this comment.
13688	86	17	86	18	The half-sentence "but only two of those (Friedrich et al., 2016; Köhler et al., 2018) take into account orbital forcing." is wrong. All mentioned papers of Köhler et al consider orbital forcing, and probably some others. I believe this is not only wrong but also misleading here, and suggest deletion. The half-sentence before should be changed and extended on some details: My suggestions: "... (section 7.4.4.) by considering only the warm phases of the Pleistocene, which are, however, defined differently in the various approaches." [Peter Köhler, Germany]	Accepted. Taken out comments on orbit, and added text stating that that warm phase definition varies between studies.
13678	86	21	87	6	The contributions to Table 7.12 based on data of the last 800 kyr should be optimized. Column 4 states the climate sensitivity classification, for which a quantification is given. At best, this should include as many slow feedbacks as possible. Most of the analysis of the various papers of Köhler et al started on CO2 and land ice albedo feedback, ending with S[CO2,LI], however, in the disucssion / conclusions any given estimate of climate sensitivity is based on approximation of missing processes, and therefore S[GHG,LI,AE,VG] can be given. Based on that I suggest the following changes: - study Köhler et al (2015): column 2: warm states of the last 800,000 years of glacial-interglacial cycles; column 4: S[GHG,LI,AE,VG], column 5: 3.7°C (2.5-5.5°C)^b; column 6: uncertainty in temperature and radiative forcing of CO2 and land ice albedo - study Köhler et al (2017): column 2: full interglacials of the last 800,000 or 2,100,000 years of glacial-interglacial cycles; column 4: S[GHG,LI,AE,VG], column 5: [4.7 - 6.4°C]^b; column 6: Range of 2 data sets covering differnt time intervals - study Köhler et al (2018): column 2: full interglacials of the last 800,000 years with no/little temperature-CO2 divergence; column 5: footnote need to change from c to b - study Stap et al (2019, new entry): column 2: full interglacials of the last 800,000 years with no/little temperature-CO2 divergence; column 3: Direct; column 4: S^epsilon_[GHG,LI,AE,VG]^d, column 5: 5.6°C (4.3-6.9°C)^b; column 6:1 sigma uncertainty on efficacy of land ice albedo radiative forcing; new footnote d: S^epsilon_[GHG,LI,AE,VG (comment: epsilon use greek symbol) is not included in Rohling et al., 2012, but is a modification of S_[GHG,LI,AE,VG] which includes different efficacies epsilon of radiative forcing of CO2 and land ice albedo - study Friedrich et al (2016): column 2: warm states of the last 784,000 years of glacial-interglacial cycles; column 5:footnote need to change from c to b - delete existing footnote (c), add new footnote (named d above in entry for Stap et al 2019) - New reference: Stap, L. B., Köhler, P., and Lohmann, G.: 2019. Including the efficacy of land ice changes in deriving climate sensitivity from paleodata, Earth Syst. Dynam,10, 333-345, https://doi.org/10.5194/esd-10-333-2019. [Peter Köhler, Germany]	Rejected. As in response to comment above, in AR6 we are considering non-CO2 biogeochemical feedbacks such as CH4, N2O etc . as part of ECS, as these are included in many AR6 models. This is now made clearer in this section.
13204	86	22	87	6	Consider including a column that specifies which proxies are used and clarify in the table description what is meant by "direct" vs. "paleoclimate constraint." [Nora Richter, United States of America]	Accepted. Added proxies in a column, and taken out methods column because emergent constraints are now in a different section.
18812	86	23	86	23	Table 7.12: the PlioMIP data paper is listed here, but no result is available (I suppose the article is in preparation). The same occurs with Sherwood et al. Can those papers be listed if they do not provide even with preliminary results? [Gwenaelle GREMION, Canada]	Accepted. For the FGD, only final accepted papers are cited.
15004	86	23	87	1	since there are comments here on emerging papers for PlioMIP and DeepMIP papers on climate sensitivity, it may be worth being aware that a MPWP synthesis paper by the PlioVAR working group for the same KM5c interval as PlioMIP is in prep and should be submitted summer 2019. [Erin McClymont, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Pliovar SSTs are included in polar amplification section in the FGD.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
33328	86		87		Suggest adding the type of paleoclimate proxy used to draw conclusions in each study to Table 7.12. Chapter in general had several vague references to "paleoclimate data" without specification; this would be a good place to have that information if not specified in text. [Erika Wise, United States of America]	Taken into account. Identical comment below from reviewer Nora Richter.
38806	87	9	87	10	Estimates based on the LGM to preindustrial Holocene using the Kohler et al (2010; doi: 10.1016/j.quascirev.2009.09.026.) best estimate of the forcing change (the most comprehensive estimate of which I am aware) and the 3 - 7 K temperature change range given in section 7.5.4.1 would give a range for ECS best estimates of 1.25 K to 2.91 K (rebasing to a F_2xCO2 of 4.0 W/m2). Might the reason why none of the Table 7.12 ECS estimates are less than 1.9 K be publisher or researcher bias? Certainly, published best estimates of LGM forcing and temperature changes support a range extending down to 1.25 K. This point is important and should be made. Note also that the top of the 3-7 K LGM to PI warming range given exceeds the higest estimate given in Chapter 2, of 6.2 K for Synder et al (2016), [Nicholas Lewis, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. Here, we assess those estimates of ECS (or S[CO2,LI]) published in the literature. We do not unpack those estimates by combining separate estimates of forcing with separate estimates of temperature, from different studies. Also, note that there are now more recent estimates of LGM ice sheet forcing than Kohler et al (2010), e.g. Tierney et al (submitted).
45576	88	1			There is a lot of repetition in this section from earlier secions. [Steven Sherwood, Australia]	Rejected. Given that this is a summary section, I think that it is OK to include some repetition.
54826	88	6	88	7	These conclusions seem quite strong - is this well reviewed from palaeo specialists? Eg Timmermann etc? as a palaeo person I wouldn't necessarily have looked for ECS in chapter 7 [Gabriele Hegerl, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. For the SOD review process, we targeted paleo reviewers for this and other paleo sections.
58120	88	6			If this is an assessment conclusion with quantified likelihood, avoid using the word 'suggests' in this sentence. [Nathan Gillett, Canada]	Accepted. Replaced "suggests" during re-write.
58122	88	6			The authors assess high confidence in this very likely range of 2-5 C for ECS based on the paleo record. Looking at Table 7.12, while some studies certainly derive ranges supporting the 5 C upper limit, some have much higher upper limits, and many of the ranges derived in these studies are non-overlapping. The fact that the ranges are non-overlapping should limit the confidence in any individual study. The authors mention subselection of studies which carry out uncertainty analysis and consider orbital forcing, but this selection is not fully justified in the text. More justification of the basis on which the authors derive the 2-5C range from the studies in Table 7.12 is needed. [Nathan Gillett, Canada]	Accepted. We have now expanded this section on assessing the paleo estimate of ECS, and now the justification of the final assessed range is clearer.
14402	88	11			This subsection is of key importance and seems to be well explained given the lack of CMIP6 published work so far [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The section was removed.
18934	88	18	88	19	why not give the ECS definition here and mention that this is about equilibrium and not transient climate change [Gwenaelle GREMION, Canada]	Taken into account. The section was removed.
56142	88	20	88	20	incmplete reference? [Rolf Müller, Germany]	Taken into account. No revision made, reference appears to be complete.
36614	88	21	88	22	There are several most recent papers that deserve to be mentioned here, such as, Bellomo K, Clement AC, Murphy LN, Polvani LM, Cane MA (2016) New observational evidence for a positive cloud feedback that amplifies the Atlantic Multidecadal Oscillation. Geophys Res Lett 43(18):9852–9859 [Carlos Mechoso, United States of America]	Rejected. The utility of this type of study of variability pattern cloud feedbacks in constraining global climate change cloud feedback is limited.
18936	88	24	88	25	what is meant here with symmetric/asymmetric? [Gwenaelle GREMION, Canada]	Taken into account. The section was removed.
56144	88	26	88	26	the recovery of the ozone layer is more than >expected< : there is celaer evidence in the upper stratosphere and there is emerging evidence for a healing of the ozone hole. Make sure these statements are not in contradiction with WMO 2018 [Rolf Müller, Germany]	Rejected. The comment appears to be misplaced as the text pointed to does not deal with ozone.
25822	88	45	88	45	"occupy"is a strange verb here [Stephen E Schwartz, United States of America]	Accepted. Changed to "exhibit"
18520	88	46	88	46	"ensembles of opportunity" seems unclear to me, please clarify [Gwenaelle GREMION, Canada]	Taken into account. The section was removed.
18938	89	1	89	2	"Tuning of ECS" is tricky: tuning to the 20th century warming (as some modelling centers admitted to do) implies they tune the ECS? Since ECS is -F/lamda, and 20th century warming is ~(F/(kappa-lamda)) even if targetting F and lamda, doesnt the uncertainty in kappa provide some uncertainty too to ECS? The phrase "tuning of ECS" implies the models target certain values for surface temperature change DT after 2xCO2, thus ECS is not an emergent property but a predecided value, isnt this a risky statement for a policy making document? [Gwenaelle GREMION, Canada]	Rejected. This is a description of what some modelling centres do when they try to improve the match with the instrumental record. This can be done using ECS or aerosol forcing, and we are not aware of cases that use ocean heat uptake, or pattern effects which would be another option.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
15290	89	12	89	12	I'm not sure anything is yet published about the PPE performed with the Hadley model this time around but my recollection is that it was not effective in moving the ECS dial practically at all. This was performed and discussed within the UKCP18 project. I don't think there was a clear understanding of why the PPE this time around was not effective in exploring the ECS range. Maybe some author from the HC may be tapped into. [Claudia Tebaldi, United States of America]	Noted. The comment contains no suggestions.
36616	89	20	89	22	Are the ideas of Webb et al. (2015) explained in the following paragraph? [Carlos Mechoso, United States of America]	Taken into account. The section was removed.
18940	89	21	89	22	Webb et al. Should be explained more [Gwenaelle GREMION, Canada]	Taken into account. The section was removed.
36618	89	25	89	26	Even if the feedbacks were included, their importance may not be properly simulated. Here is a paper that talks about two models reacting very differently to the same perturbation, due to the different representation of feedbacks: Mechoso, C. R., T. Losada, S. Koseki, E. Mohino-Harris, N. Keenlyside, Belen Rodriguez-Fonseca, A. Castaño-Tierno, T. A. Myers, and T. Toniazzo, 2016: Can reducing the incoming energy flux over the Southern Ocean in a CGCM improve its simulation of tropical climate? Geophys. Res. Lett. 43, 11,057–11,063. [Carlos Mechoso, United States of America]	Rejected. Although this study is certainly interesting, it is not clear how the spatial structure to this idealised zonal band shortwave forcing experiment is relevant to ECS.
14404	89	30			suspect --> suspected [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
14406	89	36		39	I didn't understand these lines [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The section was removed.
27244	89	47	89	47	The 2 of CO2 is incorrectly written as an exponent instead of an indice. [François GERVAIS, France]	Not applicable. Table has been removed.
49152	89	47	90	1	Does this "radiative forcing" mean ERF? [Yu Kosaka, Japan]	Accepted. updated
56668	89	47			It is not immediatedly clear to me from the caption and the table, which "radiative forcing" is shown in the third column … and how comparable that is across CMIP3, CMIP5, and CMIP6, i.e. how exactly that was derived… [Malte Meinshausen, Australia]	Accepted. The radiative forcings shown in the table are not actual radiative forcing, rather intercepts of a linear fit. The table has been replaced with Table 7.SM.5 which does not contain estimates of ERF.
25824	90	1	90	1	Referring to table 7.13. Although results are available from only six CMIP6 models, even these six models show a strong correlation between radiative forcing and alpha, r^2 = 0.34, showing that the current CMIP models continue to compensate forcing and sensitivity. This should be noted and discussed. [Stephen E Schwartz, United States of America]	Taken into account. The forcing displayed in the table is half the intercept from a linear fit to an abrupt4xCO2 simulation. Supposedly, the reviewer refers to the compensation of forcing and sensitivity in a historical warming simulation, so does not apply here. The table was removed.
25826	90	1	90	1	Referring to table 7.13.There is some variation in F2x, evaluated as ECS*alpha across the several models. avg 3.4 ±1., W m-2, 1 std dev. The mean is well below the value given for effective forcing of doubled CO2 of 4.0 W m-2 and indeed the greatest of the values is 3.9 W m-2, to a low as . This might be discussed. [Stephen E Schwartz, United States of America]	Taken into account. The reason these numbers are lower than the assessed effective radiative forcing (ERF) from a doubling of CO2 is that they are intercepts from linear fits to an abrupt4xCO2 simulation. Many models exhibit a change towards weaker slope during the course of such simulations and therefore a simple linear fit leads to a lower (ERF). The table was removed.
33210	90	6	90	6	Suggest citing in this section the following recent studies evaluating published emergent constraints on ECS: Caldwell et al (2018) [doi:10.1175/JCLI-D-17-0631.1] and Qu et al (2018) [doi:10.1175/JCLI-D-17-0482.1]. [Mark Zelinka, United States of America]	Taken into account. These overview papers are now discussed.
49154	90	6	92	11	There is considerable overlap with Section 7.5.3.3. [Yu Kosaka, Japan]	Taken into account.
45578	90	6			Much material in this section repeats material in earlier sections. Some reorganisation is needed. [Steven Sherwood, Australia]	Accepted. The material on emergent constraints was reorganised.
58124	90	6			Some of the results assessed in this section are repetitive of previous sections. Several sections seem to discuss estimates based on fitting models to some sort of observational constraint. Try to reduce repetition. [Nathan Gillett, Canada]	Accepted. The material on emergent constraints was reorganised.
18522	90	8	90	10	The first sentence of the paragraph is really unclear to me. Please clarify what "...in diverse ways instead embrace model spread in ECS…" means. [Gwenaelle GREMION, Canada]	Taken into account. The statement has been revised.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
38810	90	8	92	16	I note that Lipat et al (2017; doi:10.1002/2017GL073151) is not mentioned anywhere in this section. It shows that only 9 out of 20 CMIP5 models have climatological SH Hadley Cell edge latitudes matching the observations within uncertainty, and that 8 of those 9 models have an ECS below 3 C (the 9th model having an ECS of 4.1 C). It also gives a solid physical explanation for this relationship. In 4×CO2-forced runs, models with excessively equatorward climatological HC extents produce stronger SW cloud radiative warming in the lower mid-latitude region and tend to have larger climate sensitivity values than models with more realistic climatological HC extents (Lipat et al 2017; doi: 10.1002/2017GL073151). Accordingly, it provides credible emergent constraint evidence for an ECS of no more than 3 C, notwithstanding that it does not formally estimate a constrained ECS range. [Nicholas Lewis, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The text was revised.
14408	90	8			These opening lines of section 7.5.6 were difficult to understand; is emergent contraint defined? [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The text was revised.
18528	90	20	90	25	Being not an expert in this field, it is not very clear to me how and equilibrium climate response can be inferred from volcanic eruptions, which seem to operate on a shorter time scale. Unless this is very obvious to most readers, I would suggest adding one short comment why this is justified. [Gwenaelle GREMION, Canada]	Rejected. An emergent constraint shows that there is a statistical relationship between the response and ECS.
18524	90	21	90	24	Is it justified to claim that the result of one single study leads to a "likely" range of ECS? Especially given that the next sentence says that the best estimate is higher in later studies and ECS from volcanic activity is low-biased? It seems incoherent to me [Gwenaelle GREMION, Canada]	Rejected. The comments seems to be misplaced and we can not identify what it refers to.
18526	90	24	90	25	It is not clear where the numbers in "Median ECS of 2.5-2.9°C" come from, since only two best estimates are referenced in the text above and those are 2.4°C and 2.7°C. Also "fairly tight uncertainty ranges" is not clear, particularly considering the possible biases discussed in the previous sentence. [Gwenaelle GREMION, Canada]	Taken into account. The text has been reorganised and rewritten.
58126	90	27	91	11	Repetitive of 7.5.3.3, last paragraph. Also the assessment in Section 7.5.3.3 concluded that approaches based on variability were not very robust. [Nathan Gillett, Canada]	Taken into account.
19306	90	28	90	28	Is the best estimate the median ? Perhaps remind at the beginning of the chapter what is the best estimate . [Gwenaelle GREMION, Canada]	Noted. Usually, yes, although different studies may apply different definitions.
18942	90	28	90	28	Explain here the fluctuation-dissipation theorem , even if briefly [Gwenaelle GREMION, Canada]	Rejected. This is not deemed necessary.
18530	90	30	90	30	The ECS estimate of 1.1°C and the range of 0.6°C to 1.7°C seem to contradict the statement on p. 89, line 40f "it es extremely likely that ECS is greater than 1.5°C". This should be discussed [Gwenaelle GREMION, Canada]	Noted. The study is referenced as background to understand the subsequent discussion.
15292	90	30	91	1	my recollection of this study is that it had a very serious shortcoming in its assumptions, namely the single time scale thing, and if it is still the received wisdom about this paper I think it should be stated, rather than presenting the result in this neutral way. [Claudia Tebaldi, United States of America]	Rejected. The text is not deemed neutral. The study is interesting in that the idea underpins more recent studies (Cox et al. 2018).
25828	90	30	91	1	The principal reason that Schwartz (2007) found a low ECS is that he did not account for heat transport from the mixed layer into the deep ocean; thus the sensitivity he determined is more akin to a TCS than to an ECS. This is discussed in Schwartz (2008) Schwartz SE (2008) Reply to comments by G. Foster et al., R. Knutti et al., and N. Scafetta on ‘‘Heat capacity, time constant, and sensitivity of Earth’s climate system’’. J Geophys Res 113:D15105. doi: 10.1029/2008JD009872 [Stephen E Schwartz, United States of America]	Rejected. The comment contains no concrete suggestions.
58128	90	54			Show the evidence supporting this assessed range. [Nathan Gillett, Canada]	Rejected. The comment refers to a non-existing line.
18532	91	1	91	2	please add citation to "recently it was proposed… giving a median ECS of 2.8°C…" [Gwenaelle GREMION, Canada]	Accepted.
18944	91	1	91	10	the paragraph in 1-10 is difficult to understand, and needs more details. [Gwenaelle GREMION, Canada]	Taken into account. The paragraph has been revised.
18534	91	3	91	7	The long sentence "A particular challenge… " seems obscure. Please improve clarity, possible by diving into several sentence which give enough room to each of the arguments being made. [Gwenaelle GREMION, Canada]	Taken into account. The paragraph has been revised.
18536	91	10	91	11	It is not clear, which "same filter" is applied. Maybe improve wording and use the term "filter" also above, when the filter is actually described. Also, if I understand correctly, the method leads "inevitably […] to an overestimated ECS […] " (see line 8f). Is the ECS of 3°C in line 11 also overerstimated? [Gwenaelle GREMION, Canada]	Taken into account. The paragraph has been revised.
36620	91	28			Also, Qu X, Hall A, Klein SA, Caldwell PM (2014) On the spread of changes in marine low cloud cover in climate model simulations of the 21st century. Clim Dyn 42(9):2603–2626 [Carlos Mechoso, United States of America]	Rejected. The study did not add new information to that in the other references.
33212	91	31	91	31	I'm confused how this value could be -0.2 yet earlier in the text (p. 60, line 13) it is stated that the anvil feedback is "likely small and negative, counteracting a small portion of the positive high-cloud altitude". Since the high cloud altitude feedback is +0.2, this would counteract it fully, not a small portion of it. [Mark Zelinka, United States of America]	Taken into account. The second order draft now makes an explicit assessment of this feedback.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
18540	91	54	91	54	Please explain, how the upper bound of 5°C is assessed (from table 7.14?) [Gwenaelle GREMION, Canada]	Taken into account. The explanation is now more explicit.
18948	92	10	92	11	Where is this range mentioned in the table 7.14? [Gwenaelle GREMION, Canada]	Rejected. The range is in the table.
18538	92	13	93	3	In table 7.14 (Brient et al., 2016) and (Brient and Schneider, 2016) give uncerrtainty ranges from 2.4°C or 2.3°C upwards. This seems to contradict the text passage on page 91, lines 24 - 35, where it says "only small probability values below 3°C.". Please clarify. [Gwenaelle GREMION, Canada]	Accepted. The text was revised to say "and in many cases provide only small".
18542	92	13	93	3	Not all studies mentionned in the main text of the chapter are found in the table, e.g.Schwartz, 2007, Po-Chedley et al., 2018b, Rydpal et al., 2018. [Gwenaelle GREMION, Canada]	Noted. These particular studies are included for context. The final table contains now only studies that are used in the assessment.
18946	92		92		Table 7.14: What is meant: model range consistent with observations? [Gwenaelle GREMION, Canada]	Taken into account. The text was revised.
30362	93	5	95	2	Great synthesis! Looking forward integrating this in the TCRE assessment in Chapter 5. [Joeri Rogelj, Austria]	Noted. Thanks.
53720	93	5	95	2	After heavy sections it was great to come to this useful synthesis. [Jan Fuglestvedt, Norway]	Noted. Thanks.
45582	93	5			This is a nice discussion and synthesis, which reaches a reasonable conclusion using straightforward qualitative reasoning. But it seems to imply a subjective assessment of the likely range, which is at odds with the later calculation of a pdf. [Steven Sherwood, Australia]	Rejected. There is no calculation of a pdf.
38812	93	7	93	11	It should be stated that both process-understanding and emergent constraint evidence, although not "derived directly from climate models", is heavily dependent on the accuracy of representation in complex models of processes that are critically important to ECS - including those relating to clouds, convection and circulation, all of which are known to have substantial shortcomings. The substantial adjustments made in this chapter to historical energy budget ECS/effective sensitivity estimates are also derived directly from complex models. Many paleoclimate ECS estimates are also heavily dependent on complex models. [Nicholas Lewis, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The text was revised.
38814	93	13	93	22	The lines of evidence referred to are not all nearly independent, in particular process-based and emergent constraint evidence are closely related. Central estimates of ECS derived from historical records of surface temperature and the Earth's energy imbalance and revised forcing estimates do not support a central ECS estimate of 3 K without a number of biased choices and unjustified adjustments. The increase in ECS found in GCMs in much warmer than preindustrial climates is irrelevant for assessing ECS, save perhaps if used to scaling down ECS estimates derived from past much warmer climates. [Nicholas Lewis, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The text was revised.
26884	93	21	93	22	"The assessed statements are summarized in Table 7.14 for ECS and Table 7.15 for TCR." The Table numbers appear to be incorrect. They are 7.15 for ECS and 7.16 for TCR. [Tomoo Ogura, Japan]	Accepted.
49156	93	21	93	22	References to Tables 7.14 and 7.15 should be 7.15 and 7.16, respectively. [Yu Kosaka, Japan]	Accepted
38816	93	24	93	34	The Bayesian method for combining evidence used in the cited studies has been shown to be invalid for producing even approximately correct confidence intervals: Lewis 2018 (Lewis, Nicholas. "Combining independent Bayesian posteriors into a confidence distribution, with application to estimating climate sensitivity." Journal of Statistical Planning and Inference 195 (2018): 80-92.) and Lewis and Grunwald 2018 (Lewis, Nicholas, and Peter Grünwald. "Objectively combining AR5 instrumental period and paleoclimate climate sensitivity evidence." Climate dynamics 50.5-6 (2018): 2199-2216.) Those two studies should be cited and their criticisms of the methods used in the two currently cited studies should be explained. If no one in the chapter 7 author team understands objective Bayesian methods than an external statistician who does so should be brought in. [Nicholas Lewis, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. The issues do not affect the approach taken here.
14822	93	24	93	43	I don't see where this Bayesian analysis – which underpins the overall assessment of ECS – is carried out or referenced. Therefore, there is a lack of traceability into the final assessment of ECS given at lines 36-43. The details of exactly how the Bayesian analysis is carried out – how multiple lines of evidence are combined and what priors are uses is presumably crucial to the overall final assessment yet is not clearly described or referenced. [Peter Stott, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. There is no formal Bayesian assessment carried out here, as described in the beginning of the section. The text was revised to make this clearer.
58130	93	24		27	As noted elsewhere, this generally relies on the lines of evidence being independent. [Nathan Gillett, Canada]	Rejected. As explained in the synthesis section the method used here does not rely on strict independence.
15294	93	27	93	30	What does "combined probability" mean here? THe only way I can think of interpreting it is through conditional probabilities. You are saying that P(ECS<1.5|A and B) is smaller than either conditional probabilities (conditioning on the individual evidences, here denoted by A and B) but I don't think that holds: P(E|A,B) (for short) = P(E,A,B)/(P(A)P(B)) which is smaller than P(E,A)/P(A) only if P(E,A|B) is smaller than P(E,A) (or put B in place of A, and the logic is the same). I don't think that is necessarily true. But maybe you mean something else with that combined probability... [Claudia Tebaldi, United States of America]	Taken into account. The text was revised to not confuse terms from statistics with the heuristic approach taken here.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
38818	93	36	93	43	These ECS ranges and the central estimate are unjustified and the high confidence level assigned to them is absurd. Also, it should be pointed out that ECS is far less relevant to warming over the next 200 or more years than is the effective sensitivity on that timescale, which is better constrained by observational evidence. [Nicholas Lewis, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. That effective ECS is more relevant for projections is strictly speaking not the theme here, but a new section (7.5.7) discusses this and many other related issues.
58132	93	38		40	How was this sythesis range derived exactly? This sythesis might benefit from a figure showing ECS ranges from individual studies, as in AR5. [Nathan Gillett, Canada]	Rejected. We do not wish to show a figure with all estimates as this would be counter to the philosophy of identifying and assessing multiple lines of evidence.
38820	93	45	94	8	It seems very possible that the claimed consensus of median ECS estimates [from different lines of evidence] is at least in part a reflection of group think, of national governments choosing IPCC authors who reflect "consensus" views and of choices made by those authors in all areas of this chapter as to what studies to cite and give a high weight to and which to ignore or downweight, and what adjustments to make to evidence from historical period energy budget estimates. In addition, it may well partly reflect decisions by researchers as to what it is likely to get published without difficulty in peer reviewed journals and what will or will not favour their career prospects. [Nicholas Lewis, United Kingdom (of Great Britain and Northern Ireland)]	Noted. This is the reason we explain that there could be group think.
53722	93	45	94	8	useful reflection here. Hope you keep it. [Jan Fuglestvedt, Norway]	Noted. Thanks.
15080	93	45	94	47	Perhaps it's also worthwhile noting that the perceived source of the groupthink mentioned is the consensus surrounding IPCC reports which are unavoidably biased by a conflict of interest where unless the ECS is as high as stated, the policy goals of the UNFCCC and by extension, the IPCC, become unsupportable. Something else to consider are the asymmetrically harsh political consequence of a low ECS. If this leads to a cover up whose motivation is to hide the scientific truth for political reasons, the consequences of exposure would be far worse and would negatively impact the veracity of legitimate science for decades to come. How political affiliation affects an individuals position on the science is highly inappropriate and reinforces the claims of groupthink by both sides. Much like religion, politics must not take sides of controversial science, nor should one side of controversial science court political support. Let the scientific method do what it's meant to do and accept the results, especially with regard to falsification. Whether or not science conforms to a specific political narrative must be irrelevant. [George White, United States of America]	Noted. Unclear what revision is requested here.
14410	93	45			I like the frank and philosophical discussion of potential implicit biases in the concensus on ECS [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Noted, thanks.
18544	94	11	94	16	The synthesis value of the likely range (2.5°C to 4.0°C) does not seem to relate to the only value given in this table, which is the likely range emergeing from process understanding (2.3 - 4.1 °C). It is not clear, why for the synthesis a tighter range was chosen. [Gwenaelle GREMION, Canada]	Taken into account. It is now explained that we use a 0.5C precision for ECS. Moreover, the other lines of evidence imply similar likely ranges, even if the literature did not allow us to assess these explicitly.
38822	94	11	94	16	In my view the very likely ranges given in Table 7.15, even if correctly derived, can reasonably only be regarded as likely ranges. I also strongly disagree with the absurd claim that warming over the instrumental record indicates that it is extremely likely that ECS exceeds 1.7 K. [Nicholas Lewis, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The comment that very likely should really mean likely relates to a legacy in previous IPCC reports wherein often extra uncertainty has been added. A more explicit discussion of where extra uncertainty has been added to the lines of evidence is in the updated draft.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
15082	94	13	94	14	Table 7.13: The maximum possible surface emissions sensitivity is 2 W/m^2 of surface emissions per W/m^2 of forcing corresponding to a sensitivity factor of 0.37 C per W/m^2, or an ECS of about 1.5C based on an ERF of 4 W/m^2 from doubling CO2. This unconditionally precludes any ECS greater than about 1.5C. The emissions sensitivity limit of 2 W/m^2 of surface emissions per W/m^2 of forcing is easier to grasp as an upper limit of 1 W/m^2 more per W/m^2 of forcing than an ideal BB whose emission sensitivity is 1 W/m^2 of surface emissions per W/m^2 of forcing. The extra W/m^2 of emissions can only be replaced by older surface emissions that were absorbed by the atmosphere and later returned to the surface. After some delay, the absorbed energy is evenly split between outer space contributing to the planets radiant balance and the surface contributing to its radiant balance as it replaces emissions beyond what can be replaced by solar power alone. Geometry dictates that the relative proportions of radiant energy absorbed by the atmosphere and ultimately emitted into space or sent back to the surface are roughly the same. The upper limit can only occur when the atmosphere absorbs 100% of the incremental surface emissions. Consider that 1 W/m^2 of solar ERF is contributing 2 W/m^2 to the surface emissions, all of which are absorbed by the atmosphere. In the steady state, half of this absorbed energy is emitted into space offsetting the W/m^2 of solar ERF while the remaining half is added to the W/m^2 of solar ERF to offset the additional W/m^2 emitted by the surface. To achieve more than 2 W/m^2 of surface emissions per W/m^2 of forcing literally requires creating energy out of thin air as the atmosphere will need to absorb more than 100% of the radiant emissions by the surface. [George White, United States of America]	Rejected. The surface perspective on climate sensitivity has long been abandoned, and for good physical reasons the top of the atmosphere is now used for energy balance. Moreover, there exists other feedback mechanisms than the emission of infrared radiation from the surface.
18482	94	13	94	15	This information in this table (Table 7.15) could be better represented as a bar chart figure, especially since one of the main discussion points of the summary paragraphs above it discusses how the asymmetry in ranges for low probability-high impact makes it challenging to rule out such a scenario. A figure representation would make those ranges more immediately apparent. [Gwenaelle GREMION, Canada]	Noted. It was considered to make a visual figure of this, but the idea was turned down.
9308	94	13	94	16	I suggest a line be added to this table, in order to recall the "likely" range quoted by AR5. Showing how this bracket has been narrowed illustrates in a concrete way the progress achieved; at the same time it shows there is still some work ahead. [philippe waldteufel, France]	Taken into account. AR5 assessed range was removed from the table.
25830	94	14	94	14	Synthesis in table: Is the difference in the likely range between the synthesis and the process due to some reasoning or just round-off? [Stephen E Schwartz, United States of America]	Taken into account. It is now explained that we use a 0.5C precision for ECS. Moreover, the other lines of evidence imply similar likely ranges, even if the literature did not allow us to assess these explicitly.
9620	94	14	94	15	It is unclear why the likely range is narrower than the very likely range? Perhaps a figure illustrating these ranges would be useful. [Katarzyna (Kasia) Tokarska, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. A likely range has to cover at least 66 percent probability, whereas a very likely range must cover at least 90 percent. Therefore a likely range can be narrower than a very likely range.
14824	94	14	94	15	With gaps in the table entries here I don't see how the Bayesian synthesis has been carried out. [Peter Stott, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. A Bayesian analysis has not been carried out.
18950	94	19	94	19	Shouldn't here in line 19 have a new section for TCR? Also remind here how TCR is defined (so that the reader does not go back to the beginning to search for definitions) [Gwenaelle GREMION, Canada]	Rejected. It is neither deemed necessary to start a new section, nor to define again TCR. We avoid having definitions in multiple places.
38824	94	21	94	23	This statement, like many in this chapter, is wrong. Pattern effects arising from internal variability, if they had occurred over the full historical period, would cause TCR estimates from the historical record to depart from the "true" forced transient climate response. [Nicholas Lewis, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The comment contains no concrete suggestions.
18952	94	22	94	23	what is meant here? This sentence doesn't seem to make sense to me . [Gwenaelle GREMION, Canada]	Taken into account. The likely range must be inside the very likely range, therefore a narrower range was chosen in the absence of likely ranges from all lines of evidence. The text was revised.
38826	94	25	95	2	Both emergent constraints and process understanding provide relatively little constraint on TCR, since they relate to feedbacks in near-equilibrium. It is notable that in AOGCMs TCrRcan be very much lower than ECS. The GFDL-ESM2 CMIP5 models have TCRs of only 1.1 K and 1.3 K according to IPCC AR5 WG1 Table 9.5 (although the true values appear to be slightly higher), but ECS values (from very long integrations) of marginally over 3 K. [Nicholas Lewis, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The comment contains no suggestions.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
18546	94	30	95	2	It is not clear, why the synthesis of the liekly range gives 2.0°C as an upper bound, while all values above have 2.2°C as upper bound. [Gwenaelle GREMION, Canada]	Taken into account. The likely range must be inside the very likely range, therefore a narrower range was chosen in the absence of likely ranges from all lines of evidence. The text and later the assessment was revised.
58134	94				Table 7.15. The justification for the upper bound of 5C for the paleo and emergent constraint ranges is not fully clear. [Nathan Gillett, Canada]	Taken into account. The relevant sections have been revised.
18954	95	7	95	7	Line 7 could be removed . I personally found the whole paragraph difficult to understand.It could be improved, by explaining for example more clearly how the 2 methods of calculating ECSs differ, since both methods use GCMs. What is meant with the last sentence in line 27? [Gwenaelle GREMION, Canada]	Taken into account. The text was merely a placeholder and has been rewritten.
38828	95	16	95	19	The Dessler study range quoted is for ECS estimates based on 10 year means and are the outliers from 100 runs. The 95% ECS range using 20 year means is a more realistic measure of uncertainty from internal variability in this model, and is much narrower: 2.7 - 3.5 K. That range should be quoted as well. [Nicholas Lewis, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. This text has been removed.
38830	95	24	95	27	The Andrews et al (2018) inferred ECS estimates are greatly different when using a more recent SST and sea-ice dataset, in the models for which its use was tested. No reliance at all should be put on the primary results of that study. [Nicholas Lewis, United Kingdom (of Great Britain and Northern Ireland)]	Noted.
52062	95	32			There is a lot of cross-talk between this section and several other chapters. As presently written from a cursory read there may be inconsistencies. Some careful though is warranted over this section in conjunction with several other chapters (at least 3, 4, 6 and possibly 8 and 9) [Peter Thorne, Ireland]	Noted. The comment contains no concrete suggestions.
14412	95	36			is this just paleorecord or also instrumental record? [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. This section has been moved up in the outline (now discussed before feedback dependence on SSTs). It compares models to both the instrumental and paleo record, so the text has been modified.
24488	95		105		Important and useful discussion but there needs to be more on how the content and conclusions of Chapter 7 relate to Chapter 3 on attribution, and also Chapter 2 on radiative forcing. [Joanna Haigh, United Kingdom (of Great Britain and Northern Ireland)]	Noted.
25832	96	13	96	13	" Warming contributions (units of C)"; this use of "warming" contradicts the definition at chapter 7,page 5 line 7: "total earth system heat content change" . What is the sense of a definition if you don't stick to it? [Stephen E Schwartz, United States of America]	Taken into account. 'Total earth system warming' is meant to convey changes to the global energy budget (Wm^-2), while warming here refers to surface warming. The text has been revised in introduction to clarify the difference and the text here has been revised to emphasize that this refers to surface warming only.
38832	96	19	96	22	It should be pointed out that radiative kernel methods are only approximate, with substantial residual errors often existing, and that results also vary according to the model used to derive the kernel. [Nicholas Lewis, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. The residual you are referring to is shown explicitly in the figures. It is not negligible and thus is shown for completeness, but does not affect this assessment. Likewise, the choice of kernel (e.g., based on CAM3 for results shown in Goose et al. 2018 vs ECHAM for results shown in Pithan and Mauritsen 2014) do not substantially modify the conclusions discussed here.
44822	96	25			Figures 7.26 and 7.27: define what "residual" means. [Astrid Kiendler-Scharr, Germany]	Rejected. Not deemed necessary.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
38834	96	42	96	44	The spread in CO2 radiative forcing in CMIP5 models is almost 2 to 1 (Chung and Soden 2015), so there must be compensating errors if the overall contribution of forcing to the CMIP5 ECS range is small. [Nicholas Lewis, United Kingdom (of Great Britain and Northern Ireland)]	Noted. This article highlight the importance of having a correct method to estimate the radiative forcing, otherwise the spread of the forcing is large. This paper confirms that feedbacks are the main source of uncertainty: "As noted in previous studies (e.g., Dufresne and Bony 2008; Andrews et al. 2012; Forster et al. 2013; Vial et al. 2013), intermodel differences in climate sensitivity are dominated by uncertainties in the feedback processes in the troposphere rather than uncertainties in direct radiative forcing, rapid radiative adjustments, or heat uptake."
44824	96	44	96	47	It is not obvious from the figure that "the inter-model differences in cloud feedbacks alone is about three times larger….", more details and numbers needed. [Astrid Kiendler-Scharr, Germany]	Not applicable. This text has been modified.
49158	97	12	97	12	Are thee "emulators" the same as the two-layer EBM in Section 7.5.2.2? [Yu Kosaka, Japan]	Taken into account. The text has been revised to clarify.
18548	97	17	97	20	The sentence "Another emulator resolves …" is very diffucult to comprehend, especially the part with "mimicking the effect of the pattern effect". To me it is not clear, what the pattern effect is and why it is important in that context [Gwenaelle GREMION, Canada]	Taken into account. This text has been revised to clarify.
25834	97	22	97	22	"variance"; To what does this variance refer? Is it variance among models? And if so, is the variance perhaps underestimated because the ratio of aerosol and ghg forcing is maintained in the model studies? [Stephen E Schwartz, United States of America]	Taken into account. Yes, this refers to variance among models. Text added to clarify, and to note that this is for a 1%/yr CO2 ramping scenario, rather than for realistic forcing scenarios for which forcing differences would be important.
18550	97	36	97	37	What is GFDL? [Gwenaelle GREMION, Canada]	Nor applicable. This text has been removed.
25836	97	50	97	50	"Uncertainty in radiative forcing plays an important but generally secondary role." This statement would hold only if the ratio of aerosol and ghg forcings were maintained constant. If that ratio changes, in particular if aerosol forcing decreases rapidly with decrease in emissions whereas ghg forcing remains relatively constant, if aerosol forcing is large, and consequently climate sensitivity is high, there could be a substantial resultant increase in global temperature; the present uncertainty in aerosol forcing therefore contributes substantially to potential future change in GMST. [Stephen E Schwartz, United States of America]	Taken into account. The text has been modified to clarify that this is only for increasing or stabilizing emissions scenarios.
25840	98	1	98	1	Do you mean "warming" here in the sense defined at Chapter 7, page 5 line 7? maybe better "temperature increase" [Stephen E Schwartz, United States of America]	Taken into account. This refers to surface warming. Text revised to clarify.
25838	98	1	98	3	The point here is not "The spread in historical warming across GCMs" as stated. Actually the models all do a fairly good job of capturing historical increase in GMST. The point is rather that a consequence of the models' being able to do this with a spread of sensitivities compensated by a spread in forcings is that if the mix of forcers (ghg, aerosol) changes in the future, as it must, the models will greatly diverge. [Stephen E Schwartz, United States of America]	Taken into account. This text has been revised in the FGD.
8620	98	3	98	3	In Lembo et al. 2017 we analysed the zonal mean patterns of energy budgets anomalies at TOA and at the surface, and the implied anomalies in meridional heat transports, with prescribed SST-SIC and radiative forcing ECHAM5-HAM historical simulations. We studied the sensitivity experiments with SST-SIC changes and aerosol forcing on and off, finding that the energy asymmetry triggers energy transports across the Equator, that are unequivocally related to aerosol forcing, and may have positive feedback on NH surface warming. Lembo, V., Folini, D., Wild, M. et al. Clim Dyn (2017) 48: 1793. https://doi.org/10.1007/s00382-016-3173-9 [Valerio Lembo, Germany]	Noted. This section assesses contributions to uncertainty in global mean warming rather than energy transports.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
25842	98	5	98	7	"dominate the uncertainty" hardly captures the sense of the findings and importance of these papers. The key finding is that within present uncertainty of aerosol forcing and transient sensitivity, taking into account the coupling between these quantities, the reduction of anthro aerosol forcing might lead to substantial (up to 1 K in the limit of cessation) increase in GMST in a decade, a major component of so called "committed warming"; better, committed temperature increase. This so-called "Faustian Bargain" (Hansen and Lacis, 1990; should cite) is a potential time bomb lurking behind any strategy to reduce emissions from fossil fuel combustion. Hansen, J. E. and A. A. Lacis, Sun and dust versus greenhouse gases: An assessment of their relative roles in global climate change, Nature, 346, 713-719, 1990. [Stephen E Schwartz, United States of America]	Taken into account. The text has been modified to point to Chapter 4 which assesses the zero emissions climate commitment.
25844	98	7	98	7	Add Xu and Ramanathan (2016); Schwartz (2018); Schwartz, S. E. (2018). Unrealized global temperature increase: Implications of current uncertainties. J. Geophys. Res. Atmospheres, 123, 3462–3482. https://doi.org/10.1002/ 2017JD028121 Xu, Y. and Ramanathan, V., 2017. Well below 2 C: Mitigation strategies for avoiding dangerous to catastrophic climate changes. Proceedings of the National Academy of Sciences, 114(39), pp.10315-10323. [Stephen E Schwartz, United States of America]	Taken into account. Reference to Schwartz (2018) added.
14414	98	8			uncertainty in [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
25846	98	11	98	14	Do you mean "warming" here in the sense defined at Chapter 7, page 5 line 7? maybe better "temperature increase" [Stephen E Schwartz, United States of America]	Taken into account. This refers to surface warming. The text has been revised to clarify.
25848	98	13	98	13	Not just aerosol ERF; also transient sensitivity, because these two quantities coupled. [Stephen E Schwartz, United States of America]	Rejected. This is a subtle point and possibly not true for CMIP5 and CMIP6, where aerosol ERF and feedbacks appear to be less-well correlated than for CMIP3 (e.g., Forster et al. 2013 for CMIP5). This text is meant to convey that although feedback uncertainty dominates transient warming uncertainty, ERF uncertainty becomes important for strong emissions reductions. This text has thus been left as is as it serves this point.
49160	98	14	98	15	The importance of carbon cycle in far future is assessed is outside of the Chapter 7's scope and instead citation of Chapter 5. It is thus strange that Chapter 7 has an assessment statement on importance of carbon cycle in uncertainty of far future climate. [Yu Kosaka, Japan]	Taken into account. The text has been modified to point to Chapter 5's assessment.
6259	98	27	98	27	General: Regional energy consumption pattern also should be considered.(Ref. Jafari, M., Smith, P., (2018). Climate Change as a Driving Force on Urban Energy Consumption Patterns. In Encyclopedia of Information Science and Technology (4th ed., pp. 7815-7830). IGI Global. https://doi.org/10.4018/978-1-5225-2255-3.ch680 [Mostafa Jafari, Iran]	Rejected. This confuses what we mean by energy budget, it is clarified that we do not talk about societies use of energy within the chapter. This is covered in WG3
56692	98	32	100	6	Additional recent references on polar amplification include Kim et al. (2019) in Scientific Reports Volume 9(1): 1184 on "Vertical Feedback Mechanism of Winter Arctic Amplification and Sea Ice Loss." [Kilkis Siir, Turkey]	Taken into account. Reference added.
58136	98	32	101	37	Chapter 4 has subsections on polar amplification assessing much of the same literature in 4.5.1.1, which isn't referenced here. Agree with Chapter 4 where these will go, and remove repetition. [Nathan Gillett, Canada]	Taken into account. References to chapter 4 added. Chapter 4 gives a broad overview of polar amplification and discusses the role of non-CO2 forcing agents, while Chapter 7 discusses detailed mechanisms driving polar amplification under CO2 forcing.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
40758	98	32			Section 7.6.2.1 general. A very informative section, but I came away with a different conceptual interpretation. I envisage a forcing versus feedbacks framework. The large and positive local radiative feedbacks in the Arctic act to warm the Arctic more than other latitudes (a forcing for temperature differential). Increased latent heat transport also forces polar amplification. This amplification is limited either by more thermal emission from the Arctic or by more sensible heat transport (feedbacks). Viewing it this way helps me understand the apparent contradiction between atmospheric transport not contributing to amplification (page 99 line 19) and amplification occurring in the absence of differential forcing (page 100 paragraph starting line 14). It isn't that the atmospheric transport doesn't contribute, it is more that there is a (probably fortuitous) cancellation between the amplification forcing due to latent heat transport and the amplification feedback due to sensible heat transport. [Daniel Murphy, United States of America]	Taken into account. This is consistent with what we were attempting to describe here. The text has been revised to clarify, borrowing some of your suggested framing.
36622	98				Section 7.6.2.1. The role of polar amplification on the modes of variability is discussed in Chapter 4. Are these cross-references considered here? [Carlos Mechoso, United States of America]	Taken into account. References to Chapter 4 added.
11666	99	1	99	34	In this section, the reason and the seasonality of Arctic amplification should also consider the aerosol effect. As shown in some studies (e.g., Garrett and Zhao , 2006; Zhao and Garrett, 2015; Chen et al., 2018), aerosols played a considerable role on the cloud and hence impact the radiative forcing and seasonality of Actic amplification. Garrett, T. J., & Zhao, C. (2006). Increased Arctic cloud longwave emissivity associated with pollution from mid-latitudes. Nature. Zhao, C., & Garrett, T. J. (2015). Effects of Arctic haze on surface cloud radiative forcing. Geophysical Research Letters. Chen, Yuyang, Chuanfeng Zhao* and Yi Ming (2019), Potential impacts of Arctic warming on Northern Hemisphere mid-latitude aerosol optical depth, Climate Dynamics [Chuanfeng Zhao, China]	Rejected. This section considers mechanisms of polar amplification under CO2 forcing only. Chapters 3 and 4 discusses polar amplification under more realistic forcing scenarios where these effects will become important, so that is where these studies may be cited.
31440	99	9	99	21	Can the effect of boundary layer structure be neglected here ? (Bintanja et al., Nature Geoscience 2011) [Gerhard Krinner, France]	Rejected. The boundary layer structure already included in lapse-rate feedback in this framework, and is thus included in the energy budget analysis, if not explicitly.
14416	99	17			This appears to contradict the later discussion on p.100, 14-17. I thought Siler et al. (2018b) found moist static energy transport is enhanced and contributes toward polar amplification in addition to feedbacks? [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Without large positive Arctic feedbacks, heat transport increases into the Arctic (Siler, Armour papers) contributing to polar amplification. But with realistic feedbacks, heat transport stays roughly constant at 70N in CMIP5 models owing to large cancellation between moist and dry components. Text added to clarify and make sure of no contradictions.
40760	99	18	99	22	"little of no role for changes in atmospheric heat transport in climate models". What about ocean heat transport? See also my comment for page 98. [Daniel Murphy, United States of America]	Taken into account. Yes, ocean heat transport is thought to play a role as well. See text below this.
12528	99	23	99	34	Also changes in anthropogenic aerosol forcing and associated changes in heat transport can contribute to Arctic amplification (e.g. Navarro et al., 2016). See also Chapter 3 (section 3.4.1.1). Navarro, J. C. A., Varma, V., Riipinen, I., Seland, Ø., Kirkevåg, A., Struthers, H., Iversen, T., Hansson, H.-C., and Ekman, A. M. L.: Amplification of Arctic warming by past air pollution reductions in Europe, Nat. Geosci., 9, 277–281, https://doi.org/10.1038/ngeo2673, 2016. [David Neubauer, Switzerland]	Rejected. This section considers mechanisms of polar amplification under CO2 forcing only. Chapter 3 and 4 discusses polar amplification under more realistic forcing scenarios where these effects will become important, so that is where these studies may be cited.
14418	99	33			Although the Planck fundtion is weaker at the poles, this is modulated by lower atmosphere opacity and surface net LW flux response to warming is generally stronger than in the tropics where downward LW can increase at a faster rate than the surface upward LW due to increases atmospheric water vapour continuum emission so it's worth double checking this discussion [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Noted. We believe this discussion is an accurate reflection of the mechanisms discussed in Pithan & Mauritsen, and other cited papers. In these analyses, the Planck response is separated from the water vapor feedbacks. While Planck contributes to polar amplification, water vapor contributes to tropical amplification.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
18956	99	40	99	46	If I understand correctly, there is more polar amplification in the Arctic because , even if there is ocean heat uptake (as in the Southern Ocean, which stalls SH amplified warming) this is compensated by increased northward heat transport in the Arctic? I think it is very condense sentence here, as it summarizes different regions (subpolar North Atlantic, where ocean northward heat transport -OHT- is expected to decrease) with the Arctic, where the OHT into the Arctic is expected to increase. These concepts need to be described better and over more detail so as to be better understood. [Gwenaelle GREMION, Canada]	Taken into account. The text has been revised to clarify link between ocean heat uptake and ocean heat transport.
36624	99				Section 7.6.2,1. Possible additional references. Årthun, M, Eldevik, T., Smedsrud, L. H. (2019). The role of Atlantic heat transport in future Arctic winter sea ice variability and predictability. Journal of Climate, 32, 3327–3341. Årthun, M., Eldevik, T., Smedsrud, L. H., Skagseth, Ø., Ingvaldsen, R. B. (2012). Quantifying the influence of Atlantic heat on Barents Sea ice variability and retreat. Journal of Climate, 25, 4736–4743, doi:10.1175/JCLI-D-11-00466.1. Ding, Q., Schweiger, A., L'Heureux, M., Steig ,E. J., Battisti, D. S., Johnson, N. C., Blanchard - Wrigglesworth, E., Po-Chedley, S., Zhang, Q., Harnos, K., Bushuk, M., Markle, B., Baxter, I. (2019). Fingerprints of internal drivers of Arctic sea ice loss in observations and model simulations. Nature Geoscience, 12(1), 28-33. [Carlos Mechoso, United States of America]	Rejected. These references were considered, but they would fit better within Chapter 2,3 and 9, which discuss observed changes in Arctic sea ice, variability, and mechanisms governing ocean heat transport. This discussion of polar amplification is meant to focus on large-scale factors influencing polar amplification under CO2 forcing.
43200	100	5	100	5	Include in the references here thae invesigation of Luo, B.et al., 2017: Atmospheric circulation patterns which promote winter Arctic sea ice decline. Environmental Research Letters, 12, 054017, doi: 10.1088/1748-9326/aa69d0. [Ian Simmonds, Australia]	Taken into account. Reference added.
33214	100	12	100	12	Citation should be Zelinka and Hartmann (2012) [DOI: 10.1175/JCLI-D-11-00096.1] [Mark Zelinka, United States of America]	Accepted.
28202	100	38	100	41	Boeke and Taylor 2018 (Nature Communications: https://doi.org/10.1038/s41467-018-07061-9) is highly relevant to this discussion. [Chad Thackeray, United States of America]	Taken into account. Reference added.
18552	100	38	100	44	Two sentences at the beginning and the end of this paragraph seem to contradict each other. 1) sea ice thinning in early winter increases heat flux from ocean to atmosphere and leads to strong surface warming. 2) Sea ice melting stabilizes temperatures in summer because it uses excess energy. Maybe the first sentence should read "Sea ice growth in early winter..." ? [Gwenaelle GREMION, Canada]	Taken into account. This text has been revised to clarify.
18554	100	49	100	49	Could you explain, which process leads to the continuation of Arctic amplification, similar as for the emergence of SH polar amplification. [Gwenaelle GREMION, Canada]	Taken into account. What was meant here was that warming will continue to be larger in the Arctic (relative to tropics or global average) in the future. This text has been revised to clarify.
13206	101	4	102	45	A paragraph should be included that discusses possible changes in tropical sea-surface temperature gradients in the North Atlantic and in the Indian Ocean. [Nora Richter, United States of America]	Rejected. We choose to leave a detailed discussion of these regions to Chapter 9 who assess changes in sea-surface temperatures in different ocean basins. The reason we focus on the tropical Pacific and Southern Ocean is their key relevance for actuating radiative feedbacks (the pattern effect) that is relevant for estimates of ECS from global energy budget constraints.
53724	101	9	101	9	I think this section needs some coordination with ch4 and probably also chap 10. [Jan Fuglestvedt, Norway]	Not applicable. This subsection has been removed due to space constraints and overlap with Chapter 4.
14420	101	9			There could be a link made to 8.2.1.2 which mentions the effect of land ocean warming contrast on hydrological responses [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. Good idea, but this subsection has been removed due to space constraints and overlap with Chapter 4.
44826	101	29	101	31	Check sentence grammar. [Astrid Kiendler-Scharr, Germany]	Editorial
36626	101	34	101	37	This section is very clear and informative. [Carlos Mechoso, United States of America]	Noted. Thank you.
40762	101	40			Section 7.6.2.3 A good example of a section with informative physical reasoning. Good work in many places. [Daniel Murphy, United States of America]	Noted. Thank you!

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
49162	102	26	102	40	The underrepresentation of the Pacific decadal variability and its potential association with Atlantic bias are discussed in Chapter 3 Section 3.7.6. Also please explicitly mention potential role of internal variability. [Yu Kosaka, Japan]	Taken into account. Reference to Chapter 3 added, and text revised to discuss this.
39402	102	34	102	36	Decreasing trend of TCs over the Indochina Peninsula was showed. The trend corresponded to the rainfall trend. Ref. Takahashi, H.G., and T. Yasunari, 2008: Decreasing trend in rainfall over Indochina during the late summer monsoon: Impact of tropical cyclones. J. Meteor. Soc. Japan, 86, 429-438, doi:10.2151/jmsj.86.429. Takahashi, H.G., T. Yoshikane, M. Hara and T. Yasunari, 2009: High-resolution regional climate simulations of the long-term decrease in September rainfall over Indochina, Atmos. Sci. Lett., 10, 14-18, doi:10.1002/asl.203. Takahashi, H.G., 2011: Long-term changes in rainfall and tropical cyclone activity over South and Southeast Asia. Adv. Geosci., 30, 17-22, doi:10.5194/adgeo-30-17-2011. [Hiroshi Takahashi, Japan]	Rejected. Due to space constraints, we must maintain a chapter focused on Earth's energy budget and radiative feedbacks and are not able to add a discussion of these studies. They may be a better fit for other chapters where these topics are discussed.
36628	102	37	102	39	This conjecture is particularly weak due to the possible influence of decadal variability in the modes, and GCMs have serious errors in the tropics that affect the SST gradient. The following paragraph seems to upgrade this to “medium confidence”? [Carlos Mechoso, United States of America]	Taken into account. Text revised to clarify that the low confidence statement refers to recent observations while the medium confidence statement below refers to an assessment based on multiple lines of evidence, such as the proxy record.
38836	102	37	102	40	Are you using strengthened to mean weakened, as you do when talking about negative feedbacks? [Nicholas Lewis, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Typo fixed.
49164	102	38	102	38	"strengthened" should be "weakened". [Yu Kosaka, Japan]	Taken into account. Typo fixed.
52064	102	43	102	45	On the face of it this finding clashes with those of chapters two through 4 with regard to observed, modelled and projected El Nino behaviour. Some reconciliation would appear warranted here. [Peter Thorne, Ireland]	Taken into account. The text has been revised to clarify that this refers to long-term trends in SSTs, rather than changes in ENSO amplitude or variability which is assessed in Chapter 4.
18606	102	50	102	50	I would write “Paleoclimate observations can provide evidence” instead of “Paleoclimate data can provide observational evidence”. I think this would help to make clear the distinction between the two types of paleoclimate data : paleoclimate observations (proxy) and paleoclimate model simulations. [Gwenaelle GREMION, Canada]	Accepted. Reworded.
18608	102	50	103	1	I think it is important to make more clear the distinction between the two types of paleoclimate data (observations and models) and what they are used for when studying climate warming: -Paleoclimate observations provide constraints on patterns of warming, and these constraints are useful to evaluate the performance of paleoclimate models. -Paleoclimate model simulations provide information about the underlying mechanisms which explain the patterns of warming. Particularly, I think you can merge the two sentences from lines 52 (p102) – 1 (p103) into one. You mention how paleoclimate models can help us understand “warming in response to high CO2 and other forcings” and also “the possible mechanisms that led to these patterns of warming”, but you can actually mention that in on sentence. For instance: "Paleoclimate model simulations provide information about patterns of surface warming that result from the climate system response to forcings such as CO2, as well as the mechanisms that led to these patterns of warming. [Gwenaelle GREMION, Canada]	Accepted. Reworded.
27106	102	50	103	12	This section (7.6.3) addresses palaeoclimates which are relevant to conditions similar to today, and future global warming scenarios. There is a promise also of additional content related to the LGM. But I suggest that instead (or in addition to) this LGM content, there should be at least a mention of studies of the Eemian where temperatures and sea levels were a little higher than pre-industrial levels, but CO2 levels were about the same. This is an important time period as it representst the last interglacial period before humans had an influence on the climate system. Nice examples of studies which integrate modelling and palaeoclimate records are Salonen et al., (2018) Nature Comms DOI: 10.1038/s41467-018-05314-1 and Plach et al., (2018) Clim. Past, 14, 1463–1485, 2018 [Chris Satow, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. Here we focus on paleo time periods which are different from modern due to CO2. The Eemian is orbitally-forced compared with mdoern. Some Eemian model-data comaprsons are included in Chapter 3.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
15084	102	50	103	12	The paleoclimate record includes ice cores like Vostok and DomeC where the unambiguous relationship between CO2 levels and temperature is that CO2 changes follow temperature changes by centuries (about 800 years for Vostok and about 250 years for DomeC) and not the other way around as often implied. This much delay can only be explained by biology adapting to temperature changes and the varying amount of the surface suitable for plant life as surface ice ebbs and flows. The more biomass there is, the more CO2 is required, the more biomass is decomposing and the more CO2 and CH4 will be present in the atmosphere. I've repeated the correlation analysis establishing the temporal relationship between temperature and CO2 concentrations via multiple methods, moreover; the delay between temperature and CH4 is consistent with the delay between temperature and CO2 changes which is another strong indicator that decomposition plays an important role relative to the CO2 and CH4 concentrations observed in the ice cores. The longer delay in the Vostok data is a consequence of a very low temporal resolution of CO2 concentrations which got much better in the analysis of the DomeC cores. [George White, United States of America]	Rejected. Here we are focusing on time periods where greenhouse gas forcing is the primary driver of climate change. For the ice-core record, orbital changes are a significant component of the forcing. Furthermore, higher-resolution ice core records now indicate a tighter coupling between CO2 and d18O. Beeman et al, 2019, Climate of the Past.
15086	102	50	103	12	There's overwhelming evidence that the signatures of cycles related to the variable tilt of the Earth, the precession of perihelion and the ellipticity of Earth's orbit are all present in the ice cores and other paleoclimate reconstructions. Exactly how orbital dynamics effects the climate is not widely understood, but that it's primarily responsible for the cycles of glaciation and warmth is undeniable. Whatever effect CO2 variability has on the temperature is insignificant by comparison. This is clear in this plot of ice core data smoothed to 22K years in order to cancel out the 22K cycle of change related to the precession of perihelion. The data shows the influence of changes in the Earth's tilt and changes in the ellipticity of the Earth's orbit. http://www.palisad.com/co2/ic/orbit.png The correlation gets fuzzier the further back in time owing to the high uncertainty in the time line relative to the bags of ice core samples. Note that DomeC was processed with more modern techniques and shows better correlation further back in time than Vostok. The 22K year smoothing averages out the effects from the precession of perihelion whose peak to peak magnitude of effect is also dependent on both the axial tilt and the ellipticity of the orbit. [George White, United States of America]	Rejected. On the timescales considered here, CO2 is a greater forcing than orbital variability. And the Pliocene timeslice of KM5C used in PlioMIP is especially chosen because it has orbital characteristics similar to modern.
15006	102	51	102	52	since there are comments here on emerging papers for PlioMIP and DeepMIP papers on climate sensitivity, it may be worth being aware that a MPWP synthesis paper by the PlioVAR working group for the same KM5c interval as PlioMIP is in prep and should be submitted summer 2019. [Erin McClymont, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Paper cited in SOD
19070	102	54	102	54	using "can provide insights" plus "possible mechanisms" seems very low confident, consider eliminating can or possible [Gwenaelle GREMION, Canada]	Accepted. Reworded.
18610	103	1	103	6	There have been several “Greenhouse” periods during the history of the Earth. Why do you mention the MPWP and the EECO in particular? I think it would also be interesting to mention the Paleocene-Eocene Thermal Maximum (PETM) (55.5-55.3 Ma), which is included in Annex II. This “Greenhouse” period was caused by a massive release of CO2 to the atmosphere. The global average temperature reached 23 degrees and there were no ice caps in the poles. What is interesting about this period is that a recent study showed that the current rate of CO2 emission from anthropogenic sources is between 9 to 10 times higher than the rate of greenhouse gas emission during the PETM. If the emission of CO2 from anthropogenic sources keeps on increasing, in 140 years the total amount of CO2 emitted by humans could be as high as the total amount emitted during the PETM. Reference : Gingerich, P.D. 2019. Temporal Scaling of Carbon Emission and Accumulation Rates: Modern Anthropogenic Emissions Compared to Estimates of PETM Onset Accumulation. Paleoceanography and Paleoclimatology, vol 34, issue 3. [Gwenaelle GREMION, Canada]	Accepted. Now clearer that we choose these the time periods because they have been the focus of international efforts.
18612	103	1	103	9	Before naming the warm periods of interest, it would be better to explain why it is interesting to analyse those periods. I would write something like this: “In this context, there has been a focus on past global warming periods caracterised by CO2 concentrations that are higher than pre-industrial levels, such as the mid-Pliocene warm period (...) and the early Eocene climatic optimum (...). The paleoclimate data corresponding to these periods provides constraints on patterns of warming that result from CO2 forcing. Two patterns of warming that have been analyzed by several studies are the meridional and longitudinal temperature gradients”. [Gwenaelle GREMION, Canada]	Accepted. Changed the order of this introductory section.
18614	103	7	103	9	When you mention polar amplification in brackets, it seems that “polar amplification” refers to “ long-term changes in meridional temperature gradients”, which is not the right definition of polar amplification. [Gwenaelle GREMION, Canada]	Accepted. Reworded.
18616	103	17	103	23	You mention that, for the estimates of temperature, there were uncertainties associated with proxy calibrations. What are the uncertainties that were associated with proxy data for estimating CO2 concentrations? I would include a sentence about it at the very beginning of the paragraph, because I think it’s more logic to talk first about the uncertainty of proxies for estimating forcing (e.g. CO2 from ice cores) and then about the uncertainty of proxies for estimating response (e.g. temperature from Mg/Ca). Then, as you did, you can mention if the results of climate model simulations agreed with both types of proxies or not. [Gwenaelle GREMION, Canada]	Accepted. I think that the order is OK here, but we include some discussion of uncertinties in forcing as well as temperature..
18634	103	20	103	21	You put as reference the paper of Winguth et al. (2010), but this paper is about the PETM (55.5-55.3 Ma, according to Annex II), not about the EECO (52-50 Ma, according to Annex II). You also put the reference of the paper of Hubert and Caballero (2011), but it seems to be about the early Eocene in general (∼56–48 Ma), and not just about the EECO. [Gwenaelle GREMION, Canada]	Accepted. Yes, made this more general to "early Eocene"

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
18618	103	25	103	29	I would join together the first sentence and the first half of the second sentence because both refer to proxies for estimating temperature (if you don't do that then you'll have to specify that when you say "reconstructions of the MPWP" you mean "temperature reconstructions of the MPWP"). The second part of the second sentence should be a sentence on its own because it refers to proxies for estimating CO2. [Gwenaelle GREMION, Canada]	Accepted . Reworded.
18620	103	29	103	31	The phrase "as such, the dregree of polar amplification (...)" should be one sentence on its own. I would also change some words to make more clear the relation between forcing (CO2) and response (polar amplification). In concrete, I would write something like this: "Consequently, we can better quantify the degree of polar amplification resulting from high CO2 concentrations during these past time periods. In turn, the ability of the climate models to reproduce polar amplification can be better assessed. [Gwenaelle GREMION, Canada]	Accepted. Reworded.
18736	103	43	103	46	What is the reference for this sentence? [Gwenaelle GREMION, Canada]	Accepted. PlioMIP modelling added.
18622	104	1	104	1	It's more adequate to use the word "reproduced" instead of "predicted". [Gwenaelle GREMION, Canada]	Accepted. Reworded.
18626	104	3	104	3	I would write "thus improving agreement" instead of "and improve agreement", because the improvement in the agreement between models and proxy data is due to the changes made to model parameters. [Gwenaelle GREMION, Canada]	Rejected. It is "and", because some of the improved agreement is due to changes in climate sensitivity, not just polar amplification.
18624	104	3	104	4	When you say "data" (line 4), which data are you referring to? Simulation (model) output is also data, so when you say "agreement between models and data", do you mean "agreement between models and proxy data"? [Gwenaelle GREMION, Canada]	Accepted. Reworded.
18636	104	4	104	4	The paper of Kiehl and Shields (2013) is about the PETM, not the EECO. The paper of Sagoo et al. 2013 seems to be about the Early Eocene in general (~56–48 Ma), and not just about the EECO (52-50 Ma, according to Annex II). [Gwenaelle GREMION, Canada]	Accepted. Yes, made this more general to "early Eocene" where appropriate.
18630	104	5	104	8	Where is the reference for this statement ? [Gwenaelle GREMION, Canada]	Accepted. Added Zhu et al (2019), plus the DeepMIP paper.
19072	104	8	104	8	need some reference here [Gwenaelle GREMION, Canada]	Accepted. Added Zhu et al (2019), plus the DeepMIP paper.
19074	104	8	104	8	The quality of the figure is really low, cannot read labels, numbers, legend… [Gwenaelle GREMION, Canada]	Accepted. This problem occurred during the compilation. The actual figures have a better resolution and the final report will not have this issue anymore.
18628	104	10	104	10	It would be more clear to say "equator-to-pole temperature gradients" rather than "meridional temperature gradients" [Gwenaelle GREMION, Canada]	Accepted. Reworded.
19076	104	12	104	12	Some reference is needed for "which are more consistent with the proxies...". [Gwenaelle GREMION, Canada]	Accepted. Added Lunt et al (2019); Haywood et al (2019; in prep)
43762	104	22	104	27	UK37 index should go with capital K. Eventhough in some papers it appears in regular k, it was originally defined in capital K. The index was originally developped in Bristol, UK, I guess this is why both U and K went in capital. The first deffinition of the index is: Brassell, S.C., Eglinton, G., Marlowe, I.T., Pflaumann, U., Sarnthein, M., 1986. Molecular stratigraphy: a new tool for climatic assessment. Nature 320, 129-133. Please check that this is changed in the whole IPCC report. [Carles Pelejero, Spain]	Taken into account - this section has been substantialyl re-written in the FGD so comment is no longer relevant.
19078	104	26	104	35	Haywood et al. (2016) would be a good reference here: Haywood, A. M., Dowsett, H. J., & Dolan, A. M. (2016). Integrating geological archives and climate models for the mid-Pliocene warm period. Nature communications, 7, 10646. [Gwenaelle GREMION, Canada]	Taken into account - this section has been substantialyl re-written so no longer relevant.
18646	104	38			Section 7.6.4 : You have not included the information about land-ocean warming contrast (section 7.6.2.2), particularly the fact that "there is high confidence that near-surface air temperature over land will, on average, increase by more than that over the oceans in both the transient and equilibrium response to greenhouse gas forcing". [Gwenaelle GREMION, Canada]	Not applicable. This comment is no longer applicable in the new structure of this section.
18638	104	44	104	54	When talking about the level of evidence it's written twice : "The paleoclimate proxy record of past warm climates, GCM simulations of those past climates, and GCM projections of climate response to CO2 forcing provides". Couldn't you mention just once in the first sentence that all the conclusions about level of confidence in this section are based on those types of data? [Gwenaelle GREMION, Canada]	Rejected. These are important summary statements that may be pulled through to the Exec Summary, so it is important that they stand alone.
9310	104	47	104	49	"has yet emerged" is certainly not what you mean to say. [philippe waldteufel, France]	Accepted. Reworded.
36630	104	51	104	54	Has it been suggested that the sign of the gradient could actually reverse in paleo time scales? [Carlos Mechoso, United States of America]	Noted. Not as far as I am aware.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
18640	105	2	105	9	In p.105, when you talk about levels of confidence, it's not clear which types of evidence the conclusions are based on (proxy data, observational data during the instrument period, past climate simulations, climate projections) [Gwenaelle GREMION, Canada]	Noted. Confidence comes from knowledge of the gas lifetimes and the forcing-response relationship. The same basic result will fall out of any plausible equation or climate model, as long as it preserves the lifetime of the forcing agents appropriately.
18642	105	5	105	7	I think it is important to specify that "the observed strengthening of the east-west SST gradient (..) will eventually give way to a weakening of the gradient on centennial timescales" UNDER greenhouse gas forcing (mentionned in section 7.6.2.3). [Gwenaelle GREMION, Canada]	Not applicable. Wrong section.
18644	105	7	105	9	This sentence should go in paragraph from lines 44-49 (p.104), where you talk about polar amplification. Since it actually overlaps some of the statements made in that paragraph, you could merge them. [Gwenaelle GREMION, Canada]	Taken into account. This text has been revised and moved to a section on polar amplification.
25850	105	7	113	16	it seems essential to have graph of the various potentials of CO2 as a function of time. the impulse response function; the AGWP, the AGTP etc. These graphs should indicate best value, used for normalizing the potentials of other substances, and also cumulative uncertainty, the latter to give the user a sense of accuracy. I propose absolute quantities for all other substances as well, and the normalized quantities if there is a reason for this (historical or user demand), but I am greatly concerned over the implied accuracy of these quantities if simply given without uncertainties. [Stephen E Schwartz, United States of America]	Taken into account. Uncertainties are given in section 7.6.1 of the FGD in several places. Table 7.15 of the FGD now gives overall uncertainties in the emission metrics due to uncertainties in radiative efficiencies, lifetimes and the climate response function.
53770	105	12	105	12	I notice that no values are given for BC, OC, NOx and CO. With new ERFs in the literature, the metric values could also be updated. An assessment of utility and robustness would be useful. [Jan Fuglestvedt, Norway]	Taken into account. See 7.3.4.3 and Ch. 6 for forcing updates. However, metrics for BC, OC, Nox and CO have not been covered in 7.6 due to space limitations and limited new relevant literature to assess.
53772	105	12	105	12	As far as I can see, the indiret effects on trop O3 and strat H2O form methane are just breifly mentioned in section 7.7. What about lifetime changes? [Jan Fuglestvedt, Norway]	Taken into account.This is now assessed in Section 7.6.1.3.
47576	105	12	105	12	In my view, there are five missing topics that need to be covered in this metric section 7.7 as an update since AR5. Below is a list of such topics with relevant recent references. I assume that papers related to economics (e.g. costs of using suboptimal metrics; the interpretation of discount rate in terms of metric time horizon) will be covered by WG3, so they are not included in the list below. 1) Metric application for life cycle impact assessments Cherubini et al. (2016, Environmental Science & Policy, doi:10.1016/j.envsci.2016.06.019). Cherubini and Tanaka (2016, Environ. Sci. Technol., doi:10.1021/acs.est.6b05343). Levasseur et al. (2016, Ecol. Indicators, doi:10.1016/j.ecolind.2016.06.049). Levasseur et al. in Global Guidance for Life Cycle Impact Assessment Indicators Vol. 1 (eds R. Frischknecht & O. Jolliet) Ch. 3, 59-75 (UNEP, 2016). Mallapragada and Mignone (2017, Environ. Res. Lett., doi:10.1088/1748-9326/aa7397). 2) Multi-metric approach (and also multi-basket approach) Ocko et al. (2017, Science, doi:10.1126/science.aaj2350). Fesenfeld et al. (2018, Nature Climate Change, doi:10.1038/s41558-018-0328-1). Reisinger et al. (2017, Ecol. Indicators, doi:10.1016/j.ecolind.2017.04.026). Tanaka et al. (2019, Nature Climate Change doi:10.1038/s41558-019-0457-1). 3) The use of metrics to support the Paris Agreement targets Fuglestvedt et al. (2018, Phil. Trans. R. Soc. A., doi:10.1098/rsta.2016.0445). Tanaka and O'Neill (2018, Nature Climate Change, doi:10.1038/s41558-018-0097-x). 4) Emission metric of H2O Sherwood et al. (2018, Environ. Res. Lett., doi:10.1088/1748-9326/aae018). 5) Metrics accounting for regional dimensions Aamaas et al. (2016, Atmos. Chem. Phys., doi:10.5194/acp-16-7451-2016). Aamaas et al. (2017, Atmos. Chem. Phys., doi:10.5194/acp-17-10795-2017). Tanaka et al. (2019, Nature Climate Change doi:10.1038/s41558-019-0457-1). [Katsumasa Tanaka, Japan]	Taken into account. Thank you for the suggestions and references. We have added material on these.
25852	105	12	105	12	Not to "evaluate" emissions; better to "compare climate effects of emissions of different substances" [Stephen E Schwartz, United States of America]	Taken into account. Thank you. Text amended accordingly.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
25854	105	12	105	12	"exchange rates". Implies some sort of transfer between compartments, which is not intended. better "compare climate effects of emissions of different substances". [Stephen E Schwartz, United States of America]	Taken into account. It has precedent in previous AR but we have removed the phrase "exchange rates".
49542	105	12	105	48	Significant part of the introductory section 7.7.1 refers to the matrics use in policy discussions and I think it should also include a reference to for example Pierrehumbert (2014) paper quoted further on page 109, line 4-6) that point to weaknesses and potential for for missinterpretation of resutls when using particular metrices. At the same time, the post AR5 advances in that matter and development of new metrices can be mentioned right away; the discussion of details shall stay of course it belongs in further sections of this chapter. [Zbigniew Klimont, Austria]	Taken into account. Thank you for the suggestion. We have edited this section, retained a reference to Pierrehumbert (2014) and then expanded on this in 7.6.2.
53776	105	12	113	16	Coordination with chapter 6 authors is needed. They write about the substances that introduces challenges to the metric issue (short lifetimes, heterogeneous responses etc) [Jan Fuglestvedt, Norway]	Accepted.
44306	105	12	113	16	This section presents an interesting discussion of physical metrics for climate, which is good, but I feel it would be much more valuable (and true to the IPCC goal of being policy-relevant) if it considered the link between policies that affect climate and the broader world. In SR1.5 we put a great deal of effort into showing these kinds of linkages, in particular between climate-related policies and SDGs. For example, large scale deployment of BECCS can lead to severe pressure on land and water resources, whereas shifting away from coal (and fossil fuels more generally) has enormous public health benefits. I realize AR6 is back to being divided by WG, but to my mind it'd be unhelpful to lose the links betwe4en climate and SDGs. There are in fact metrics that include such links, as discussed in SR1.5, and I think this section would benefit from including those as well. [Drew Shindell, United States of America]	Accepted. It is important for WGI to be able to present the new science aspects, but it also important to show how new and old metrics can be used in different ways. As the chapter evolves we will try to point to linkages with other Chapters and with WGII and WGIII, which are often better places to discuss co-benefits.
53778	105	12	113	16	Coordination with chapter 5 authors is needed in order to obtain consistency and avoid repetitions. [Jan Fuglestvedt, Norway]	Accepted. See Box 7.3
53726	105	12	113	16	Section 7.1. is an important section for connecting knowledge on climate effects of the individual gases to development of climate policies. I think the section contains the main elements that are needed, but could perhaps benefit from a stricter structure around choice of impact parameter (RF, dT, SLR, damage), treatment of the time dimension (integral vs "snapshot"), global vs regional dimension on both driver and response side. You may also consider relating metric type to UNFCCC goal, Kyoto, Paris; that may help the reader to understand the relation between choice of metric and goal of climate policies. [Jan Fuglestvedt, Norway]	Taken into account. Thank you. We have attempted to reduce redundancy and sharpen this section.
18740	105	12			Section 7.7 : If it is possible to change the structure of this section, I would organise it as follows: 7.7.1 "Introduction to metrics": definition, types and what they are for (the current section 7.7.1 doesn't really talk about innovations since IPCC AR5, so the title of this section should me modified). 7.7.2 "Physical description of metrics" - General explanation of how they are calculated - Summary of key findings in AR5 - Summary of new key findings 7.7.2.1 "Radiative efficiency" - Why the knowledge of radiative efficiency is important for emission metrics - New radiative efficiencies of the most relevant GHGs and how they have been obtained 7.7.2.2 "Emission metrics" - Definition of the two main types of metrics ("instantaneous or endpoint values" metrics and "integrated" metrics), main differences between them, and what each metric is useful for. - "Instantaneous or endpoint values" metrics: definition of each metrics (AGTP, etc.), its equation, units, and what it is used for. - "Integrated" metrics: definition of each metrics, its equation, units, and what it is used for. - Ratio of two metrics (which would include the information that is in he current section 7.7.2.4) : definition, what they are for, how they are computed. 7.7.2. 3 "Emission metrics by species" : This section is built from the knowledge of radiative efficiencies (from section 7.7.2.1) and the equations to compute emission metrics (section 7.7.2.2). For each relevant species, it should be succintly explained how the new emission metrics (instantaneous metrics, integrated metrics, ratio of two metrics) was obtained, and, if possible, the degree of confidence of the results [Gwenaelle GREMION, Canada]	Taken into account. The structure has been revisited and amended.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
18742	105	12			Section 7.7: I think it is important to be consistent in the use of words and expressions, and to make clear what the meaning of each concept is. For instance: - Climate metrics vs emission metrics: The distinction is not clear cut, but it's important to use the same name for the same type of metrics that are mentioned in this section. - The text includes the terms "effective radiative efficiency", "radiative efficiency" and "effective radiative forcing". Do they mean the same thing? If so, it would be better to use just one of the expressions throughout all the text. Other two details I would like to point out are these: - Global temperature change potential (GTP): There's no hyphen between "temperature" and "change" (Shine et al. 2005). The accronym GTP should first appear in line 52 of page 106, which is where this metrics first appears. There is no need to mention what GTP means again in line 7 of p.109. - AGTPxs, AGTPx : To be consistent, the "x" should be either a subscript or a superscript in all equations. [Gwenaelle GREMION, Canada]	Taken into account. We have tried to improve terminological consistency and added words to the WGI Glossary.
56672	105	12			Maybe use consistently "emission metrics" instead of "metrics" for clarity. [Malte Meinshausen, Australia]	Accepted.
33414	105	12			Overall chapter 7.7 comment: too much focus on the "scientifically perfect metric", too little focus on "what's good enough to get the job done". E.g., GWP* is a great scientific tool, but is unlikely to be practical for use in a GHG trading protocol. Two-basket approaches are also interesting, but make it difficult to discuss the total climate impact of, say, a given year's national emissions. I think it would be great to actually have the chapter start with the premise that the 100 year GWP is the standard approach being used, discuss how changes since AR5 lead to changes in 100 year GWP estimates, and review briefly some of the key caveats and simplifications. Then have a separate section for all the other metrics, in which the strengths of each metric can be discussed (e.g., the GWP* for being the best representation of temperature impact) along with challenges (e.g., the lack of practicality in comparing single pulses to constant emission streams) [Marcus Sarofim, United States of America]	Rejected. The mandate of WGI is the Physical Science Basis, so the main aim here is to review developments in metrics from the physical science perspectives. Since the main development in this area has been the development of step-pulse metrics, this is where we have concentrated.
33416	105	12			Add a citation to Balcombe et al. (https://pubs.rsc.org/en/content/articlelanding/2018/em/c8em00414e#!divAbstract) as a review of metrics & timescales. [Marcus Sarofim, United States of America]	Suggestion noted.
53740	105	14	105	48	I think section 7.7.1. also could mention the use of metric for calculation net zero or grennehouse gas balance. For a multi-gas GHG balance as described in The Paris agreement, metrics are needed and will have an impact on how the balance is achieved and its effects. Three relevant papers: Tanaka and O'Neill, NCC, 2018; Allen et al, NCC 2016 and Fuglestvedt et al, 2018. [Jan Fuglestvedt, Norway]	Taken into account. Thank you, we now discuss balance.
18664	105	14			Section 7.7.1 In general, it’s somewhat hard to follow the basic ideas covered by this section, and some sentences are repetitive. Moreover, several references are quite old, so it would be a good idea to add newer references. [Gwenaelle GREMION, Canada]	Agreed. The section has been heavily edited.
18648	105	16	105	16	Greenhouse gases are forcing agents, so it would be better to say “forcing agents such as greenhouse gases” rather than “gases and forcing agents” [Gwenaelle GREMION, Canada]	Taken into account. Text amended.
53728	105	16	105	17	The first sentence is a bit uncelar; it does not relate emission metrics to emission. (As formulated it also works for RF). I suggest you make it more clear that metrcis are for measuring a response to a unit of emission. [Jan Fuglestvedt, Norway]	Taken into account. Changes made to clarify this here (7.7) and in the subsequent section on matching metrics and policy goals (7.7.2) and Box.
18652	105	16	105	28	I think these two paragraphs are, in a sense, repeating each other. And, in my opinion, they are not very clear. It would be a good idea to write just one paragraph that answers these questions : What are emission metrics? How do they work? What are they for? [Gwenaelle GREMION, Canada]	Taken into account. This section has been heavily edited, and some redundancy eliminated.
33404	105	16	105	48	This introduction does not highlight what I consider as perhaps the most important driver for use of metrics, which is to take advantage of "what" flexibility in policy design. E.g., Böhringer, C., Löschel, A., and Rutherford, T. F.: Efficiency gains from “what”-flexibility in climate policy an integrated CGE assessment, Energ. J., 0, 405–424, 2006. [Marcus Sarofim, United States of America]	Not applicable. This is probably a level of detail beyond WGI's domain. It's probably enough for WGI purposes to point out that metrics can be value for a range of purposes, including trading, and that this leads to allocative efficiency gains. The rank-ordering of the contributions to those efficiency gains is probably best left to WGIII (if they want to explore these).

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
47578	105	20	105	20	A chapter of the previous IPCC Assessment Report is cited. In principle, I believe that the IPCC Assessment Report is a secondary source because it is aimed to serve as a summary of current literature. If possible, I think that some primarily sources (i.e. original peer-reviewed literature) should be considered, although I understand that it may be difficult to find a primary source for a relatively general statement like this. [Katsumasa Tanaka, Japan]	Noted. It is common practice to summarise the state of undertstanding in the previous AR by referring to statements from that AR, and in each new assessment primarily cite recent litterature that revises that understanding.
53730	105	20	105	21	I dont tink readers will understand "imperfect summary of the "exchange rates". I hope you can try to make it more clear. [Jan Fuglestvedt, Norway]	Taken into account. "Exchange rates" replaced in Chapter.
49544	105	23	105	23	Figure 7.2; in the block " Emissions of greenhouse gases" the formula for nitrous oxide (N2O) is incorectly written as "N2o" [Zbigniew Klimont, Austria]	Editorial
19080	105	23	105	35	Consider referncing Cherubini et al. (2016): Cherubini, F., Fuglestvedt, J., Gasser, T., Reisinger, A., Cavalett, O., Huijbregts, M. A., ... & Strømman, A. H. (2016). Bridging the gap between impact assessment methods and climate science. Environmental Science & Policy, 64, 129-140. [Gwenaelle GREMION, Canada]	Taken into account. We have added a range of possible metrics applications in 7.7.1 and expanded on these, including examples, in 7.7.3.
13476	105	26	105	26	Expand GWP, GWP*, GTP and MGTP [Govindasamy Bala, India]	Accepted.
53732	105	26	105	26	the metrics mentioned here are not defined, and not shown in the fig [Jan Fuglestvedt, Norway]	Taken into account. We have expanded the acronyms on first use. See section 7,6,1
18654	105	30	105	32	The first and second sentence mean more or less the same thing. You could say the same thing using just one sentence, for instance: " Emission metrics are a valuable tool for climate policy decision-making since they facilitate the comparison between the effects of different forcing agents". [Gwenaelle GREMION, Canada]	Taken into account. The section has been amended.
56670	105	30	105	35	This is a good paragraph and would ideally inform how IPCC AR6 presents metrics, i.e. not as one being superior to the other (as the Exec summary seems to say), but as different metrics being finetuned for different goals (or mixtures of goals)… [Malte Meinshausen, Australia]	Taken into account. Thank you. We have attempted to write more on this issue in a nonprescriptive way in Box7.3, where we explain that some metrics are more suited to some purposes than others. The relationship between physical emissions metrics and economic variables was explored in some depth in AR5. In this report we have focused more on the relationship between physical emissions metrics and physical climate variables.
53734	105	30	105	35	useful para. (You may change "suggest" to "help prioritize") [Jan Fuglestvedt, Norway]	Taken into account. Thank you. Text changed.
18656	105	32	105	35	The sentence "The most appropriate metric..." is repetitive. You can make it shorter. Moreover, I don't understand the sentence, What do you mean by "which aspects of climate change are most important to a particular application"? Could you give an example? [Gwenaelle GREMION, Canada]	Taken into account. We have revised the section. We don't give examples in this section, but go into more depth in Box 7.3.
43630	105	36			It would be helpful to provide further clarity and examples of different policy goals. E.g. a cost-benefit approach would need to weigh gases based on the climate damages caused by emissions of one unit mass of each gas, whereas a cost-effectiveness approach (where a long-term target has been set externally) would need to weigh gases based on their effect on the specific exogenous targets. Based on Tol et al (2012), GWP is an example of the former (but relies on additional assumptions/judgements), and dynamic GTP an example of the latter if the exogenous target is to limit peak temperatures (but relies on assumption when those peak temperatures will occur). This provides a useful bridge to other metrics as flagged on page 106 lines 1-4 and the fact that any metric choice has implications for the overall cost at which a given climate change outcome will be achieved. [Andy Reisinger, New Zealand]	Rejected. Much of this material was traversed in AR5. We would prefer to point to that material rather than to rehash it here.
53736	105	37	105	37	Yes, important to stress that many of the challenegs are avoided (or lifted to a different level in policymaking) if we don't aggregate. [Jan Fuglestvedt, Norway]	Accepted.
18658	105	37	105	38	You wrote that "While metrics can provide a useful way of comparing the effects of different gases, they may not be required if gases or forcing agents are treated separately", so it seems to me that you're saying that emission metrics are always relative (i.e. we are comparing the effect of forcing agents in relation to the effect of a particular forcing agent, which is CO2 normally). Nevertheless, in lines 18-19 you wrote that "Although absolute metrics exist, most metrics are usually relative", so it is confusing. [Gwenaelle GREMION, Canada]	Noted. Metrics are optional - people are not required to choose one. That depends on policy choices. Metrics are also usually relative to CO2.

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12878	105	37	105	48	SLCP's contribute to the rate of warming, which is important for particularly vulnerable areas like the Arctic and the speed with which we approach tipping points and self-reinforcing feedbacks. Fast mitigation of SLCPs yields fast results, avoiding 0.6ºC of warming by mid-century and 1.2ºC of warming by 2100; comparatively, avoided warming of CO2 at 2100 is 1.6ºC if CO2 emissions peak at 2030 and 1.9ºC if CO2 emissions peak at 2020. Xu Y. & Ramanathan V. (2017) Well below 2 °C: Mitigation strategies for avoiding dangerous to catastrophic climate changes, PROC. NAT'L. ACAD. SCI. 114(39):10315–10323; Report of the Committee to Prevent Extreme Climate Change (Chairs: V. Ramanathan, M. L. Molina, and D. Zaelke) (2017) Well Under 2 Degrees Celsius: Fast Action Policies to Protect People and the Planet from Extreme Climate Change; Haines et al (2017) "Short-lived climate pollutant mitigation and the Sustainable Development Goals"; Molina M., et al. (2009) Reducing abrupt climate change risk using the Montreal Protocol and other regulatory actions to complement cuts in CO2 emissions, PROC. NAT'L. ACAD. SCI. 106(49):20616–20621, 20616; Arctic Monitoring and Assessment Programme (AMAP) (2017) SNOW, WATER, ICE, AND PERMAFROST IN THE ARCTIC: SUMMARY FOR POLICYMAKERS, 8 ("The Arctic is still a cold place, but it is warming faster than any other region on Earth. Over the past 50 years, the Arctic's temperature has risen by more than twice the global average. Increasing concentrations of greenhouse gases in the atmosphere are the primary underlying cause: the heat trapped by greenhouse gases triggers a cascade of feedbacks that collectively amplify Arctic warming."). [Durwood Zaelke, United States of America]	Noted. SLCPs and LLCPs both contribute to the warming rate, with the growth in LLCPs being the more important term since 1980 (e.g. figure 8.20 in AR5). Metrics such as GWP100, GWP20 and GTP100 do not capture the effect of SLCPs on warming rates when emissions are falling, whereas the new metrics (CGTP, GWP*) that preserve the distinction between stock and flow pollution do capture the appropriate effects on warming rates as well as warming.
52066	105	37	105	48	It feels strange to have all this discussion and not refer to either chapter 5 or chapter 6. [Peter Thorne, Ireland]	Taken into account - we try to blend where relevant.
42348	105	37	105	48	SLCP's contribute to the rate of warming, which is important for particularly vulnerable areas like the Arctic and the speed with which we approach tipping points and self-reinforcing feedbacks. Fast mitigation of SLCPs yields fast results, avoiding 0.6ºC of warming by mid-century and 1.2ºC of warming by 2100; comparatively, avoided warming of CO2 at 2100 is 1.6ºC if CO2 emissions peak at 2030 and 1.9ºC if CO2 emissions peak at 2020. Xu Y. & Ramanathan V. (2017) Well below 2 °C: Mitigation strategies for avoiding dangerous to catastrophic climate changes, PROC. NAT'L. ACAD. SCI. 114(39):10315–10323; Report of the Committee to Prevent Extreme Climate Change (Chairs: V. Ramanathan, M. L. Molina, and D. Zaelke) (2017) Well Under 2 Degrees Celsius: Fast Action Policies to Protect People and the Planet from Extreme Climate Change; Haines et al (2017) "Short-lived climate pollutant mitigation and the Sustainable Development Goals"; Molina M., et al. (2009) Reducing abrupt climate change risk using the Montreal Protocol and other regulatory actions to complement cuts in CO2 emissions, PROC. NAT'L. ACAD. SCI. 106(49):20616–20621, 20616; Arctic Monitoring and Assessment Programme (AMAP) (2017) SNOW, WATER, ICE, AND PERMAFROST IN THE ARCTIC: SUMMARY FOR POLICYMAKERS, 8 ("The Arctic is still a cold place, but it is warming faster than any other region on Earth. Over the past 50 years, the Arctic's temperature has risen by more than twice the global average. Increasing concentrations of greenhouse gases in the atmosphere are the primary underlying cause: the heat trapped by greenhouse gases triggers a cascade of feedbacks that collectively amplify Arctic warming."). [Gabrielle Dreyfus, United States of America]	Noted. SLCPs and LLCPs both contribute to the warming rate, with the growth in LLCPs being the more important term since 1980 (e.g. figure 8.20 in AR5). Metrics such as GWP100, GWP20 and GTP100 do not capture the effect of SLCPs on warming rates when emissions are falling, whereas the new metrics (CGTP, GWP*) that preserve the distinction between stock and flow pollution do capture the appropriate effects on warming rates as well as warming. (Same comment as 1492.)
43628	105	37	105	48	This paragraph is not wrong but it needs to recognise that multi-basket approaches simply shift the issue of metric, or more broadly, how much weight to place on emissions of different gases, elsewhere. In a single-basket policy, there is usually one (more or less clearly) defined target, and the policy needs a metric to decide how much of each gas to abate in order to meet the target. A multi-basket policy does not need a metric to compare emissions across the different baskets, but policymakers need to decide somehow how to set targets for the different baskets. This involves the same value judgements about near- and long-term impacts, time horizons and climate commitments as emission metrics. It is therefore wrong to give the impression, as I feel the paragrah does, that the use of multiple emission baskets somehow makes the problems inherent in emission metrics go away. The paragraph should recognise this clearly and explicitly. For a discussion of this in a national context, see e.g. https://motu.nz/assets/Documents/our-work/environment-and-agriculture/climate-change-mitigation/emissions-trading/2018-09-21-Submission-from-Leining-Kerr-and-Winchester-Improvements-to-the-New-Zealand-Emissions-Trading-Scheme-Final.pdf, pages 24-26 [Andy Reisinger, New Zealand]	Noted. Agree it is important that policymakers are aware of the relevant trade-offs and make decisions in the light of the best available science. And we agree that the same value judgements are in play irrespective of the number of baskets. We have extensively rewritten parts of this section.
33406	105	37	105	48	I acknowledge a number of arguments for a two-basket approach (Jackson 2009, and Daniel 2012, but also Smith 2012, https://doi.org/10.1038/NCLIMATE1496, and Sarofim 2007, Chapter 2 "Methane Policy: An Integrated Approach argues against GWP based trading" in https://globalchange.mit.edu/publication/13758). However... [Marcus Sarofim, United States of America]	Noted. The point here is that there are options, and that metrics are not required for all purposes. We think this is important to state.

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12708	105	37	105	48	SLCP's contribute to the rate of warming, which is important for particularly vulnerable areas like the Arctic and the speed with which we approach tipping points and self-reinforcing feedbacks. Fast mitigation of SLCPs yields fast results, avoiding 0.6ºC of warming by mid-century and 1.2ºC of warming by 2100; comparatively, avoided warming of CO2 at 2100 is 1.6ºC if CO2 emissions peak at 2030 and 1.9ºC if CO2 emissions peak at 2020. Xu Y. & Ramanathan V. (2017) Well below 2 °C: Mitigation strategies for avoiding dangerous to catastrophic climate changes, PROC. NAT'L. ACAD. SCI. 114(39):10315–10323; Report of the Committee to Prevent Extreme Climate Change (Chairs: V. Ramanathan, M. L. Molina, and D. Zaelke) (2017) Well Under 2 Degrees Celsius: Fast Action Policies to Protect People and the Planet from Extreme Climate Change; Haines et al (2017) "Short-lived climate pollutant mitigation and the Sustainable Development Goals"; Molina M., et al. (2009) Reducing abrupt climate change risk using the Montreal Protocol and other regulatory actions to complement cuts in CO2 emissions, PROC. NAT'L. ACAD. SCI. 106(49):20616–20621, 20616; Arctic Monitoring and Assessment Programme (AMAP) (2017) SNOW, WATER, ICE, AND PERMAFROST IN THE ARCTIC: SUMMARY FOR POLICYMAKERS, 8 ("The Arctic is still a cold place, but it is warming faster than any other region on Earth. Over the past 50 years, the Arctic's temperature has risen by more than twice the global average. Increasing concentrations of greenhouse gases in the atmosphere are the primary underlying cause: the heat trapped by greenhouse gases triggers a cascade of feedbacks that collectively amplify Arctic warming."). [Kristin Campbell, United States of America]	Noted. SLCPs and LLCPs both contribute to the warming rate, with the growth in LLCPs being the more important term since 1980 (e.g. figure 8.20 in AR5). Metrics such as GWP100, GWP20 and GTP100 do not capture the effect of SLCPs on warming rates when emissions are falling, whereas the new metrics (CGTP, GWP*) that preserve the distinction between stock and flow pollution do capture the appropriate effects on warming rates as well as warming. (Same comment as 1492.)
47580	105	37	105	48	The discussion in this paragraph needs updating because there have been related advances in the literature since AR5. A multi-metric approach, which is different from multi-basket approach, has been proposed (Ocko et al., 2017, Science, doi:10.1126/science.aaj2350; Fesenfeld et al. 2018, Nature Climate Change, doi:10.1038/s41558-018-0328-1). But there are disagreements over which combination of metrics should be used (Tanaka et al., 2019, Nature Climate Change doi:10.1038/s41558-019-0457-1). Furthermore, the Tanaka paper compared multi-metric and multi-basket approaches and showed that a multi-basket approach may lead to a misleading outcome if it is applied to climate impact assessment. [Katsumasa Tanaka, Japan]	Taken into account. Thank you for these references. We have attempted to incorporate some of these points into the text.
47582	105	37	105	48	This paragraph may fit better in Section 7.7.3. [Katsumasa Tanaka, Japan]	Not applicable. We have revised and simplified this section.
18660	105	37	105	48	It would be a good idea to make clear that in this paragraph you are talking about climate policy-making, and to explain more clearly what the single-basket, two-basket and multi-basket approaches are and what they are they used for. For instance, in a conference paper, Tanaka and Cherubini (2015), succintly explain the one and the two-basket approaches. The reference is: Tanaka and Cherubini (2015). Global and regional temperature metrics under a two-basket approach. Geophysical Research Abstracts, vol. 17. Available at : https://meetingorganizer.copernicus.org/EGU2015/EGU2015-10032.pdf Unfortunately, I am not an expert in this subject, so I cannot give you other useful references. [Gwenaelle GREMION, Canada]	Taken into account. We have revised the section.
18650	105	38	105	38	Greenhouse gases are forcing agents, so it would be better to say "forcing agents such as greenhouse gases" rather than "gases and forcing agents" [Gwenaelle GREMION, Canada]	Taken into account. Text amended.
47584	105	38	105	38	The meaning for "if gases or forcing agents are treated separately" did not come very clear to me when I first read it. The terminology "command-and-control approach" may help clarify the sentence. [Katsumasa Tanaka, Japan]	Rejected. It's not necessarily a "command and control" approach to treat gases separately - gases could simply be priced separately, or some regulated and some priced. We think the current text is adequate here.
53738	105	40	105	40	You may add a ref to an old paper suggeting this; Fuglestvedt et al 2000, GRL; and Daniell et al 2012, Climatic Change [Jan Fuglestvedt, Norway]	Thank you. We have revised this section extensively.
44828	105	40	105	43	Only primary species listed here - what about secondary (e.g. ozone)? [Astrid Kiendler-Scharr, Germany]	Noted. This section is largely introductory, so we have attempted to keep it relatively simple.
18662	105	40	105	43	It would be more accurate to mention the order of magnitude of the lifetime of the different gases (see WG1 AR5 report): 10 for CH4... 100 for N2O... 1000 for SF6... 10000 for CF4... (For CO2 it's not clear) If the reader understands that gases have different lifetimes, then it will be easier to understand the need for the multi-basket approach. [Gwenaelle GREMION, Canada]	Rejected. There is a table of species with this information in the appendix.

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49546	105	45	105	46	Maybe an explicit reference to the Kyoto Protocol can be made here when singe-basket application is mentioned [Zbigniew Klimont, Austria]	Taken into account. We have amended the text to reflect that climate policy has single- and multi-basket precedents.
18666	105	53	106	7	The sentence in lines 53-54 (p.105) and the sentence in lines 6-7 are very similar : they mention again what the emission metrics are. [Gwenaelle GREMION, Canada]	Taken into account. Thank you. We have tried to address several instances of low level redundancy in the text in this section.
25856	106	1	106	1	"equate emisions"; this sort of jargon does not belong here. better "compare the climate effects" [Stephen E Schwartz, United States of America]	Accepted: This has been changed to "on the same scale"
47586	106	3	106	4	Papers that have originally proposed metric ideas should also be cited. For example, Manne and Richels (2001, Nature, doi:10.1038/3507054) is an important paper first proposing cost-effective metrics. [Katsumasa Tanaka, Japan]	Rejected: This reference does not add additional information
47588	106	3	106	4	If these economic metrics are discussed in AR6 WGIII report, it should be stated so here. It is perhaps better placed earlier in Section 7.7 to clarify the structure and scope of this subsection. [Katsumasa Tanaka, Japan]	Accepted: This has been moved
53742	106	4	106	4	It was only covered in WGIII chapter 3 - not 4. But there was also a box in the WGIII TS. [Jan Fuglestvedt, Norway]	Accepted, wording changed.
25858	106	6	106	7	"Emission metrics are a simple way of representing the magnitude of the climate effect of a unit emission of a species." This sentence is well phrased. [Stephen E Schwartz, United States of America]	Noted. Thank you.
25860	106	6	106	7	"Since AR5 there has been further understanding of the radiative effects of emitted species (see 7.3.2)." Better "Since understanding of the radiative effects of emitted species has increased." [Stephen E Schwartz, United States of America]	Accepted: Wording has been changed
47590	106	8	106	8	The original reference for GWP is Lashof and Ahuja (1990, Nature doi:10.1038/344529a0). [Katsumasa Tanaka, Japan]	Accepted: Reference has been changed.
32970	106	8			global mean sea level [Aimee Slangen, Netherlands]	Accepted. Wording has been changed.
13478	106	10	106	10	Expand AGWP and AGTP [Govindasamy Bala, India]	Accepted: These have been expanded
47592	106	11	106	11	As implied here, the reference gas does not have to be CO2. An idea of using CH4 as a base gas for SLCP metrics was put forward by Cherubini and Tanaka (2016, Environ. Sci. Technol., doi:10.1021/acs.est.6b05343). This idea was applied to a climate impact assessment (Tanaka et al., 2019, Nature Climate Change, doi:10.1038/s41558-019-0457-1). This idea can be briefly introduced here. [Katsumasa Tanaka, Japan]	Taken into account: References added.
47594	106	13	106	13	This sentence can be moved to Section 7.7.1 to inform the reader in advance what will be discussed in this section. [Katsumasa Tanaka, Japan]	Accepted: This has been moved.
13480	106	15	106	16	"Radiative efficiency": Is this same as efficacy? Please clarify. I see that down on line 42, the unit is Wm-2 per ppm. Can you also explain the statement that the radiative efficiency vary with background? Do you mean control climate state? Or background concentration of the gases? [Govindasamy Bala, India]	Taken into account: Clarified that it is the background concentrations of the gases.
43632	106	15	106	17	Yes metrics evolve with background concentration but the main reason for changes in the GWP of CH4 has been an evolving understanding of indirect forcing, mainly via tropospheric ozone. A lot of policymakers think that because GWP of CH4 has increase from 21 to 23 to 25 to 28 in subsequent assessment reports, that this is an inevitable increase over time. The changes are largely due to changes in scientific understanding of indirect forcing, whereas the change due to changing background concentrations alone would have been much smaller (see e.g. Reisinger et al 2011, doi: 10.1088/1748-9326/6/2/024020) [Andy Reisinger, New Zealand]	Noted: This comment is noted, no changes to text made.
18700	106	18	106	19	It is mentioned : “short-lived species (SLCFs)”, but SLCF stands for “short-lived climate forcers”. [Gwenaelle GREMION, Canada]	Accepted: This has been reworded
19082	106	22	106	22	Might consider Sarofim and Giordiano (2018): Sarofim, M. C., & Giordano, M. R. (2018). A quantitative approach to evaluating the GWP timescale through implicit discount rates. Earth System Dynamics, 9(3), 1013-1024. [Gwenaelle GREMION, Canada]	Rejected: This study is already cited on the next page where it is more appropriate.
45594	106	24	106	28	The bullet-point list needs an introductory sentence (e.g. "Further key updates since AR5 include:") or it could be embedded in the text in the paragraph above. [Øivind Hodnebrog, Norway]	Not applicable: The bullets have been removed.
43634	106	24	106	28	The role of those three bullets is not clear. They are normative/prescriptive statements. Are these the assessment conclusions of the authors (in which case, I feel the prescriptive tone is a problem and use of calibrated uncertainty language essential), or is this attempting to summarise the sentiment in recent literature (in which case I don't feel the recent literature has been cited adequately). This needs clarification/revision. [Andy Reisinger, New Zealand]	Taken into account: The bullets have been removed
18668	106	24	106	28	Why are these statements writen in bullet points? Where are the references for each statement? [Gwenaelle GREMION, Canada]	Taken into account: The bullets have been removed
18670	106	24	106	28	You mention “climate metrics” here, but you’ve been talking about “emission metrics” in this section so far. Page 8 of this chapter explains the distinction between “climate metrics” and “emission metrics”, and says that the distinction is not clear cut. But I think it’s important to make sure that reader knows if we’re are talking about “climate metrics” or “emission metrics”, and be consistent when we use these words. [Gwenaelle GREMION, Canada]	Accepted, wording changed.
53744	106	24	106	28	the bullets comes abruptly [Jan Fuglestvedt, Norway]	Taken into account: The bullets have been removed

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25862	106	37	106	38	"The re-evaluated effective radiative efficiency forcing for CO2 will affect all relative climate metrics." This is not good. It is a consequence of using non-systematic units, i.e., normalizing to the AGWP of CO2. Suggeste abandon GWP concept and report only AGWP's. Otherwise there will be a proliferation of GWP's every time the radiative efficiency or removal rate of CO2 is changed. [Stephen E Schwartz, United States of America]	Rejected: Relative metrics are used widely and therefore need to be assessed in AR6
25864	106	40	106	43	Suggest give in table. These are very important quantities. Uncertainties should also be given. [Stephen E Schwartz, United States of America]	Taken into account: These have been added to table 7.17
43636	106	40	106	46	This is too brief: the para says "these are the assessed values" but gives no detail on how the assessment was done, e.g. Etminan revisions for CH4 but what specific assumption for rapid adjustments etc. More detail is needed at the beginning to clarify the methodology, then it's fine to give the results. [Andy Reisinger, New Zealand]	Taken into account: Reference to 7.3.2 added.
47596	106	40	106	46	If the background atmospheric composition in 2017 is assumed for all metric values presented in AR6, it should be clearly stated so. [Katsumasa Tanaka, Japan]	Accepted: background concentration is stated as 2017.
25866	106	41	106	42	Give these numbers in a table. They are important and need to be brought out.That said the changes are small compared to the uncertainties. So perhaps the implications are overstated. [Stephen E Schwartz, United States of America]	Taken into account: These have been added to table 7.17
19084	106	43	106	44	"For.....background", confusing sentence [Gwenaelle GREMION, Canada]	Taken into account: This sentence has been reworded
58144	106	43			The CO2 value is the same as the AR5 value, not an increase. [Nathan Gillett, Canada]	Accepted, wording changed.
53746	106	47	106	47	You may consider using an Annex or Supporting Online Material for all numbers and calculations. [Jan Fuglestvedt, Norway]	Taken into account: Material has been moved into supporting online material.
47598	106	49	106	49	I feel it may be implicitly assumed that metrics need to be calculated by simple analytic formula or response functions presented here. The simplicity and transparency of this approach probably contributed to a wide acceptance of GWP and GTP, but metric calculations should be open to other methods, and there are studies using more complex models to derive metric values (e.g. Gillett and Matthews, 2010, Environ. Res. Lett., doi:10.1088/1748-9326/5/3/034011; Reisinger et al., 2010, Geophys. Res. Lett., doi:10.1029/2010gl043803; Tanaka et al., 2013, Clim. Change, doi:10.1007/s10584-013-0693-8; Gasser et al., 2017, Earth Syst. Dynam., doi:10.5194/esd-8-235-2017). It can be noted somewhere here or around that models can be directly employed to calculate emission metrics. [Katsumasa Tanaka, Japan]	Taken into account: Discussion of model-derived metrics has been added.
18682	106	49			Section : 7.7.2.2 Physical quantities : In general, I think the content is adequate and well explained, but there are some changes I suggest to make to the structure of this section so as to make it clearer. This is the order I would follow : 1st. Explain the difference between the two types of metrics : a) instantantaneous or endpoint values, b) integrated from the time of emission. This is what you explain in lines 39 (p.107) to 8 (p.108). 2nd. Explain the metrics that are instantantaneous or endpoint values --> What is written from line 6 (p.107) to line 37 (p.107). I also suggest you write the information contained in the first paragraph of the section (from line 51,p.106, to line 4,p.107) where it suits better, that is, the information about the temperature change potential and global sea level rise should be written just before showing the corresponding equations, not in the very beginning of the section. The sentence that goes from line 2 (p.107) to line 4 (p.107) would be more appropriate just after explaining the equations of AGRF, AGTP and AGSR, because it is easier to understand it once we've seen these equations. 3rd Explain the metrics that are integrated from the time of emission. [Gwenaelle GREMION, Canada]	Taken into account. The main structure has been kept the same, but some of these suggestions have been taken into account to make it clearer.
18684	106	49			Section : 7.2.2.2 In the equations, there are the variables t and t'. What "time" do they represent (ex. when the gas is emitted...)? I say this because the variable H also represents time, so it can be confusing for the reader. [Gwenaelle GREMION, Canada]	Taken into account: Variable t explained.
18674	106	51	106	51	It's written "All the emission metrics are related to top of atmosphere (effective) radiative forcing", but "top of the atmosphere radiative forcing" is not the same as "effective radiative forcing" (see p.9 of this chaper). [Gwenaelle GREMION, Canada]	Accepted: "top of atmosphere (effective)" deleted.

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15088	106	51	108	8	A more accurate mathematical formulation starts with the instantaneous energy balance, Pi(t) = Po(t) + dE(t)/dt, where Pi(t) is the instantaneous power entering the system and equal to (Psun*(1-albedo)), Po(t) is the instantaneous power leaving the planet and their instantaneous difference, dE/dt, is functionally equivalent to the ERF and adds to or subtracts from the energy stored by the planet, E. Po is a function of the surface emissions consequential to its temperature, cloud emissions when present and a radiant model of atmospheric absorption, transmittance and re-transmission Since the derivative of E must be the same form as Pi and Po, solutions for E are limited to powers of Euler's number, including imaginary powers representing sinusoidal solutions. Since Pi(t) is a periodic function (diurnal and seasonal) the solutions for the causal response to Pi are readily obtained. The solution space includes exponential approaches to a constant steady state in response to step functions of Pi and sinusoidal solutions in response to sinusoidal functions of Pi. A wrinkle occurs since while the temperature, T, is linearly proportional to E (and not dE/dt), Po is proportional to T^4, thus Po approaches equilibrium (average dE/dt == zero), faster than it would if Po was linear to E. This represents an under damped system which will overshoot the desired steady state and is what manifests natural variability around the mean. The data tells us that for climate averages, Ps/Po = 1.62. Even monthly averages from pole to pole are within 20% of this ratio and yearly averages deviate by far less. Po can be restated as e*Ps, where e is 1/1.62 is the effective emissivity of a gray body at T emitting Po. Ps is proportional to T^4, while T is also linearly related to E. Given that in the steady state, average Pi == average Po, the ECS can be exactly quantified as 1/(4*e*o*T^3), where e is 0.62, o is the SB constant and T is the average temperature. [George White, United States of America]	Rejected: This does not relate to the emission metrics being discussed here. It does not reference any literature to support the statements. No changes to the text are suggested.
32972	106	54	107	17	Is this really total GMSL or only the steric part? If it is total, it seems a quite crude assumption: there might besome linearity in individual processes but for sea level as a whole there is the issue of timescales. [Aimee Slangen, Netherlands]	Accepted: "thermosteric component" added.
18672	107	3	107	4	To avoid ambigüity, you can write: "Thus, SLCFs become relatively more important for SLR than for temperature or radiative forcing". [Gwenaelle GREMION, Canada]	Accepted: This has been reworded
47600	107	6	107	10	Although there is nothing wrong here, I think it unusual to introduce AGRF at the beginning. I imagine that readers would expect to see AGWP or GWP first, but GWP comes a page after. For the purpose of clarification, I would think it better to have a statement here that AGRF is different from AGWP (or GWP) as a note of caution. [Katsumasa Tanaka, Japan]	Taken into account: The text has been modified to introduce AGWP at an earlier stage.
13484	107	12	107	12	Equation 7.2: This integral looks similar to the one in Equation 8.1 of AR5 report that relates emission to temperature change. Each emission pulse would yield a T change after some time. Hence Eqn. 8.1 in AR5 sums up all the T change due to prior emissions. However, equation 7.2 is not so clear. Are you saying RF at any point of time would correspond to a later T change and summation of all T changes due to prior RF would give the total T change? I hope AGRFs are still tagged to separate emission pulses. As written, it looks like the effect of a emission pulse is counted multiple times. [Govindasamy Bala, India]	Rejected: Equation 7.2 is not related to 8.1 in AR5
44830	107	12	107	24	Make consistent use of sub/supper scripts. Define R_T, R_SLR, …. [Astrid Kiendler-Scharr, Germany]	Rejected: The sub/super scripts are consistent. R_T and R_SLR are defined.
18690	107	15	107	15	Eq. 7.3 : The t' of the integral symbol should be t (without the prime). [Gwenaelle GREMION, Canada]	Accepted: prime removed.
42062	107	19	107	29	Even though I am an originator of the GPP, I wonder, given its policy relevance (and/or lack of uptake in the literature) whether it deserves so much detail here - perhaps the passing mentions in earlier paragraphs are enough? [Keith Shine, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account: The description of GPP has been reduced considerably
18676	107	20	107	20	The concept of "effective atmospheric forcing" has not been defined in the chapter. What is it? What is the difference between "effective atmospheric forcing" and the other two concepts that have been explained before (""effective radiative forcing", "top of the atmosphere radiative forcing")? [Gwenaelle GREMION, Canada]	Accepted: Atmospheric forcing now defined.
18678	107	21	107	21	Instead of "the effective radiative forcing", it would be more appropriate to write "the absolute global radiative forcing metric", because that is what the equations show (the variable AGRF). When talking about metrics, there are several terms that are very similar, so I think it is important to make sure we use the same terms when talking about the same thing in order to avoid confusion. It's hard enough for the reader to remember all the concepts that are explained! [Gwenaelle GREMION, Canada]	Rejected: This sentence is describing the physical processes so to refer to the physical term "effective radiative forcing" is more appropriate.
13482	107	24	107	24	Equation 7. 4: there is an extra k in the first term. [Govindasamy Bala, India]	Not applicable. This equation has been deleted.
18680	107	33	107	33	What do global damage potentials mesure exactly? What represents "damage"? [Gwenaelle GREMION, Canada]	Taken into account: This has been reworded to explain that "damage" refers to economic and social costs.
47602	107	34	107	34	Here again the original reference is required for the Global Damage Potentials. [Katsumasa Tanaka, Japan]	Taken into account: It has been clarified that this was a recent example rather than the originator.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
25868	108	2	108	40	Not sure if this discussion even belongs here. It seems to be dealing with mitigation issues, not climate science issues per se. That said, it is not all that well said. The discussion starts with a criticism of the approach instead of the value of the approach. A strength of the GTP approach versus the AGTP approach is that the dependence of the result on climate sensitivity cancels out, a point not mentioned here. Panels a and b clearly depend on the chosen climate sensitivity (which is unspecified, but should be), and are thus uncertain to a factor of 1.6 likely range. The discussion of the consequence of lifetime is pretty trivial and well understood. On the other hand the normalization to CO2 is hostage to knowledge of the rate at which a pules emission of CO2 would decrease from the atmosphere, which is highly uncertain, factor of several, Schwartz, 2018, Figure 3.So any discussion of ACTP of CO2 needs to present the decay curve of a pulse of CO2 in addition to the climate sensitivity, and for that matter also the forcing associated with incremental CO2, but that is minor relative to the other uncertainties. All of these issues would have to be explicitly presented instead of just ignored. S oa lot of explaining for a fairly trivial result that arguably doesn't belongin this chapter at all or even in WG1 report.. [Stephen E Schwartz, United States of America]	Taken into account: This discussion does belong here as it discusses the physical science involved in comparing forcing agents. The climate sensitivity used is now reported in the table caption. As pointed out by the review the climate sensitivity does not affect the final metric.
52068	108	11			Much of this has also been covered, perhaps from a slightly different angle, in chapter 5 FOD and, to a lesser xtent, chapter 6. Some reconciliation may be warranted here. At a minimum cross-referencing would be adviable. [Peter Thorne, Ireland]	Accepted: Ch7 provides the definitive discussion on forcing in the report. Overlap with ch. 5 and ch. 6 have been addressed and extensive cross-referencing has been added in the FGD.
47604	108	15	108	15	Because climate-carbon feedbacks are now included in standard metric values in AR6, I think such feedbacks need to be explicitly indicated in Figure 7.2. [Katsumasa Tanaka, Japan]	Not applicable: figure 7.2 will not remain as it is
53748	108	18	108	18	I suggest adding "tentatively" after "response was" [Jan Fuglestvedt, Norway]	Accepted: Change made
53750	108	18	108	18	I suggest you insert something like this after "metrics": "this introduce an inconsistency and bias in the metric values." [Jan Fuglestvedt, Norway]	Accepted: Change made
58138	108	19			Gillett (2010; https://iopscience.iop.org/article/10.1088/1748-9326/5/3/034011/meta) evaluated the effect of carbon cycle feedbacks on GWP and GTP of various gases and was assessed in the AR5. [Nathan Gillett, Canada]	Taken into account: Reference to Gillett added.
18688	108	22	108	22	What is Γ ? It is not explained anywhere in chapter 7. According to Collins et al. (2013b), “A way of obtaining a simple estimate of this effect is to make the crude assumption that the change in the land-atmosphere flux of CO2 is linearly proportionally to the surface temperature change with a coefficient Γ“. [Gwenaelle GREMION, Canada]	Accepted: Gamma now defined
45596	108	22	108	22	I suspect the reference should be "Collins et al. (2013b)" and not "Collins et al. (2013a)"? [Øivind Hodnebrog, Norway]	Editorial: updated in SOD
30302	108	22	108	22	Define all symbols used. [Joeri Rogelj, Austria]	Accepted: Gamma now defined
47606	108	22	108	22	Please define the mathematical notation here. [Katsumasa Tanaka, Japan]	Accepted: Gamma now defined
18686	108	22	108	22	Don’t you mean Collins et al. (2013b) (which is the study you have just mentioned in line 19 of the same page)? [Gwenaelle GREMION, Canada]	Combine with #18686
53752	108	24	108	24	insert a delta before Agxx ? [Jan Fuglestvedt, Norway]	Accepted: Change made
18692	108	29	108	30	The new method to take into account the carbon cycle response has been developped, according to the text, by Gasser et al. (2017) and Sterner and Johansson (2017). Even if the results of both studies agree that “Including the carbon cycle response for non-CO2 treats CO2 and non-CO2 species consistently” (high agreement), I think that the fact that there are just two studies is not enough to state that “Confidence in the method for calculating the carbon cycle response is high”. [Gwenaelle GREMION, Canada]	Taken into account: The confidence statements are now separate for the methodology and the magnitude.
47608	108	29	108	30	Regarding the statement "Confidence in the method for calculating the carbon cycle response is high," I do not doubt this, but I suggest that such a statement should not be made unless there are more substantial evidences. In AR5, the method for calculating climate-carbon feedbacks for metrics relied just on a single paper (i.e. Collins et al., 2013, Atmos. Chem. Phys. doi:10.5194/acp-13-2471-2013). Gasser et al. (2017, Earth Syst. Dynam., doi:10.5194/esd-8-235-2017) was published after AR5 and essentially showed that the AR5 approach to quantifying climate-carbon feedbacks for metric numerators had been wrong. Now the situation appears similar because the feedback calculation method in AR6 is based solely on a single paper again (i.e. the Gasser paper). I served as a reviewer of the Gasser paper, and I do not question the quality of the paper. But I do think that relevant authors should make more self-reflection before making such a bold statement. We need more independent lines of evidence before declaring high confidence. The Gasser paper is based on just one model OSCAR. More models like MAGICC, FAIR, ACC2 etc need to be employed to quantify the effect of such feedbacks for metric values. As another issue, Sterner and Johansson (2017, Environ. Res. Lett., doi:10.1088/1748-9326/aa61dc), which also looked into this problem, should be more integrated in the discusssion here. [Katsumasa Tanaka, Japan]	Taken into account: The confidence statements are now separate for the methodology and the magnitude.
30304	108	29	108	34	There seems to be a conflict between the high confidence in the method and the +-100% uncertainty. Maybe add an additional confidence statement for the total effect (probably medium confidence as there might be strong evidence but low agreement). [Joeri Rogelj, Austria]	Taken into account: The confidence statements are now separate for the methodology and the magnitude.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
33402	108	29	108	34	I certainly agree that it is clear that including climate-carbon feedbacks for both CO2 and non-CO2 species is a superior approach to including climate-carbon feedbacks for CO2 but not non-CO2 species. However, I think that the chapter authors should consider the value of a GWP calculation that does not include climate-carbon feedbacks in either the CO2 or non-CO2 species. Some of my reasoning is laid out in a comment on Gasser et al. (2017). The basic summary is that for the 100-year GWP, which is the most widely used metric, inclusion of climate-carbon feedbacks has an effect of less than 2% on GWP calculations. The cost of the inclusion is increased complexity and the loss of the ability to calculate a 100 year GWP provided only a lifetime and a radiative efficiency. The 100 year GWP already includes a number of simplifications with much larger effect than ignoring climate-carbon feedbacks, such as the assumption of constant background concentrations (which is perhaps a good parallel to ignoring climate-carbon feedbacks), the choice of 100 years as an integration period. But it is these simplifications which make the GWP appealing, and even with the simplifications, the 100 year GWP does a reasonable job at approximating the relative damages resulting from emission pulses of different gases (Sarofim et al. 2018), and when incorporating in an integrated assessment model, is not far off from the optimal emissions pathway calculated in those models (Reisinger et al. Climatic Change 117, 677, 2013, Smith et al. https://doi.org/10.1007/s10584-013-0861-x, 2013) (as noted in a key sentence in Myhre et al: "However, under idealized conditions of full participation in mitigation policy, the increase is relatively small at the global level, particularly when compared to the cost savings resulting from a multi-(as opposed to single-) gas mitigation strategy even when based on an imperfect metric (O'Neill, 2003; Aaheim et al., 2006; Johansson et al., 2006; Johansson, 2012; Reisinger et al., 2013; Smith et al., 2013)" [Marcus Sarofim, United States of America]	Rejected: Adding a no-feedback metric probably confuses this issue -not to mention being biased low (even if only by a few %).
33452	108	29	108	34	I wanted to reiterate that for the 100 year GWP, it would be extremely valuable for the IPCC to present an estimate not including any climate-carbon feedbacks (in both CO2 or non-CO2 gases), or at the very least, to provide a CO2 lifetime multi-exponential approximation that excludes the climate-carbon feedbacks that would allow other users to calculate the no-feedback version. I personally think that for the 100 year GWP, the no feedback version would be more useful. I recognize that for other metrics (e.g., GTP), feedbacks make a larger difference. [Marcus Sarofim, United States of America]	Rejected: Adding a no-feedback metric probably confuses this issue -not to mention being biased low (even if only by a few %).
19086	108	30	108	31	confusing sentence, some commas are needed. [Gwenaelle GREMION, Canada]	Accepted: Commas have been added
43640	108	32	108	34	Given the range of climate-carbon cycle feedbacks across the C4MIP and CMIP5 model results (I'm not on top of what evaluation may have been done yet for CMIP6 results), I find it problematic to use a single model result (Gasser et al) without any assessment of where this specific model sits within the spectrum of results across the range of models. The use of this specific model needs to be justified more clearly if this will then determine THE official IPCC numbers for updated metric values. [Andy Reisinger, New Zealand]	Taken into account: Discussion of the feedback terms in the two simple carbon cycle models has been added.
53768	108	33	108	33	I suggest putting table 7.A.1 in Online Supporting Material. And only have a short table with selected components in the chapter. [Jan Fuglestvedt, Norway]	Taken into account: Some components have been moved to supporting online material.
44832	108	37	108	40	References seem very limited here. Add reference for effects of aerosol on light utilization by plants (e.g. Mercado et al., Nature 2009). [Astrid Kiendler-Scharr, Germany]	Accepted: reference added
9622	108	37	108	41	Perhaps also include a reference to Tokarska et al. 2018, who showed that that the non-CO2 effects on the carbon cycle (and carbon budgets) is secondary, and only the direct warming effect is important. Ref: Tokarska, K.B., Gillett, N.P., Arora, V.K., Lee, W.G., and Zickfeld, K. The influence of non-CO2 forcings on cumulative carbon emissions budgets. Environmental Research Letters, 13, 034039 (2018). [Katarzyna (Kasia) Tokarska, United Kingdom (of Great Britain and Northern Ireland)]	Accepted: reference added
53754	108	37	108	41	you may add that ideally , all indirect effects should be included in order to be consistent, but knowledge limits how far one can go. [Jan Fuglestvedt, Norway]	Accepted: Text added.
18694	108	38	108	39	The expression "fertilize the carbon cycle" does not seem very clear. It would be more appropriate to say that the emissions of reactive nitrogen species can "facilitate the fertilization of plants, thus increasing carbon sequestration", if that's what you mean. [Gwenaelle GREMION, Canada]	Accepted: This text change has been made.
18696	108	39	108	40	What's the reference for the sentence "emissions of aerosols or their precursors can affect the utilisation of light by plants"? Also, it would be a good idea to specify how aerosols or their precursors affect the carbon cycle. I suppose that emission of aerosols or their precursors prevents some light reaching plants, thus reducing carbon uptake by plants. [Gwenaelle GREMION, Canada]	Taken into account. Mercado reference added.
19088	108	40	108	40	"…. of light by plants." Can cite Cohan et al. (2002) here: Cohan, D. S., Xu, J., Greenwald, R., Bergin, M. H., & Chameides, W. L. (2002). Impact of atmospheric aerosol light scattering and absorption on terrestrial net primary productivity. Global Biogeochemical Cycles, 16(4). [Gwenaelle GREMION, Canada]	Accepted: Cohan cited.
44834	108	43	108	44	Refer to Chapter 6. [Astrid Kiendler-Scharr, Germany]	Accepted: reference added

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
45598	108	43	108	50	It could be worth mentioning that non-methane hydrocarbons have indirect effects through ozone formation and reaction with OH, which leads to longer CH4 lifetime - see Collins et al. (2002, Clim. Ch., http://doi.org/10.1023/a:1014221225434) and Hodnebrog et al. (2018, Atm. Sci. Lett., http://doi.org/10.1002/asl.804). [Øivind Hodnebrog, Norway]	Accepted: text and references added
47610	108	47	108	50	AR5 presented two types of CH4 metrics: one for general purpose (GWP=28) and another for fossil fuel to account for CH4 oxidation (GWP=30). The current AR6 draft does not distinguish between the two, but is the distinction really no longer needed? As the text states, it may be possible to include the oxidized CH4 as part of CO2 accounting (Muñoz & Schmidt, 2016, International Journal of Life Cycle Assessment, doi:10.1007/s11367-016-1091-z). However, the 2019 IPCC Methodology Report (Volume 1, Chapter 7.2.1.5) considers different treatments for CH4, depending on how each country reports CH4 oxidation in its emission inventory. This point requires more discussion in the text. Clarifications are needed also for the sake of maintaining the continueity with AR5. [Katsumasa Tanaka, Japan]	Taken into account. Discussion of the fossil fuel vs biogenic methane has been added.
47612	108	47	108	50	CO2 released as a result of oxidizing CH4 can enter into the global carbon cycle. Could this significantly affect the carbon cycle? The answer is probably no, but this question occurred me because CH4 oxidation is discussed in the same section with carbon cycle responses. It may be worth clarifying it. [Katsumasa Tanaka, Japan]	Rejected: This new chemically produced CO2 acts the same as directly emitted CO2. The carbon cycle response to this is already included in metrics and this discussion does not need to be repeated here
58140	108	47		48	What about methane affects on sulphate? (https://science.sciencemag.org/content/326/5953/716) [Nathan Gillett, Canada]	Taken into account: A discussion on the effects of methane on aerosols has been added.
30306	109	2	109	29	For balance also a (potentially short) section should be included on comparing long-lived gases to CO2. Alternatively section 7.7.2.4 can simply be called "Comparing forcers of different lifetimes with CO2", and include a para on long-lived forcers and subsequent paragraphs on short-lived forcers as is currently the case. [Joeri Rogelj, Austria]	Taken into account: A sentence on LLCFs has been added.
53756	109	4	109	5	When you mention that metrics are critizised for either over or underating the importance, i think uo need to discuss the context and timescales briefly. It depends on wat you consider; rate vs level, short vs long term, etc [Jan Fuglestvedt, Norway]	Taken into account: This sentence has been removed.
18704	109	4	109	10	I would slightly modify the structure and content of these sentences to make it shorter and more clear. I would write : “Emission metrics are often criticised for over or understating the role of different species, mainly SLFCs, on future globally averaged surface temperature change (e.g. Pierrehumbert, 2014). SLCFs are particularly sensitive to the choice of time horizon because the temperature change following a pulse of SLCF emission declines with time. For instance, for methane, GTP corresponding to 50-year and 100-year time horizons range from 15 to 6.6, respectively. In contrast, the temperature change following a pulse of CO2 emissions is roughly constant in time (see Table 7.17 and Table 7.A.1). [Gwenaelle GREMION, Canada]	Taken into account: This paragraph has been reworded
18698	109	5	108	6	The meaning of SLCFs is already explained in the title of section 7.7.2.4, so ther is no need to mention it again. [Gwenaelle GREMION, Canada]	Accepted: This change has been made
18702	109	7	109	8	According to table 7.A.1, for CH4 : GTP (50) = 15 and GTP (100) = 7 According to table 7.17, for CH4 : GTP (50) = 14.5 and GTP (100) = 6.6 So, for methane, GTP for 50-year and 100-year time horizons actually range from 15 to 6.6, respectively (not from 14 to 4) [Gwenaelle GREMION, Canada]	Accepted: Change made
45600	109	8	109	8	The values range from 15 to 7, not 14 to 4, according to the tables. [Øivind Hodnebrog, Norway]	Accepted: These has been changed
43638	109	8			"from 14 to 4" -> "from 14.5 to 6.6" as per Table 7.17 [Andy Reisinger, New Zealand]	Accepted: Change made
30310	109	12	109	12	This statement is vague and therewith not very useful. Please, fefine "significant". Does this mean one, two, three, or an order of magnitude larger than the lifetime of the SLCF? [Joeri Rogelj, Austria]	Accepted: Clarified that this is a few times larger.
43642	109	12	109	14	Given Figure 7.29 and basic climate science principles, I don't think it is justified to talk about a "roughly constant" temperature change - the temperature clearly continues to increase for several centuries, albeit at a declining rate. Also the authors should reconfirm that the calculations include climate-carbon cycle feedbacks. [Andy Reisinger, New Zealand]	Accepted: Changes made, carbon cycle response has been added.
18706	109	14	109	16	The “metrics for the step emission changes” is the same thing as the “integrated metrics”(lines 1-8, p.108), isn’t it? If so, then I would suggest combining that information, and putting it in section 7.7.2.2, since it’s the section which explains the basics of the emission metrics. I also suggest using equation 7.6 to represent integrated metrics rather than the one that appears in line 4, p.108, because the former is more clear. If the “metrics for the step emission changes” is NOT the same thing as the “integrated metrics”, then it would be a good idea to explain the difference between them. [Gwenaelle GREMION, Canada]	Taken into account: The equivalence between step emissions and integrated metrics has now been stated in the text. The two however are conceptually different so they are not combined.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
18710	109	18	109	29	What is the reference for the concept of “mixed-GTP” metrics and its peformance? I assume it’s probably the same reference as the figure 7.29, but this figure has no reference either. Also, it would be more accurate to say “GTP ratio” than “mixed-GTP”. [Gwenaelle GREMION, Canada]	Taken into account: This reference has been added (Collins et al. submitted). The naming has been changed to "combined GTP"
18708	109	19	109	19	The fact that “This has the units of years, (the standard GTP is dimensionless)” has already been mentioned in section 7.7.2, so you don’t need to mention it again. [Gwenaelle GREMION, Canada]	Rejected: The dimensions of the combined GTP have not been discussed earlier.
18712	109	19	109	20	It’s hard to compare those figures because these two metrics don’t have the same units. Moreover, that figure doesn’t really show that “ This mixed-GTP shows much less variation with time than the standard GTP”. [Gwenaelle GREMION, Canada]	Taken into account: The shape of the graphs can be compared even if they have the different units. "much less" has be changed to "less".
30312	109	21	109	22	The carbon budget concept is used to denote various things in the lilterature. Using a more specific description like "cumulative CO2 emissions" is a more robust way of communicating here. Alternatively, first define how the "carbon budget" term is to be understood in this particular context. [Joeri Rogelj, Austria]	Accepted: suggested description used.
18714	109	22	109	22	Allen et al (2016) explained that “ Ref. 2 also notes that the ratio AGTPS(x)/AGTPS(CO2) is approximately equal to GWPH(x) for time horizons H much longer than the lifetime of an agent x”, but they don’t mention the name GWP* in this article. The metrics GWP* is actually explained in Allen et al. (2018b). According to the authors, GWP* “considers a sustained one-tonne-per-year increase in the emission rate of an SLCP (...) to be equivalent (in terms of temperature impact) to a one-off pulse emission of GWPH×H tonnes of CO2 where GWPH is the value of that SLCP’s GWP for a time-horizon H”. The reference is (already written in chapter 7): Allen, M. R., Shine, K. P., Fuglestvedt, J. S., Millar, R. J., Cain, M., Frame, D. J., et al. (2018b). A solution to the misrepresentations of CO 2 -equivalent emissions of short-lived climate pollutants under ambitious mitigation. npj Clim. Atmos. Sci. 1. doi:10.1038/s41612-018-0026-8. [Gwenaelle GREMION, Canada]	Accepted, reference to Allen et al. (2018b) added.
30314	109	24	109	24	Quantify "much less" [Joeri Rogelj, Austria]	Taken into account: The has been quantified to less than half
30316	109	24	109	24	The use of the phrase "most useful" reflects an implicit value judgement by the authors. It should be made explicit which criteria the authors use to judge the usefulness. [Joeri Rogelj, Austria]	Accepted: Now defined as "least dependence on time horizon"
44302	109	25	109	26	Yes, tother emissions can be accounted for exactly using CO2-fe, but it'd ber useful to point out that this requires negative values once you go to timescales longer than the lifetimes of SLCFs, so is inherently peculiar and likely impractical to use. [Drew Shindell, United States of America]	Taken into account: more details on CO2-fe have been added.
30318	109	25	109	26	Maybe useful to cite the primary sources here. This concept was first presented by Wigley (1998). [Joeri Rogelj, Austria]	Accepted: Wigley has been added
47614	109	26	109	26	Please define the CO2-fe metric and clarify what is the difference between CO2-fe and GWP*. [Katsumasa Tanaka, Japan]	Taken into account: more details on CO2-fe have been added.
43644	109	26	109	29	A more fundamental challenge to the use of GWP* in policy is that this metric is based on a change in SLCF emissions in perpetuity. Climate policy instruments usually have a hard time dealing with an assumption that an action that occurs in a given year will be sustained in perpetuity. [Andy Reisinger, New Zealand]	The comment is incorrect. GWP* accurately estimates the temperature effect of a time-series of gases. Its accuracy does not rely on perpetual emissions. See Figure 7.22.
47616	109	28	109	29	It is unclear to me how policies can deal with pulse CO2 emissions and step (or sustained) SLCP emissions in the same basket. More concretely, how does the global stocktake process every five years in UNFCCC evaluate step CH4 emissions lasting 20 years? GWP* (or mixed-GTP) has been proved to be a good metric for geophysical applications, so are TEMP (Tanaka et al., 2009, Clim. Change, doi:10.1007/s10584-009-9566-6) and FEI (Wigley, 1998, Geophys. Res. Lett., doi:10.1029/98gl01855) (because they reproduce tempearture and forcing pathways, respectively, as precisely as possible). But such theoretically fine metrics are not necessarily good for policies. Even though this is part of WGI focusing on the physical aspects of climate change, given the strong applied nature of emission metrics, I think that the potential problem for equating pulse and step emissions in policy settings needs to be clearly spelled out here. [Katsumasa Tanaka, Japan]	GWP* provides a means of placing the temperature effects of SLCFs within a cumulative emissions framework. As a physical metric it is very successful - much more so than GWP100. However, as has been recognised by its developers from the start, the insights it produces probably suggest separate treatment of SLCFs from LLCFs. It is hard to think of other important parts of pollution or environmental health regulation where long-lived pollution is treated as fungible with short-lived pollution, so the strange practice of bundling long-lived and short-lived gases in climate change is something of an outlier.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
18716	109	28	109	29	This sentence does not seem clear to me. What does "These" refer to? And what do you mean by "the high relative value of changes in short-lived pollutants": changes in their concentration, their emissions..? [Gwenaelle GREMION, Canada]	Taken into account: This sentence has been reworded
30308	109	32	109	38	This figure seems to suggest a too high degree of confidence and low level of uncertainties. It would be appropriate if this figure also illustrates ranges/uncertainties in the quantities shown, unless they are irrelevant. However, in the latter case evidence should be provided to support this position. [Joeri Rogelj, Austria]	Taken into account: Uncertainty analysis has been added.
19090	109	34	109	34	Figure 7.29 does not have letters (a),… [Gwenaelle GREMION, Canada]	Taken into account. Figure revised
18718	109	34	109	36	In figure 7.29 (p.199), a), b), c) and d) are not written.Also, note that figure 7.29 d) shows the ratio [step emission]/[pulse of CO2 emission], rather than step vs pulse. [Gwenaelle GREMION, Canada]	Taken into account. Figure revised
25870	109	41	113	16	It would seem essential to show in a figure the time dependence of the AGWP , AGTP, etc for CO2, as these quantities serve as the denominators for the GWP, GTP etc for all other species. Might be valuable to show for a few of the other species, perhaps with shorter or longer lifetimes; yes the AGWP(time) is simply 1 - exponential, but the other quantities are more complicated, so it would be valuable to lend some concreteness to otherwise pretty abstract quantities. [Stephen E Schwartz, United States of America]	Rejected: These were shown in AR5 and have not changed since.
25872	109	41	113	16	Perhaps most important here is the absence of a similar treatment developing aerosols. The aerosols themselves and their radiative effects (direct and indirect) disappear in a week, so a pulse emission corresponds to a pulse forcing, and thus results in an impulse response function, IRF, (albeit negative) onto the AGTP. Provision of this IRF (graphical, data) would permit treatment of aerosols in a way similar to nonCO2 ghgs, but would also through the using the IRF as a green's function permit ready evaluation of the AGTP of any longer-lived species by convolution with the pertinent exponential characterizing the decay of the substance and its forcing. This could result in a valuable tool for both understanding and for formulating policy. [Stephen E Schwartz, United States of America]	Rejected: These were shown in AR5 and have not changed since.
25874	109	41	113	16	One would also like to see (graph and data) the IRF of the temperature response to a pulse input. This could be convolved with the forcing IRF to obtain the temperature response to a pulse emission of the pertinent substance. Ditto for any other of the response quantities. [Stephen E Schwartz, United States of America]	Rejected: The pulse inputs formulae are provided, so they are not needed to be added to the figure.
50792	109	43	109	45	As phrased in the second sentence here you leave an impression that IPCC earlier has recommended the use of GWP(100), which in my view is not the case. Also on page 113, line 10-16 you state that this happened in AR4. When checking back with what was actually written in AR4 I do not find any recomandation from IPCC to use GWP(100). On the contrary there is a long part that describes both cons and pros with not only GWP(100) but also with GWPs in general. I think you have to seperate better in your statement the time-horizon aspect and choice of metrics method. Please consider to either make a clear reference to exactly where in AR4 it was recommended to use GWP(100) or rephrase the sentence to make it factual. [Ole-Kristian Kvissel, Norway]	Accepted: word "continue" removed.
18720	109	44	109	45	I think there's no need to mention "GWP values are not presented for other time horizons", because you only need to mention what is presented in the table. [Gwenaelle GREMION, Canada]	Rejected: This clarification is intentional
53758	109	45	106	45	I suggest you delete "continued" before "recommendation". It sounds as if the previous report (AR5) did recommend GWP100 - which it did not. [Jan Fuglestvedt, Norway]	Accepted: Change made
30320	109	45	109	45	Please double check the facts underlying this statement. I'm unsure whether the IPCC has ever "recommended" GWP, particularly in context of what is written on page 111, lines 41-43. [Joeri Rogelj, Austria]	Accepted: word "continue" removed.
47618	109	45	109	46	Why are the GWP values for other time horizons like 20 years not listed in the table? There are interests in (or even needs for) applying GWP20 to highlight the effects of SLCPs (CCAC https://www.ccacoalition.org/ru/node/1923; Ocko et al., 2017, Science, doi:10.1126/science.aaj2350; Tanaka et al., 2019, Nature Climate Change, doi:10.1038/s41558-019-0457-1). I agree with the fact that metrics with longer time horizons like GWP100 and GTP100 are more relevant to climate policies, but I disagree with abandoning short-term metrics completely from the AR6. If there are reasons for doing so, they need be stated clearly for further examination. [Katsumasa Tanaka, Japan]	Taken into account: Discussion of time horizons has been added
47620	109	45	109	46	MGTP should be renamed because this acronym can be confused with mean GTP (MGTP) proposed earlier by Gillett and Matthews (2010, Environ. Res. Lett., doi:10.1088/1748-9326/5/3/034011). This paper was discussed in the metric section of AR5. Could mixed-GTP be re-written as GTP* to be comparable with GWP*? I am not fully clear if this suggestion actually makes sense, though, because I do not know details in mixed-GTP (no peer-reviewed paper for mixed-GTP). [Katsumasa Tanaka, Japan]	Accepted: Changed to Combined GTP - CGPT
18722	109	45	109	46	It is written that MGTPs stands for mixed metrics comparing step changes in SLCFs with pulse emissions of CO2. Nevertheless, in the literature, it is clear that MGTPs stands for "mean global temperature change potential" (see for ex. Gillett and Matthews 2010). So, to avoid confusion, it is better not to use the accronym MGTP if you're not using it with the well-established meaning. Reference: Gillett N P and Matthews H D 2010 Accounting for carbon cycle feedbacks in a comparison of the global warming effects of greenhouse gases Environ. Res. Lett. 5 034011 [Gwenaelle GREMION, Canada]	Accepted: Changed to Combined GTP - CGPT

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18724	109	47	109	52	The radiative properties of the different species (CO2, CH4,N20) are already explained in section 7.7.2.1 (not 7.2.2.2), so it is repetitive to mention this again. And the radiative properties of halocarbons should be mentioned in that section too, instead of in this section. Also, when talking about how climate (or emissions) metrics have changed since AR5, it is important to state from the beginning of the paragraph that you're comparing these new metrics to those that appear in AR5 (you only mention it in line 52, but it would have been more appropriate to mention it in the beginning). So, in this paragraph, you can just write something like this for each species : "Compared to what was found in AR5, emission metrics for "x" have increased (decreased) due to the increase (decrease) in the "x" effective radiative efficiency. The reference Etminan et al. (2016) should be mentioned just once. [Gwenaelle GREMION, Canada]	Taken into account: This paragraph has been reworded.
58142	109	47			CO2 radiative efficiency is the same as AR5 value according to 7.7.2.1. [Nathan Gillett, Canada]	Rejected: The unadjusted efficiency has decreased, but the adjustments bring it back to the AR5 value as stated in the text here.
33434	109	51	110	2	When comparing to AR5, it is important to distinguish between comparing to the AR5 values with or without climate-carbon feedbacks for non-CO2 gases [Marcus Sarofim, United States of America]	Taken into account: Climate-carbon feedback discussion has been added.
47622	110	1	110	2	The authors need to elaborate why the carbon cycle responses are assessed to contribute less than in AR5. I don't think that general readers would understand why, unless it is more clearly explained. [Katsumasa Tanaka, Japan]	Taken into account: Explanation of the decrease in carbon cycle responses added to section 7.7.2.3.
18726	110	1	110	2	This sentence is not clear: - It is written "the carbon cycle responses...", but responses to what : CO2 emissions, halocarbon emissions..? -It is written "are assessed to contribute less than in AR5", but to contribute to what and how (positive, negative contribution)? -It is written: "for all species the climate metrics are slightly smaller than in AR5". Are you referring just to halocarbons? I guess so, because the climate metrics are not slightly smaller than in AR5 for all species. [Gwenaelle GREMION, Canada]	Taken into account: Sentence reworded.
43646	110	1	110	2	The AR5 used a very simple approximation, so I think this sentence should be revised to make clear that the assessment of climate-carbon cycle feedbacks in climate metrics in the AR6 is based on more complex models and methods than were available in the AR5, and that this has resulted in a lesser contribution of ccfbs to metric values than in the AR5. Otherwise it sounds too much as if the AR5 had generically overestimated climate-carbon cycle feedbacks and I don't think that would be justified. [Andy Reisinger, New Zealand]	Taken into account: Explanation of the decrease in carbon cycle responses added to section 7.7.2.3.
44836	110	5	110	15	What is the basis for selecting species in talber 7.17? Does Methane include efects from resulting ozone and H2O? [Astrid Kiendler-Scharr, Germany]	Taken into account: Table 7.17 has been clarified
42064	110	7	110	7	I think the reader needs a warning/commentary about the very large MGTP values here, as otherwise they could be a bit shocked. Relate to the fact that these now refer to a sustained emission of x kg per year rather than a pulse. Perhaps a simple example would be helpful (e.g. how many years worth of emissions of CO2 would be equivalent to a total permanent removal of methane emissions)? (using Annex 1 2017 emissions, I get that it is 20 years, but I did this very quickly). [Keith Shine, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account: Discussion of the CGTP values has been added..
25876	110	7	110	10	Seems essential to include AGWP etc for CO2 here as the other quantties are normalized to that for CO2. Table should present uncertainties. This hard given uncertainties in both numerator and denominator. Another reason to give absolute potentials. [Stephen E Schwartz, United States of America]	Taken into account: Date have been included in online supporting material.
25878	110	7	110	10	Are these metrics given per mass? per mole? and for CO2, per mass of CO2 or per mass of C? Need to explicitly specify. [Stephen E Schwartz, United States of America]	Taken into account: This has been clarified
42076	110	7	110	13	perhaps some care/caveats needed to make clear that 50 and 100 years are chosen for the MGTP for illustrative reasons and so not constitute a recommendation in anyway, although the point that the 50 and 100 year values are similar is well taken. Perhaps 75 years would be an ideal choice! [Keith Shine, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account: Time horizons has been changed to 75 years.
53766	110	7	110	14	Add years to the "lifetime" collumn [Jan Fuglestvedt, Norway]	Accepted: This has been added
47624	110	7	110	14	Some communities need emission metrics with a shorter time horizon (e.g. 20 years). I don't think that short-term metrics should be entirely dropped. These should be retained for continuity with AR5. Furthermore, I find it confusing to see MGTP with GWP and GTP in the same table because the way how to use MGTP is dfifferent from those for GWP and GTP. [Katsumasa Tanaka, Japan]	Rejected: The reasoning behind times horizons has been explained in the text. It is most convenient to include CGTP and GTP in the same tables.
30322	110	7	110	15	It is odd that no values are provided for mixed GTPs for gases with lifetimes longer than 20 year. Does this mean that this concept is not useable for these gases? This needs clarification in the text. [Joeri Rogelj, Austria]	Taken into account: Discussion of the maximum useful timescales for CGTP has been added
25880	110	8	110	8	caption refers to "carbon cycle responses as described in 7.7.2.4" but no such responses are described there. [Stephen E Schwartz, United States of America]	Accepted: Changed to 7.7.2.3

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18728	110	8	110	9	I don't think you need to write "(see section 7.7.2.5)", "as described in 7.7.2.4", etc..The references to the figures are written in the text, but there is no need to cite a section of the text in the figures. [Gwenaelle GREMION, Canada]	Reject: These are added for clarity
25882	110	10	110	10	"climate response function" does not seem to be defined. [Stephen E Schwartz, United States of America]	Accepted: Changed to temperature response function.
25884	110	10	110	10	"climate response function" should be plotted as a function of time; not enough to state "is from Geoffroy". And uncertainty in this quantitiy should be shown in the plot. [Stephen E Schwartz, United States of America]	Rejected: This information is available in the Geoffroy reference, so it is not repeated here.
18734	110	10	110	10	Which reference : Geoffroy et al. (2013a) or Geoffroy et al. (2013b)? [Gwenaelle GREMION, Canada]	Editorial
18730	110	13	110	13	Table 7.17 : It would be interesting to include CO2 in the table, specially if you mention it in section 7.7.2.5. It is also important to include the unit for lifetime, which are years. [Gwenaelle GREMION, Canada]	Taken into account: Units for lifetime added. Metrics are all unity for CO2 so are not included.
18732	110	13	110	13	Why are some of the values that appear in table 7.17 different from those in table 7.A.1? For instance, it is striking that for HFC-32, according to table 7.17, MGTP(50)= 72500 and MGTP(100)=86500, whereas according to table 7.A.1, MGTP(50)=72488638 and MGTP(100)=86484542.There's a difference of an order of 10^3 in these and other values for HFC-32. Same thing for HFC-134a and CFC-11. There are also slight differences in some values. For instance, for CH4, MGTP(50) is 3050 in Table 7.17 and 3048 in table 7.A.1. I have also noted that the values of CF4 are not in table 7.A.1 [Gwenaelle GREMION, Canada]	Taken into account: The rounding has been made more consistent
47626	110	18	111	12	Although figures are missing in this draft, this box intends to show how each metric performs on RCP pathways. I think it equally (or practically more) important to show how each metric works for the Paris Agreement targets. This has been explored by Tanaka and O'Neill (2018, Nature Climate Change, doi:10.1038/s41558-018-0097-x) and Fuglestvedt et al. (2018, Phil. Trans. R. Soc. A., doi:10.1098/rsta.2016.0445), which should be treated more prominently in AR6 (not mentioned in this draft at all). Based on cost-effectiveness approach, the Tanaka paper shows that, if GWP100 is used to implement the net zero GHG emission target by 2060, it leads to declining temperatures below 2C. If GTP100 is used instead, it gives almost stabilized temperature at 2C. If GWP20 is used, it is not possible to reach net zero because the residual CH4 emissions are weighed too high to be compensated by negative CO2 emissions that are assumed possible in the model. [Katsumasa Tanaka, Japan]	Not applicable. Box 7.3 has been entirely rewritten. But as well as comparing on successful achievement of Paris, it is also important to consider how metrics affect scenarios in which Paris targets are not met.
19092	110	20	111	10	Since Figures are missing is not possible to assess whether [Gwenaelle GREMION, Canada]	Not applicable. Comment unfinished.
56674	110	20			In its current form, Box 7.3 under the heading METRIC TYPES, seems to solely focus on the stock and flow pollutant issue. There is much to say about GWP, GTPs etc. and their history under the heading of METRIC types. Also, given that this box seems to be biased towards fleshing out the MGTP/GWP* foundataion, it would be pertinent to discuss the real-world implications if those kinds of metrics (rather than time-updated GWPs, GTPs or separate accounting baskets were pursued. Having a 3000 value on CH4 for any reduction of CH4 would be quite simply the end of any CO2 mitigation in any kind of crediting or ETS schemes for the next 10 years, until the CH4 reduction options are exhausted ... That would amount to exactly the opposite effect to what many GWP* / MGTP proponents claim ... i.e. that GWP is at the moment putting too much weight on CH4, as the GTP would be at the moment lower (but soon higher) for a peak warming of 1.5C and 2C... the only issue that the time-integration (i.e. accounting for step changes in the emissions, rather than emissions themselves) solves, is that there are the time-changing values of GTPs the closer we get to the peak warming... but that is acquired by creating a metric that would be utterly counterproductive for the next 10 years by completely disregarding efforts to reduce the stock pollutants. Anyway, I want to raise a word of great concern, if this Chapter maintains is somewhat theoretical and policy-blind focus on proclaiming MGTPs or GWP* would solve real world problems... [Malte Meinshausen, Australia]	The step-pulse or stock flow issue is the main development in the physical science domain in the period since AR5, which is why it receives the focus it does. We do not believe the box is biased. Also, the consequences of metric choice depend not just on the metric, but on how the metric is used and on how it fits into policy structures. No one has advanced simply substituting GWP* or CGTP in for GWP100 in all uses. Where the LLCF vs SLCF distinction has been discussed as a policy issue in light of step-pulse metrics, policy discussions have focused on two basket approaches. This is consistent with how cumulative and short-lived pollutants are treated in other parts of environmental regulation.
25886	110	22	110	22	"single valued"; strange terminology; I think that what the author is trying to suggest is that it is difficult to quantify the time-dependent or integrated radiative effect of a substance and compare to that of CO2 by a single quantity. [Stephen E Schwartz, United States of America]	Taken into account: This has been reworded to clarify
43648	110	22	110	23	I think this could be stated even more strongly: "ALL single-valued emissions metrics struggle to capture SOME aspects of the climate response under different scenarios AT DIFFERENT TIMES". (Although it's not clear to me what is meant by "single-valued" emission metrics - what's the alternative? multiple values for the same point in time?) [Andy Reisinger, New Zealand]	Taken into account. Text amended for clarity.

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15174	110	25	110	30	The evidence contradicts a long multi-decadal residence time for CO2. If we calculate all the CO2 that has been emitted since the IR, a significant fraction of it is missing. Biology consumes CO2 rapidly and without decomposition replenishing it, albeit it a slightly reduced rate, biology would quickly starve to death. Based on the multi-million year trends in atmospheric CO2 it's clear that biology sequesters carbon at a faster rate than it can be replenished by natural sources. Without mankind's intervention, the long term destiny of carbon based life will be to perish by running out of carbon. The language also implies that CO2 is a pollutant which it most certainly is not. CO2 along with water and sunlight are the 3 equally important resources at the base of the food chain. Without CO2 in the atmosphere life as we know it could not exist. Failing to point out the crucial nature of CO2 to life is irresponsible. [George White, United States of America]	Noted. For details of the carbon cycle see Chapter 5.
18738	110	25	110	37	What are the references for the statements in this paragraph? [Gwenaelle GREMION, Canada]	Not applicable. Box 7.3 has been entirely rewritten.
13474	110	25	110	37	"flow of pollution". I am not familiar with this terminology but I think "flux pollution" sounds like a scientifically better terminology and nicely conttasts with "stock pollution" [Govindasamy Bala, India]	Rejected. It is a common distinction and phrase in engineering and economics.
25888	110	25	111	1	Suggest avoid "pollution" as pejorative; the discussion is valid for any substance introduced into the environment, whether the effects are beneficial or adverse or neutral. [Stephen E Schwartz, United States of America]	Rejected. It is a common distinction and phrase in engineering and economics.
25890	110	25	111	1	The box is fairly trivial and arguably does not belong in this chapter or WG1 report. [Stephen E Schwartz, United States of America]	Not applicable. Box 7.3 has been entirely rewritten.
44838	110	29			"Short lived climate forcers" instead of "short lived cliamte pollutants" [Astrid Kiendler-Scharr, Germany]	Accept: This has been changed.
19094	110	31	110	31	What does GSAT mean? Where can be seen this near-linear relationship? [Gwenaelle GREMION, Canada]	Rejected. Global Surface Air Temperature. See glossary and defined in chapter.
43650	110	33			The word "pattern" is not ideal here since it could be understood to mean a spatial pattern (which isn't the case for any of the well-mixed GHGs including methane and HFCs). [Andy Reisinger, New Zealand]	Accept: replaced with "time evolution"
30324	110	36	110	36	It would be more useful to keep this statement referring more generally to long-lived and short-lived forcers instead of the two specific examples of CO2 and CH4. No reason is provided why all other short-lived forcers are neglected here. [Joeri Rogelj, Austria]	Rejected. The emphasis is because CO2 and CH4 are the most important instances of each.
19096	110	39	110	41	Quite complex sentence, might need reformulation [Gwenaelle GREMION, Canada]	Not applicable. Box 7.3 has been entirely rewritten.
42072	111	3	111	3	Here and elsewhere - sometimes GWP20 and sometimes GWP(20) etc [Keith Shine, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We have improved the consistency of use.
42070	111	6	111	6	It's a bit of an (understable) shame that this expansion isnt here, as I think one of the key aspects of the mixed metrics is rather hidden, and only comes later - that is the (relative) cooling effect of a sustained decrease in emissions of an SLCF which is completely misrepresented by the GWP. I guess this aspect is one which will be illustrated here, but I feel it could be introduced earlier to engage the reader's attention to this key aspect. [Keith Shine, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. Box 7.3 has been entirely rewritten.
43652	111	6			Just a note for the SOD revision: the authors will have to decide whether to show warming from a given baseline trajectory, or whether to show the temperature impact of various deviations from the baseline (involving different amounts of mitigation of SLCFs vs LLGHGs). From a policy perspective, the latter can be more relevant, since decision-makers want to know how much their abatement in any given reduces climate change. But in this case, the magnitude of differences between realistic alternative mitigation scenarios will be much smaller than if RCP2.6 and RCP6 are compared as baseline scenarios. Policymakers by and large do not rely on GWP or other metrics to determine warming from RCP6 vs RCP2.6. [Andy Reisinger, New Zealand]	Not applicable. Box 7.3 has been entirely rewritten.
30326	111	8	111	10	This conclusion is only true if you consider the full pathway of emissions to be defined. For annual emissions benchmarks, GWP100 provides a much stronger constraint than GWP* or MGTP. For balance and to avoid policy prescription, this distinction should be made clearly by contrasting the strengths and drawbacks of the various methods for cumulative targets as well as for targets in single years. [Joeri Rogelj, Austria]	Not applicable. Box 7.3 has been entirely rewritten.
53760	111	15	111	15	In this sectin you may mention how metrcis are used, simply by saying Emission(i) x Metric(i, H) etc [Jan Fuglestvedt, Norway]	Taken into account. We have rewritten the section and tried to make it much clearer.
25892	111	15	113	16	Again arguably does not belong in this chapter or WG1 report. "value laden" is not science. [Stephen E Schwartz, United States of America]	Rejected. Where choices are value-laden we should point them out. This does not mean we should provide an answer.

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48800	111	15	113	16	This section is very unclear. I doubt there should be admission of guilt regarding recommendations made in AR4. More important is to clearly describe the limittations and advantages of the different metrics used. A table comparing the effects of different emissions by radiative forces, by temperature rise, by sea level rise, by dollar-denominated climate change damages would be very illustrative, as well 20, 100 (and 500) year values of GWP and GTP. [Birgit van Munster, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We have extensively revised this section, but given the recent developments in the literature, we are discussing how emissions metrics can and cannot map to key aspects of the Paris Agreement. We are also editing for clarity.
48802	111	15	113	16	Give as illustration a theoretical example of what GWP means by explaining the temperature effect of a near term fast reduction of a short lived GHG with high GWP like CH4 compared to continued high CH4 emissions, and set this next to a near term fast reduction of a long lived GHG like CO2 compared to continued high CO2 emissions. [Birgit van Munster, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. This is basically the point we are making: rapid reductions in SLCPs lead to cooling, while rapid reductions in LLCPs lead to a slowing of cooling (perhaps to zero, if emissions of LLCPs decline to zero). This is captured in GWP* and CGTP but is masked in GWP and GTP.
18506	111	17	111	17	7.7.3.1. does not really addresses the interpreation of metrics, but is rather a collection of examples which illustrate that you need metrics appropriate for your question. There is also much overlap with 7.7.1. and 7.7.2. and no new information which would be needed for the rest of the section. Suggestion: Leave only the first two paragraphs (line 19 -31) and remove the rest (line 33 - p.112 line 5). And do not have 7.7.3.1 as distinct section, leave the remaining sentences as introductionary words of 7.7.3 and "Matching metrics and policy goals" would be the new 7.7.3.1. [Gwenaelle GREMION, Canada]	Taken into account. We have extensively rewritten this section and removed some redundancy. We have also expanded and revised the section to consider a range of applications for metrics.
30328	111	17	112	5	I very much like this discussion. It could be further improved (although this goes relatively far into WG3 domains) by not just discussing what policy-makers are assumed to do in theoretical models (page 111, lines 45ff) but also how they are actually used. For example, I have not seen any real world example of metrics being used to compute economy-wide carbon-equivalent prices for mitigation. Even the UK CCC net zero report provides separate targets for different sectors, despite being informed by pathways that use different metrics and approaches. [Joeri Rogelj, Austria]	Noted. Thank you.
47628	111	17	113	16	The discussion in Section 7.7.3 is important, raising many fundamental aspects of metrics. Even though most of these themes are recurring, I think it good to see this debate in the AR6 draft, and this discussion should be useful for metric users. But, reading through from the beginning of Section 7.7, I feel that the discussion here somewhat goes back to the beginning and are repetitive. The entire Section 7.7.3 can be shortened and merged with Section 7.7.1. [Katsumasa Tanaka, Japan]	Taken into account. Thank you. We have revised this section to discuss some of the possible uses of metrics, and review recent literature. In general, we think AR5 reviewed the economic aspects of metric use quite thoroughly, so we propose in general to lightly revisit that, but to discuss some of the other issues.
18960	111	19	111	22	This example should be more specific or broken up into two sentences: the first a more general statement and the second a very specific example. As it reads right now, it is confusing with lots of vague identifiers (i.e., “some”, “more or less”, “this”). Couldn’t this read as simply as “the choice of the metric depends on the policy or scientific goals in question.”? [Gwenaelle GREMION, Canada]	Rejected. It is a general sentence describing a process with a range of possible outcomes, some of which may be more, or less, useful for policy.
18990	111	19	113	16	Unless I’m unfamiliar with the language of policy, section 7.7.3 could be streamlined and written more clearly. The first paragraph starting on line 19 should clearly lay out the points to be discussed, i.e., (1) the fact that the choice of metrics depends on the values of the scientist, decisionmaker, or policymaker; (2) one value being timescale & functional form, and (3) another value being metrics based on economic costs. In fact, the FAQ7.2 could be used as aid in streamlining 7.7.3. What is the point of each paragraph? Furthermore, based on the lack of citations, I think emphasis should also be placed on how little we actually know about the use of appropriate metrics. [Gwenaelle GREMION, Canada]	Taken into account .Thank you. The section has been extensively rewritten.
19016	111	19	113	16	Overall, more emphasis should be placed on the gap between physical climate science and social sciences in the metrics used for climate science. More research is required regarding which metrics are actually useful for policymakers and or at least how to obtain those metrics. Right now, the section reads as though we are to just leave it to the policymakers to decide on the best metrics. But how useful is this report if it’s unrelatable and full of science that is uninterpretable to policymakers? [Gwenaelle GREMION, Canada]	Rejected. The IPCC's brief is to be policy relevant but not policy prescriptive. This means we do have to leave it to policymakers. We can set out relationships and uncertainties and point out matters of choice, but it is not our role to recommend things (including the status quo).
18962	111	26	111	29	I think there should be some differentiation between physical and economical choices. i.e., add the words on line 28 “economical-based choices such as” after “including”. Or perhaps add a sentence verifying that these choices may be motivated by physics, biology, or socioeconomics. [Gwenaelle GREMION, Canada]	Taken into account. We have expanded and revised the section to consider a range of applications for metrics.

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45602	111	33	111	36	AR5 also included 50-year values for GTP. [Øivind Hodnebrog, Norway]	Noted. We do not propose to do this. To include more metrics we have restricted the number of time horizons.
33436	111	33	111	43	An interesting question (to which I don't necessarily have an answer): GWP has traditionally been reported at 20 year, 100 year, and sometimes, 500 year timescales since its inception. Recently, Sarofim & Giordano (2018) have produced an analysis suggesting that the 100 year timescale for the GWP is compatible with a 3% discount rate if the global-damage-potential is considered to be a measure of the appropriate relative impact of gases. The GTP has followed the example of the GWP, and so the 100 year GTP is sometimes considered to be an alternative to the 100 year GWP: but it is unclear to me that it makes sense to do so. To the extent that the GTP is promoted because of the 2 degree temperature target, then the appropriate timescale for the GTP might be the estimated time to meet that temperature (considerably less than 100 years). This would, of course, require updating the GTP timescale on a regular basis, and it is unclear what timescale to use for the GTP is the temperature target is exceeded. [Marcus Sarofim, United States of America]	Noted. Yes - various papers discuss time-varying GTP in this way. Some of this literature was touched on in AR5.
18966	111	45	112	5	What is the point of this paragraph? Is the point that scientists have a hard time quantifying the cost of reducing emissions, so they resort to the use of physical metrics? Perhaps I'm not familiar with the vernacular, but this paragraph is written in a very verbose way. [Gwenaelle GREMION, Canada]	Taken into account. We have extensively rewritten this section and removed some redundancy. We have also expanded and revised the section to consider a range of applications for metrics.
33438	111	45	112	5	While I am not going to claim that Sarofim and Giordano (2018) uses anywhere close to a perfect damage function, it does present a framework in which to estimate the relative damages of different GHGs under different damage function assumptions (and is not the only study to do so). Additionally, there is a large literature on the Social Cost of Carbon which provides additional ways to estimate damages (see, e.g., the National Academies report on Valuing Climate Damages). I think that these efforts could be noted, even if the conclusion of the authors is that the shape of damage functions is still too uncertain to inform metrics choices. [Marcus Sarofim, United States of America]	Thank you for the reference. We have extensively revised this section.
18964	111	46	111	50	Either break up into two sentences or simplify. The first sentence could be about the fact that the role of metrics that is most useful to policymakers is based on the economic value of reducing greenhouse gas emissions, and then the second sentence specifying the comparison of "discounted marginal abatement costs", etc [Gwenaelle GREMION, Canada]	Rejected. It is a general sentence describing a process with a range of possible outcomes, some of which may be more, or less, useful for policy.
44304	111	54	112	5	Seems a bit overly pessimistic to me to say we haven't made any real progress since this 2001 paper. Hsiang et al, Science, 2017 showed that bottom-up analyses of all the quantified damages associated with climate change led to values that were, somewhat surprisingly in my opinion, quite consistent with the fairly simplistic damage functions long used by economists. Though that was only for the US, where there is ample economic data on damages (unlike much of the world), this is still progress and suggests we can go beyond just physical metrics with some confidence. [Drew Shindell, United States of America]	Taken into account. We have extensively rewritten this section. We have also removed the more pessimistic points about damages functions not having made much progress.
43654	112	1	112	5	Quite a few studies (Boucher, Johansson, Sarofim) have looked at economic damage potentials. They found that a significant part of the uncertainties that besets absolute damage potentials cancel out (i.e. we don't have to know exactly the shape of the damage function because the errors are symmetrical for the numerator and denominator). They are not fully symmetrical but the error is not as large as this paragraph suggests (as evidenced by the relative robustness of against a range of assumptions in economic interpretations of GWP, e.g. in Sarofim and Giordano). [Andy Reisinger, New Zealand]	Taken into account. Thank you. The section has been amended.
18958	112	8	113	16	Perhaps this section should be about why GWP is not always the most appropriate metric [Gwenaelle GREMION, Canada]	Noted. We believe that it is.
18968	112	10	112	12	This sentence is verbose, vague, and should be rewritten. To start, the words "more or less well-" are not needed. [Gwenaelle GREMION, Canada]	Editorial
43656	112	10	112	12	add "and over all times", since this is usually where metrics diverge substantially. E.g. GWP is an excellent predictor of temperature change from a constant rate of CH4 vs a constant rate of CO2 exactly 100 years into the future, but it underpredicts temperature change from CH4 before that date and overpredicts it after that date (as per Shine 2005). [Andy Reisinger, New Zealand]	Taken into account. We have amended the section.
18970	112	12	112	12	Remove "No matter how it is done" [Gwenaelle GREMION, Canada]	Rejected. We think the current text is adequate.
18972	112	13	112	13	"play out" --> "emerge" [Gwenaelle GREMION, Canada]	Taken into account. Text amended to reduce informal language.
18974	112	14	112	15	Sentence should be rewritten: "GWP(100) is customary based on the fact that the GWP(100) was the only metric discussed in the IPCC FAR (Houghton et al. 1990)." [Gwenaelle GREMION, Canada]	Rejected. That is perhaps not the only reason. See Shine 2009.
18976	112	16	112	16	"as an illustration" should be removed [Gwenaelle GREMION, Canada]	Accepted

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43658	112	16	112	17	The quote is a partial one and is selective towards the negative. The FAR did not say "DO NOT USE" but the partial quote makes it sound as if it had. The full text reads: "A simple approach has been adopted here to illustrate the difficulties inherent in the concept, to lllustrate the importance of some ot the current gaps in understanding and to demonstrate the cunent range of uncertainties. However, because of the importance ot greenhouse warming potentials, a preliminary evaluation is made." [Andy Reisinger, New Zealand]	Rejected. A preliminary evaluation does not constitute a preliminary endorsement.
15090	112	21	112	21	From the language used here and in the dozens of other references to policy goals spread throughout this chapter alone, it's clear that the policy goals are driving how the science is being assessed. This reinforces one of the big criticisms against these reports. From all outward appearances, and confirmed herein, the policy goals of the UNFCCC are too strongly influencing how the science is evaluated and presented by the IPCC. If the IPCC wants to remain the authority on climate science, it should be more independent, more objective, more transparent, go only where the scientific method sends it and avoid getting trapped by unreasonable expectations set by otherwise unsupportable policy goals. While many now defer to the IPCC as the authority on climate science, given the serious nature of the errors in the latest report, this is an unsustainable position. It should be clear that owing to increased skepticism world wide that many are awakening to the errors in the IPCC's reports and this includes scientists associated with the current US administration. The constant claims of a catastrophe that never arrives is not helping the credibility of the IPCC/UNFCCC and it's this drumbeat of alarmism that many consider to be a hoax. [George White, United States of America]	Noted. No suggestion for text change.
18978	112	21	112	23	What "properties"? Rewrite sentences: "The alignment between policy goals and metrics is important, but also the uncertainty associated with either the physical variable or the policy target requires consideration. For example, well-being is notoriously difficult to measure. Policymakers often resort to either well-measured variables that are only imperfectly aligned with well-being (such as financial income) or to variables that may be better aligned with the goals of policy but are more uncertain poorly measured (such as composite indicators of well-being). This often leads to a trade-off between properties such as well-being. [Gwenaelle GREMION, Canada]	Not applicable. Section has been rewritten extensively.
18992	112	27	112	27	"proxy" needs an example [Gwenaelle GREMION, Canada]	Rejected. We think this would disrupt the flow of the paragraph - readers can consult the reference (AR5) for further details.
18994	112	31	112	31	"Metrics can be more or less well-aligned" --> "Metrics can be chosen purposefully to be well-aligned with a target or goal." [Gwenaelle GREMION, Canada]	Editorial
33440	112	33	112	34	I'd add Sarofim and Giordano (2018) as a paper supporting the linkage between GWP100 and the GDP (though I'd add that the choice of discount rate is key in making this comparison - S&G find that the GWP100 aligns with the GDP only when the discount rate is near 3%) [Marcus Sarofim, United States of America]	Taken into account. Thank you for the reference. We have edited this section
33408	112	33	112	35	I would suggest adding Sarofim and Giordano 2018 here, along with a note that the global damage potential depends on the choice of discount rate, and the 100 year GWP aligns with the GDP with a 3% discount rate choice. [Marcus Sarofim, United States of America]	Taken into account. Thank you, the section has been amended.
43660	112	35	112	37	GTP aligns well with a cost-effectiveness framework only if the time horizon matches the expected peak temperature. This should be clarified since GTP(100) has been demonstrated in numerous studies (reviewed in the AR5) to be highly ineffective in helping cost-effective decisions to limit warming to below 2 degrees. [Andy Reisinger, New Zealand]	Taken into account. We have edited this sentence to add "could be a good choice [...] under some circumstances."
42066	112	36	112	36	It isnt clear to me why the GTP time-horizon is included here (and the GWP time horizon two lines earlier). It is the GTP that fits the cost effectiveness framework (at least as I understand it) rather than GTP(100) and the temperature change in, say 2119, is rather arbitrary [Keith Shine, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We have removed the time-horizon, and edited the sentence.
47630	112	39	112	46	I would think it more reasonable to say that Article 4 gives a numerical target (i.e. net zero GHG emissions during the latter half of this century). For example, people are debating what are the differences between 80% emission cut and 100% emission cut (i.e. zero emissions). Zero emission target is regarded as a numerical target in this context. [Katsumasa Tanaka, Japan]	Rejected. There are no numbers in Article four, which is why we use the word "numerical". People may make substantively different things regarding what a "balance of sources and sinks" implies, as was shown in Fuglestvedt et al., 2018.
53762	112	41	112	41	re "numerical targets": It has, in Art 4, the GHG gas balance target (as mentiond below) , which needs metric for being operational. [Jan Fuglestvedt, Norway]	Noted. There are no numbers in Article four, which is why we use the word "numerical". People may make substantively different things regarding what a "balance of sources and sinks" implies, as was shown in Fuglestvedt et al., 2018.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
30330	112	41	112	46	There are actually three additional targets in Article 4: peaking as soon as possible, undertake rapid reductions thereafter, and reach that balance. The full text reads: "In order to achieve the long-term temperature goal set out in Article 2, Parties aim to reach global peaking of greenhouse gas emissions as soon as possible, recognizing that peaking will take longer for developing country Parties, and to undertake rapid reductions thereafter in accordance with best available science, so as to achieve a balance between anthropogenic emissions by sources and removals by sinks of greenhouse gases in the second half of this century, on the basis of equity, and in the context of sustainable development and efforts to eradicate poverty." [Joeri Rogelj, Austria]	Noted. There are no numbers in Article four, which is why we use the word "numerical". People may make substantively different things regarding what a "balance of sources and sinks" implies, as was shown in Fuglestvedt et al., 2018.
18996	112	43	112	43	remove words after colon "these are" [Gwenaelle GREMION, Canada]	Editorial
18998	112	45	112	45	add "also" before "contain" [Gwenaelle GREMION, Canada]	Editorial
42068	112	48	112	48	SCLF not SLCP in this report? And methane is an SLCF but probably deserves particular mention in this context. [Keith Shine, United Kingdom (of Great Britain and Northern Ireland)]	Noted. We had inconsistent feedback on this point. Other LAs have argued that SLCF refers to a science concept, but to call them "pollutants" is more of a political expression. Hence we have gone with SLCFs throughout, which is also more consistent with LLCFs (both are forcers - we should use the same description).
30334	112	48	112	48	Please use a consistent acronym for short-lived forcers (SLCF). Methane is part of this group, so no need to repeat it. "SLCPs" are a political concept that refers to warming SLCFs only. [Joeri Rogelj, Austria]	Taken into account. Have made terminology consistent.
30332	112	48	112	49	This is policy prescriptive and a value judgment in itself. A balanced way to formulate this is to stick to the scientific facts, for example: "When expressed in GWP100, CO2-equivalent emissions of declining SLCF can have a sign opposite to their implied warming trend." (This is also only true if SLCFs decline markedly on timescales shorter than their lifetimes). [Joeri Rogelj, Austria]	Rejected. Observing that GWP100 gives a positive sign on warming instead of a negative sign is a mathematical matter, not a values claim.
33410	112	48	112	49	I find this sentence misleading or confusing. A pulse of methane emissions will have both a positive GWP100 and a positive impact on warming regardless of whether emissions are increasing, declining, or constant, would it not? [Marcus Sarofim, United States of America]	Noted. The sentence is not about pulse emissions. The point of the new metrics is that they show that a pulse emissions of LLCF can be equivalent to a sustained step change in SLCFs; but not a pulse emission of SLCFs. If a time-series of SLCFs implies constant emissions, then the level of warming is constant (ie no additional warming). If emissions are declining, then warming associated with that source is declining. This contrasts to stock pollutants, where the warming effects are cumulative in emissions.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
43662	112	48	112	49	This statement and following discussion fundamentally misconstrue what the GWP and other metrics aim to represent and what makes them right or wrong in a particular context. The issue is not about integrated metrics giving the wrong sign (this is simply incorrect since eg GTP also gives the 'wrong' sign in that sense). Every emission of methane makes the world warmer than if this emission had not occurred, and this is independent of whether emissions in general are rising or declining. Thus the sign for the warming caused by one individual tonne of methane emitted vs one individual tonne of carbon dioxide emitted is entirely correct. What the authors should be pointing to is the fact that if GWP (or any other pulse-emissions metric) is used to evaluate climate outcomes from a multi-year TRAJECTORY of emissions it gives increasingly incorrect answers about the cumulative temperature outcome from this time series of emissions. In other words, the issue is not about integrated metrics, but about using pulse emission metrics to ascertain climate outcomes from a continuous flow of emissions. That is a very different problem and limitation to the (in my view incorrect and unsubstantiated) claim that GWP gives the wrong sign about the contribution of SLCFs to warming. Every SLCF emission makes the world warmer than it would have been without that emission, regardless of what emissions are prior or later, and GWP and other pulse emission metrics are emminently useful and correct in their conclusions regarding the sign (and, subject to time horizon) magnitude of warming caused by each individual emissions pulse. [Andy Reisinger, New Zealand]	Taken into account. The issue is equivalence. In this paragraph we are showing one important way in which the customary metric, as well as others, fail to map to important climate impacts. This stream of work is important, and has been developed since AR5, which is why it is highlighted. Integrating metrics are particularly prone to this error, we think. We also make the point of GTP. We did not claim that SLCFs lead to cooling in a pulse sense, but said "when emissions are declining". Cooling is defined (in the Oxford English Dictionary) as a negative warming trend, so it seems a reasonable word to use. Nevertheless, we have tried to edit the text to add the point that a pulse emission can warm the world relative to the counterfactual world in which it had not been emitted.
30338	112	48	113	2	Please also include a discussion of which metric constrains temperature outcomes the most when single year targets are being achieved (as is the case in NDCs). [Joeri Rogelj, Austria]	Rejected. This is too specific a request for the current text. If we were to retain a box that compared metrics under a range of scenarios a range of difference performance measures could be applied.
30336	112	50	112	50	Please make the value judgment made in this sentence explicit, by explicitly clarifying in what way and under which circumstances this metric is "least erroneous". [Joeri Rogelj, Austria]	Not applicable. This section has been heavily edited, and the sentence has been removed.
33412	112	52	113	2	I recognize that this is a quote from Myhre et al., but I don't find it useful. Some people consider the term "Greenhouse gas" to be misleading in the same way, because greenhouses work by limiting convective transfer whereas GHGs work by changing radiative transfer, but I think it is a useful metaphor that's a quick way to elicit a mental mapping for the average person that is in the right ballpark (e.g., both work to trap heat). Honestly, cumulative radiative forcing and warming are even better mapped to each other than greenhouses and GHGs. The average policymaker would have no intuition about what "relative cumulative forcing index" would mean, whereas their intuition about "global warming potential" would be a decent approximation of what the GWP actually indicates. [Marcus Sarofim, United States of America]	Rejected. We think the quote is relevant, and it shows the heritage of this line of argument.
19000	112	53	112	54	Should “Global Warming Potential” be GWP to maintain consistency and prevent confusion? [Gwenaelle GREMION, Canada]	Rejected. Spelling out the acronym's claim to represent "warming" seems appropriate here.
45604	112	54	112	54	Myhre et al. (2013) -> Myhre et al. (2013b) [Øivind Hodnebrog, Norway]	Editorial
19002	112	54	112	54	Which Myhre et al. 2013 citation? [Gwenaelle GREMION, Canada]	Taken into account. Chapter 8, IPCC AR5.
25894	113	1	113	1	Since the name "relative cumulative forcing index" would be more appropriate, why not take the opportunity to change the name here, stating that the quantity formerly denoted as "global warming potential" is now denoted "relative cumulative forcing index RCFI". You could put "(formerly known as GWP)" whenever RCFI is used in the report to get the community accustomed to the new, more accurate name. Otherwise forever into the future we are obliged to note that the name Global Warming Potential may be somewhat misleading. Why perpetuate a misleading name? [Stephen E Schwartz, United States of America]	Noted, thank you. This was discussed at the LAM, but given that GWP is such an established term the chapter team was not in favour of introducing a new name at this point.
19004	113	4	113	4	Rewrite: “Longstanding critiques of the customary metric GWP have continued to develop. These critiques have been extended…” [Gwenaelle GREMION, Canada]	Taken into account. Text amended slightly.
43664	113	4	113	10	As per my comment on page 112 lines 48/49, the authors need to emphasise more clearly that the issue with GWP arises when the climate outcomes of trajectories (i.e. time series) of emissions are evaluated. The problem is not that GWP doesn't get the warming from an individual pulse emission right (it certainly gets the right direction, and the magnitude is just a question of what time horizon we care about), it's that GWP and indeed any metric that only compares pulse emissions by design is blind to what emissions occurred in the years prior to the emission in question, or emissions that will occur in future years. [Andy Reisinger, New Zealand]	Taken into account. We have rewritten the section for clarity, and would value further feedback on that.
53764	113	4	113	16	You may mention that AR5 did not recommend a metric but emphasized that choice of metric depends on the policy goal. [Jan Fuglestvedt, Norway]	Taken into account. We have edited the text.

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19006	113	5	113	5	What is meant by "preserving the distinction"? [Gwenaelle GREMION, Canada]	Taken into account. "preserve" changed to "draw"
19008	113	7	113	7	Comparing what about the greenhouse gases? [Gwenaelle GREMION, Canada]	Taken into account. "comparing the warming effects of greenhouse gases"
19010	113	7	113	7	Break up sentence after “have been developed” and “some of these” [Gwenaelle GREMION, Canada]	Accepted
19012	113	10	113	14	This sentence is verbose and vague. Avoid passive voice and the words "it" & "those" [Gwenaelle GREMION, Canada]	Editorial
9194	113	10	113	14	I would emphasize some practical aspects in the choice of metrics for climatic forcers. A wide application range where we need metrics is life-cycle type assessments for policy programs, technologies, agricultural practices, and many kinds of products. When planning and evaluating emission reduction projects that implement emission reductions for companies and states, we need forcer metrics. Thousands of people work with such projects in the world, and one of the best aspects of the most common weighting method, GWP100, has been that the method is easy to use, although many researchers have criticized the disadvantages of the method. The debate has also been triggered by the 100-year time horizon of GWP100. The main objective of the Paris Agreement is to limit the temperature rise to 1.5-2 degrees, which would require that the net emission are zero by the middle of the century, in about 30-40 years, not in 100 years. If the IPCC AR6 report describes new, more accurate metrics, it would also be helpful to outline how they fit into lifecycle-type assessments, and possibly, how they are applied. [Ilkka Savolainen, Finland]	Taken into account. We have attempted to discuss some aspects of the life-cycle assessment and metrics literature, but on the whole our focus is on the physical climate science relationship between gases under different metrics.
42074	113	10	113	16	This is a fascinating passage, particularly concerning the wrist-slap to AR4. But from my limited experience it misses a key aspect of the adoption of climate emission metrics in policymaking. One of the judgements that policymakers have to make is the relative value of continuity in policymaking versus "disruptive" changes. For many parties to UNFCCC treaties even the apparently small step of adopting GWP(100) values from more recent IPCC assessment reports has proven a step too far. I think this really illustrates the difficulties in adopting a quite different metric, even when there are compelling reasons to do so. There are various forces at work, including pragmatism, conservatism and downright stubborness/ignorance. [Keith Shine, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We have edited the section to improve clarity.
43666	113	13	113	14	While I applaud the author's willingness to challenge past IPCC conclusions, they should not distort them in order to make them easier to attack. The AR4 did not "recommend" the use of GWP, it simply said that it is a USEFUL metric to compare emissions. It might have been clearer to say that it is useful to compare pulse emissions (rather than time series of emissions), but more importantly, usefulness is not a recommendation. The same statement in the next sentence also drew attention to shortcomings especially when applied to SLCFs. GWP is no doubt useful if one's interest is in the impact of an individual pulse emission on radiative forcing for a time period of roughly the next 4 human generations. To the extent that climate policy deals with abatement choices and trade-offs between gases in individual years, I don't see much wrong with an IPCC report calling GWP "useful" to compare emissions of LLGHGs - especially since at the time of the AR4 there wasn't really a wide range of alternatives out there and GTP shares the same limitations as GWP in terms of giving the wrong direction of travel when applied to a time series of emissions. [Andy Reisinger, New Zealand]	Taken into account. We have edited the section and removed this sentence. However, we do not believe we are distorting AR4. The relevant reference in AR4 is "Thus, GWPs remain the recommended metric to compare future climate impacts of emissions of long-lived climate gases. "Section 2.10.1, P211. That is a clear and explicit recommendation regarding the use of metrics in policy. It contravenes IPCC's policy relevant but not police prescriptive mandate.
58146	113	13		14	Avoid criticising AR5 in this way. [Nathan Gillett, Canada]	Taken into account. The section has been edited and the sentence removed. But there is an issue here - there was an explicit prescriptive recommendation in AR4, which sits awkwardly with IPCC's non-prescriptive mandate. The relevant reference in AR4 is "Thus, GWPs remain the recommended metric to compare future climate impacts of emissions of long-lived climate gases." Section 2.10.1, P211. That is a clear and explicit recommendation regarding the use of metrics in policy.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
9196	113	14	113	16	Examples of situations cases where forcer metrics can be needed: 1. CO2 emissions from ships will be reduced by about 25% by moving from diesel fuel to Liquefied Natural Gas (LNG). However, 2-3% of the natural gas is leaked as methane into the air in the gas production, transport and use chain, depending on e.g. area and system (Alvarez et al. 2018). What is the net impact of CO2 emission decrease and CH4 emission increase in the middle of the century? 2. Lifecycle emissions from ruminant meat and milk products compared with vegetable protein sources. [Ilkka Savolainen, Finland]	Noted. This depends on the variable through which you wish to compare "net impacts". If it is temperature, then pulse metrics are likely to have serious limits that become evident if the emissions associated with the ship are considered on a multi-annual basis.
47632	113	19	113	19	As a key knowledge gap related to emission metrics (Section 7.7), it is currently unclear what kind of emission metric the scientific community can recommend for the Paris Agreement implementation, while the international political discussion is converging to GWP100 (to be negotiated until COP25 in December 2019). My observation is that the scientific community has not been able to propose a metric that is both technically sound (i.e. physically and economically correct) and practically useful. As far as I understand, GWP* (Allen et al., 2016, Nature Clim. Change, doi:10.1038/nclimate2998) reproduces peak temperatures by equating pulse emissions of stock gases like CO2 with sustained emisssions of flow gases like CH4, but it remains unclear to me how this metric can be applied to policies because greenhouse gas inventories and emission exchanges do not deal with sustained emissions. Section 7.7 discusses an even newer metric called Mixed-GTP, but it is still premature to evaluate this metric because there is no peer-reviewed literature on Mixed-GTP. On the other hand, there have been a few studies that looked into how existing metrics can support the Paris Agreement targets (Fuglestvedt et al., 2018, Phil. Trans. R. Soc. A., doi:10.1098/rsta.2016.0445; Tanaka and O'Neill, 2018, Nature Climate Change, doi:10.1038/s41558-018-0097-x). Both studies showed that GTP100 can lead to near stabilized temperatures if it is implemented to define the net zero GHG emission target. GWP100 can lead to temperature decline if it is used in the same way. However, the evidence is still limited in my view and more research is needed. I think it worth listing this open question for metrics as a key knowledge gap of this chapter. [Katsumasa Tanaka, Japan]	Not applicable - section removed.
15092	113	19	113	19	A key knowledge gap is how the climate system distinguishes the next Joule from all the others, so that the next W/m^2 of CO2 related forcing contributes well over 4 W/m^2 to the surface emissions while each of the W/m^2 concurrently arriving from the Sun uniformly contributes only 1.62 W/m^2 to the surface emissions. In other words, how can the incremental effect from the next W/m^2 be so much larger than the average effect of all W/m^2 considering the most basic rules of physics that Joules measure work, all Joules are equivalent and it takes work to warm the surface. [George White, United States of America]	Not applicable - section removed.
25896	113	19	113	35	Seems like a very short list. [Stephen E Schwartz, United States of America]	Not applicable - section removed.
13472	113	23	113	24	The rapid adjustments could be also a huge component of climate response (specifically in the atmospheric component of the climate system) in the case of some forcing agents such as BC. This is discussed in a paper that is recetly published in ERL by Modak and Bala (https://iopscience.iop.org/article/10.1088/1748-9326/ab21e7). This study also find that the efficacy of BC aerosols is only 0.69. [Govindasamy Bala, India]	Not applicable - section removed.
19014	113	36	113	36	Shouldn’t there be one key knowledge gap regarding section 7.7, i.e., the ability to translate physical terms like “radiative forcing” and “global warming” to a metric useful to policymakers? [Gwenaelle GREMION, Canada]	Not applicable - section removed.
30340	114	1	115	39	Really nice FAQ. I like the much simpler first couple of paragraphs and the more technical second half. Maybe this could be made visually clear by inserting a subheading? [Joeri Rogelj, Austria]	noted: Thanks, but the FAQ style does not use subheading.
33216	114	1	115	39	The language here is very informal at times, almost distractingly so. Is this intentional, and if so, can it be dialed back a bit? Also, of all the cloud feedbacks to highlight specifically in this FAQ, it seems odd to discuss the iris when this is still very much debated in terms of whether it even exists and its radiative impact. I disagree on multiple levels that "Climate scientists now believe that a reduction of anvil cloud area will occur in association with a greater clustering of convective storms with warming." In my opinion, other, better-researched feedback mechanisms for which substnatial progress has been made (i.e., tropical low cloud feedbacks) should be highlighted if this FAQ is to be titiled "What have we learned since IPCC AR5?" [Mark Zelinka, United States of America]	taken into account: The language style was intentionally made informal as FAQ plays an educational role. We may emphasize improved understanding of low-cloud feedback and discuss a bit less about the iris in SOD
47970	114	1	116	53	Chapter 7 FAQs could benefit with a question describing the different types of feedbacks (climate, biogeochemical, long-term) to be used as a science communication product and to help understand of the SPM key points on feedbacks. [WGI TSU, France]	noted: It has been explained in Box 7.1
47972	114	1	116	53	Chapter 7 FAQs could benefit with a question explaining ECS - considering the focus the topic could receive. [WGI TSU, France]	noted: It has been explained in Box 7.1
47974	114	1	116	53	FAQ 7.2: this is a technical question, not really suitable for a general audience. SR1.5 had a box on metrics including GWP* - could similar be apprpriate here? If the FAQ is kept could the title be rephrased to something like 'How do we compare the strength of different greenhouse gases?' [WGI TSU, France]	Taken into account. We have drafted a box on metrics for further consideration.
19018	114	3	114	3	Given that majority of the response is about the basics of clouds and cloud feedbacks, perhaps the question should read “Why are clouds a large source of uncertainty and what have we learned since IPCC AR5?” [Gwenaelle GREMION, Canada]	noted: This FAQ describes advances after AR5, which had explicitly stated that clouds are the largest source of uncertainty in climate feedback.

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16350	114	3	115	39	This FAQ is seems to be answering two questions: what is the effect of clouds on climate change, and what do we know now that we didn't in AR5. One way forward would be to divide it into two and have two FAQs on clouds. [Renee van Diemen, United Kingdom (of Great Britain and Northern Ireland)]	rejected: the two questions are so tightly coupled that they cannot be divided (first one was not actually a question but a basis to answer the second question)
19024	114	5	114	6	remove "in fact have made a tonne of progress and" [Gwenaelle GREMION, Canada]	Noted: We acknowledge that in CMIP5/6 models there is a compensation between ERF to doubling CO2 and the net climate feedback. A possible influence of this has been discussed in 7.5.1 and assessed if it is trustworthy from process point of view.
25898	114	9	114	16	"We see the reflections from these little drops of water as clouds. When the drops grow large enough, they can fall to the surface as rain. If they get cold enough, they can freeze to make ice crystals that can grow and fall to the surface as snow" Who is the audience for this language? The scientific community or third graders? [Stephen E Schwartz, United States of America]	noted: We provide FAQ for non-experts (but stakeholders) so it is written in plain language
19020	114	9	115	30	Better topic sentences would aid the reader. [Gwenaelle GREMION, Canada]	rejected: We do not understand how to respond to this vague suggestion.
19022	114	9	115	30	Are references back to the chapter section or citations needed? At the very least, I think some mention of where the reader may find this in the current IPCC report would be useful. [Gwenaelle GREMION, Canada]	noted: FAQ does not cite references but referred to 7.4.2 as a link to the chapter main document
18508	114	10	114	10	add: ["...condenses out of air"] on aerosol particles [Gwenaelle GREMION, Canada]	The sentence has been rewritten.
18510	114	12	114	12	add: ["...to the surface as snow"] or rain. Especially in mid-latitudes precipitation goes always over the ice phase regardless of if it ends up as snow or rain at the surface [Gwenaelle GREMION, Canada]	accepted
18512	114	14	114	14	the increasing number of campaigns with measurements from below, in and above the clouds or even ground based remote sensing, should not remain unmentioned. Suggestion: add ["... numerous satellites"] and aircraft-based instruments ["measured..."] [Gwenaelle GREMION, Canada]	accepted
18514	114	14	114	16	sentence should be more nuanced than just high and low clouds. Suggestion:The net radiative effect of a cloud depends on its properties like altitude, thickness, and the number of liquid droplets and ice crystals, but also on the albedo of the underlying surface. On a global average clouds reflect more sunlight than they trap thermal radiation, hence clouds tend to cool the climate. [Gwenaelle GREMION, Canada]	rejected: We preferred to explain the cloud radiative effects in a simple way, following FAQ7.1 of AR5
19026	114	16	114	16	Rewrite: "clouds cool the climate because they reflect more radiation than they trap." [Gwenaelle GREMION, Canada]	rejected: This sentence is simple enough in explaining the basic role of clouds in climate.
37824	114	19			Yes, clouds are one thing that shapes the circulation, but it should be added that circulation, with its associated fluxes of moisture, also influences cloud. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	accepted: We have added such sentences.
19028	114	20	114	20	remove "For decades it has also been known that" [Gwenaelle GREMION, Canada]	rejected: This sentence is necessary for articulating what was known in the past research.
15094	114	22	114	22	The only places biomass is mentioned is here and in one other place, always as a fuel being burned. Given the extreme concern over CO2, biology isn't mentioned once, yet biology and CO2 couldn't be more tightly coupled with a strong mutual feedback connection between them. More CO2 -> more biomass -> more decomposition -> more CO2 and which is evidenced in the paleoclimate record as it manifests the delay between temperature and CO2/CH4 seen in the ice cores. The delay arises because it takes time to sequester enough natural CO2 into the short term carbon cycle in order to sustain a larger biomass, even as a fraction of the carbon consumed is sequestered in long term carbon storage as fossil fuels and carbonaceous rocks. [George White, United States of America]	Noted. The carbon cycle and relevant feedbacks are assessed in Ch. 5
19030	114	24	114	24	Remove "predictably" [Gwenaelle GREMION, Canada]	Editorial
19032	114	26	114	26	"a challenge" --> "challenging" [Gwenaelle GREMION, Canada]	Editorial
19034	114	26	114	26	"has also been proposed" --> "has been proposed" [Gwenaelle GREMION, Canada]	Editorial
19036	114	26	114	26	"the more numerous" --> "more numerous" [Gwenaelle GREMION, Canada]	Editorial
19040	114	29	114	31	Citation needed [Gwenaelle GREMION, Canada]	rejected: normally FAQ does not cite references
19038	114	31	114	31	What is meant by "on balace"? [Gwenaelle GREMION, Canada]	Noted. "on balance" is synonymous for "when all factors are taken into account" (Oxford Dictionary)
19042	114	32	114	32	citation needed [Gwenaelle GREMION, Canada]	rejected: normally FAQ does not cite references

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19044	114	37	114	37	citation needed [Gwenaelle GREMION, Canada]	rejected: normally FAQ does not cite references
13464	114	38	114	39	Name a few ways in which clouds change: cloud fraction, cloud liquid water, vertical profile, ice/liquid ratio, etc [Govindasamy Bala, India]	accepted: a few words added
18516	115	2	115	2	replace "represented in fine detail" with "included in model runs". There are so many model studies which e.g., have fixed CCN/INP concentrations or use CCN/INP parametrisations which may practical or convienient, but not necessarily realistic. Therefore "fine detail" is in my eyes an exaggeration. [Gwenaelle GREMION, Canada]	accepted: The sentence has been revised.
19046	115	6	115	6	I believe 7.4.2 should be 7.4.2.4 [Gwenaelle GREMION, Canada]	Accepted.
25900	115	27	115	27	"Climate scientists now believe" maybe better "Some climate scientists now believe". [Stephen E Schwartz, United States of America]	Accepted. The sentence has been revised.
25902	115	38	115	39	I am not sure that this self congratulatory language is appropriate. Suggest just stick to the facts. [Stephen E Schwartz, United States of America]	Accepted. The sentence has been revised.
16348	116	1	116	1	The question could be interpreted differently based on the audience reading the question - 'I' could also imply what emission metric an individual should use to measure their impact. One way forward would be to rephrase the question slightly to focus on metrics for policy targets as the answer seems to address that topic. [Renee van Diemen, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable - FAQ 7.2 has been replaced
53040	116	1	116	53	The question "Which emission metric should I use?" is NOT purely a physical science question, but also involves questions of what the purpose the metric is being applied. This question cannot be answered purely in a WG I context. This box should be moved to a portion of the report where expertise across disciplines can be brought in. For example, integrated assessment of metric use has found that results for the CO2/CH4 tradeoff can be quite insensitive to the value of the metric actually used (e.g., Harmsen et al. 2016, doi:10.1007/s10584-016-1603-7 and a number of other similar journal papers). This sort of nuance is critically important to be applied to this question. Dealing with this purely as a physical science question does not serve decision-makers and can be misleading. [Steven Smith, United States of America]	Not applicable - FAQ 7.2 has been replaced
13584	116	1	117	14	Can we say that GWP accounts only for the radiative forcing and GTP represents the total response of the climate system and hence it accounts for both radiative forcing and feedbacks? If yes, it may be discussed explicitly [Govindasamy Bala, India]	Not applicable - FAQ 7.2 has been replaced
13470	116	1	117	14	The efficacy of forcing agents could be different and GTP could be better. This may be discussed when the shortcomings of GWP is discussed. [Govindasamy Bala, India]	Not applicable - FAQ 7.2 has been replaced
48804	116	1	117	14	The answer to this FAQ is very unclear. Can examples be used to explain the usefulness and the shortcomings of the different metrics. The problem and the power of using GWP? [Birgit van Munster, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable - FAQ 7.2 has been replaced
9404	116	1	117	14	The question is very policy relevant. However, the answer is difficult to understand. Given the temperature goal specified in the Paris Agreement and given the different character of long lived Climate forcers and short lived climate forcers: what would be the recommended metrics for both groups? [Klaus Radunsky Radunsky, Austria]	Not applicable - FAQ 7.2 has been replaced
43624	116	1	117	53	I have no problem with the main statements made in this FAQ (except one, see separate comment), but I suspect it to be rather impenetrable for people who are not experts in metrics and are not steeped in the jargon that is used. This FAQ should receive substantial support from policy and communications experts (after which it will be important that attempts to simplify and sharpen any messages have not changed the scientific balance). [Andy Reisinger, New Zealand]	Not applicable - FAQ 7.2 has been replaced
30346	116	7	116	22	I don't understand what is meant by this sentence, and infer from my own failure to understand the sentence that the broader public might also benefit from rewording. [Joeri Rogelj, Austria]	Not applicable - FAQ 7.2 has been replaced
30344	116	10	116	10	"Greenhouse gas accounting" instead of "carbon accounting". [Joeri Rogelj, Austria]	Not applicable - FAQ 7.2 has been replaced
30348	116	33	116	34	Neither the convention nor the Paris Agreement mentions temperature stabilisation, and this statement is thus imprecise. To avoid this the sentence could be rephrased to read: "The global climate change regime complex contains many elements, but greenhouse gas stabilization and capping temperature below a maximum limit have central roles." [Joeri Rogelj, Austria]	Not applicable - FAQ 7.2 has been replaced
13466	116	34	116	34	"Article 2 of Convention" I believe the reference is to "Article 2 of UNFCCC". Please make this clear. Same may be clarified when reference to Article 4 is made. [Govindasamy Bala, India]	Not applicable - FAQ 7.2 has been replaced
30350	116	44	116	47	I think this is inaccurate. Article 4 says the balance should be achieved in the second half of the century. Even if not extremely precise, it is quantitative i.e. between 2050 and 2100. [Joeri Rogelj, Austria]	Not applicable - FAQ 7.2 has been replaced
19048	116	49	116	49	remove “it has been shown repeatedly that the” [Gwenaelle GREMION, Canada]	Not applicable - FAQ 7.2 has been replaced
19050	116	49	116	49	“Temperature targets like those listed in the Paris Agreement” [Gwenaelle GREMION, Canada]	Not applicable - FAQ 7.2 has been replaced

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
43626	116	49	116	50	I feel that "poorly-matched" is too strong here and needs either qualifying or disclosure of underlying assumptions. Yes GWP does not predict temperature outcomes at all times with precision, but the same studies that show this also demonstrate that the amount by which of GWP gets global temperatures 'wrong' is much less than the extent to which poorly designed policy (e.g. policy that fails to address substantial shares of emissions) or insuffcently ambitious targets get temperature outcomes wrong (in the sense of failing to achieve the goal set out in the Paris Agreement). It is important that this FAQ keeps in perspective just how wrong it might be to use GWP compared to any other metric, within the range of choices e.g. across different emission scenarios for non-CO2 gases from the integrated assessment model literature. If realistic alternatives (rather than idealised thought experiments) are used, then using the 'wrong' metric is far less important than having insufficiently ambitious targets or policies (regardless of what metric those targets or policies are expressed in). [Andy Reisinger, New Zealand]	Not applicable - FAQ 7.2 has been replaced
30352	116	49	116	50	This statement makes too much of a generalisation to be acceptable. This statement is true if cumulative GWP-weighted CO2 equivalent emissions are compared to temperature increase, but not in other contexts. When used to define single-year benchmarks, GWP emissions can be shown to be much more precise in defining and constraining a temperature outcome than alternative trajectory based approaches like GWP*, which in turn are better if targets would be CO2-eq budget based. [Joeri Rogelj, Austria]	Not applicable - FAQ 7.2 has been replaced
19052	116	50	116	50	remove "it is" [Gwenaelle GREMION, Canada]	Not applicable - FAQ 7.2 has been replaced
19054	116	50	116	51	remove "that they" [Gwenaelle GREMION, Canada]	Not applicable - FAQ 7.2 has been replaced
30354	116	50	116	51	This statement is only true when considering cumulative emissions, not when considering annual emissions. [Joeri Rogelj, Austria]	Not applicable - FAQ 7.2 has been replaced
19056	116	51	116	51	should "cannot" be "can"? [Gwenaelle GREMION, Canada]	Not applicable - FAQ 7.2 has been replaced
30356	117	1	117	1	Insert "instantaneous" before "temperature effects". [Joeri Rogelj, Austria]	Not applicable - FAQ 7.2 has been replaced
13468	117	1	117	1	"Trajectory based approaches": Can an example be given here? Is "Target-based approaches" meant here? A clarification would be helpful [Govindasamy Bala, India]	Not applicable - FAQ 7.2 has been replaced
30358	117	2	117	4	Worded as such, this statement is wrong. It is only true when considering cumulative emissions. For annual emissions, the level of emissions expressed with GWP or similar metrics is much more closely related to the overall temperature outcome. Please correct. [Joeri Rogelj, Austria]	Not applicable - FAQ 7.2 has been replaced
30360	117	4	117	8	This discussion misses consideration of how well the various metrics perform when used to set single-year benchmark targets (as is common in the NDCs). [Joeri Rogelj, Austria]	Not applicable - FAQ 7.2 has been replaced
19058	117	14	117	14	Perhaps add a sentence about how political science or social scientists could aid in determining the best metrics. [Gwenaelle GREMION, Canada]	Not applicable - FAQ 7.2 has been replaced
18804	118	51	118	54	Remove the repeated text in this reference. [Gwenaelle GREMION, Canada]	Editorial
18792	127	3	127	3	The list of authors in this reference is incomplete. It is only listed one of five authors. [Gwenaelle GREMION, Canada]	Editorial
18778	136	31	136	31	Authors names are repeated in this reference. Authors are “Lewis and Curry”. [Gwenaelle GREMION, Canada]	Taken into account. Combine with #18776
18780	144	48	144	48	The reference is incomplete. The article is inaccessible [Gwenaelle GREMION, Canada]	Accepted: all submitted literature available on request from TSU
18782	144	53	144	53	The reference is incomplete. The article is inaccessible [Gwenaelle GREMION, Canada]	Accepted: all submitted literature available on request from TSU
48834	148	19	148	20	B.M.Smirnov J.Phys. D:Appl.Phys 51 214004 (2018) [Pekka Sunila, Finland]	Noted. See reply to comment #48890
25904	156	6	156	6	Table 7.A.1. Suggest add column for present forcing, W m-2. [Stephen E Schwartz, United States of America]	Reject: Present forcing is not relevant for the emission metrics.
25906	156	6	156	6	Table 7.A.1. Suggest much of this table can go into an Annex. The really important quantities are for CO2 through F12; Then simply state the present forcing by large categories, eg HCFC's [Stephen E Schwartz, United States of America]	Accept: This has been moved to supplementary online material
27246	156	6	168	3	Why SO2 is not in the list? It may be more abundant in the atmosphere than many others from the list. Since the molecule contains two different atoms, it absorbs infrared radiation and is, therfore, a greenhouse gas. [François GERVAIS, France]	Rejected: SO2 has negligible burden in the atmosphere. The reviewer has not provided any evidence for documentation of its radiative efficiency.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
50852	169	2	169	7	I assume that this figure is still under development - particulalry the graphical realisation needs further development. Some specific comments: text for TOA: the cooling and warming is linked to the imbalanc,e not balance.. So either use 'impbalance' or 'changes''.; text to the bottom right: not readable. If elements such for example forcing are included in such a shematic, they should be complete (e.g. only a ship and industry can be misleading to represent the whole CO2 foring); the satellite icon alone to adress the budget constraint is also not correct, and misses to hoghloght the importance of global observation programs such as Argo vital for this concept; And what platform type is shown in the ocean? And all Earth system components and the 'state-of-the-art knowledge need to be talen into account in such an approach: what about the land storage campeigns/estimates? For the cryopshere there is also much to say; ... There is particularly much new knowledge under the way on these topics, which needs to be assessed, and included in such an overview schematic. [Karina von Schuckmann, France]	Taken into account. The figure has been updated for clarity
6443	169		169		Figure 7.1: The quality of the figure does not allow reading the small text, e.g. the red text on the blue background. It is also difficult to understand from the figure how the advancements relate to what is shown in the schematic, e.g., why put the statement on the temperature response over the land surface, when the ocean is playing a key role. [Stephanie Fiedler, Germany]	Taken into account. The figure has been updated for clarity
27096	170	2	170	4	Figure 7.2 is useful, but doesn't quite do as the caption promises, i.e. link to the areas of the other working groups. A simple addition of some colour coding to denote the areas for each of the IPCC working groups would show nicely how they all relate to one another. [Chris Satow, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Agree, it has been redrawn
48576	170		170		In general the chapter is very nicely written however I feel Fig. 7.2 (page 170) schamiatic needs to be redrawn as it not clear. [Pushp Raj Tiwari, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Agree, it has been redrawn
26150	171	1	171	7	I do not like, and have never liked, these heat flow diagrams especially the figures on the lower right hand side. I do not believe the diagrams takes sufficient account of convective processes. OK someone has made the numbers balance but that doesn't make them right. However this image is too low res and fuzzy, so I unfortunately cannot read the detail on this therfore I cannot review it, which is a terrble disappointment. [Stephen Taylor, United Kingdom (of Great Britain and Northern Ireland)]	Noted, the quality of the figure displayed in FOD is much lower than the originals provided by the authors. The figure should be included in much better resolution in the SOD. The convective processes of sensible and latent heat flux are explicitly shown in the diagram, and balance the radiative energy surplus at the surface and the related atmospheric radiative deficit in the atmosphere.
6445	171		171		Figure 7.3 also needs to be revised for readability. [Stephanie Fiedler, Germany]	Noted, the quality of the figure displayed in FOD is much lower than the originals provided by the authors. The figure should be included in much better resolution in the SOD.
45586	172				This figure is hard to understand, it looks like a diagram of the ocean [Steven Sherwood, Australia]	NOT APPLICABLE - the figure has been removed.
25908	175	0	170	0	On the several panels, draw line segments with slopes corresponding to W m-2 [Stephen E Schwartz, United States of America]	Accepted. Thanks for the suggestion.
25910	175	1	175	1	The text in the box refers to ERF but the figure shows time integral of ERF, ERF being the slope. Suggest show ERF instead (or perhaps additionally, if the integral is considered necessary). [Stephen E Schwartz, United States of America]	Taken into account. Box 7.2 Text has been revised in the FGD. Figure showing historical ERF is included in Ch. 2.
25912	175	1	175	1	The quantties plotted are integrals, which do not reveal the time dependences of the several quantities very well. Suggest also plotting time series of the derivatives of these integrals to better illuminate the several quantities. [Stephen E Schwartz, United States of America]	Noted. The assessment of an energy budget requires the integral quantity expressed in joules. ERF timeseries are presented elswhere in the report.
25914	175	1	175	1	panels d and e; some indication of uncertainty seems essential. [Stephen E Schwartz, United States of America]	Taken into account. Figure revised to more clearly show the associated uncertainty for these timeseries.
25916	175	1	175	1	Panel a lower bound shows negative slope (climate system losing energy) over much of the time range and indeed net negative energy change between the beginning and the end of the time period examined . I find this hard to imagine. What is responsible for this? My guess is that this is some sort of mistake. [Stephen E Schwartz, United States of America]	Taken into account. The assessment and figure have been revised substantially.
18888	175	1	175	1	labels on figures (d), (e) and (f) are slightly difficult to read. [Gwenaelle GREMION, Canada]	Taken into account. Labels have been revised to increase readability.
25918	175	2	175	9	Data sources need to be specified. All quantities showin i n line graphs should also be tablulated, perhaps in supplemental. [Stephen E Schwartz, United States of America]	Taken into account. The underlying time series will be made available as part of the final report.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
38838	175	2	175	9	Please, in both the SOD and final versions of AR6 WG1, make a copy of all the data plotted in Box 7.2 Figure 1 available in an appendix, in the interests of transparency, adequacy of review and scientific progress. [Nicholas Lewis, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The data will be made available as part of the final report.
25920	175	4	175	5	Uncertainties represented by shadings; not clear in panel a uncertainty in which quanttity [Stephen E Schwartz, United States of America]	TAKEN INTO ACCOUNT. The figure has been substantially revised for the SOD.
25922	176	1	176	1	It seems essential to show radiative forcings associated with the two RCPs. [Stephen E Schwartz, United States of America]	Not applicable. Figure has been removed in the FGD.
25924	176	1	176	1	In addition to plots ofextensive quantity DeltaT shown, Suggest graphs of time series of intensive variable DeltaT/Forcing, which is pretty good approximation of transient sensitivity, to compare models and to assess change with time. [Stephen E Schwartz, United States of America]	Not applicable. The Figure has been removed in the FGD.
25926	176	1	176	1	I am pleased to see results calculated with 2 compartment energy balance model. Parameters of the model should be stated (transient and equilibrium senstivities and heat capacities of two compartments and, although not independent, time constants.) [Stephen E Schwartz, United States of America]	Not applicable. The text and figure have been removed in the FGD.
18890	176	3	176	3	it might be beneficial to include the other two scenarios (6.0 and 8.5), or if it is preferred to not include these scenarios, provide an explanation. [Gwenaelle GREMION, Canada]	Not applicable. Text and figure have been removed in the FGD.
25928	177	1	177	1	The value given in the text for F2x is 4.0 W m-2 yet the bar is well less than 4.0. Fix or explain. [Stephen E Schwartz, United States of America]	Accepted. Updated in the Second Order Draft: Smith et al results have been modified for the definition of ERF that includes land surface temperature changes.
18892	177	3	177	3	labels and text overlaid on the plots are difficult to read. [Gwenaelle GREMION, Canada]	Accepted. Updated in the Second Order Draft
25930	178	1	178	1	This figure, Fig 7.8, is enormously important. Climate sensitivity is independent of forcing agent, within 20%. Totally vindicates the forcing/response/sensitivity hypothesis. This figure also supports reporting climate sensitivity, as of models, in units of K/(W m-2), which acknowledges that ECS, TCS are geophysical properties of Earth's climate system, rather than being CO2 centric. I have been advocating this for several AR cycles, to no avail. I try once again. Maybe not this report, maybe not in my lifetime, but some day. In the meantime, I would suggest the use of two scales, one in units of K/F2x and one in units of K/(W m-2). [Stephen E Schwartz, United States of America]	Noted - thank you for positive comments. Not terribly clear that scale of K/F2x adds to the comprehension.
25932	178	1	178	1	Why such large uncertainty on CFC 11? Discuss. [Stephen E Schwartz, United States of America]	Rejected: It is not necessary to discuss each species in this figure in detail. The figure is purely to illustrate that the alphas are similar between species.
25934	178	1	178	1	I call your attention to the fact that 1/alpha (and hence alpha) are positive quantities, as I advocate, but not consistent with the defintion of alpha given at page 7-10. Oh what a tangled web we weave when we mess with definitions. [Stephen E Schwartz, United States of America]	Taken into account. Figure updated in SOD to be negative alpha
6447	180		280		Figure 7.10: Define in the caption the difference between the satellite-based estimates and the observational constraints. I also suggest to add a best estimate for the aerosol ERF in Fig. 7.10a, and write ERFari and ERFaci instead of aerosol and cloud in Fig. 7.10b. These would make the content clearer. [Stephanie Fiedler, Germany]	Accepted - figure improved
25936	181	1	181	2	Fig 7.11 If more than one cmip model forcing is the same, spread the circles vertically to show this. [Stephen E Schwartz, United States of America]	Taken into account. Model estimates now removed, they were causing confusion
25938	181	1	181	2	Fig 7.11 Give estimated values and associated uncertainties for ERFari and ERFaci separately, and also for sum. The reason for that is that ERFari and ERFaci will be correlated so the uncertainty should reflect that correlation. [Stephen E Schwartz, United States of America]	Taken into account. ERFari and ERFaci uncertainties in fig. 7.8.
25940	181	1	181	2	Fig 7.11 spread in aerosol forcing by cmip models seems to greatly exceed spread in total anthro, which is hard to understand unless there is anti correlation between aerosol and ghg forcing, which is also hard to understand; needs to be discussed. [Stephen E Schwartz, United States of America]	Taken into account. Model estimates now removed, they were causing confusion
25942	181	1	181	2	Fig 7.11 Specify in caption meaning of uncertainties [5-95]? [Stephen E Schwartz, United States of America]	Accepted.
25944	181	1	181	2	Suggest give cmip forcings 1750 - 2017 to give better comparison. If the cmip numbers are not available, then suggest adjust them by appropriate amounts at each end of range, stating how they were adjusted and show adjusted numbers in figure and appropriate table for interested readers (could be supplemental). [Stephen E Schwartz, United States of America]	Taken into account: The CMIP6 forcings have been removed from this figure
33442	181	1	181	8	I am hoping that in addition to this figure, there will be a figure like 8.17 from AR5 (with supporting table 8.SM.8): I find these estimates of the current day forcing attributable to historical emissions of each greenhouse substance to be very useful. [Marcus Sarofim, United States of America]	Taken into account: This figure is being developed by chapter 6.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
25946	182	1	182	11	These figures are very misleading at best and probably wrong, as they do not take into account the covariance (entanglement) of aerosol forcing and sensitivity. I think it would be hard to do this in one figure but should be possible in three panels, one for best estimate aerosol forcing and sensitivity; one for high magnitude aerosol forcing and high sensitivity; one for low aerosol forcing magnitude and low sensitivity. Assume aerosol forcing magnitude is low; then sensitivity must be low; so temp increase due to ghgs will be roughly equal to obsed temp increase about 1 K. On the other hand, if aerosol forcing magnitude is large, sensitivity must be high, so temp increase due to ghg's will be about 3 K, with an offset by aerosols of about 2 K. If that were shown, this could be an enormously vauable figure, illustrating the Faustian bargain (Hansen and Lacis, 1990) and the consequences of abrupt cessation of aerosol forcing (Armour and Roe, 2011; Schwartz, 2018) [Stephen E Schwartz, United States of America]	Taken into account. The calculations have been redone to be more of a bottom up estimate. The approach to uncertainty has been clarified
25948	182	1	182	11	How can the total anthro contribution to temp change range from 0.8 to 2.15 K as shown in graph, when the obsd increase is 1.0 ± 0.2 K? this just makes no sense. Suggest revise figure to be constrained by observations, somehow indicating the cancellation that results from GHG and aerosol. This might require several panels, as the sensitivity is related to the aerosol forcing by the observed Delta T constraint. . [Stephen E Schwartz, United States of America]	Taken into account. We agree and have considered how to explain the figure as a bottom up estimate and put into the context of Chapter 3 attribution
25950	182	1	182	11	Suggest same format for both panels, with the underlying model clearly indicated in each panel. [Stephen E Schwartz, United States of America]	Not applicable - figure revised and bottom panel removed
54824	182				This figure is remarkably confidently attributing temperature contributions to individual forcings even contrails! This disagrees in level of rigour quite dramatically with the usual attribution barchart and at the minimum needs clear explanation but I would be cautious here - this is just the RF implication right? it does not account for possible enhancing/diminishing factors eg in dynamics and all the othe rthings attribution does [Gabriele Hegerl, United Kingdom (of Great Britain and Northern Ireland)]	Noted. It is made clear that this is not a statistical attribution but a bottom up estimate of response
25952	183	1	183	1	It is hard to understand how temp observations are constraining GHG contribution to temp increase in panel c. Explain. Is this on the forcing side or the sensitivity side? Seems unlikely on forcing side because of panel b. So must be on the TCR side, panel a. But given uncertainty in ECS and the slight difference between initial and constrained distribution in panel a, it is hard to see how there can be much if any constraint at all on TCR from temp change observations. Much more detail explanation is requd in text. [Stephen E Schwartz, United States of America]	Not applicable. This part of the diagram has now been removed
25954	184	1	184	1	Give uncertainties by bars and whiskers at the right of the time series as in Figure 8.18 of AR5. [Stephen E Schwartz, United States of America]	Taken into account. The figure has been redrawn for clarity. Uncertainties are presented in Figure 7.10
42756	184		185		Figures 7.14-7.15 - Very clean and useful overall, consider making lines slightly thicker (especially in legend) to make identification easier. Consider also including short summaries of categories in caption and/or legend, such as total natural (volcanic + solar). Doesn't need a lot of detail, but anything that helps a graph and caption stand on their own better makes for better comprehension. [Stephanie Courtney, United States of America]	Noted. Thank you for the feedback. Figure 7.14 has been transferred to chapter 2 - taken on board for 7.15 (now 7.12) and designed in conjunction with TSU best practice.
25956	185	1	185	1	This figure is terribly misleading as it does not convey the consequences of uncertainties. Because of the anticorrelation between total forcing and transient climate senstivity under the constraint of observed temperature increase over the instrumental record (Kiehl, 2007; Schwartz, 2018) these quantities are not independent. I refer you to Figure 1 of Schwartz, 2018. What might work rather well here is a three panel figure with one panel for high, best-estimate, and low-forcing with the corresponding transient sensitivities. The important point, and it is terribly important, is that a high magnitude aerosol forcing requires a high sensitivity, and vice versa. That is totally missing from the figure. Kiehl, J. T. (2007), Twentieth century climate model response and climate sensitivity, Geophys. Res. Lett., 34, L22710, doi:10.1029/2007GL031383. Schwartz, S. E. (2018). Unrealized global temperature increase: Implications of current uncertainties. J. Geophys. Res. Atmospheres, 123, 3462–3482. https://doi.org/10.1002/ 2017JD028121 [Stephen E Schwartz, United States of America]	Rejected. The figure is designed to show the best estimate of response, the two layer model used takes autocorrelation into account but note there is zero autocorrelation in CMIP6 models so the references cited are out of date
25958	185	1	185	1	Another important reference that speaks to what they denote as the "entanglement" of aerosol forcing and sensitivity (nice phrase that might be incorporated into a discussion of the phenomenon) is Xu, Y. and Ramanathan, V., 2017. Well below 2 C: Mitigation strategies for avoiding dangerous to catastrophic climate changes. Proceedings of the National Academy of Sciences, 114(39), pp.10315-10323. [Stephen E Schwartz, United States of America]	Rejected. The figure is designed to show the best estimate of response, the two layer model used takes autocorrelation into account but note there is zero autocorrelation in CMIP6 models so the references cited are out of date
25960	186	1	186	1	Something appears amiss here. First column gives total approx -1.2. It would seem that total is sum of succeding bars,approx -0.7, + 1.1, + 0.3 +0.4, +0.1, with total being +1.2; At minimum this apparent discrepancy needs to be explained. Also if uncertainties are similarly additive, it would ssem that uncertainty in cloud alone exceeds uncertainty in total. [Stephen E Schwartz, United States of America]	Taken into account. It was confusing that the Planck feedback was shown with a 1/4 factor to fit the range of others. This figure has been revised.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
33218	187	1	187	9	"less clouds" is not grammatically correct, I think. It should be "fewer clouds" or "reduced cloud fraction". [Mark Zelinka, United States of America]	Taken into account. Fixed, thanks.
19120	189	3	189	11	The caption for panel a) does not provide sufficient information for a correct reading of the figures. What are bold black and red lines? Furthermore, the caption provide for panel b) is misleading: can you be more precise on what do you mean for "elsewhere"? [Gwenaelle GREMION, Canada]	Taken into account. This figure has been revised.
6449	189		189		Fig. 7.19a: Increase the font size of the small text in the figure. [Stephanie Fiedler, Germany]	Taken into account. This figure has been revised.
49548	189				I was wondering if the c) and d) maps should not use the same scale, e.g, extending -1.5 to +1.5 for both maps [Zbigniew Klimont, Austria]	Rejected. One shows warming over historical warming while the other shown warming under abrupt CO2 quadrupling, so their magnitudes are expected to be different. The important feature, as highlighted in the text, is that the spatial pattern of warming is different; this is best seen with colorbars spanning different ranges for each given their different maximum warming rates. Note that this figure has been revised.
33220	190	1	190	1	I find it confusing/misleading to label panel (d) with "historical" when it is actually the value from year 100 of the 1% per year CO2 run. [Mark Zelinka, United States of America]	Taken into account. Fixed.
28118	190	1	190	23	colors in panel a should be identified: Different models? Suggest different models in other panels be identified. [Stephen E Schwartz, United States of America]	Not applicable. This figure has been revised.
25962	191	1	191	1	Abscissa scale not clear; is this temp change relative to preindustrial? So if preindustrial were, say 13 C, then 10 means 23 C? The range on this seems totally beyond comprehension; Up to 23 degrees above preindustrial. That would be a different planet for sure. But why are there no points going to zero, corresponding to preindustrail. And temp change so far rel to pre industrial is only about 1 degree. So no data in the range 0 to 1 C on the abscissa scale. On the other hand, maybe the scale should be labeled Actual global mean temp, (not relative to PI); in which case we are interested in range 13 to 16 for PI to present, and LGM going to perhaps 9 to 5 on the scale, which would make a lot more sense. but all that said, for present sensitivity 3 ± 1.5 K for CO2 doubling, alpha would seem to take range (-4/ECS) or -0.9 to -2.7, which swamps the entire range shown. So all this needs to be explained. [Stephen E Schwartz, United States of America]	Taken into account. The x axis has been explained more clearly now in the caption.
9624	191	4	191	9	It is unclear how the preindustrial baseline is defined (shown on the x-axis). [Katarzyna (Kasia) Tokarska, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The x axis has been explained more clearly now in the caption.
25964	192	0	192	0	This figure is difficult/impossible to understand, in a variety of ways. First the choice of abscissa. Why is F_2x the independent variable. In chapter 7, page 79, line 6 it is stated that it is taken as 4± 0.7 W m-2. But the response of the climate system, as reflected in the ordinate quanttiy (so-called climate feedback parameter, better climate response parameter) which being a geophysical quantitiy should not depend on the magnitude of forcing of doubled CO2. [Stephen E Schwartz, United States of America]	Taken into account. The figure has been extensively revised for the second order draft.
33222	192	1	192	12	Please explain how the location of the thick and thin curves in the ellipse are determined (i.e, the path that they trace out) [Mark Zelinka, United States of America]	Taken into account. The figure has been extensively revised for the second order draft.
6451	195		195		Fig. 7.25: Lines instead of shading would be better to show the difference between CMIP3+5 and CMIP6. [Stephanie Fiedler, Germany]	Taken into account. A new version of the figure was prepared.
25966	199	1	199	1	The mixed step vs pulse is an utterly confusing quantitiy and should be nipped in the bud. Just present AGWP etc for each substance and let the user deal with those quantities. [Stephen E Schwartz, United States of America]	Rejected. We think it is important to highlight the step pulse distinction in WGI because, along with multi-metric approaches it represents a significant innovation in the relevant physical science literature. We should make that clear. There are ways policymakers can deal with the stock-flow distinction - they do so in the face of pollutants ranging from cigarette smoke to lead pollution. We should not omit this material because we are under the (mistaken) impression that policymakers will be confused by it.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
25968	200	0	200	0	This is a very glitzy figure but the glitiziness detracts from information transmittal. Why not square the figures up and present two panels like we are accustomed to. And why brightness temperature; why not W m-2? And provide a color bar. Otherwise its just eye candy that has no place in this report. And at the end of the day, what is the point of the figure? That the model is doing a pretty good job? Then show a third panel with differences in W m-2 so that the reader can have a sense of that; Is it meant to show short range spatial variation, that is not captured in lower resolution models? Then show another panel with lower resolution and yet another panel to show the difference. Is it meant to show the difficulty that attaches to spatially integrating such short range variation? Then that point should be made by showing integrals that reflect this. Is it meant to show the large dynamic range in OLR. The scale bar would help make that point. [Stephen E Schwartz, United States of America]	Taken into account. The figure has been extensively revised for the second order draft.
18518	200	1	200	1	image quality is poor, the pseudo-3D does not help to illustrate the message of the figure. No legend. A four panel style may help the viewer to compare the satellite observations and model results. Also the monthly- and zonal crossection from satellite and model are enough to deliver the message, that satellite and model are in good agreement. Assuming that the goal is to show the reduction of cloud related uncertainties since AR5, then a global map showing the global distribution of the cloud-related uncertainty or model-observation discrepancy may be worth a consideration [Gwenaelle GREMION, Canada]	Taken into account. The figure has been extensively revised for the second order draft.
30342	200	1	200	9	Maybe a schematic with the various cloud types and their effects and level of understanding could work here. [Joeri Rogelj, Austria]	Taken into account. The figure has been extensively revised for the second order draft.
13462	200	6	200	6	Expand NICAM [Govindasamy Bala, India]	Taken into account. The figure has been extensively revised for the second order draft.
19300	4-00	24	4-00	24	To avoid confusion between SLCFs and what you mean by short-lived species in the text, I suggest you delete “short-lived “, and just say “it is most useful for species with lifetimes much less than the time horizon of the metrics”. [Gwenaelle GREMION, Canada]	Not applicable. Section has been extensively revised.
19298	4-00	38	4-00	38	It should be “emission metrics” instead of “climate metrics”, following the definition given in p.8 of this chapter. [Gwenaelle GREMION, Canada]	Not applicable. This comment didn't come with any page/line numbers, so difficult to tell what it refers to
19296	4-00	41	4-00	44	The value of newly-assessed radiative efficiency of CO2 is the same as the value of the former assessment. However, the paragraph says that the computed radiative efficiency of CO2 has increased, which seems contradictory… [Gwenaelle GREMION, Canada]	Taken into account: This has been clarified in SOD
19308	4-00	54	4-00	54	You can make the sentence shorter by writing : “(...) to derive a sea level rise (SLR) response function RSLR(t) to either radiative forcing or global surface temperature”. [Gwenaelle GREMION, Canada]	Taken into account. Paragraph rewritten for clarity.
44770	all				While the chapter makes ample reference to AR5, references to SR15 and SR on Land and Oceans are completely missing and should be added in SOD. [Astrid Kiendler-Scharr, Germany]	Noted.
44772	all				The information provided in Box 7.1 is repeated at multiple places in the chapter. Check where and to what extent this is really necessary. [Astrid Kiendler-Scharr, Germany]	Noted.
44774	all				Resolution is too low for many of the figures, making it impossible to read some of the annotations. [Astrid Kiendler-Scharr, Germany]	Taken into account. The figure quality was improved for the second order draft.
44782	all				ensure that abbreviations are spelled out at first mentioning (e.g. ECS in Box 7.1) [Astrid Kiendler-Scharr, Germany]	Taken into account.
18916	DsF6421Q				IA: I found the graph very useful and very well-represented. It would be great if you add “download CSV” option to each graph, so everybody can reproduce the graph and use the data in their analyses - that would make the work of scientists so much easier! [Gwenaelle GREMION, Canada]	Noted. Thank you for the positive comments. Unfortunately it was not clear which figure was being referred to here. It is not within the remit of the chapter author team to provide a “download CSV” option for each graph, but this feedback will be passed on to TSU to take into account for the web-based interface to AR6. We do intend in chapter 7 to make all figure data available following publication of the report.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
45580					It seems odd for this chapter to first present a line of reasoning leading up to an assessed range of ECS and TCR, which depends on GCMs, and then have a separate section on model evaluation. It would be far more logical to do it the other way around, would't it? Why would we accept the ECS or feedback values in GCMs if we haven't yet evaluated them? Isn't much of the previous discussion effectively model evaluation (e.g., the emergent constraints are effectively a targeted model evaluation? Wouldn't a natural constraint on ECS be to select GCMs that give the right historicl warming under reasonable aerosol forcing?) And surely the material in Section 7.6.1 would be important to consider before making an assessment of ECS based at least in part on GCMs. [Steven Sherwood, Australia]	Taken into account. In response to this and other comments the climate model line of evidence has been removed.
45584					I am thoroughly confused about how the authors obtained a pdf, or likelihood ranges, for ECS. The latter seem to be first given in 7.3.5.3 with a very cursory explanation and references to "prior" and "posterior" quantities. Later, ranges are given based on each of the lines of evidence (often without being explicit that those ranges are evidently based only on that evidence). Then a final range is given in the summary section 7.5.7 that is different from the one given in 7.3.5.3. There are discrepant "likely" ranges everywhere. The methodology for deriving a final pdf is not explained anywhere that I can find and is not a straightforward problem. [Steven Sherwood, Australia]	Taken into account. The Synthesis section, formerly 7.5.7, contains a description of method applied. The text has been revised.
45588					The executive summary starts out with the most boring findings and ends with the most interesting. Is that really what you want to do? Surely the most interesting results are the revised ECS, TCR and aerosol forcing numbers, new finding that feedbacks will become more positive in the future, and statements that polar warming anomalies are understood. [Steven Sherwood, Australia]	Taken into account. We have added better signposting in the ES in terms of themes
54832					Overall very nice and interesting chapter I didn't manage to read it all but focused on sensitivity [Gabriele Hegerl, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Thank you for the positive feedback!
11644					A self-regulation mechanism has been identified in the climate system, which to my knowledge, is properly assessed by the IPCC WG1 report. It is observed that the temperatures at 500 hPa are almost always bracketed between -42C and -3C in the northern hemisphere. The cold limit occurs despite these cold temperatures being reached in November even before the solstice is reached. On the warm limit, despite air being situated at times over hot dry tropical deserts, such as the Sahara, it does not get warmer than that value. The only exception is very limited, and is found in hurricanes where it can exceed 0C. 500 hPa is used as this is well recognized in synoptic meteorology as the best standard pressure level to assess synoptic weather patterns including extratropical cyclone development. We investigated this issue in these papers Chase, T.N., B. Herman, R.A. Pielke Sr., X. Zeng, and M. Leuthold, 2002: A proposed mechanism for the regulation of minimum midtropospheric temperatures in the Arctic. J. Geophys. Res., 107(D14), 10.10291/2001JD001425. http://pielkeclimatesci.wordpress.com/files/2009/10/r-246.pdf Tsukernik, M., T.N. Chase, M.C. Serreze, R.G. Barry, R. Pielke Sr., B. Herman, and X. Zeng, 2004: On the regulation of minimum mid-tropospheric temperatures in the Arctic. Geophys. Res. Letts., 31, L06112, doi:10.1029/2003GL018831. http://pielkeclimatesci.wordpress.com/files/2009/10/r-270.pdf Herman, B., M. Barlage, T.N. Chase, and R.A. Pielke Sr., 2008: Update on a proposed mechanism for the regulation of minimum mid-tropospheric and surface temperatures in the Arctic and Antarctic. J. Geophys. Res.-Atmos., 113, D24101, doi:10.1029/2008JD009799. http://pielkeclimatesci.wordpress.com/files/2009/10/r-339.pdf Chase, T. N., B. M. Herman, R. A. Pielke Sr., 2015: Bracketing mid-tropospheric temperatures in the Northern Hemisphere: An observational study 1979 - 2013. J. Climatol. Wea. For., 3,2,http://dx.doi.org/10.4172/2332-2594.1000131. The IPCC WG1 should report on this issue, as it provides a negative feedback on the atmosphere warming due to added greenhouse gases. Unless 500 hPa can warm above this cold limit, for example, extratropical cyclones and other polar front dynamics will not change much. [Roger Pielke Sr, United States of America]	Rejected. The topic of these papers, having to do with climatological mid-tropospheric temperatures and their relationship to the jet stream and barotropic storm dynamics, is not within the scope of Chapter 7, which covers Earth's energy budget and feedbacks. The mechanisms hypothesized within these papers to regulate the mid-tropospheric temperatures relate to moist-adiabatic convection, and thus are included in climate models and implicitly included in the assessment of the lapse-rate feedback within this chapter.
36500					Overall I found this chapter to be well written, with lots of new science since AR5 assessed, and updates on AR5 assessments. It generally has appropriate treatments of uncertainties, and a good balance of thoroughly reviewing the literature, and drawing out assessment conclusions. [Nathan Gillett, Canada]	Noted. Thank you for the positive feedback!
36502					I think the ERF framework would benefit from further discussion clarifying which biogeochemical feedbacks are included within ERF and which are not. For example, the discussion of the ERF of land use change discussed a possible contribution of LUC-induced aerosol changes to the ERF of land use change. But, for example, there is literature on the affects of methane on sulphate aerosol concentration (e.g. Shindell et al., 2009; https://science.sciencemag.org/content/326/5953/716) - if such feedbacks are included for LUC, then they should also be included for methane. Or changes in halegonated species, methane, N2O, CO2 can affect stratospheric and tropospheric ozone concentration through processes which are not mediated by surface temeprature. Practically I think it would make sense to exclude biogeochemical feedbacks via the concentrations of other species from the ERF calculations. [Nathan Gillett, Canada]	Accepted: Biogeochemical feedbacks have been discussed in greater depth in the chapter in the FGD, and assessments in Ch. 5 and Ch. 6 are referred to where appropriate.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
36504					I think the use of an EBM to convert radiative forcing changes to temperature changes is a useful way to communicate the affects of changes in radiative forcing. But for several reasons I would argue against calculating and reporting observationally-constrained attributable warming estimates, such as those shown by the solid lines in Figure 7.13. Firstly, this appears to be out of scope of Chapter 7, and rather in the scope of Chapter 3. Second, the distributions derived in this way are artificially narrow, because they use observed global mean temperature changes as a constraint multiple times. Observed GSAT evolution is used as a constraint on the Ch 7 assessed ranges on TCR, ECS and ERF (via the aerosol contribution). Then the authors again use the observed GSAT evolution to contrain these distributions via an EBM fit, as shown in 7.13. The fitting procedure is not fully described, but I suspect that it includes the assumption that the prior distributions are independent from the observed evolution of GSAT, which is applied as a constraint. For this reason, it likely underestimates the uncertainties. This could be addressed by simply removing the posterior curves from Fig 7.13c, and taking out the corresponding discussion. [Nathan Gillett, Canada]	Taken into account. We have discussed this extensively with chapter 3 and now show unconstrained temperatures. Text now distinguishes approaches
36506					This reads like an attribution of observed surface temperatures, whereas it is in fact the simulated temperature response to assessed ERF changes. I suggest moving this bullet to pg 6, ln 14, after ERF has been introduced, and indicating that these changes in ERF can be translated into surface temperature using a simple model, and then giving these results. [Nathan Gillett, Canada]	Accepted. Agree, the test is now clarified
56988					Figure general comments Chapter 7: ideally, figures should be a bit more independent from the caption => Titles can be added to the figure to enhance the understanding at first glance //Figures and caption should be more independent from the main text => spell out acronyms in figure and/or caption wherever possible //units have to be in ( ) and not in [ ] and font is Arial. For more information about Visual guidelines, please refer to the IPCC visual style guide (https://www.ipcc.ch/site/assets/uploads/2019/04/IPCC-visual-style-guide.pdf) [WGI TSU, France]	Taken into account. All figures edited for the second order draft.
56990					Figure 7.1: the schematic could benefit from a more engaging design. For more guidelines, contact the TSU's graphic officer [WGI TSU, France]	Taken into account. The figure has been updated .
56992					Figure 7.2: the schematic could benefit from a more engaging design. For more guidelines, contact the TSU's graphic officer // a title (like in Figure 7.3) would help understand quickly what the schematic is all about // the association with WGs, as stated in the caption, is not present in the figure [WGI TSU, France]	Taken into account. We have designed a more engaging figure.
56994					Figure 7.5: font should be Arial [WGI TSU, France]	Taken into account. Figure font has been harmonised in the FGD.
56996					Figure 7.6: Ideally, reflected whortwave, emitted longwave, net TOA flux could be added next to the respective panel label (which are not correctly labelled at the moment). See fig 1 box 7.2 for example// TOA should be spell out in caption at least. [WGI TSU, France]	Not applicable - the figure was replaced in the second order draft.
56998					box 7.2 fig 1: Ideally, OHC should be spelled out [WGI TSU, France]	Not applicable - the figure was replaced in the second order draft.
57000					box 7.2 fig 2: According to the visual style guide, the color shading for RCP 2.6 and 4.5 should be RGB 146 197 222 and 67 147 195 respectively. [WGI TSU, France]	Taken into account. This has been addressed in the FGD.
57002					Figure 7.7: ERF and IRF should be spell out in caption and/or (ERF) and (IRF) added next to legend in panel (a). [WGI TSU, France]	Taken into account.
57004					Figure 7.8: This figure would benefit from a short general title right on top of the panel, if any comes to mind (e.g. climate feedback parameter (α) // ideally a short label explaining that the numbers in the figures are "number of model analyzed" could enhance comprehension at first glance [WGI TSU, France]	Taken into account.
57006					Figure 7.9: Here some comments in case the figure can be later modified (it seems to be an already-published figure): it is unclear how the small panel relates to the 1990-2010 section of the "total" curve. At the moment, it seems like the total curve somehow relates to the OsloCTM3 curve since they are in the same color. It is also not easily visible that the small panel is linked to this section of panel (a) (dotted black line not ideal here) // it is not clear what is the difference between dotted lines and full lines of the same color. [WGI TSU, France]	Not applicable - figure removed
57008					Figure 7.10: is always better to have full words in the figure instead of Scat, amt, absorption (this saves space in the caption as well). [WGI TSU, France]	Taken into account.
57010					Figure 7.11: The substance of this figure is confusing: how can the graphic report on ERF from the last 150 years without a change of value over the years? Is this the mean ERF of the period 1750-2017? [WGI TSU, France]	Taken into account. We believe from the reviewer's comment that it is not the substance of the figure that needs changing but the figure caption and title, which has been done to emphasise the change in 1750-2018 ERF.
57012					Figure 7.11: does the color difference (red orange yellow) in the positive ERF section carry a meaning? it seems like CH4 is "more" than N2O, which is "more" than Halogen, regardless of the ERF absolute value. Visually, the reader uncontiously understand that importance decreases from red to yellow. see figure 7.12 for example. // the total anthropogenic bar should be in red as well - currently it seems like there is a connection with halogens. [WGI TSU, France]	Taken into account. Figure updated.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
57014					Figure 7.12: yellow shades could be removes (e.g. fig. 7.11). Figure 7.12 and 7.11 should have the same design. [WGI TSU, France]	Taken into account. The figures (what is now 7.9 and 7.11) have been updated.
15288					I find the section about ECS extremely interesting and well argued. I had several comments but they all related to the emerging CMIP6 models' ECSs so I decided to hold off when I saw the italics on page 95. I am left with just small points and *I think* calling out an ambiguous statement involving probabilities. [Claudia Tebaldi, United States of America]	Noted. We look forward to more comments on the second order draft.
57016					Figure 7.13: ECS and TCR should be spell out in caption [WGI TSU, France]	Not applicable. The figure has been removed.
57018					Figure 7.17: are some annotations superfluous? The schematic could be simplified and less cluttered by keeping only the necessary information to understand cloud fregimes and/or that is present in the main text [WGI TSU, France]	Taken into account: The figure was revised in SOD.
57020					Figure 7.18: it is unclear how stratocumulus and trade cumulus relate to 6 LES simulations [WGI TSU, France]	Not applicable: This figure has been dropped in the FGD.
57022					Figure 7.20: legend for the different colors is missing in (a) // font should be Arial [WGI TSU, France]	Not applicable. Sub-figure needing labels have been removed.
57024					Figure 7.21: alpha refers to "radiative feedback " (fig 7.20), "climate feedback parameter" (fig 7.8) and "feedback parameter". If alpha is the same parameter in all these figures, it would be good to have a standardized name to refer to it. [WGI TSU, France]	Taken into account. The chapter uses "feedback parameter" consistently in the FGD.
57026					Figure 7.24: ECS and TCR should be spell out in caption // font should be arial [WGI TSU, France]	Taken into account. Spelled out, and font has been corrected in the FGD.
57028					Figure 7.26: font should be arial // color scheme should ideally be changed to highlight the negative vs positive contribution inputs rather than highlighting different catergories (labels are already here, so colors are redundant). Suggestion: use red for positive and blue for negative, as it was done in previous figure of the chapter. // the radiative feedback could be link with an arrow or a line to the right plot box.. [WGI TSU, France]	Taken into account. Figure has been updated, and font has been changed in the FGD.
57030					Figure 7.27: font should be arial // for (a), use the temperature palette in the Visual Style guide (from RGB 247 247 247 to RGB 103 0 31) // color scheme should ideally be changed to highlight the negative vs positive contribution inputs rather than highlighting different catergories (labels are already here, so colors are redundant). Suggestion: use red for positive and blue for negative, as it was done in previous figure of the chapter. // the radiative feedback could be link with an arrow or a line to the right plot box.. [WGI TSU, France]	Taken into account. The figure has been updated with changed colors and arrow, and color scheme adjusted in the FGD.
57032					FAQ 6.1 figure: this figure is not appropriate for a lay audience. [WGI TSU, France]	Taken into account: The figure was fully revised in SOD.
45550					Some parts of this chapter seem to be mixing up confidence and probability statements in a way that doesn't seem consistent with the IPCC uncerainty guidelines. [Steven Sherwood, Australia]	Taken into account. Confidence is now used more uniformly