Comment ID	From Page	From Line	To Page	To Line	Comment	Response
27738	0	0	0	0	et al in italics, bibliographical citations in chronological order. [Poot Delgado Carlos Antonio, Mexico]	Taken into account. Technical issue with Mendeley that cannot be solved at chapter level. Might be solved now for SOD.
27740	0	0	0	0	Unify the use of "and" in bibliographic citations [Poot Delgado Carlos Antonio, Mexico]	Accepted. SOD strives to be consistent.
27742	0	0	0	0	delete the two points and followed by the legends of figures (Figure 4.19:) [Poot Delgado Carlos Antonio, Mexico]	Noted. This is a technical issue for cross referencing (text and figure legend) in MS word. Those are deleted in the SOD.
28806	0	0	0	0	Although relatively unfinisihed due to lack of data, Chapter has really nice structure and figures and there are great nuggets here which should be spm headlines, such as expected increase in rate of warming, even under mitigation [Piers Piers Forster, United Kingdom (of Great Britain and Northern Ireland)]	Noted, thank you.
28812	0	0	0	0	RFMIP will hopefully give projected ERFs, which can be used with Chapter 7 ECS and TCR estimates to emulate range of temps for Figure 4.1, and comparing to CMIP5/6 ensembles. We should work on this in time for SOD. If no RFMIP results to evaluate we can diagnose ERFs from FAIR and MAGICC for projections.. [Piers Piers Forster, United Kingdom (of Great Britain and Northern Ireland)]	Accepted, the SOD follows this strategy. Box 4.1 , Section 4.4.1, and the synthesis GSAT assessment in 4.3.4 use the Ch7 ECS and TCR assessed ranges as they are converted to GSAT ranges with the emulator.
46732	0	0	0	0	lack linkage with SROCC, duplication with chapter 9 on projection of ocean and crosphere [WGI TSU, France]	Taken into account. Cross-referencing to previous reports and other chapters is more extensive in the SOD.
46734	0	0	0	0	this chapter introduced a method quantifying sources of projection uncertainty, but statements on quantified uncertainty are scarely found [WGI TSU, France]	Taken into account. The FOD was based on a small subset of CMIP6 results; the SOD contains a full uncertainty assessment and defines how probabilistic statements have been arrived at. As it turns out, though, the literature is essentially on GSAT. Box 4.1 makes this clear now.
48020	0	0	0	0	Scoping Outline Check: All bullets from approved outline are covered in the first order draft. Please note some overlp with chapter 9. Chapter 4 should assess global indicators only, as stated in the approved outline. [WGI TSU, France]	Taken into account. The SOD explains selection choices more explicitly. "Global" is defined here as having global import. Also, please note that Arctic sea-ice extent is indeed the global integral of all Northern-Hemisphere sea ice.
28866	0	0	0	0	FAQs all have excellent titles, I would like to see all of these retained [Piers Piers Forster, United Kingdom (of Great Britain and Northern Ireland)]	Noted, thank you.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
57450	0		0		General comment #1: Nice work putting together so much with so few CMIP6 simulatoins available. There was enough here to see the arc of what you are planning once you have everything. My overall reaction is that I found the flow to be difficult to follow at times. If a reader wants to learn about how sea ice is projected to change, or ENSO, or precipitation, they have to look in something like 4 different places (21st century section, near-term section, mid-to-long term section, and very long term section). In many cases the discussions in these sections about sea ice or precipitation, say, were not very different from one another since there isn't that much new that happens between these timescales (except perhaps an emerging dominance of forced response over internal variability). For instance, it's stated in at least three different places in the chapter that global precip is expected to change by 1-3%/K. I'm not sure if it is too late, but a better flow may be to group sections and subsections by climate system component and to then discuss all timescales sequentially and in one place (e.g., short term sea ice projections, followed by mid-to-long, followed by very long). [Kyle Armour, United States of America]	Taken into account. The SOD makes more explicit within-chapter cross-references and aims at avoiding unnecessary duplication. However, the overall chapter structure has not been changed, because it moves from global indicators to near term (emphasising internal variability) to mid- and long-term.
57452	0		0		General comment #2: Section 4.2 (Methodology) would benefit from some figures to point to, e.g., it's useful to see how similar the patterns of warming and precip are at different times or emissions scenarios when discussing pattern scaling, otherwise it is quite abstract. Perhaps plot a few figures (e.g., global annual mean temperature and precip projections; maps of annual temp and precip at a couple different warming levels) near the beginning to aid in this discussion. Can show these again with seasonal information or additional warming levels later. [Kyle Armour, United States of America]	Rejected. Given the space limitations and in the interest of communicating visually primarily the results and not the methodologies, we decided to keep as Figure 4.1 the time series of projections of global climate indicators.
57454	0		0		General comment #3: Throughout the chapter I was having trouble keeping track of when likelihood statements came from assessment of the model spread or percentage of models that show something, and when likelihood statements came from a that combined with additional information such as mechanistic understanding or consistency with observed changes. Perhaps discuss somewhere what information is used for these statments and be more explicit about each when you can be. [Kyle Armour, United States of America]	Taken into account. The FOD was based on a small subset of CMIP6 results; the SOD contains a full uncertainty assessment and defines how probabilistic statements have been arrived at.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
38428	0				I enjoyed reading this chapter, and find that the authors have done a great job in outlining recent insights regarding the future evolution of the climate system. In particular, I find that the differentiation between near-term and long-term changes works well in principle. However, at the same time I feel that the current framing misses the opportunity to more strongly emphasise recent developments focusing on carbon budgets rather than specfic scenarios. Hence, the chapter possibly too much emphasises the somewhat passive temporal aspects of climate change, rather than emphasising the impact of the forcing. [Dirk Notz, Germany]	Taken into account. However, carbon budgets are assessed in Ch5. The dual perspectives of time/scenarios in Sections 4.3--4.5 and warming levels in Section 4.6.1 is now made more explicit in the Introduction.
38432	0				The chapter repeatedly talks about results from „the five climate models“, or „the three climate models“, or „the two climate models“, but often it is not clear which specific subset the term „the xxx models“ refers to. I expect, however, that this will automatically be addressed as more model results become available. [Dirk Notz, Germany]	Taken into account. The issue has turned moot due to the availability of more CMIP6 models.
49440	0				This chapter needs to be careful not to excessively repeat information (e.g. on observed ocean temperature changes) that has been covered in the previous two chapters. Whereas Ch 2 should focus on observations to date and Ch 3 on attribution of those observed changes to anthropogenic forcing, Ch 4 should specifically focus on the future, not the present or past. [Sonya Legg, United States of America]	Taken into account. However, part of the remit for this chapter is to use information from the most recent past to infer near-term future changes, and further to assess models' adequacy-for-purpose. Some inclusion of the past in hence required.
9262	0				The documentation of key results could be improved by adding the CMIP5/CMIP6 data references in addition to model references. A table with basic information on the model and experiment together with the data references is suggested. CMIP6 references are accessible via ESGF or following the furtherInfoUrl in the NetCDF data headers. CMIP5 data is archived in the IPCC DDC at http://www.ipcc-data.org/sim/gcm_monthly/AR5 . [Martina Stockhause, Germany]	Taken into account. A cross-chapter Technical Annex is included in the SOD that contains basic model characterisations.
30272	0				There should be little bit more detail information about the new scenarios, SSPs [Nazan An, Turkey]	Taken into account; the SOD contains ERF time series. However, the basic description of the scenarios is contained in Ch1.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
48474	0				The modes of variability sections seem disconnected from their impacts e.g. we are primarily interested in how modes of variability will change due to their impact on surface climate but their projected impacts on temperature and rainfall patterns are not assessed. Their impacts could be better woven into the discussions of the projected changes in temperature and rainfall patterns e.g. papers like Lim et al 2016:10.1002/2016GL069453 [Julie Arblaster, Australia]	Taken into account. The SOD contains a fuller assessment. Subsections 4.4.3 and 4.5.3 in the SOD include more comprehensive assessment of teleconnection and temperature changes associated with changes in modes of variability. However, their impacts on precipitation are mainly discussed in Chapter 8 (water-cycle chapter Section 8.4.2).
47202	0				Congratulations for this well-written and comprehensive draft of chapter 4. I only have minor comments (see hereafter). [Hervé Douville, France]	Noted, thank you.
11626	0				Where in this chapter do authors present model hindcasts for the pre-industrial 2000 and 10,000 years? Use e.g. the composite global records of PAGES 2k 2013 and Marcott et al. 2013 for a start. [Sebastian Luening, Portugal]	Rejected. Simulation of distant-past climates is not in the chapter's remit.
36522	0				General Comment: Very informative and comprehensive. The limited availability of CMIP6 results appears to weaken several conclusions and can make it somewhat difficult to evaluate the progress from AR5 to AR6. [Carlos Mechoso, United States of America]	Noted, thank you.
36524	0				Organization: In some way, the organization in near, mid-and long-term changes results in many repetitions. Also, many times the same reference is included several times in the same paragraph. A technical editor with ability in streamlining presentations may help in this regard. [Carlos Mechoso, United States of America]	Taken into account. The SOD makes more explicit within-chapter cross-references and aims at avoiding unnecessary duplication. However, the overall chapter structure has not been changed, because it moves from global indicators to near term (emphasising internal variability) to mid- and long-term.
56246	0				It seems questionable to base all projections on current CMIP6 projections, and to overlook CMIP5 results, in particular since the CMIP6 and CMIP5 ensembles seem to differ (e.g. with respect to climate sensitivity). A valid question is: Should we trust the CMIP6 ensemble? Is there evidence that it is more reliable, or does the CMIP5 seem more robust? Maybe the most robust approach would be a mix/combination of CMIP6 and CMIP5 assessments. [Sonia Seneviratne, Switzerland]	Taken into account. However, CMIP5 and CMIP6 results cannot be mixed straightforwardly, because the forcings differ substantially. CMIP5-based publications are already assessed extensively. The SOD contains a full uncertainty assessment of CMIP6-based results.
51902	0				This is another very well written chapter. For the ES is there a reason why there are no statements related to the final two sections? These would seem to contain information that should be summarised within the ES even if they are low confidence high uncertainty statements? [Peter Thorne, Ireland]	Taken into account; thank you. The SOD ES represents the entire chapter more broadly.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
56254	0				I am deeply concerned that this chapter does not seem to consider any RCP1.9 scenarios, which would be the only ones most consistent with the Paris agreement (which it is important to remember is what the world's countries committed to). [Sonia Seneviratne, Switzerland]	Taken into account. No SSP1-1.9 scenario were available for the FOD, but they are for the SOD and have received due consideration.
51922	0				Try to be more consistent when summarising the AR5 findings and use the past tense consistently so that its clear what were the findings of prior assessments compared to what your new findings are. Quite often the present tense is used and it is therefore not clear whether these are antecedant findings or your new assessment [Peter Thorne, Ireland]	Accepted; the SOD tries to follow this sound advice consistently.
53460	0				Re "emulators": i) please check consitency with ch1 and ch7 ii) may also be worth mentionng very briefly why they are used. [Jan Fuglestvedt, Norway]	Accepted; emulators are properly introduced in the SOD and harmonised with Ch1 and Ch7.
51928	0				The choice to split the short-term from the long-term leads toa certain creeping feeling of déjà vu on a number of occassions To keep things short and avoid repetition might it be better to have instead short and longer term subsections to each thematic section in turn rather than having short and long-term with exactly the same headings? It feels to me like a reader may want to see e.g. the short and long-term precipitation issues and may prefer to see them cheek to jowl rather than having to search for and then reconcile the two sections? [Peter Thorne, Ireland]	Taken into account. The SOD makes more explicit within-chapter cross-references and aims at avoiding unnecessary duplication. However, the overall chapter structure has not been changed, because it moves from global indicators to near term (emphasising internal variability) to mid- and long-term.
35808	0				The authors should be commended on completing a chapter based largely on CMIP6 scenario simulations which appeared very close to the submission deadline for the draft. The Executive Summary tells a consistent story based on six CMIP6 models, and subsets of these. The chapter includes valuable assessment on some new topics not covered in previous assessments, such as polar amplification. [Nathan Gillett, Canada]	Noted, thank you.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
35810	0				A problem with the current chapter structure is that it is in parts repetitive, and in parts the chapter lacks overall coherency. For example, Sections 4.3.3.2, 4.4.3.2 and 4.5.3.2 all independently assess changes in ENSO variability through the 21st century. 4.3.3.2 and 4.5.3.2 both define and describe ENSO. 4.3.3.2 and 4.4.3.2 both summarise the AR5 findings on changes in ENSO variability. And all three sections reach slightly different assessments: 4.3.3.2 - 'we conclude that ENSO variability is likely to weaken under the SSP1-2.6 and SSP2-4.5 beginning in the near-term (2021-2040) while there is no consensus on ENSO variability change in the SSP3-7.0 and SSP5-8.5 scenarios (low confidence because of the limited number of models available).' 4.4.3.2: 'At the time of composing the FOD of Chapter 4, the ability of CMIP6 models in simulated different ENSO flavours as well as projected changes in SST variability in the near-term remain to be explored'. 4.5.3.2: 'While a subset of CMIP5 models that simulated linear ENSO stability realistically exhibit a decrease in ENSO amplitude by the latter half of the 21st century, there is no strong consensus among models on long-term Nino3.4 SST changes when considering all models.... However, an increase of Eastern Pacific ENSO SST variance has been shown when taking into account the biases in the ENSO pattern simulation by different models'. Moreover, none of these three sections references any other of these three sections. Some other sections of the chapter are better linked, but overeall for the SOD, the chapter would benefit from editing to remove repetition and all authors familiarising themselves with the rest of the chapter to ensure a consistent assessment. I also propose some suggestions for merging sections	Taken into account. The SOD makes more explicit within-chapter cross-references and aims at avoiding unnecessary duplication. However, the overall chapter structure has not been changed, because it moves from global indicators to near term (emphasising internal variability) to mid- and long-term. The changes in ENSO variability and its teleconnection/impacts are treated more consistently with minimization of overlaps between 4.3.3.2, 4.4.3.2 and 4.5.3.2 in the Chapter 4 SOD and Chapter 8 Section 8.4.2..
26148	1	1	163	1	This chapter should be called “Climate Model Predictions” and is an extension of the previous chapter. Topics are repeated within the chapters, a sure sign the chapter layout and titles are wrong.. Chapter titles do not follow the natural flow of subject topics. [Stephen Taylor, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The SOD makes more explicit within-chapter cross-references and aims at avoiding unnecessary duplication. However, the overall chapter structure has not been changed, because it moves from global indicators to near term (emphasising internal variability) to mid- and long-term. The chapter title follows the approved outline.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
32044	1		163		I like the section structure of this chapter which is helpfully logical. However, I have an important question about section 4.8. I am pleased to see this section included but it implies that in the rest of the chapter (i.e. nearly all of it) the focus will be almost entirely on assessment of the likely range for key parameters. If this is the case, it should be clearly stated at the outset and justified. Alternatively (which I think would be better) the importance of assessing potential high impact scenarios (of course climate response scenarios not just SSPs) - in addition to the likely range - should be identified at the beginning of the chapter as a cross-cutting requirement to meet the needs of policy makers; and appropriate discussion and assessments should then be included in every major section of the chapter (i.e. each section should assess both the likely range and specific high impact scenarios). Section 4.8 might still be required in order to explore some cross-cutting aspects of high impact scenarios in more detail. Please see Sutton, BAMS early online release, 2019, and Sutton ESD, 2018, for further explanation and justification. [Rowan Sutton, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The SOD explains selection choices more explicitly. We feel that focusing the low-probability, high-impact instances in one subsection better serves the overall chapter purpose.
6942	1				In this review I have primarily focused on the summary for policy-makers, and on the topic of multi-model weighting since this has been the subject of my recent research. [Olson Roman, Republic of Korea]	Noted.
56122	2	31	2	31	Why is >Antarctic sea ice> omitted here? See discussion in Chapter 2 [Rolf Müller, Germany]	Noted. Ch4 can only assess a subset of the quantities covered in Ch2.
50100	2	33	2	39	Modes of variability in the list have a different terminology from those in Chapter 2 for the similar modes. Suggest to make them two consistent with each other. [Hong-Li Ren, China]	Accepted. SOD has harmonised definitions across WGI. To help the harmonization and avoid unnecessary overlaps, the technical annex on MoVs has been worked out in coordination with Chapters 2, 3, and 8.
26186	2	37	2	37	word "Basin and" to be deleted [iman babaeian, Iran]	Rejected. Distinction is needed. The subsection discusses both Indian Ocean Basin mode and Indian Ocean Dipole mode.
46320	2	37	2	37	word "Basin and" to be deleted [sadegh zeyaeyan, Iran]	Rejected. Distinction is needed. The subsection discusses both Indian Ocean Basin mode and Indian Ocean Dipole mode.
57596	2	37	2	37	word "Basin and" to be deleted [Sahar Tajbakhsh Mosalman, Iran]	Rejected. Distinction is needed. The subsection discusses both Indian Ocean Basin mode and Indian Ocean Dipole mode.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
9004	3	3			Is there a difference between near-surface air temperature and surface air temperature? [Anna Merrifield, Switzerland]	Taken into account. They are used synonymously in the FOD; harmonised in SOD. Still to be done in 4.4 and 4.5.
26188	3	16	3	16	word "Basin and" to be deleted [iman babaeian, Iran]	Rejected. Distinction is needed. The subsection discusses both Indian Ocean Basin mode and Indian Ocean Dipole mode.
46322	3	16	3	16	word "Basin and" to be deleted [sadegh zeyaeyan, Iran]	Rejected. Distinction is needed. The subsection discusses both Indian Ocean Basin mode and Indian Ocean Dipole mode.
57598	3	16	3	16	word "Basin and" to be deleted [Sahar Tajbakhsh Mosalman, Iran]	Rejected. Distinction is needed. The subsection discusses both Indian Ocean Basin mode and Indian Ocean Dipole mode.
47204	3	19			What about postponing this section at the end of chapter 4 (since climate policies also have strong implications for very-long-term and/or low-probability changes) and possibly rename this section « Implications of and for climate policy » (since any revised assessment of the future climate response to a specified radiative forcing / global temperature target has potential implications for the expected benefits of a lower target) ? [Hervé Douville, France]	Rejected. Most of this assessment concerns the 21st century. "Implications for" would seem policy-prescriptive.
47206	3	40			I fully support this focus but it might be better to have it as a systematic analysis framework in all former sections which could emphasize both the most likely range but also the low-probability-high-impact changes on different timescales or for different global mean temperature targets. If you don't make this decision, you may at least consider moving subsection 4.7.3 into section 4.8 since the potential for abrupt climate change before 2100 cannot be excluded, especially given the new definition that is proposed in the AR6. This might be also more consistent with the possible outcome of chapter 3 and other chapters that the Earth System is currently experiencing an abrupt change in multiple and important respects. [Hervé Douville, France]	Taken into account. Keeping 4.8 separate retains a clearer focus, though, especially given the high ECS numbers in some CMIP6 models. No change in structure.
48058	5	1	5	1	The Executive Summary is longer than recommended (2 pages) and key messages do not appear in bold. [WGI TSU, France]	Taken into account. Key messages are in bold in SOD. Note, however, that the FOD length was almost exactly on target, at 1916 words, which translates into two printed pages.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
52392	5	1	8	3	Once more work is completed, this Executive Summary should be re-written to be far more accessible to policy makers, as it is key to understanding of AR6 overall that these (in essence) replacements for RCP scenarios be well-understood by the policy world. The rest of Chapter 4 can be far more technical, but this section is important to proper understanding and use of AR6 going forward. [Pam Pearson, Sweden]	Taken into account. It is not clear, however, whether the ES is already the point where the general reader is being addressed, or whether this occurs higher up the ES--TS--SPM ladder. Insisting on traceability to the main text, tight length limitations, and on dropping jargon might pose an impossible target.
47998	5	1	8		Executive summary format: please bold the first sentence of each paragraph to highlight the main assessment conclusion, followed with additional details in unbold text. [WGI TSU, France]	Accepted and implemented in SOD.
26544	5	1	9	51	The reading of the whole chapter is rather complex and laborious. The overall suggestion is to streamline the chapter, avoid quite frequent repetitions and better balance the contributions referring the different varaibles under study within each paragraph, so as to make the reading more pleasant and easy. [Antonia Longobardi, Italy]	Taken into account. The SOD makes more explicit within-chapter cross-references and aims at avoiding unnecessary duplication. However, the overall chapter structure has not been changed, because it moves from global indicators to near term (emphasising internal variability) to mid- and long-term.
38960	5	1			Any entry for temperature patterns such as land-ocean contrast and polar amplification in Executive Summary? [Masahide Kimoto, Japan]	Taken into account. Unfortunately, there has not been enough space.
43886	5	3	5	4	time horizons from the near-term (2021–2040) out to year 2300???, this satement is not clear in as it is in the executive sumaary, it should be improve by stating "the changes are assessed in selected global climate indices out to year 2300" [Michael Mugarura, Germany]	Rejected. The juxtaposition appears useful.
53368	5	3	5	6	This start of the ES is very useful. [Jan Fuglestvedt, Norway]	Noted, thank you.
9142	5	3	5	6	As in comment 8 above, in view of the model uncertainties, projecting beyond 2100 to 2300 better belongs in the realm of science fiction, [Jim O'Brien, Ireland]	Noted. No required action discernible from this comment.
56248	5	3	5	6	It seems questionable to base all projections on current CMIP6 projections, and to overlook CMIP5 results, in particular since the CMIP6 and CMIP5 ensembles seem to differ (e.g. with respect to climate sensitivity). A valid question is: Should we trust the CMIP6 ensemble? Is there evidence that it is more reliable, or does the CMIP5 seem more robust? Maybe the most robust approach would be a mix/combination of CMIP6 and CMIP5 assessments. [Sonia Seneviratne, Switzerland]	Taken into account. However, CMIP5 and CMIP6 results cannot be mixed straightforwardly, because the forcings differ substantially. CMIP5-based publications are already assessed extensively. The SOD contains a full uncertainty assessment especially of GSAT changes.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
57600	5	4	5	5	1995-2014 is not a climatological normal period and exasperate metrics of climate change. recommend to change it into the most recent climatology normal of 1981-2010, which is defined by WMO. [Sahar Tajbakhsh Mosalman, Iran]	Rejected. Choice of reference period is explicitly and carefully justified later. Besides, the period of 1995-2014 is used as the definition of recent past across WGI.
26190	5	4	5	5	1995-2014 is not a climatological normal period and exasperate metrics of climate change. recommend to change it into the most recent climatology normal of 1981-2010, which is defined by WMO. [iman babaeian, Iran]	Rejected. Choice of reference period is explicitly and carefully justified later. Besides, the period of 1995-2014 is used as the definition of recent past across WGI.
46012	5	4	5	5	If the 1995-2014 reference benchmark is precisely measured, as compared to the approximated 1850-1900 period, should it be clearly identified as such? [Isaac Pearlman, United States of America]	Rejected. Choice of reference period is explicitly and carefully justified later. Besides, the period of 1995-2014 is used as the definition of recent past across WGI.
46324	5	4	5	5	1995-2014 is not a climatological normal period and exasperate metrics of climate change. recommend to change it into the most recent climatology normal of 1981-2010, which is defined by WMO. [sadegh zeyaeyan, Iran]	Rejected. Choice of reference period is explicitly and carefully justified later. Besides, the period of 1995-2014 is used as the definition of recent past across WGI.
33324	5	5	5	5	This line states "pre-industrial period (1850-1900)" -- This runs contrary to Chapter 1, which outlines that this period is now considered the "early industrial" (Chap 1, p.69) [Erika Wise, United States of America]	Noted. Usage in Ch1 FOD is not the one agreed upon at LAM2, nor at LAM3.
6910	5	5	5	6	The phrase "provides the global reference" is unclear. [Olson Roman, Republic of Korea]	Noted. Seems clear to us and has not drawn any other comment.
30662	5	6	5	6	the plural "regional changes" is likely more appropriate [Annalisa Cherchi, Italy]	Noted. Our usage seems consistent with singular in "climate change".
11524	5	7	7	27	It needs noted that these statements/conclusions are similar to what is in earlier IPCC reports. [Roanld Stouffer, United States of America]	Taken into account. The ES has been reconstructed, focusing on its own storyline rather than the chapter's.
57602	5	8	5	9	need to incorporate more CMIP6 models to decrease uncertainty. [Sahar Tajbakhsh Mosalman, Iran]	Accepted and implemented in SOD.
26192	5	8	5	9	need to incorporate more CMIP6 models to decrease uncertainty. [iman babaeian, Iran]	Accepted and implemented in SOD.
46326	5	8	5	9	need to incorporate more CMIP6 models to decrease uncertainty. [sadegh zeyaeyan, Iran]	Accepted and implemented in SOD.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
29972	5	8	5	10	I must say I find it bizarre that Chapter 4 is taking the view that our expectations for the future need to be preliminary because all the CMIP6 simulations are not in the can. What about CMIP5? The notion that we have to start from zero and throw away our previous knowledge is a real problem with this whole exercise. You could have written the FOD based on current literature and then tweaked things (probably only very slightly) once you saw the CMIP6 results, which would also have been less stressful! However, it seems quite possible that the substantially higher ECS in many of the CMIP6 models will lead to quantitative conclusions that differ from those in AR5 (for the same GHG scenario). Yet unless it is shown that the ECS values in CMIP6 models are somehow more credible, there would then be no sound reason to accept CMIP6 numbers over those from CMIP5. The authors need to consider this issue very carefully in their crafting of the findings, since it will affect the messaging. (This is a good reason for expressing things in terms of global warming levels!) [Theodore Shepherd, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The SSP provide a broader range of forcings. The SOD contains an explicit comparison against CMIP5 and a comprehensive assessment of GSAT changes. It is useful during FOD review to distinguish between preliminary wrt. later drafts and preliminary wrt. later assessment reports. The FOD must make a plausible prediction of what will be possible for SOD. Things have turned out well.
8264	5	8	5	10	There are only 5 models now. It might be changed the most results projected by fully CMIP6 in SOD. [Zong Ci Zhao, China]	Noted.
54018	5	12	5	16	Oh boy, this could become really confusing. We are now calling these SSPs, even though they are essentially RCPs that reference an SSP-marker which happens to provide forcing that is close to the original AR5 RCPs or to other new forcing levels by 2100 specified in ScenarioMIP. The earlier RCPs didn't reference the drivers used, even though they were effectively based on rudimentary SSP markers too. I suppose this is an agreed lexicon across all WGs, but I wish you well in explaining it to the governments! It might have been preferable to reverse the order, and have the forcing value first, with the SSP supplying that forcing listed second. That way, the SSP acronym could have been dropped and maybe a subsitute for RCP (e.g. RF for radiative forcing) used instead. Hence SSP1-2.6 would become RF2.6-1; SSP3-7.0 would be RF7.0-3. I suppose this comment should have been raised four years ago when the CMIP6 runs were being planned. - oh well! [Timothy Carter, Finland]	Noted. No discernible action item.
49504	5	12	5	16	I understand that the choice of the priority scenarios is driven by the ability to compare to AR5 (?). I was wondering if the authors conisdered/discussed if the SSP1-1.9 woud be worthy to be included in the priority list and consequently in presented comparisons. [Zbigniew Klimont, Austria]	Taken into account. No results from SSP1-1.9 were available for the FOD, but they are now.
43888	5	14	5	14	Give a reason for jumping SSP 4 as a world deepening with inequality is also a possible future outcomde [Michael Mugarura, Germany]	Rejected. Ch4 can only assess the simulations carried out by research groups.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
57338	5	18	5	18	The chapter is generally admirably clear when it is talking about GSAT, with a few exceptions: I found a few stray uses of GMST when I think you almost certainly meant GSAT. That said, given Chapter 2 doesn't give a clear reason for favouring GSAT over GMST (and states clearly that there is higher confidence in the latter), why not show at least one figure using the Cowtan/Richardson code sampling models where observations actually exist and computing GMST using some kind of frozen data mask for the future? I appreciate there are issues over sea-ice, but there aren't many observations up there anyway. [Myles Allen, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. GMST sometimes must be used if it was used in primary literature. We see no value in carrying GMST in projections otherwise; Ch2 SOD provides the conversion.
53018	5	18	6	5	It's a bit too wordy. I think this can be more concise, especially given that it's an "excecutive summary". For example, paragraphs 1 and 2 can be further lumped into 1 paragraph. [Anson Cheung, United States of America]	Taken into account. The ES has been reconstructed, focusing on its own storyline rather than the chapter's.
56250	5	20	5	20	Again: It seems questionable to base all projections on current CMIP6 projections, and to overlook CMIP5 results, in particular since the CMIP6 and CMIP5 ensembles seem to differ (e.g. with respect to climate sensitivity). A valid question is: Should we trust the CMIP6 ensemble? Is there evidence that it is more reliable, or does the CMIP5 seem more robust? Maybe the most robust approach would be a mix/combination of CMIP6 and CMIP5 assessments. [Sonia Seneviratne, Switzerland]	Taken into account. However, CMIP5 and CMIP6 results cannot be mixed straightforwardly, because the forcings differ substantially. CMIP5-based publications are already assessed extensively. The SOD contains a full uncertainty assessment especially of GSAT changes.
46014	5	20	5	20	Is it possible to begin with mid-term (2021-2040) results , and then give longer-term results in chronological order? This comment goes for all sections (global temp, precip, etc.). [Isaac Pearlman, United States of America]	Taken into account. The ES has been reconstructed, focusing on its own storyline rather than the chapter's.
35822	5	20	5	21	In the SOD, introduce the method used to estimate uncertainties in projections when the first such projections are given (could be in a footnoote). [Nathan Gillett, Canada]	Accepted.
27202	5	20	5	26	The difference of the projections towards the end of this century and the observations, 0.4°C since 1945, beginning of acceleration of CO2 emissions, are too large for comparable periods of time to be convincing. [François GERVAIS, France]	Noted. No discernible action item.
53370	5	20	5	26	I dont think you need to mention development of CO2 concentrations here. Even if it is the main driver there are other forcings as well. And mentioning CO2 makes the text heavy to read. [Jan Fuglestvedt, Norway]	Taken into account. The ES has been reconstructed, focusing on its own storyline rather than the chapter's.
29858	5	20	5	26	Although it is about the average temperature, it might be better if the projected CO2 levels can be highlighted (e.g. xxx ppm for the period of 2040-2060 etc.) [Mustafa Tufan Turp, Turkey]	Taken into account. The ES has been reconstructed, focusing on its own storyline rather than the chapter's.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
9144	5	20	5	35	As in comment 9 above, my suggestion is to delete SSP5-8.5 projections are these are unrealistic and present only distracting figures to the public and media. [Jim O'Brien, Ireland]	Rejected. SSP5-8.5 forms part of the material that must be assessed here. As written explicitly, there is currently no scientific basis for considering one scenario as more or less plausible than any other scenario.
57604	5	21	5	21	need to mention the difference between GSAT between 1995-2014 and pre- industrial period. [Sahar Tajbakhsh Mosalman, Iran]	Taken into account. The ES has been reconstructed, focusing on its own storyline rather than the chapter's.
32046	5	21	5	21	It is entirely unclear what is meant by a 5-95% range, i.e. any interpretation involves a whole set of assumptions, so I don't think this metric should be used, particularly not in the Executive Summary. If it is used the assumptions should be very clearly stated. [Rowan Sutton, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account (cf., comment 35822).
26194	5	21	5	21	need to mention the difference between GSAT between 1995-2014 and pre- industrial period. [iman babaeian, Iran]	Taken into account. The ES has been reconstructed, focusing on its own storyline rather than the chapter's.
15592	5	21	5	21	Would it be better to use Global Mean Surface Temperature (GMST) instead of GSAT? [Izidine Pinto, South Africa]	Rejected. Ch2 SOD provides the conversion.
46328	5	21	5	21	need to mention the difference between GSAT between 1995-2014 and pre- industrial period. [sadegh zeyaeyan, Iran]	Taken into account. The ES has been reconstructed, focusing on its own storyline rather than the chapter's.
49916	5	21	5	22	Instead of percent range, give mean value, deviations and range of model spread, as in Table 4.2 . Present text giving whole range of model results (0.7 – 1.7 C) is difficult to comprehend, particularly by policy makers. The median value can even be replaced by a most likely value when a larger ensemble of model results become available. This suggestion also applies to all subsequent scenario results presented in the various sections. [Govindarajalu Srinivasan, Thailand]	Taken into account (cf., comment 35822). Note, however, that Table 4.2 also gives the 5-95% and not the total ensemble range. The total range is very sensitive to outliers.
47634	5	21	5	26	For consistency with previous reports, for understanding and policy relevance, it is important that inthese lines in the summary the projected global average temperature for 2081-2100 relative to pre-industrial is also mentioned, since the international goals are relative to pre-industrial levels. [Birgit van Munster, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The ES has been reconstructed, focusing on its own storyline rather than the chapter's.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
15332	5	22	5	22	As this is a key takeaway for the entire report, I'd suggest that you provide additional language to describe the phrase "where CO2 concentrations peak" in terms that lay people understand, e.g. "where CO2 concentrations peak, i.e. when (or after) anthropogenic emissions reach zero or net zero...". Most lay people will have a difficult time understanding the difference between CO2 concentrations peaking and CO2 emissions peaking. [Lia Cairone, United States of America]	Taken into account. The ES has been reconstructed, focusing on its own storyline rather than the chapter's.
51620	5	23	5	26	Just to be clear, my understanding from the AR5 was that RCP8.5 highest level to 2100 was +4.8C and now the SSP5-8.5 are up to 6.1C. I appreciate there are new combinations for the AR6 (SSP+RCP), and that this increase in 8.5 temperature increase is due to new SSP combinations, but want to share my understanding of what you are now saying in case I am mistaken and others might also be confused. [Lindsey Cook, Germany]	Taken into account. Ch4 SOD makes comparisons between CMIP5 and CMIP6 forcing and responses explicit. Moreover, the GSAT assessment includes lines of evidence in addition to the CMIP6 ensemble.
56424	5	24	5	25	"emissions scenario" may be misleading; it suggests that the chapter uses emissions-driven SSP simulations. [Kirsten Zickfeld, Canada]	Accepted. "Emissions" dropped here.
47636	5	28	5	30	In Chapter 2 it is mentioned that the budget for 1.5°C will be exhausted by 2030 (67% chance) or 2035 (50% chance) at constant 2017 emissions. This seems conflicting with a 1.5°C rise of GSAT relative to pre-industrial by on average 2025 [Birgit van Munster, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Note that the result quoted here is based on a small sample containing high-ECS models. Clarification provided in SOD.
32656	5	28	5	35	Consistent with other chapters presenting key results, this paragraph needs to be in bold. [Michael MacCracken, United States of America]	Taken into account, although not entire paragraphs are to be in bold, only the first sentence.
56252	5	29	5	30	Questionable conclusion, which differs substantially from the IPCC SR15. Is this alone due to the differing global climate sensitivity of the considered 5 models compared to the CMIP5 ensemble, are there other aspects that play a role? Is this because no RCP1.9 scenarios were considered? The IPCC SR15 concluded that from a geophysical point of view alone, past emissions do not commit us to a world of more than 1.5°C warming. Is this shown to be invalid in the CMIP6 simulations so far? [Sonia Seneviratne, Switzerland]	Taken into account. Note that the result quoted here is based on a small sample containing high-ECS models. Clarification provided in SOD.
37636	5	29			"pre-industrial (1850-1900)" should be changed to "early-industrial baseline (1850-1900)" to be consistent with the terminology established in Chapter 1. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. Usage in Ch1 FOD is not the one agreed upon at LAM2, nor at LAM3.
15264	5	30	5	30	The use throughout of the phrase "priority SSPs" I'm afraid would communicate somehow a policy implication, or a judgment of relevance/likelihood. At least it was my reaction while reading. [Claudia Tebaldi, United States of America]	Taken into account. Wording is straight from original research paper but has been reconsidered.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
37638	5	30			See comment 188. Here the 2025 date for the 1.5ºC crossing first appears. If other CMIP6 results confirm this, there must be some summary statement here as to why this date is so early compared with what is stated as likely in SR1.5. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Assessment updated and referring explicitly to SR1.5.
15260	5	37	5	39	This connection between transient warming ranges and the ECS range is not clear to me both in the way it is derived (is it really just comparing the two ranges, or does it involve some subsetting of models that fall into the ECS range of interest, and their expressions of transient warming?) and in its meaning (why should we care about that?). I'm not saying this is wrong of course, but as the item is written it does not clearly tell me why I should be interested in knowing that. [Claudia Tebaldi, United States of America]	Taken into account. The ES has been reconstructed, making clearer the relationship between the CMIP6 ensemble and the ch7-assessed ECS range. .
56426	5	37	5	42	Unclear what the key message associated with this paragraph is. The "warming range inferred from the very likely ECS range" has littlle policy relevance. [Kirsten Zickfeld, Canada]	Taken into account. The ECS range permits assessment of CMIP6-simulated warming, as has been made clearer in SOD.
57456	5	37	5	42	This statement is unclear -- I initially read it that their projected warming falls within 2-5 K over this time, when you mean something more like "models that simulate the highest warming beyond 2060 generally have higher ECS than 2-5 likely range from Chap 7" [Kyle Armour, United States of America]	Taken into account. Statement clarified.
11518	5	37	5	42	This summary seems missing leading. CMIP6 does not have any experiments by design to estimate equil. Climate sensitivity, just eff. Climate sensitivity. More is needed if the paragraph is kept. I recommend deleting it. [Roanld Stouffer, United States of America]	Rejected (deletion). But statement has been clarified.
32048	5	37	5	50	There are references here to the assessed range for ECS but arguably the assessed range for TCR is more relevant [Rowan Sutton, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Assessed TCR is also used in SOD.
49918	5	37	5	50	Linking results to ECS make this section lose clarity and also complicated, also frequent reference to a later Chapter 7. Consider presenting the excecutive summary without (or with minimal) refernce to ECS. Details of this link maybe presented in later sections as in the present draft [Govindarajalu Srinivasan, Thailand]	Rejected. The ECS range permits assessment of CMIP6-simulated warming.
35824	5	38	5	39	Say how the warming range is inferred from ECS. As well as the estimate of ECS a projection of GSAT with an EBM also requires an esimate of ocean heat uptake, and an estimate of radiative forcing including aerosol forcing, both of which are uncertain and will affect the result. Summarise the approach used to sample forcing and heat uptake efficiency here, and provide more details in the caption to Fig 4.8 or chapter text. [Nathan Gillett, Canada]	Taken into account. The ES has been reconstructed, making clearer the relationship between the CMIP6 ensemble and the ch7-assessed ECS range. .
38424	5	38			The term „inferred“ requires somewhat more explanation [Dirk Notz, Germany]	Taken into account. Assessment reformulated.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
15262	5	44	5	44	Related to the previous it is not clear how this is arrved at and why we should care. Are the authors using ECS range as a surrogate for models structural uncertainty? [Claudia Tebaldi, United States of America]	Taken into account. The ES has been reconstructed, making clearer the relationship between the CMIP6 ensemble and the ch7-assessed ECS range. .
6912	5	44	5	50	I worry that uncertainty in global mean atmospheric temperature due to the "likely" and "very likely" uncertainty range in equilibrium climate sensitivity is compared to that one due to internal variability, without specifying which range is used here: likely or very likely. This may create confusion. [Olson Roman, Republic of Korea]	Taken into account. The ES has been reconstructed, making clearer the relationship between the CMIP6 ensemble and the ch7-assessed ECS range. .
57336	5	44	5	50	It is misleading to label this uncertainty as arising from the ECS, when it in fact arises from the TCR, and my understanding is that the range of TCRs in CMIP6 not as inconsistent with observationally-constrained ranges as the range of ECS values. [Myles Allen, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Assessed TCR is also used in SOD. Whether TCR is indeed and not only theoretically the better predictor for 21st-century warming is not clear, however (e.g., Grose et al. 2018).
11520	5	45	5	45	Delete "irreducible". It is not clear to me what is meant. If kept, need to define "irreducible uncertainty". [Roanld Stouffer, United States of America]	Rejected. The term is important.
32658	5	46	5	47	This is pretty full of jargon. I'd urge the whole Executive Summary be looked at with the intent of making it more understandable for a general scientific reader, making clear why the various points are important instead of just what they are. [Michael MacCracken, United States of America]	Taken into account. It is not clear, however, whether the ES is already the point where the general reader is being addressed, or whether this occurs higher up the ES--TS--SPM ladder. Insisting on traceability to the main text, tight length limitations, and on dropping jargon might pose an impossible target.
35826	5	46	5	47	Is the period missing here (near-term)? As written this sentence says that GSAT uncertainty arising from the ECS likely range is similar to the internal variability uncertainty (no period specified), which contradicts the previous sentence which says that the former is substantially larger. [Nathan Gillett, Canada]	Noted. No, the statement applies over all periods.
32660	5	47	5	47	Do you really mean "predictions"--should this not be "projections, or are all factors being considered here (natural variability, solar, volcanic eruptions, ENSO, and so on)? Is this not a conditioned forecast, so a projection instead of a prediction? [Michael MacCracken, United States of America]	Noted. Indeed "prediction" is meant in its strict sense, as expressed by "initialized".
37640	5	47	5	50	What are the prospects for additional predictions for 2019 to 2028 from other systems? One would expect that there are at least results available from the Met Office decadal prediction system. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. SOD contains two more CMIP6 predictions for 2019--2028. Note, though, that it has been surprisingly hard to get the combination of CMIP6 and 2019--2028 predictions.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
32050	5	47	5	50	This conclusion is almost certainly sensitive to reference period; this issue needs to be acknowledged. [Rowan Sutton, United Kingdom (of Great Britain and Northern Ireland)]	Noted. FOD already specifies the period.
51622	5	52	4	55	Can you give some life to this sentence, so that readers appreciate the human/nature suffering/loss of life related to 'significantly and detectably further removed', otherwise the profound nature of what you are saying is too easily lost. [Lindsey Cook, Germany]	Taken into account; assessment reformulated d. Note, however, that "impacts" are not in the Ch4 remit.
35828	5	52	5	54	The meaning of this statement is not clear. It is not clear if the text means that RCP8.5 is further removed from today's climate than it is from RCP 4.5. Or if RCP8.5 is further removed from today's climate than RCP4.5 is. Both conclusions are probably true, but it isn't clear which is meant. Further, the meaning of ''the range of regional climate states is.. significantly and detectably further removed.. even in the presence of internal variability' isn't clear. Is this saying that the range of climate states is non-overlapping in the different scenarios? (the use of the word 'range' implies this). Or just that the regional climate states are significantly different in the different scenarios? Either way, these statements are in my view not very meaningful unless they specify an actual climate variable and spatial and temporal averaging. For example, 30-year mean continental-mean SAT will have non-overlapping 5-95% ranges in different climate scenarios before daily-mean grid-cell-mean precipitation. [Nathan Gillett, Canada]	Accepted. Statement removed.
9006	5	52			What is the "latter portion of the 21st century" in terms of the near-, mid- , long-, very long- term descriptors used elsewhere? [Anna Merrifield, Switzerland]	Taken into account. Assessment reformulated.
6924	5		7		The executive summary is very well structured and well organized. I appreciate the provision of information for different variables using the IPCC uncertainty language [Olson Roman, Republic of Korea]	Noted, thank you.
53372	6	1	6	5	This is an important issue and statement. Need to develop this further. [Jan Fuglestvedt, Norway]	Taken into account. Length limitations apply, though.
9146	6	1	6	5	It seems an extraordinary statement that there is low confidence on whether achieving a Zero Emissions Commitment would actually cause subsequent warming or cooling. [Jim O'Brien, Ireland]	Noted.
56428	6	2	6	2	I assume "all emissons" should read "CO2 emissions"? [Kirsten Zickfeld, Canada]	Taken into account; clarified.
35830	6	3	6	5	Carbon budgets have not been introduced or explained at this point in the ES. I suggest omitting the discussion on the effect on carbon budgets, and just focussing on the ZEC itself. [Nathan Gillett, Canada]	Taken into account. Conflicting suggestions on this in FOD review.
15590	6	7	6	7	Would it make sense to have the tile as 'Global land precipitation"? [Izidine Pinto, South Africa]	Noted. Subheadings have been reduced in number.

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11522	6	7	6	21	It needs noted that while the precipitation over land increases, the demand for water (i.e. P-E) increases more. This means that most land areas will be DRIER in spite of the increased P. [Roanld Stouffer, United States of America]	Noted. P-E is covered in Ch8.
13182	6	7	6	21	What about evaporation rates? Will certain regions get drier over time and how certain are we of that occurring? [Nora Richter, United States of America]	Noted. Evaporation is covered in Ch8.
47210	6	9	6	11	Replace « higher » by « only slightly higher » or better give numbers (the increase in global land precipitation is expected to much less than in global mean precipitation, e.g. Samset et al. 2017) in order to avoid a misleading message about increased water availability at the global scale. I would even suggest to add a sentence to this paragraph suggesting that this increase in precipitation does not necessarily translate into increased water availability (P-E) given the projected increase in land surface evapotranspiration (with a possible link to Chapter 8). [Hervé Douville, France]	Taken into account. In SOD, ES precipitation statement is considerably changed from the FOD one.. The likely range of precipitation change is clearly given.
36526	6	9	6	11	: “Based on results from the five CMIP6 models available, it is very likely that global land precipitation will be higher during the period 2081–2100 than during the period 1995–2014, under all scenarios considered here (medium confidence due to limited data availability)” Could you add, as you did for GSAT, “The uncertainty in projecting precipitation that arises from the ECS very likely range is substantially larger than the irreducible uncertainty arising from internal variability for the period…. This is a concern for precipitation associated with monsoons, which has a large natural variability. [Carlos Mechoso, United States of America]	Noted. In SOD, ES precipitation statement is considerably changed from the FOD one.. The likely range of precipitation change is clearly given.
15574	6	9	6	11	When the authors state that " is very likely that global land precipitation will be higher" are the authors referring to global averaged mean precipitation? I would suggest make clear what statistical quantity of precipitation are the authors reffering to. [Izidine Pinto, South Africa]	Taken into account. In SOD, ES precipitation statement is considerably changed from the FOD one. The likely range of precipitation change is clearly given.
35832	6	9	6	14	These two statements about long-term changes in global precipitation are inconsistent. Ln 9-11 says that there is only medium confidence that it is very likely that global land precipitation will be higher in 2081-2100 in all scenarios considered. Ln 13-15 says that it is virtually certain that in the long-term (2081-2100?) global mean precipitation will increase as GSAT rises. Since no confidence statement is specified, the assumed level of confidence is at least high confidence according to the IPCC GPGP on uncertainties. Thus the two statements have inconsistent confidence levels. [Nathan Gillett, Canada]	Taken into account. In SOD, ES precipitation statement is considerably changed from the FOD one.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
32662	6	9	6	21	These statements all need to be qualified or there will be tremendous misinterpretation of them. First, it needs to be said HERE and not just elsewhere that the distribution of the changes in rainfall will not be uniform, that there will be areas of both increases and decreases. Second, it needs to be said that the trend toward an increased whare of precipitation coming in intense events is expected to continue (I presume). Third, it needs to be said that evaporation will also be increasing, and then fourth, what really matters for agriculture, water resources and more is the integrated timing and effects of all of this. So, more precipitation is not helpful to farmers if it comes all at once, or in the wrong season, if there are longer dry episodes, and so on (similar complications for water resources, etc.). It seems to me what needs to be said here is that the hydrologic cycle over land change, and how this will play out in particular regions and for particular economic sectors will need to be evaluated, etc. As stated here (the last, rather vague and innocuous sounding sentence on lines 20-21 not withstanding), the impression given is generally that this is a positive, and that simply is not the case when one considers all aspects. [Michael MacCracken, United States of America]	Taken into account.. The SOD makes clear on changes in rainfall pattern which is not uniform. However, note that the complete water cycle is covered in Ch8 and change in extreme rainfall event is assessed in Ch11.
57840	6	9		21	Strongly agreed on extreme precipitation, temperature variability of the monsoon regions and the high latitudes. Precipitation changes between 2007,2009,2011, and 201u shown in the climate models for each regions, projected an increase in the precipitation in near term, which would be greater between the year 2081-2100. In skeletal geography of the world; precipitation fluctuations may occur in the future, which had pccured in the past two decades. But an integrated approach on precipitation models could identify the needs for climatic indices and Atmospheric parameters in relation to precipitation globally. [Abiodun Adegoke, Nigeria]	Noted. The level of detail requested here goes beyond the Ch4 remit, though. For the integrated approach suggested, this chapter already includes changes in global climatic indices related to precipitation change globally such as global land precipitation, global monsoon precipitation and global monsoon circulation index.
40514	6	13	6	16	I refer only to this paragraph since I am looking at monsoon aspects, but this is a more general issue: I'm a little troubled that the Executive Summary is taking information only from CMIP6 data. Surely the IPCC assessment should be taking information from whatever is available: all types of models, including CMIP5. There have been multilple CMIP5 studies published since the AR5 (covering all aspects of climate science). To restrict the Executive Summary to CMIP6 only suggests an underlying assumption that CMIP6 data are somehow "better" than CMIP5 - which is untested. Comparisons of CMIP3 and CMIP5, for example, showed that they could be treated as entirely separate datasets rather than a simple improvement of one over the other. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The SOD is based on multiple lines of evidence including CMIP6 and CMIP5.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
6914	6	13			It is unclear what the "long term" means. Is it beyond the year 2100? [Olson Roman, Republic of Korea]	Taken into account. Clarified in SOD.
57606	6	14	6	14	need to mention the decreasing of rainfall in mid latitude belt. [Sahar Tajbakhsh Mosalman, Iran]	Taken into account. In SOD, ES precipitation statement is considerably changed from the FOD one. Pattern of precipitation change is assessed comprehensively including regional contrast.
26196	6	14	6	14	need to mention the decreasing of rainfall in mid latitude belt. [iman babaeian, Iran]	Taken into account. In SOD, ES precipitation statement is considerably changed from the FOD one. Pattern of precipitation change is assessed comprehensively including regional contrast.
46330	6	14	6	14	need to mention the decreasing of rainfall in mid latitude belt. [sadegh zeyaeyan, Iran]	Taken into account. In SOD, ES precipitation statement is considerably changed from the FOD one. Pattern of precipitation change is assessed comprehensively including regional contrast.
49920	6	14	6	15	Pl. add values for SSP2-4.5 and SSP3-7.0, to maintain consistency with earlier temperature scenarios paragarphs [Govindarajalu Srinivasan, Thailand]	Taken into account. In SOD, ES precipitation statement is considerably changed from the FOD one. Precipitation changes under all five SSPs are mentioned in ES.
38430	6	14	6	18	I am confused that in line 13, the link between GSAT and precipitation is assessed as virtually certain, while in line 18/19, apparently the same link is assessed as very likely. [Dirk Notz, Germany]	Taken into account. In SOD, we assess very likely for both aspects
35834	6	18	6	19	This seems to be identical to the assessment on lines 13-14, except that it is based on three models instead of five, and the likelihood leavel is 'very likely' instead of 'virtually certain'. [Nathan Gillett, Canada]	Taken into account. The statement is revised as 'very likely',
6916	6	18	6	21	It seems that some of the information in this paragraph overlaps with information in the previous paragraph. It seems that the previous paragraph meant to replace some of the text in this paragraph. Please check. [Olson Roman, Republic of Korea]	Taken into account. The ES statement is revised to avoid any overlap with other statement in ES.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
37642	6	20	6	21	It is stated here that it is "very likely" that extreme precipitation events will increase. But in Chapter 11, page 11-7, line 2, it is stated that it is "likely" that observed upward trends in heavy precipitation will continue. Some reconciliation of these two likelihoods is needed. Also, it needs to be clear what increases in heavy preciptation events means. Is it increases in the number of events, the rainfall per event, or both? [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. ES for extreme precipitation change is taken out in the SOD in coordination with Ch11. Extreme precipitation assessment in main text of the SOD is now consistent with Ch11.
15582	6	20	6	21	Is extreme precipitation here including heavy precipitation and lack of rainfall ("droughts")? If referring only to heavy precipitation the confidence language in chapter 11 is 'likely' while in here is 'very likely' [Izidine Pinto, South Africa]	Taken into account. . ES for extreme precipitation change is taken out in the SOD in coordination with Ch11. Extreme precipitation assessment in main text of the SOD is now consistent with Ch11.
47212	6	25	6	27	This paragraph is illustrative of the potential underestimation of internal variability when talking about near-term climate change. While I agree that there is (at least) medium confidence about opposite changes in monson circulation and precipitation on a long-term (2081-2100) perspective, a similar paradox for the near-term perspective (2021-2040) is more difficult to reconcile with the strong natural multi-decadal variability of the global (and mostly regional) monsoon(s). The first sentence of the paragraph may be true for the forced monsoon response but may be wrong for the actual near-term evolution of the global (regional) monsoon(s), a speculation that could be confirmed by the use of a large ICE. [Hervé Douville, France]	Taken into account. In SOD, ES precipitation statement is considerably changed from the FOD one. The SOD clearly separates long-term and near-term precipitation in ES including the role of internal variability.
32664	6	25	6	32	This is phrased in terms of those thinking about global energetics--but the likely reader is going to be the individual in a particular location wanting to know what is going to happen to their monsoon. Text needs to be added on spatial aspects--namely that the extent and timing of the monsoons is going to shift somewhat--and those on the edges where the shifts occur are likely to be significantly affected. I really think some further development of the nuances involved needs to be expressed. [Michael MacCracken, United States of America]	Not applicable. This chapter mainly assesses monsoon precipitation and circulation in a global context. The details of regional monsoon change including onset, rainy period, and intensity as well as ITCZ shift and water cycle change are well covered by Ch8.
56430	6	29	6	30	Metrics such as precipitation index and circulation index need to be defined. [Kirsten Zickfeld, Canada]	Rejected. The metrics of global monsoon precipitation and circulation index is well introduced in the main text. Due to tight length limitation in ES, metrics to define indices and modes of variability cannot be included here.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
49922	6	29	6	32	About monsoons, one of the often-demanded information by policy makers and users is “onset time” and “duration”. Any indication, or lack of it, stated here will be very relevant [Govindarajalu Srinivasan, Thailand]	Not applicable. Note that the complete water cycle is covered in Ch8. The metrics of global monsoon precipitation and circulation index is well introduced in the main text. Due to tight length limitation in ES, metrics to define indices and modes of variability cannot be included here.
9008	6	29	6	32	Is a weakening of the global monsoon circulation index physically consistent with a strengthening in monsoon precipitation? [Anna Merrifield, Switzerland]	Not applicable. Precipitation change can be occurred with the combination of thermodynamic and dynamic contributions. Although there will be a weakening of circulation (dynamic factor) in response to global warming, precipitation can be still increased due to increase in atmospheric moisture (thermodynamic factor).
26958	6	29	6	32	In the executive summary, it would be preferable to write what this actually means. Suggestion: replace by lines 36 - 38 from page 50. [Joachim Rock, Germany]	Taken into account. The sentence is revised more clearly in the SOD.
40516	6	32			Insert "s" at the end of "simulation" [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Corrected, thank you.
32054	6	36	6	36	To be of any use for policy makers some quantitative information much be provided on the magnitude of changes not just the sign. [Rowan Sutton, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. However, it is very challengeable to provide quantitative information on changes in circulation magnitude due to large discrepancy among model projections.
51898	6	36	6	46	This key finding would be more digestible if it were to be split into several self-contained points which may cover different subsets of the finding in turn? [Peter Thorne, Ireland]	Accepted. The FOD ES on sea level pressure and large-scale atmospheric circulation was too abbreviated. The SOD ES on this issue is expanded with several separated key points.
57608	6	37	6	37	This is in contrast with findings of other researchers. Usually polar front jet streams (PFJ) develop in the region of maximum temperature gradient, nearing to the sub polar region. As the rate of warming in polar region is much higher than mid latitude and tropics. In this regard, by weakening the temperature gradient of equator-to-pole, the place of PFj will be unstable and hit the lower latitude more than usual. [Sahar Tajbakhsh Mosalman, Iran]	Taken into account. There is substantial disagreement in the published literature whether the weakening of the meridional surface temperature gradient is the dominant driver of change in atmospheric dynamics.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
26198	6	37	6	37	This is in contrast with findings of other researchers. Usually polar front jet streams (PFJ) develop in the region of maximum temperature gradient, nearing to the sub polar region. As the rate of warming in polar region is much higher than mid latitude and tropics. In this regard, by weakening the temperature gradient of equator-to-pole, the place of PFj will be unstable and hit the lower latitude more than usual. [iman babaeian, Iran]	Taken into account. There is substantial disagreement in the published literature whether the weakening of the meridional surface temperature gradient is the dominant driver of change in atmospheric dynamics.
46332	6	37	6	37	This is in contrast with findings of other researchers. Usually polar front jet streams (PFJ) develop in the region of maximum temperature gradient, nearing to the sub polar region. As the rate of warming in polar region is much higher than mid latitude and tropics. In this regard, by weakening the temperature gradient of equator-to-pole, the place of PFj will be unstable and hit the lower latitude more than usual. [sadegh zeyaeyan, Iran]	Taken into account. There is substantial disagreement in the published literature whether the weakening of the meridional surface temperature gradient is the dominant driver of change in atmospheric dynamics.
29968	6	37	6	38	Is the statement about a poleward shift in the midlatitude jet for the NH referring to the zonal mean? If so, this is an irrelevant metric. If not, then it is not an accurate description over the North Atantic/European sector in wintertime, at least if the CMIP5 results (reported in Zappa et al. 2013, and discussed in AR5) are holding up for CMIP6. Rather, it is more like a downstream extension. [Theodore Shepherd, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The section of the ES on atmospheric circulation projections has been rewritten in the SOD and this specific statement has been removed.
9010	6	39	6	42	Could this be separated into two sentences? It's not clear the relationship between poleward expansion and the SAM. [Anna Merrifield, Switzerland]	Accepted and implemented in SOD.
37644	6	40	6	41	The wording in these two lines implies that ozone is not a greenhouse gas. If the acronym GHG is defined as covering only a subset of greenhouse gases (CO2, CH4, N2O, etc),OK, but without such a definition, the word "other" is needed before GHGs. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The wording has been clarified in the SOD to refer to "other greenhouse gases".
29970	6	41	6	41	It is precisely because the ozone-recovery effect is opposing the GHG effect that internal variability could overwhelm the (very small) forced trends, therefore "though" should be changed to "thus" or "such that". I do not consider this an editorial comment as it affects the logic of the statement. [Theodore Shepherd, United Kingdom (of Great Britain and Northern Ireland)]	Accepted and implemented in SOD.
35836	6	41			The word 'though' isn't appropriate here. The first part of the sentence says that it is very likely that ozone recovery will oppose GHG-induced poleward expansion (i.e. make the forced trend small), the second part says 'although internal variability could overwhelm forced trend'. Ozone recovery opposing the GHG-forced trend makes it more likely that internal variability will overwhelm the forced trend. I suggest separating into two sentences. [Nathan Gillett, Canada]	Taken into account; clarified.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
9012	6	42			Perhaps "obscure" rather than "overwhelm"? [Anna Merrifield, Switzerland]	Accepted.
45778	6	48	6	48	Remove biosphere from title as not assessed [Katja Mintenbeck, Germany]	Accepted and implemented in SOD.
27204	6	50	6	53	The Danish Meteorological Bureau reports that the sea Arctic volume has not shown any decrease since 2007. http://ocean.dmi.dk/arctic/icethickness/thk.uk.php These observations seems to contradict the projections of ice-free Arctic in september. [François GERVAIS, France]	Rejected. Comment stands in stark contrast to Ch2 assessment.
42324	6	50	6	53	There is a marked decreased in the likelihood of an ice-free Arctic if warming is limited to 1.5C compared to 2C. Arctic Monitoring and Assessment Programme (AMAP) (2017) SNOW, WATER, ICE, AND PERMAFROST IN THE ARCTIC: SUMMARY FOR POLICYMAKERS; Sanderson B. M., et al. (2017) Community climate simulations to assess avoided impacts in 1.5 and 2 ºC futures, EARTH SYSTEM DYNAMICS 8:827–847; Screen J. A. & Williamson D. (2017) Ice-free Arctic at 1.5ºC?, NATURE CLIMATE CHANGE 7:230–231; Jahn A. (2018) Reduced probability of ice-free summers for 1.5 °C compared to 2 °C warming, NATURE CLIMATE CHANGE 8:409–413. [Gabrielle Dreyfus, United States of America]	Taken into account. The ES has been reconstructed, focusing on its own storyline rather than the chapter's. This also concerns the time/scenario vs warming-level perspective.
12644	6	50	6	53	There is a marked decreased in the likelihood of an ice-free Arctic if warming is limited to 1.5C compared to 2C. Arctic Monitoring and Assessment Programme (AMAP) (2017) SNOW, WATER, ICE, AND PERMAFROST IN THE ARCTIC: SUMMARY FOR POLICYMAKERS; Sanderson B. M., et al. (2017) Community climate simulations to assess avoided impacts in 1.5 and 2 ºC futures, EARTH SYSTEM DYNAMICS 8:827–847; Screen J. A. & Williamson D. (2017) Ice-free Arctic at 1.5ºC?, NATURE CLIMATE CHANGE 7:230–231; Jahn A. (2018) Reduced probability of ice-free summers for 1.5 °C compared to 2 °C warming, NATURE CLIMATE CHANGE 8:409–413. [Kristin Campbell, United States of America]	Taken into account. The ES has been reconstructed, focusing on its own storyline rather than the chapter's. This also concerns the time/scenario vs warming-level perspective.
57458	6	50	6	53	Is this true under even the lowest emission scenarios, or if we hold warming to 1.5 or 2 K? [Kyle Armour, United States of America]	Noted. Statement correct for the models available for FOD under the four priority SSPs.
13184	6	50	6	53	What about changes in Antarctica and mountain glaciers? Include a few sentences talking about current changes and uncertainites. [Nora Richter, United States of America]	Rejected. Covered in Ch9.
52394	6	50	6	53	Suggest this statement tie ice-free Septembers to temperature rather than date, eg 2100, since the status of Arctic sea ice is highly dependent on GMT at that time. This can be based on the SROCC. [Pam Pearson, Sweden]	Taken into account. The ES has been reconstructed, focusing on its own storyline rather than the chapter's. This also concerns the time/scenario vs warming-level perspective.
9148	6	50	6	53	See comment 17 above; previous predictions of an ice-free Arctic have been proven wrong! [Jim O'Brien, Ireland]	Noted. Too unspecific to be considered an action item.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
12798	6	50	6	53	There is a marked decreased in the likelihood of an ice-free Arctic if warming is limited to 1.5C compared to 2C. Arctic Monitoring and Assessment Programme (AMAP) (2017) SNOW, WATER, ICE, AND PERMAFROST IN THE ARCTIC: SUMMARY FOR POLICYMAKERS; Sanderson B. M., et al. (2017) Community climate simulations to assess avoided impacts in 1.5 and 2 ºC futures, EARTH SYSTEM DYNAMICS 8:827–847; Screen J. A. & Williamson D. (2017) Ice-free Arctic at 1.5ºC?, NATURE CLIMATE CHANGE 7:230–231; Jahn A. (2018) Reduced probability of ice-free summers for 1.5 °C compared to 2 °C warming, NATURE CLIMATE CHANGE 8:409–413. [Durwood Zaelke, United States of America]	Taken into account. The ES has been reconstructed, focusing on its own storyline rather than the chapter's. This also concerns the time/scenario vs warming-level perspective.
16020	6	51	6	51	For clarity sake, "Ocean" should be added after "Arctic". [SAI MING LEE, China]	Accepted.
9014	6	51			"Permanently" suggests a completely ice-free Arctic all year.. The sentence reads well as: … the Arctic will become effectively ice-free (..) in September by the end of the 21st century. [Anna Merrifield, Switzerland]	Accepted.
46016	6	52	6	52	Including September implies a confidence in the exact month that the Arctic will become ice-free, as opposed to what the intent of the sentence is which is to say that ice-free means less than 1 million km2 in September [Isaac Pearlman, United States of America]	Rejected. The summer minimum occurs robustly in September.
32952	6	55	7	4	It would be best to use the global mean SL projections from CH9 for cross-report consistency & discuss with CH9 about confidence statements (Kopp/Slangen) [Aimee Slangen, Netherlands]	Accepted and implemented in SOD.
32052	6		6		I support the choice to present projections for other variables conditional on GSAT [Rowan Sutton, United Kingdom (of Great Britain and Northern Ireland)]	Noted, thank you.
43890	7	1	7	2	the text in line 1 and line two about the three models used for the different scenarios should indicated that they are the same models (or different) which were used for SSP1 - 2.6 and SSP5 - 8.5 [Michael Mugarura, Germany]	Taken into account. Many more models are available for the SOD.
9150	7	1	7	4	It is interesting to see only 45cm sea level rise predicted even under the unrealistic SSP5-8.5 scenario, which actually indicates that sea level rise in very unlikely to exceed 30cm by 2100. [Jim O'Brien, Ireland]	Noted. No action item discernible. The estimation is only from thermosteric contribution. The SOD includes other contributions to global sea level rise including ice sheet in coordination with Ch9.
15266	7	3	7	3	the use of the word monotonic suggests that every year would see a sea level higher than the previous, but it would not be the case. [Claudia Tebaldi, United States of America]	Taken into account. Wording changed.
51624	7	3	7	3	While a highly educated native English speaker, I have no concept of what 'monotonic' means. Any better way to describe to policy makers? [Lindsey Cook, Germany]	Taken into account. Wording changed.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
56432	7	6	7	12	Paragraph overlaps with assessment in chapter 5. [Kirsten Zickfeld, Canada]	Taken into account. Ocean acidification should be covered here, too, because of its iconic nature, but treatment has been harmonised with Ch5.
9016	7	9	7	10	Replace "increasing" with "an increase in" in both instances. [Anna Merrifield, Switzerland]	Accepted.
47214	7	10	7	12	Add here a brief sentence about the fate of the terrestrial carbon sink, in line with the key messages from Chapter 5. [Hervé Douville, France]	Rejected. The terrestrial sink, in contrast to ocean acidification, cannot be readily covered in Ch4 without undue underpinning and hence effort.
52396	7	14	7	14	Suggest some of the conclusions of 4.7.2 on commitment and irreversibility be brought into the Executive Summary. [Pam Pearson, Sweden]	Taken into account. ES restructured for SOD.
35838	7	16	7	17	The statement that the NAM is very likely to become more positive under SSP5-8.5 appears strong when set against the low confidence statement on pg 6, ln 37-38 that models predict a poleward shift of mid-latitude jets in both hemispheres. [Nathan Gillett, Canada]	Taken into account. The section of the ES on atmospheric circulation projections has been rewritten in the SOD. We are more careful in the SOD to distinguish between confidence statements for the zonal mean circulation (NAM) and regional projections (e.g., the North Atlantic).
32056	7	16	7	19	Any assessments in this chapter (or others) should not be based solely on CMIP6 model results, since they absolutely must take into account assessment of model adequacy for the specific issue being assessed. This is a fundmental methodological issue which should be made clear at the beginning of the chapter, and applied consistently to all assessments. See also my Comment 30 on Box 4.1. [Rowan Sutton, United Kingdom (of Great Britain and Northern Ireland)]	Noted. FOD ES states that in its first paragraph following the chapeau.
39508	7	18	7	18	It is confusing to talk about "SAM weakening" considering that it is a mode of variability with a phase and amplitude associated. I suggest to describe the SAM changes as positive or negative, like it is done for NAM, or clarify if the statement describes SAM positive trend. [Carolina Vera, Argentina]	Taken into account. The SOD clarifies changes in trend and variability of the SAM.
30664	7	18	7	18	"recovers" should be "will rcover" [Annalisa Cherchi, Italy]	Noted. It's not so clear because the statement refers to present-day simulation results.
38962	7	18			Is "weaken" meant for reduced variablity? [Masahide Kimoto, Japan]	Noted; indeed that's meant.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
9018	7	21			Would it be possible to comment on tendencies in ENSO frequency and amplitude in addition to variability? [Anna Merrifield, Switzerland]	Taken into account. Literature on changes in ENSO frequency is very limited. Thus, it is not included in SOD. However, we will assess in the FGD if literature is available. The current metrics for ENSO variability change used in both FOD and SOD reflects ENSO amplitude change.
31486	7	22			Is the difference between AOGCMs and ESMs somewhere explained? [Rein Haarsma, Netherlands]	Taken into account -- explanation added. (Refers to FOD page 8).
16022	7	26	7	27	According to the main text of Section 4.5.3.2, the ENSO-related rainfall is not necessarily confined to the Nino 3.4 region. [SAI MING LEE, China]	Noted. The ENSO-related rainfall is not confined to the Nino 3.4 region.
47642	7	29	7	44	Because most 1.5°C pathways are overshoot scenarios, it seems important to include a conclusion on the knowledeg of the reversibility of the climate patterns following overshoot of a global temperature target. This seem improtant and policy relevant . [Birgit van Munster, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. ES restructured for SOD.
12800	7	29	8	3	Include the benefit of SLCP mitigation where the effects are more immediate than the effects of CO2 mitigation, helping reduce the rate of warming and warming in the near-term, which is crucial for avoiding feedbacks and tipping points. 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 ("We define ''fast-action'' to include regulatory measures that can begin within 2–3 years, be substantially implemented in 5–10 years, and produce a climate response within decades. We discuss strategies for short-lived non-CO2 GHGs and particles, where existing agreements can be used to accomplish mitigation objectives. Policy makers can amend the Montreal Protocol to phase down the production and consumption of hydrofluorocarbons (HFCs) with high global warming potential. Other fast-action strategies can reduce emissions of black carbon particles and precursor gases that lead to ozone formation in the lower atmosphere, and increase biosequestration, including through biochar. These and other fast-action strategies may reduce the risk of abrupt climate change in the next few decades by complementing cuts in CO2 emissions."); Xu and Ramanathan (2017) Well below 2 °C: Mitigation strategies for avoiding dangerous to catastrophic climate changes, Proc. Natl. Acad. Sci., doi: 10.1073/pnas.1618481114; 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; Arctic Monitoring and Assessment Programme	Taken into account. The ES has been reconstructed, including SLCF mitigation.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
12646	7	29	8	3	Include the benefit to SLCP mitigation where the effects are more immediate than the effects of CO2 mitigation, helping reduce the rate of warming and warming in the near-term, which is crucial for avoiding feedbacks and tipping points. 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 ("We define ''fast-action'' to include regulatory measures that can begin within 2–3 years, be substantially implemented in 5–10 years, and produce a climate response within decades. We discuss strategies for short-lived non-CO2 GHGs and particles, where existing agreements can be used to accomplish mitigation objectives. Policy makers can amend the Montreal Protocol to phase down the production and consumption of hydrofluorocarbons (HFCs) with high global warming potential. Other fast-action strategies can reduce emissions of black carbon particles and precursor gases that lead to ozone formation in the lower atmosphere, and increase biosequestration, including through biochar. These and other fast-action strategies may reduce the risk of abrupt climate change in the next few decades by complementing cuts in CO2 emissions."); Xu and Ramanathan (2017) Well below 2 °C: Mitigation strategies for avoiding dangerous to catastrophic climate changes, Proc. Natl. Acad. Sci., doi: 10.1073/pnas.1618481114; 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; Arctic Monitoring and Assessment Programme	Taken into account. The ES has been reconstructed, including SLCF mitigation.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
35842	7	31	7	34	This text discussing the effect of reduced GHG emissions on CO2 concentration and climate appears to suggest that GHG emissions reductions by themselves are sufficient to reverse global warming. 'There is high confidence that mitigation through reduced greenhouse gas (GHG) emissions would slow and limit the degree of climate change relative to high emissions reference scenarios.... There would be a lag between emissions peak, CO2 concentration peak, and peak in surface temperature'. First reduced GHG emissions will not necessarily lead to either a peak in CO2 concentration or a peak in surface temperature. CO2 concentration woldl only peak if the CO2 emissions are very strongly reduced to less than the level of annual removals by the sinks - the current text implies any GHG emissions reductions would lead to a peak and decline. As discussed elsewhere in the chapter, the global mean temperature may not decline for 100s to 1000s of years even if CO2 emissions are reduced to zero - this depends on the zero emissions commitment of the climate system. The full response will depend on the mix of GHG emissions reductions, as well as changes in SLCFs, but even with such considerations it is not clear that 'reduced GHG emissions' to zero would lead to 'a peak in surface temperature' on human timescales. [Nathan Gillett, Canada]	Taken into account. ES restructured for SOD.
51900	7	31	7	38	To my view it would be worth clarifying why warming may be faster in the short term (presumably that the cooling effect of aerosols goes to zero virtually instantaneously?) [Peter Thorne, Ireland]	Taken into account. Reformulated for SOD, to make clear that the effect -- should it occur -- is due to internal variability.
9152	7	31	7	44	It is interesting to see that with significant emissions reduction or CO2 removal that global temperatures might rise even faster! [Jim O'Brien, Ireland]	Taken into account. Reformulated for SOD, to make clear that the effect -- should it occur -- is due to internal variability.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
57842	7	31		54	Carbon emi,greenhouses gases inventory by countries (GHGs inventory). The gap between the developing countries acting on the GHGs is larger yhan the developed countries. If the mitigating response or measures are higher per regions as projected by the United Nations Framework convention on climate change (UNFCCC 2019) per country is adopted and sma successful along with comprehensive integrated approach ti greenhouses gases emissions need to be implemented, actions must be taken by nations, then thats when we have high confidence in mitigating process. Closely monitoring the work done per countries on the Carbon dioxide emissions (CO2) wgich is the primary and the most important part in Greenhouses gases. As stated in April 2019 by tge UNFCCC on reducing the carbon dioxide concentration per country to archive the primary goal in line the Kyoto, Montreal Protocol and Paris Agreements. [Abiodun Adegoke, Nigeria]	Noted. No action item discernible.
53374	7	32	7	32	What is "high emission reference scenario"? Please clarify what you have in mind? [Jan Fuglestvedt, Norway]	Taken into account. Reformulated for SOD. .
15334	7	33	7	34	Is it possible to describe the duration of the lag between CO2 concentration peak and peak in surface temperature using a range of years? This would be helpful to policymakers to understand. [Lia Cairone, United States of America]	Taken into account. Reformulated for SOD. .

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
35844	7	34	7	38	This text regarding the 'delay in detectability' of emissions reductions is unclear, and as written this paragraph seems to imply that reducing GHG emissions won't have any climate benefits before the mid to late 21st century, with a lot of focus on the lack of detectable benefit of emissions reductions in the near term. Recognising that the primary audience of the report is policymakers, rather than scientists, I would recommend re-phrasing along the lines 'Any reduction in greenhouse gas emissions will reduce projected warming rates, with the effects of emissions reductions following SSP1-2.6 relative to SSP5-8.5 becoming detectable in decadal means of atmospheric CO2 by xx, decadal means of global mean temperature by yy, and affects becoming detectable later on shorter timescales, regional scales, and in most other varibles. Internal variability in the climate system precludes earlier detection of the response to emissions reductions.' As written, the text refers to 'delay in detectability' without first explaining what this is. The statement that the 'delay in detectability' results from 'intertia and internal variability' in the physical climate system rather than the carbon cycle is ambiguous, and arguably wrong. It is not clear what the inertia of the carbon cycle is, but this could be taken as referring to long atmospheric lifetime of CO2. If CO2 had an atmospheric lifetime of one year rather than centuries, this would undoubtedly substantially reduce the delay in detectability of emissions reductions. [Nathan Gillett, Canada]	Taken into account. Reformulated for SOD. .
43892	7	36	7	36	the statement "Recent modelling results and our improved understanding suggest that over the near term" may require atleast one source of the modelling results [Michael Mugarura, Germany]	Taken into account. Reformulated for SOD. .
37646	7	37			Insert the word "air" before "temperature" in this line. Otherwise "global mean surface temperature" might be thought to be GMST not GSAT. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Statement re-phrased.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
35848	7	40	7	42	There are a number of issues with this assessment of the effects of CDR. First, the paragraph starts by by referring to CDR 'at scales as large as currently represented in the RCP8.5 scenario'. But RCP 8.5 doesn't require CDR - it still has large positive CO2 emissions in 2100 (see e.g. IPCC AR5 WGIII Figure SPM.4). Second the statement on the effectiveness and side effects of CDR, which is taken directly from Keller et al. (2014), refers to 'all CDR methods', but Keller et al. only consider a subset of CDR methods. In particular, Keller et al. (2014) do not consider BECCS, which is the CDR method most assessed in AR5 (e.g. SPM4.1 of IPCC AR5 WGIII). Third, according to page 66, line 37, this chapter only assess the climate response to CDR, and not the biogeochemical response, which is assessed in Chapter 5. But the potentially severe side effects referred to in Keller et al. (2014), from which this text is taken, are mainly biogeochemical. This assessment should be replaced with one focussed on the climate response to CDR, based on all relevant literature, and also considering BECCS and other CDR approaches not considered by Keller et al. (2014). [Nathan Gillett, Canada]	Taken into account. Reformulated for SOD. .
35886	7	40	7	42	This assessment appears to be overlapping with, and somewhat disagree with, the assessment made in the Chapter 5 ES that 'Deployment of CDR methods can have beneficial and adverse environmental side effects (very high confidence).' [Nathan Gillett, Canada]	Taken into account. Reformulated for SOD. .
55556	7	40	7	43	The sentence seems to misrepresent the relationship between RCP's (specifically 8.5) and emissions-reduction and removal measures. [Matthisa Honegger, Germany]	Taken into account. Reformulated for SOD. .
12802	7	40	7	44	Nature-based CDR can make a significant contribution. Natural climate solutions can provide more than a third of cost-effective CO2 mitigation needed before 2030. Brandon W. Griscom et al., Natural Climate Solutions, Proceedings of the National Academy of Sciences (2017). The Rodale Institute, in their report Regenerative Organic Agriculture and Climate Change (2014), argues that “we could sequester more than 100% of current annual CO2 emissions with a switch to widely available and inexpensive organic management practices, which we term ‘regenerative organic agriculture.’” In addition, direct air capture presents a potentially large removal technology, with limited foreseeable side effects, especially in forms of land use or competition with other uses. The key limitation is cost – but this is not represented as written (severe side effects). See Sabine Fuss et al., Negative emissions—Part 2: Costs, potentials and side effects, Environ. Res. Lett. (May 2018). [Durwood Zaelke, United States of America]	Taken into account. Reformulated for SOD. .

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
12648	7	40	7	44	Nature-based CDR can make a significant contribution. Natural climate solutions can provide more than a third of cost-effective CO2 mitigation needed before 2030. Brandon W. Griscom et al., Natural Climate Solutions, Proceedings of the National Academy of Sciences (2017). The Rodale Institute, in their report Regenerative Organic Agriculture and Climate Change (2014), argues that "we could sequester more than 100% of current annual CO2 emissions with a switch to widely available and inexpensive organic management practices, which we term 'regenerative organic agriculture.'" In addition, direct air capture presents a potentially large removal technology, with limited foreseeable side effects, especially in forms of land use or competition with other uses. The key limitation is cost – but this is not represented as written (severe side effects). See Sabine Fuss et al., Negative emissions—Part 2: Costs, potentials and side effects, Environ. Res. Lett. (May 2018). [Kristin Campbell, United States of America]	Taken into account. Reformulated for SOD. .
28808	7	40	7	44	warning: context of 8% is important here - all 1.5C scenarios rely on lots of CDR and it does what it is supposed to do in this context in ESMs, ie.e. lead to peak and decline of tempertures. The headline of of conext could cause trouble? [Piers Piers Forster, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Reformulated for SOD. .
9370	7	40	7	44	Lack of clarity: it is strange to read that it is assumed that in the RCP8.5 scenario there is a significant amount of CDR . It is expected that the highest rate of CDR is linked to RCP2.6. and that in RCP8.5 no CDR is included. [Klaus Radunsky Radunsky, Austria]	Taken into account. Reformulated for SOD. .
9372	7	40	7	44	It is also strange to read that the potential of CDR onyl allows limited (8%) warming reductions. There have been made assessments that there are CDR approaches such as direct air capture that have the potential to finally eliminate all excess CO2 from the atmosphere - the primary limitations the willingness to pay given the current considerable financial means required. Such assessment would also be more coherent with the conclusion provided on page 8, lines 1-3 related to termination of SRM. [Klaus Radunsky Radunsky, Austria]	Taken into account. Reformulated for SOD. .

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
32058	7	40	7	54	I find the framing of these assessments extremely misleading and potentially very dangerous. This is another area where a risk assessment framing is essential. One of the most fundamental issues for any geo-engineering proposal is not what is the most likey response, but rather what could go wrong and what are the potential very high impact side effects. Rather than concluding that various goals "... can be met simultaneously (low confidence)" an appropriate assessment should be that there is huge uncertainty and risks associated with all such strategies. And specific risks should be described and assessed carefully and quantitatively. [Rowan Sutton, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The SRM sections have been re-written, using as both framing and starting point the assessments from AR5 and SR1.5.
35846	7	41	7	42	The statement that carbon dioxide removal methods 'are, individually, either relatively ineffective with limited (<8%) warming reductions, or they have potentially severe side effects' is copied directly from the abstract of Keller et al. (2014). Apart from the fact that text of course shouldn't be copied verbatim from other sources, the authors should assess the breadth of literature on this topic, not just adopt the conclusions from one study. [Nathan Gillett, Canada]	Taken into account. Reformulated for SOD. .
53376	7	42	7	42	A bit unclear what 8% reduction means [Jan Fuglestvedt, Norway]	Taken into account. Reformulated for SOD. .
55558	7	43	7	44	This sentence is incorrect: The outgassing referred to in the corresponding secion 4.6.3. would not occur as a consequence of halting CDR activities, but of reducing atmospheric CO2 concentrations in the imagined case of sinks exceeding sources of CO2. This is a pretty important misrepresentation. [Matthisa Honegger, Germany]	Taken into account. Reformulated for SOD. .
35850	7	43	7	44	This statement that 'cessation of CDR is expected to cause increasing warming trends' is misleading, and the conclusions depend on the type of CDR technology used. Cessation of BECCS or clear air carbon capture would not necessarily result in outgassing of CO2 or warming. The statement regarding warming is only generally true to the extent that termination of CDR would cause increased warming compared to continuation of CDR, but I don't think this is the intended meaning. [Nathan Gillett, Canada]	Taken into account. Reformulated for SOD. .
55520	7	43	70	47	"Scheme" does not seem like the best word, as it has a negative connotation. The FOD uses it to almost exclsuively to refer to CDR and SRM yet adaptation and mitigation are equally "schemes." Perhaps "method" would be better to differentiate stratospheric aerosol injection, marine cloud brightening, etc. When peaking of SRM or CDR in general, not further word is necessary. E.g. p. 67 L. 56: SRM would intentionally..." [Matthisa Honegger, Germany]	Taken into account. The SRM sections have been re-written, using as both framing and starting point the assessments from AR5 and SR1.5.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
15584	7	46	7	47	By "...SRM can markedly diminish global and regional climate change…" does the author means SRM can markedly diminish global mean temperature change or all elements of the climate? Also by "diminish global and regional climate change" I understand as "being a good thing" [Izidine Pinto, South Africa]	Taken into account. The SRM sections have been re-written, using as both framing and starting point the assessments from AR5 and SR1.5.
51626	7	46	7	54	The way this is written, it is hard to understand projected outcomes of SRM are good/dangerous. Would a policy maker jump at 'markedly diminish global and regional climate change' as a solution, without appreciating dangers? Is it clear to the reader that 'reduce global precipitation' is a good or bad thing, in relation to cc changes? This paragraph is of serious concern because it reads as a bland solution rather than highlighting SRM as unproven to scale with dangerous side effects. In addition, SRM can be seen politically as an alternative to needed and rapid mitigation, in a world where the root causes of anthropogenic climate change are often lucrative activities. [Lindsey Cook, Germany]	Taken into account. The SRM sections have been re-written, using as both framing and starting point the assessments from AR5 and SR1.5.
55522	7	46	7	54	The SRM's speed of effect (p. 69) is relevant enough to warrant a message in the Executive Summary, as even aggressive mititgation and CDR could not do that. [Matthisa Honegger, Germany]	Taken into account. The SRM sections have been re-written, using as both framing and starting point the assessments from AR5 and SR1.5.
12804	7	46	8	3	High confidence here seems incongruous with low confidence in the point above. Both rely on the uncertainties in simulating aerosol forcing. All studies on SRM rely on modeling, as the first physical experiments are just now underway. See Jeff Tollefson, The Sun Dimmers, Nature (November 2018). [Durwood Zaelke, United States of America]	Taken into account. The SRM sections have been re-written, using as both framing and starting point the assessments from AR5 and SR1.5.
12650	7	46	8	3	When the Arctic loses ice, it allows extra RF that can further reduce ice; also, consider the timing (how soon) ice-free and the difference between lower emission scenarios that can severely limit the extent of ice loss and the likelihood of ice-free. (Note: this information is covered in below paragraph, so may be useful to comment that this paragraph just have a sign post to the information in the other paragraph.) May also be useful to mention proximity to ice-free Arctic. Overland and Wang (2013) When will the summer Arctic be nearly sea ice free?, GEOPHYSICAL RESEARCH LETTERS 40:2097–2101, 2097 (“Time horizons for a nearly sea ice-free summer for these three approaches [for estimating future ice loss covered in the study] are roughly 2020 or earlier, 2030 ± 10 years, and 2040 or later.”). [Kristin Campbell, United States of America]	Noted. However, the ES does not leave room for detailed additional explanations.
55526	7	47	7	47	This speaks of "CO2-induced warming" etc. This shoud be "GHG-induced" [Matthisa Honegger, Germany]	Accepted.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
55524	7	47	7	49	The fact that "SRM schemes would reduce global precipitation if they were implemented to offset global mean temperature change" does not seem relevant enough to warrant inclusion in the executive summary, at least in its current wording. Any response done excessively would have negative effects. If this aspect of SRM is to be included, perhaps it is better to say that SRM compensates climate change's increase in precipitation more reffectively than it does for climate change's increase in temperature. Thus, it would seem unwise to use SRM to fully compensate warming lest it reduce global precipitation. [Matthisa Honegger, Germany]	Taken into account. The SRM sections have been re-written, using as both framing and starting point the assessments from AR5 and SR1.5.
15586	7	53	7	54	Would the low confidence owing to the use of a single aerossol feeedbak algorithm or single model? [Izidine Pinto, South Africa]	Taken into account. The SRM sections have been re-written, using as both framing and starting point the assessments from AR5 and SR1.5.
42326	7		7		Include the benefit to SLCP mitigation where the effects are more immediate than the effects of CO2 mitigation, helping reduce the rate of warming and warming in the near-term, which is crucial for avoiding feedbacks and tipping points. 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 ("We define ''fast-action'' to include regulatory measures that can begin within 2–3 years, be substantially implemented in 5–10 years, and produce a climate response within decades. We discuss strategies for short-lived non-CO2 GHGs and particles, where existing agreements can be used to accomplish mitigation objectives. Policy makers can amend the Montreal Protocol to phase down the production and consumption of hydrofluorocarbons (HFCs) with high global warming potential. Other fast-action strategies can reduce emissions of black carbon particles and precursor gases that lead to ozone formation in the lower atmosphere, and increase biosequestration, including through biochar. These and other fast-action strategies may reduce the risk of abrupt climate change in the next few decades by complementing cuts in CO2 emissions."); Xu and Ramanathan (2017) Well below 2 °C: Mitigation strategies for avoiding dangerous to catastrophic climate changes, Proc. Natl. Acad. Sci., doi: 10.1073/pnas.1618481114; 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; Arctic Monitoring and Assessment Programme	Taken into account. The ES has been reconstructed, including SLCF mitigation.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
55560	8	1	8	3	This sentence is incomplete and shoudl read: There is high confidence that sudden termination of SRM in a high-CO2 concentration world would cause a rapid increase in temperature, but a gradual phase-out of SRM combined with balancing CO2 sources and sinks achieved via emissions reductions and CDR could limit the risk and impacts from sudden SRM termination. {4.6.3} [Matthisa Honegger, Germany]	Taken into account. The SRM sections have been re-written, using as both framing and starting point the assessments from AR5 and SR1.5.
49506	8	1	8	3	The statement reads nearly reassuring that sudden termination of SRM is nothing to worry about. But actually, the point is made that once SRM is implemented it MUST NOT be terminated suddenly and apart from the GHG mitigation (much slower response) help of CDR (relatively innefective with potential severe side effects - line 40-44 on previous page) shall guard us. Is the level of confidence of both statements (imapct of sudden SRM) and proposed strategy to counteract that comparable to justify such statement? [Zbigniew Klimont, Austria]	Taken into account. The SRM sections have been re-written, using as both framing and starting point the assessments from AR5 and SR1.5.
39682	8	2	8	2	Avoid the use of term "risk" when its use is not consistent with the definition provided in section 1.2.4.1 of Chapter 1 [Carolina Vera, Argentina]	Accepted an implemented.
45784	8	2	8	2	incorrect use of risk terminology, "The potential for adverse consequences for human or ecological systems" [Katja Mintenbeck, Germany]	Accepted an implemented.
51628	8	2	8	3	Please list a few points of consequence to human/ecosystems to a 'rapid increase in temperature', as a gradual 'phase out' is dependent on functioning States/social systems which cannot be guaranteed in future scenarios. [Lindsey Cook, Germany]	Rejected. Impacts are not in Ch4 remit.
29974	8	10	8	10	Change "scientific" to "physical". This is not merely an editorial comment, this has to do with epistemic values and power. [Theodore Shepherd, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Sentence revised.
37648	8	11			The acronym GSAT need not be introduced here, as it has been introduced already on the preceding page (and in earlier chapters). [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. Each chapter must be self-contained and hence define its own abbreviations. Furthermore, the ES, just like the abstract of a paper, technically is separate from the main text.
30666	8	15	8	16	I would remove "important" before "processes", and use the plural "regional changes" [Annalisa Cherchi, Italy]	Noted. Our usage seems consistent with singular in "climate change".
6920	8	19			I suggest changing "relevant substances" to "radiatively active gases". [Olson Roman, Republic of Korea]	Accepted. Relevant --> radiatively active

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
32060	8	26	8	29	As indicated in my comments on Chapter 1, I strongly suggest using the term "forcing uncertainty" in place of "scenario uncertainty" and "response uncertainty" in place of "model response uncertainty". Neither the scenarios nor the models are in fact uncertain - the uncertainty lies in the future forcing and in the response of the climate. [Rowan Sutton, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. Difference between scenarios reflects our uncertainty about future decision by humankind. By contrast, forcing as in "radiative forcing" is a model outcome, not an input. We disagree with the reviewer's semantics here, which clashes unnecessarily with common usage and hence readers' expectations.
53378	8	27	8	27	re "scenario uncertainty": Check consistency in use of this "label" with ch1. [Jan Fuglestvedt, Norway]	Noted. Usage consistent with 1.5.4.
37650	8	27	8	28	In Chapter 1, volcanic activity is regarded as an external forcing, so should this be mentioned here as a further source of uncertainty? A single large eruption may be regarded as too short-term an event to consider, but if two or three were to happen within a decade or two (and we did have El Chichon and Pinatubo about nine years apart) this might begin to matter. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The SSPs do not contain volcanoes, though, and it seems unnecessary to introduce them in this synopsis. They are covered later in the chapter.
53020	8	27	8	28	There needs to be further clarification on what "model biases" and "model spread" are . Right now, these two terms are not very informative, especially "model spread". I assume they are related to (1) errors in model and (2) uncertainties in parameterizations? [Anson Cheung, United States of America]	Rejected. Model spread is a standard term, and this synopsis must be succinct.
6922	8	27	8	29	A perhaps more thorough classification found in the literature may be: internal variability, model uncertainty (subdivided into model parametric uncertainty and model structural uncertainty), and scenario uncertainty (See Sexton et al., 2011). Reference: Sexton DMH, Murphy JM, Collins M, Webb MJ (2011) Multivariate probabilistic projections using imperfect climate models part I: outline of methodology. Clim Dyn 38:2513–2542. doi: 10.1007/s00382-011-1208-9. [Olson Roman, Republic of Korea]	Noted. We do not see, though, that this classification is more thorough. Furthermore, this synopsis must be succinct.
53022	8	32	8	32	Yeager et al. 2018 BAMS (doi: https://doi.org/10.1175/BAMS-D-17-0098.1) is a more appropriate reference instead of using Meehl et al. 2013 [Anson Cheung, United States of America]	Taken into account. We disagree, though, that Yeager is more appropriate. Meehl et al. (2014) covered all of CMIP5 decadal predictions. Yeager et al. is cited in the methods section 4.2.3.
9020	8	33			"There would be a lag.." is a bit confusing… will there be a lag? How long of a lag? Is is similar in all scenarios? [Anna Merrifield, Switzerland]	Taken into account. The ES statement is revised with clarification (note this comment is for page 7 line 33).

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
32062	8	34	8	35	The inconsistency between this assessment and the CMIP6 range is a massive issue for this chapter and I suggest it therefore merits more discussion at the outset. Also the Chapter 7 assesssment of TCR is arguably more relevant than the assessment of ECS. [Rowan Sutton, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. This is an introduction, though, and should not pre-empt the assessment. TCR is included in SOD Box 4.1.
9022	8	36	8	38	Maybe to avoid the phrase "despite emissions reductions" instead ".. suggests that, even with emissions reductions, it is possible for GSAT to rise at a faster rate in the near term than it did in the recent past ." [Anna Merrifield, Switzerland]	Agreed, modified.. (note that this comment is for page 7 line 36-38)
36528	8	39	8	46	It is my understanding that considerations of uncertainty are at the core of the “Detection and Attribution” method. One possible reference is, Barkhordarian A, von Storch H, Zorita E (2012) Anthropogenic forcing is a plausible explanation for the observed surface specific humidity trends over the Mediterranean area. Geophys Res Lett 39:L19706. https://doi.org/10.1029/2012GL053026 In my opinion, the report is right in highlighting the uncertainty issue and more references could be added. [Carlos Mechoso, United States of America]	Noted, but D&A is not in this chapter's remit.
9024	8	40	8	41	What are "scales as large as currently represented in the RCP8.5 scenario"? What is meant by "as reference"? [Anna Merrifield, Switzerland]	Taken into account. The ES statement has been considerably revised in the SOD (this comment is for page 7 line 40-41)
29976	8	41	8	41	Insert "in models" after "can be diagnosed precisely". This is not merely editorial, since the internal variability cannot be separated from the forced response in the real world. [Theodore Shepherd, United Kingdom (of Great Britain and Northern Ireland)]	Accepted; term added.
32064	8	41	8	41	diagnosed precisely but only for a given model. There remains huge uncertainty about what is the level, spectrum, and other characteristics of internal variabilty in the real world, particularly on decadal and longer timescales. This issue needs to be acknowledged and discussed. See also my comment 22, which is very relevant. [Rowan Sutton, United Kingdom (of Great Britain and Northern Ireland)]	Accepted; term added.
54652	8	41	8	42	Even with a large ensemble, internal variability can only be diagnosed precisely in the given model, not in the real world. Add "Simulated" in the beginning of the sentence, or "estimated in models" instead of "diagnosed"? [Sabine Undorf, United Kingdom (of Great Britain and Northern Ireland)]	Accepted; term added.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
35852	8	42	7	44	The statement that 'internal variability is dominant in any individual relization - including the one that will unfold in reality - in the near term' is not generally true - it depends on the variable being considered and the spatial and temporal averaging. For many earth system variables the forced response is expected to be much larger than internal variability even in the near term (2021-2040) period (relative to the 1995-2014 base period) considered here. To give some examples, global annual mean ocean heat content, sea level, atmospheric CO2 concentration, ocean carbon content or ocean pH. Even 2021-2040 mean GSAT anomaly relative to 1995-2014 is expected to be large relative to internal variability. This statement needs to identify the variables and spatio-temporal averaging being referred to. [Nathan Gillett, Canada]	Accepted. Sentence revised.
9026	8	46	8	54	Could it also be mentioned that SRM in the absence of GHG emission reductions affects ocean acidification? [Anna Merrifield, Switzerland]	Noted. The impact of SRM on ocean acidification is not addressed in ES. However, the ES statement on SRM has been considerably revised in SOD (this comment is for page 7 line 46-54) .
31488	8	54			Somewhat cryptic sentence [Rein Haarsma, Netherlands]	Taken into account. Sentence modified.
37652	9	9			Referring to 1995-2014 as the last years of the historical simulations of CMIP6 may read strangely to someone not engaged in the CMIP process. When this report is published in 2021, 2015-2020 will be history also. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Noted, but the sentence describes the facts. No change.
56434	9	12	9	12	Ocean pH is assessed in chapter 5. [Kirsten Zickfeld, Canada]	Noted. It's explained later why, as an indicator of global climate change, it is displayed also here.
9028	9	15	9	16	Regarding the choice to split the near-, mid-, and long-term changes, are the changes appreciably different from one another or do the magnitudes scale? If the latter, it may be repetitive to discuss the changes in independent sections. [Anna Merrifield, Switzerland]	Noted. The rationale behind this breakdown is explained immediately afterwards.
6926	9	16			It may be better to use the word "metrics or indices" instead of "indices" since rainfall is not an index. Also, is "precipitation" more appropriate than "rainfall". Please check. [Olson Roman, Republic of Korea]	Taken into account. "Indicators" used instead.
37654	9	22	9	23	The near term is 2021-2040, the mid-term is 2041-2060 and the long term is 2081-2100. What about 2061-2080? The mid-term could have been defined as 2041-2070 and the long-term 2071-2100. Comparability with AR5 for the last twenty years of the century would suffer, but there would not be a missing two decades. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Many different choices could have been made. More important than the reviewer's arguments is the consideration that the most recent past should not be too long a period (see 37652).

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
6928	9	26	9	27	Is consistency with the AR5 the primary motivation to define the long-term period as 2081-2100? [Olson Roman, Republic of Korea]	Noted. No, there are others. Most projections end in 2100, and the period should have the same length as the most recent past.
26548	10	1	18	9	I would definitely suggest to improve the description of the methodology. Really hard to read. [Antonia Longobardi, Italy]	Taken into account. Methodology section revised extensively for SOD
47232	10	1			Should it be highlighted somewhere in this section (may be in 4.2.5) that no bias correction will be applied in chapter 4 although some methods have been proposed for global applications and do represent another potential source of uncertainty (bias correction techniques may have strong but method-dependent effects on projected precipitation anomalies for instance) ? [Hervé Douville, France]	Rejected. Bias correction in the use of data for impacts studies is widespread but this chapter presents the model output
51904	10	1			Much of the material in this section was also covered in chapter 3. It is also germane to many other chapters. Is there a mechanism that can be used to reduce the potential for this repetition? [Peter Thorne, Ireland]	Taken into account. Chapters need to be able to stand-alone. Text has been written to keep salient to chapter 4.
31476	10	1			In this section all kind of MIPs are discussed that will be used thoughout the paper, but HighResMIP is not mentioned. HighResMIP should be included in the analysis. It can provide new information on biases and its relation to resolution, such as the too cool North Atlantic and biases in large scale atmospheric circulation patterns. [Rein Haarsma, Netherlands]	Accepted. Thank you. All MIPs used in the chapter have been listed here
46246	10	3	10	3	One of the main problems associated with the use of global climate change models is for countries that have not developed their own indigenous models. This situation makes it necessary to provide solutions to use global models in areas where these models are not built. [sadegh zeyaeyan, Iran]	Rejected. it is true not all countries have GCMs/ESMs. The ones which do exist perform simulations globally and are evaluated as such (in chapter 3)
8880	10	3	10	3	One of the main problems associated with the use of global climate change models is for countries that have not developed their own indigenous models. This situation makes it necessary to provide solutions to use global models in areas where these models are not built. [Mohammad Javad Zareian, Iran]	Rejected. it is true not all countries have GCMs/ESMs. The ones which do exist perform simulations globally and are evaluated as such (in chapter 3)
57522	10	3	10	3	One of the main problems associated with the use of global climate change models is for countries that have not developed their own indigenous models. This situation makes it necessary to provide solutions to use global models in areas where these models are not built. [Sahar Tajbakhsh Mosalman, Iran]	Rejected. it is true not all countries have GCMs/ESMs. The ones which do exist perform simulations globally and are evaluated as such (in chapter 3)
46272	10	3	10	3	developing indigenous climate model for Middle East (including Iran) and respected center is necessary. [sadegh zeyaeyan, Iran]	Rejected. IPCC is not responsible for national level modelling plans or capability
57548	10	3	10	3	developing indigenous climate model for Middle East (including Iran) and respected center is necessary. [Sahar Tajbakhsh Mosalman, Iran]	Rejected. IPCC is not responsible for national level modelling plans or capability

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
8266	10	3	11	41	A table of breif itroductions of CMIP6 projection should be shown in this part. [Zong Ci Zhao, China]	Rejected. this section describes the models. Results from projections are presented elsewhere in the chapter
8268	10	3	11	41	The various methods of CMIP6 ensembles should be discussed and give a figure to explain it. [Zong Ci Zhao, China]	Taken into account. We now have distinguished between the role of single and multimodel and perturbed parameter ensembles but only as an introduction to the more comprehensive treatment in 4.2.5. A further refinement of the description will be carried out for the FGD
6335	10	3	11	41	What factors culminate into determining the effectiveness of global and regional climate models? [Isaac Sarfo, Ghana]	Rejected. Model evaluation is assessed in chapter 3
44558	10	3			This material overlaps somewhat with sections 1.4.4-1.4.5. Let's make sure we coordinate. In particular, see the introduction to section 1.4.4 on ensemble types, and the accompanying Figure 1.10, which could be strengthened by adding some of the material you now present on page 4-11. [Bjorn Samset, Norway]	Taken into account. Thank you. In FGD we will coordinate to avoid unnecessary repetition
57610	10	7	10	8	Capital letters may use to meet the abbreviation of EMIC: Earth system Models of Intermediate Complexity (EMIC) [Sahar Tajbakhsh Mosalman, Iran]	Accepted.
26200	10	7	10	8	Capital letters may use to meet the abbreviation of EMIC: Earth system Models of Intermediate Complexity (EMIC) [iman babaeian, Iran]	Accepted.
46334	10	7	10	8	Capital letters may use to meet the abbreviation of EMIC: Earth system Models of Intermediate Complexity (EMIC) [sadegh zeyaeyan, Iran]	Accepted.
31490	10	7			Why do you not explain the difference between AOGCMs and ESMs earlier? [Rein Haarsma, Netherlands]	Rejected. This is the model description section and we explain the difference at the very start of it.
46336	10	18	10	18	Using WMO standard period of 1981-2010 is preferable. Using most recent period of 1995-2014 can exasperate the magnitude of global warming. [sadegh zeyaeyan, Iran]	Rejected. Choice of reference period is explicitly and carefully justified later.
57612	10	18	10	18	Using WMO standard period of 1981-2010 is preferable. Using most recent period of 1995-2014 can exasperate the magnitude of global warming. [Sahar Tajbakhsh Mosalman, Iran]	Rejected. Choice of reference period is explicitly and carefully justified later.
26202	10	18	10	18	Using WMO standard period of 1981-2010 is preferable. Using most recent period of 1995-2014 can exasperate the magnitude of global warming. [iman babaeian, Iran]	Rejected. Choice of reference period is explicitly and carefully justified later.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
35854	10	21	7	25	In the SOD, the chapter could also make use of the ssp245-GHG, ssp245-aer, ssp245-nat, ssp245-stratO3 DAMIP simulations to separate out the contributions of GHGs, aerosols, natural forcings and stratospheric ozone to projected changes under SSP2-4.5. [Nathan Gillett, Canada]	Rejected. At the time of the SOD there was no available literature on these simulations.
31492	10	21			HighResMIP shoudl be mentioned here [Rein Haarsma, Netherlands]	Accepted. Thank you. All MIPs used in the chapter have been listed here
54654	10	23	10	24	"climate response to chemistry and aerosol processes"? [Sabine Undorf, United Kingdom (of Great Britain and Northern Ireland)]	Accepted.
29860	10	28	10	34	Model resolutions should be stated [Mustafa Tufan Turp, Turkey]	Rejected. This table does not list models, so their resolution cannot be stated here. Model resolution will be included in the relevant model tables
44458	10	28	10	34	Coordination with table 1.3 of chapter 1 would be useful. [Anne Marie Treguier, France]	Taken into account. This table covers the MIPs for which results are used in Chapter 4.
9030	10	32			Acronyms TCR and ECS have not yet been defined. [Anna Merrifield, Switzerland]	Accepted. Table updated to not use acronyms
53380	11	7	11	8	re "no consensus": Among who? Authors? In the literature… Please be more precise. [Jan Fuglestvedt, Norway]	Accepted. text has been amended to show it meant in the literature
35858	11	12	11	29	As described in this paragraph, whereas a perturbed physics ensemble allows uncertainties associated with uncertain model parameters to be sampled over, and which is therefore relevant to obtaining probabilistic projections, stochastic physics parameterisations are really just a type of parameterisation. Like most aspects of climate model design, they have the potential to change the sensitivity of the model, but they should not warrant assessment in this section any more than all other aspects of climate model design and parameterisations. I recommend removing the discussion of stochastic physics from this paragraph. [Nathan Gillett, Canada]	Taken into account. Stochastic parametrisations are distinct from other parametrisation schemes in that they address model uncertainty, in a similar manner to perturbed parameter approaches. They have proven beneficial for initialised forecasts including seasonal climate predictions. This has been clarified. Agreed that the discussion does not fit in this paragraph, so discussion on stochastic physics has been moved to separate paragraph before initial-condition ensembles.
29978	11	13	11	13	I find it confusing to discuss stochastic physics simulations alongside PPEs. As is stated, the different realizations in PPEs represent different climates, whereas the different realizations in stochastic physics ensembles do not. A stochastic parameterization is just another form of parameterization. So aren't stochastic physics ensembles more analogous to initial-condition ensembles? Thus they can be analysed in the same way, unlike PPEs. [Theodore Shepherd, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Discussion on stochastic physics has been moved to separate paragraph before initial-condition ensembles.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
9032	11	13	11	14	Cound an example of an "uncertain model parameter" be given here? [Anna Merrifield, Switzerland]	Taken into account and original text modified. While consideration of specific model parameters is outside the scope of this introduction, we have added a sentence on the role of identification of sensitivities of results to specific parameters and the role of joint probability density functions to subselect combinations of model parameters that conform to emergent observational constraints after Collins et al., 2011, Climate Dynamics
8648	11	15	11	16	“Different ensemble members in a perturbed-parameter ensemble have different climate biases and climate sensitivities.” It depends what parameters you change and the nature of the model. Suggest “may have different climate biases and climate sensitivities”. [Julia Hargreaves, United Kingdom (of Great Britain and Northern Ireland)]	Accepted.
8650	11	16	11	18	“It is possible to weight ensemble members according to some performance metric or emergent constraint (Fasullo et al., 2015; Murphy et al., 2004) to improve the ensemble distribution.” It is the word “improve” here that I have problems with. Last time on IPCC (Yokohata et al etc) we showed that PPEs created from a single GCM are not reliable - they are much too narrow to reliably include the truth. If you weight them, you do not “improve” this. However, you might want to weight a PPE if you only want models which have certain properties. So, it is more subjective to the researchers’ desires than “improve”. Hmmm.. I think I’d just delete the sentence - it is old stuff really as the 2004 citation indicates, and if you include the Murphy et al, you kind of need to also include all the stuff showing that it does not “improve” anything. And that’s just AR5 all over again! Alternatively write a sentence that explains the new insight since AR5 in Fasullo et al. [Julia Hargreaves, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Thank you for the comment. We will revise the text in the FGD taking into account your suggestion.
36532	11	23	11	24	At this stage of the knowledge on the subject, “can” seems too strong a word. Perhaps “may” or “has shown potential”. [Carlos Mechoso, United States of America]	Accepted.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
40518	11	24	11	28	The sentence is about improving internal variability with stochastic physics, and mentions the monsoon. But it is not clear whether you are implying that the stochastic physics improves variability of the monsoon (e.g. ISV and interannual variability) or the monsoon seasonal cycle itself. This sentence should be clarified and revised. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. it improves the variability of the monsoon. Sentence has been clarified.
40520	11	26			Summer Monsoon --> summer monsoon [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted.
36530	11	31	11	41	It is my understanding that considerations of uncertainty are at the core of the “Detection and Attribution” method. One possible reference is, Barkhordarian A, von Storch H, Zorita E (2012) Anthropogenic forcing is a plausible explanation for the observed surface specific humidity trends over the Mediterranean area. Geophys Res Lett 39:L19706. https://doi.org/10.1029/2012GL053026 In my opinion, the report is right in highlighting the uncertainty issue and more references could be added. [Carlos Mechoso, United States of America]	Rejected. This chapter assess future projections. Climate up to present day, including detection and attribution, is assessed in chapter 3.
54202	11	33	11	34	this is a odd bunch of references for this sentence. Why cite Hedemann et al, not Bittner et al 2016 as the first publication with MPI-GE? There is also no reference to the Canadian Ensemble papers. See here: http://data.ec.gc.ca/data/climate/scientificknowledge/the-eccc-climate-model-datasets-for-climate-science-and-impacts-research/the-canadian-earth-system-model-large-ensembles/?wbdisable=true [Nicola Maher, Germany]	Taken into account. The references have been straightened in the SOD.
54204	11	33	11	34	should also cite Frankignoul, C., G. Gastineau, and Y.-O. Kwon (2017), Estimation of the SST Response to Anthropogenic and External Forcing and Its Impact on the Atlantic Multidecadal Oscillation and the Pacific Decadal Oscillation,Journal of Climate,30(24), 9871–9895 doi:10.1175/JCLI-D-17-0009.1 [Nicola Maher, Germany]	Rejected. See comment above.
51906	11	33	11	34	At the risk of being self-serving the Outten et al., 2015 / Bethke et al., 2017 NorESM ensembles should be cited here for completeness? [Peter Thorne, Ireland]	Accepted. See comment above.
51908	11	38	11	40	Again, this is somewhat self-serving but equally Box 3.1 cites Thorne et al., 2015 hiatus analysis in precisely the context here so for cross-chapter consistency consider adding this citation here. [Peter Thorne, Ireland]	Rejected. See comment above.
9034	11	40			Perhaps "separate" rather than "unpick"? [Anna Merrifield, Switzerland]	Accepted, changed to "extract"

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
25614	11	44	12	17	Suggest discussion as a limiting case scenario, of committed climate change, provided no further emissions of radiatio influencing substances. Committed climate change. This would provide important perspective against which to compare the several scenarios of more realistic future emissions. A key component of this discussion would be the uncertainty attached to such committed climate change, especially uncertainty associated with climate sensitivity and with aerosol forcing (they are entangled in the language of Ramanthan. 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]	Accepted. Committed changes are included in the chapter and can be described here.
35856	11	44	12	17	Somewhere the authors should mention and briefly assess the natural forcings and the climate response to those forcings included in the ScenarioMIP simulations on which this chapter is based. These simulations include projected changes in the solar cycle with long-term modulation (not just a repeating solar cycle as in CMIP5) (see Matthes et al. (2017; https://www.geosci-model-dev.net/10/2247/2017/gmd-10-2247-2017.pdf)), as well as a ramp-up of background aerosol to an average historical level used in the control simulation by 2025. In the description of ScenarioMIP O'Neill et al. (https://www.geosci-model-dev.net/9/3461/2016/) say this 'Solar time series will be provided as described on the SOLARIS-HEPPA website at http://solarisheppa.geomar.de/cmip6 and in Matthes et al. (2016). Volcanic forcing will be ramped up from the value at the end of the historical simulation period (2015) over 10 years to the same constant value prescribed for the piControl simulations in the DECK, and then will be kept fixed.' Both aspects are different to CMIP5. While the climate affect may not be large, these forcings could for example have a substantial impact on the proabilities of remaing below 2C or 1.5C in given scenarios which shoudl be assessed. For example if the probability of remaining below 2C under SSP1-2.5 depends substantially on assumptions for projected changes in solar irradiance, this is something policymakers should know about. Note that the DAMIP ssp245-nat simulations are natural-only simulation which will allow the effects of these future changes in natural forcings to be isolated. The projected natural forcings are the same in all scenario simulations, so the results can be applied equally to any scenario. [Nathan Gillett, Canada]	Accepted. In the FGD we will coordinate with chapter 1 to make sure this is covered

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
27206	11	44	12	17	https://doi.org/10.1038/s41467-019-08633-z : "The historical CO2 increase has resulted in enhanced photosynthetic carbon fixation (Gross Primary Production, GPP), as can be evidenced from atmospheric CO2 concentration and satellite leaf area index measurements. Here, we use leaf area sensitivity to ambient CO2 from the past 36 years of satellite measurements to obtain an Emergent Constraint (EC) estimate of GPP enhancement in the northern high latitudes at two-times the pre-industrial CO2 concentration (3.4 ± 0.2 Pg C yr−1). We derive three independent comparable estimates from CO2 measurements and atmospheric inversions. Our EC estimate is 60% larger than the conventionally used multi-model average (44% higher at the global scale). This suggests that most models largely underestimate photosynthetic carbon fixation and therefore likely overestimate future atmospheric CO2 abundance." [François GERVAIS, France]	Rejected. Carbon cycle is assessed in ch.5
42334	11	51	11	55	Add note and reference to SR1.5 Chapter 5 p. 467 "To date, no pathway in the literature proves to achieve all 17 SDGs because several targets are not met or not sufficiently covered in the analysis, hence resulting in a sustainability gap (Zimm et al., 2018)." [Gabrielle Dreyfus, United States of America]	Rejected. This is not an assessment that can be made by WG1, although could be mentioned in discussion in Ch.1
54970	11	53	11	53	can be more specific and say: section 1.6 in Chapter 1. [Rojas Maisa, Chile]	Accepted
38964	12	2	12	3	The way this chapter calls SSP scenarios (i.e., 1-2.6 as "sustainable", 2-4.5 as "middle-of-the-road" etc.) helped me to remember them. Other chapters might also want to adopt them. [Masahide Kimoto, Japan]	Noted. Thank you. No changes to our text required
49924	12	7	12	8	If possible, approximate magnitude of the differences between SSP and RCPs at regional levels maybe indicated. Enables a better connect for policy/adaptation planning options [Govindarajalu Srinivasan, Thailand]	Rejected. A regional assessment of SSPs versus RCP's is outside of the scope of this chapter.
49508	12	11	12	11	One could add few references here; eg. Amann et al (2013; https://doi.org/10.1146/annurev-environ-052912-173303) and Rao et al (2017) http://dx.doi.org/10.1016/j.gloenvcha.2016.05.012 where this was discussed. Few more refs are in chapter 6 too [Zbigniew Klimont, Austria]	Taken into account. Literature coverage has been expanded
49510	12	13	12	13	Recommened to also reference to Rao et al (2017) http://dx.doi.org/10.1016/j.gloenvcha.2016.05.012 as this is where the foundation of these scenarios were developed. Also refernce to Chapter 6 can be added as this discused there too. [Zbigniew Klimont, Austria]	Taken into account. Literature coverage has been expanded
44516	12	17	12	17	C4MIP I think it is CMIP [Shaukat Ali, Pakistan]	Rejected. No, we refer here to C4MIP regarding emissions driven simulations

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
32066	12	20	12	35	There is a need to add a discussion of how CMIP6 multi-model projections are sensitive to reference period, particularly for the near term (e.g. Hawkins & Sutton, BAMS, 2016). Also, we found in AR5 Ch 11 that using trends rather than anomalies from the CMIP ensemble results provided an important constraint on GSAT for the near term. It would seem a good idea to consider this in AR6 also. [Rowan Sutton, United Kingdom (of Great Britain and Northern Ireland)]	Accepted and implemented in the SOD.
30668	12	20	14	32	and what about land surface as source of near term information? [Annalisa Cherchi, Italy]	Noted, Other non-oceanic drivers of predictability are mentioned at line 46. pg.12 (i.e. Bellucci et al. 2015a) of the FOD. New findings, if any, related to land-surface as source of near-term predictability will be incorporated in the FGD.
37656	12	24	12	27	The language "observationally constrained projections" is not ideal, as "initialized" predictions are also, in a sense, observationally constrained projections. The observational constraint is a different one, on the initial state. Also, in NWP "initialization" has been traditionally used as the term for the process of balancing initial states, after or during the process of assimilating or analysing observations. At the very least, "the initialization information under 3) is applied" in line 27 could be changed to something like "the observational data used to initialize predictions under 3) are used". [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The suggested phrasing is used in the SOD.
54656	12	34	12	35	Relevant here: Shiogama, H., Stone, D., Emori, S., Takahashi, K., Mori, S., Maeda, A., … Allen, M. R. (2016). Predicting future uncertainty constraints on global warming projections. Scientific Reports, 6(January 2016), 1–7. https://doi.org/10.1038/srep18903 [Sabine Undorf, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The SOD has included this reference.
43894	13	16	13	17	the statement "A variety of methods has been employed to generate initial-condition ensembles fordecadal prediction", can be improved by including assumptions thus, it should read that: A variety of Methods and assumptions….. [Michael Mugarura, Germany]	Accepted and implemented in the SOD.
53382	13	17	13	17	re "no consensus": Among who? Authoss? In the literature… Please be more precise. [Jan Fuglestvedt, Norway]	Noted. The wording in the FGD will be more specific.
31494	13	45			This sentence is rather vague [Rein Haarsma, Netherlands]	Accepted. This will be addressed in the FGD.
37658	13	49			Change "skill" to "the estimated skill". Skill is a property of the forecast, and we try to estimate it when we evaluate forecasts. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. This change is implemented in the SOD.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
35860	13	50	13	52	The text here suggests that initialised forecasts of climate variables may be useful for assessing the quality of observational datasets. Please add supporting references for this. The text does not mention the fact that initialised forecasts have to be initialised with observations, and so for variables for which observational uncertainties are large for the forecast assessment, observational uncertainties will also be large for the initial conditions. I would think that the only conditions in which an initialised forecast might be more accurate than direct observations would be for a variable/lag with high skill, uncertain direct observations, and whose forecasts were strongly constrained by other variables which had low observational uncertainties. But in these conditions a reanalysis should do better than the initialised prediction assuming observational errors are suitably modelled and the reanalysis model is at least as good as the model used for the predictions. Are the authors suggesting that initialised forecasts of climate varaibles woudl be expected to be more accurate than reanalyes? [Nathan Gillett, Canada]	Accepted. Additional references have been provided in the SOD.
37660	13	52			The points made in this paragraph have been well known in the NWP community for many years.The mismatch between observations and forecast in data assimilation schemes is used to check the suitability for use of observations from new instruments, and to correct the perceived biases in certain types of observation, for example. So forecast information is used not only to assess observational datasets, but also to correct biases as best we can. This approach may well be used in the "initialization" of the predictions referred to on page 12. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Noted. No action required.
47216	13	54	14	2	I would temper this statement or provide a more quantitative assessment about the fraction of the global land area where the performance is significantly improved for both temperature and precipitation for instance. [Hervé Douville, France]	Accepted. A more quantitative statement is provided in the SOD.
35862	14	11	14	12	Specify the timescale for this assessment of forecast skill in the Pacific. [Nathan Gillett, Canada]	Accepted and implemented in the SOD.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
54658	14	15	14	21	If "ensemble-mean magnitudes that are much weaker than observed" and the following sentence still refer to Sahel precipitation, then it is maybe worth mentioning that this might be related to an underestimated response to forcing in this region, as found in a detection and attribution study of changes during the twentieth century, where the model response to all historical forcings had to be significantly scaled up in order to explain the observations (Undorf et al., 2018). Undorf, S., Polson, D., Bollasina, M., Ming, Y., Schurer, A., & Hegerl, G. C. (2018). Detectable impact of local and remote anthropogenic aerosols on the 20th century changes of West African and South Asian monsoon precipitation. Journal of Geophysical Research Atmospheres, 123, 4871–4889. https://doi.org/10.1029/2017JD027711 [Sabine Undorf, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. This point is well taken but our sentence is not specific to Sahel precipitation.
35864	14	17	14	19	The cited reference (Yeager and Robson, 2017) doesn't support the assessment that 'decadal predictions with large ensemble sizes appear able to predict multi-annual precipitation anomalies over certain land regions, although with ensemble-mean magnitudes that are much weaker than observed.' The paper is a review paper, and doesn't appear to say anything about decadal predictions of precipitation using modelling systems with large ensemble sizes. [Nathan Gillett, Canada]	Taken into account. Additional references have been added in the SOD.
29980	14	19	14	21	I am concerned that the so-called "signal-to-noise paradox" has become something of a meme (and note that the two cited papers are from the same group, so are not independent). There does seem to be some evidence of the phenomenon for the NAO, but since there remains no explanation for it, we should be very cautious about inferring too much generality. For example, a recent paper (Byrne et al. 2019 JGR doi: 10.1029/2018JD030173) shows that for seasonal predictions of the SAM in ECMWF S4, there is absolutely no evidence of a S/N paradox. [Theodore Shepherd, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. This point is well taken but we think that our use of "may be symptomatic" in our sentence demonstrates sufficient caution.
53024	14	23	14	26	Lovenduski et al. 2019 might be relevant also, doi: https://doi.org/10.5194/esd-10-45-2019 [Anson Cheung, United States of America]	Accepted. Thank you, we have included the reference
49362	14	23	14	26	Especify what properties of the Earth System might be related to the ocean variability. Do also define variability of the ocean (i.e. temperature and salinity). [Rafael Catany, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. Examples are provides in the following sentence. The meaning of ocean variability is self-evident.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
36534	14	28	14	32	The evidence you have presented to place higher confidence in temperature than in other climate quantities is a little weak. Perhaps you could briefly remind us of your arguments? Recall that in Lines 49-52 of the previous page you reminded us that, “skill can be degraded by errors in observational datasets used for verification, in addition to errors in the predictions.” Is this particularly true for precipitation over the oceans? [Carlos Mechoso, United States of America]	Rejected. References are provided earlier in this subsection that support our assessment.
25616	14	29	14	31	Don't just report that such a study was conducted without revealing the results; please state the results. [Stephen E Schwartz, United States of America]	Taken into account. We revised the text mentioning the results.
37662	14	30	14	31	This is an appropriate reference to the work of Smith et al. (2018), though for clarity the word "temporarily" could be added before the word "exceeded". But it may be misinterpreted by a reader as implying that 1.5ºC has not been exceeded in the recent past. Fig 1.2 of SR1.5 and the earlier journal article by Simmons et al. (2017. doi: 10.1002/qj.2949) indicate that this may well have happened in February 2016. Comment 4 discusses this in the context of the entire report. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Changed as suggested
38966	14	37	14	51	Better refer to Sec. 1.6.3 ("Temperature levels as additional tool for cross-Working Group integration"). The section title here (4.2.4 "Pattern Scaling") might also be reconsidered for easy cross-referencing. [Masahide Kimoto, Japan]	Accepted - we are now referring to section 1.6.2. However, we have retained the title "pattern scaling" since this is the appropriate terminology for associating patterns of change with levels of global warming.
35866	14	41			Delete 'mitigation', since not all scenarios considered in this chapter are mitigation scenarios. [Nathan Gillett, Canada]	Accepted, change implemented.
53384	14	55	14	55	"and mitigation scenario": You mean the scenario under consideration? Maybe this needs rewording [Jan Fuglestvedt, Norway]	Accepted. The text has been changed as suggested.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
35868	15	2	15	4	Pattern scaling does not require the assumption that external forcing does not affect the internal variability, if used to project mean climate changes, as far as I understand. A change in variability would affect the probabilities of extremes, as well as the range of projected change in a given variable, but not the mean. [Nathan Gillett, Canada]	Rejected. Please note that we are discussing here not pattern scaling in general, but specifically the traditional approach where a pattern of change associated with a given level of global warming is assumed to scale over all other levels of warming. The traditional assumption thus do require no change in internal variability in order to be sensibly applied across all warming levels. Consider for example, the case where strong El Nino events increase in frequency at higher levels of global warming. This would affect internal variability in many regions and consequently also the mean patterns of change.
15268	15	7	15	7	In Tebaldi and Knutti 2018 https://doi.org/10.1088/1748-9326/aabef2 we show that pattern scaling is accurate also for low warming scenarios. [Claudia Tebaldi, United States of America]	Taken into account. Thank you for pointing out this reference and finding, both are reported on in the SOD (see section 4.2.4 and 4.6.1).
29982	15	10	15	11	This point is illustrated very clearly for atmospheric circulation in Ceppi et al. (2018 J.Clim. doi: 10.1175/JCLI-D-17-0323.1). Note that the "fast" response discussed in that paper is mainly associated with the fast response in SSTs, not the rapid adjusment. [Theodore Shepherd, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Thank you for pointing out this reference and qualification, both are reported on in the SOD in section 3.6.1.
31496	15	14			This framework is not entirely clear to me. Please explain better. [Rein Haarsma, Netherlands]	Rejected. Please noted that this framework for studying responded to external forcing is explained in some detail in the next paragraph of the section.
35870	15	20	15	22	While I understand that the main focus here is on assessing recent literature, it may be worthwhile citing earlier literature which proposed these ideas - in particular Allen and Ingram (2002; https://www.nature.com/articles/nature01092). [Nathan Gillett, Canada]	Rejected. Thank you for pointing out this early reference on the pattern scaling related methodologies - it is being referred to in the introductory paragraphs of section 4.2.4. Our later discussion is largely informed by numerous more recent findings informed by analysis of the CMIP5 and CMIP6 ensembles.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
9036	15	22	15	23	Isn't this by construction, as per "the slow response as forcing-independent" in L18. [Anna Merrifield, Switzerland]	Rejected. Thank you for this consideration, however, this result is not an artefact of the question posed, but a fundamental property about how precipitation responds to a warmer world - that response seems largely independent of the rate of warming.
43896	15	31	15	33	Instead of using the land-sea warming contrast changes in a non-linear way with GSAT to "approximate" the role of the rate of global warming in determining regional patterns of climate change, its better fit to use "potential indicater" rather than approximation. [Michael Mugarura, Germany]	Rejected. This sentence merely makes the point that the land-sea temperature difference do not scale linearly with the increase in GSAT, rather, it scales with the rate of global warming.
36538	15	33	15	35	This seems to be repeated. [Carlos Mechoso, United States of America]	Rejected. No repetition is evident with regards to these statements.
29984	15	34	15	37	I think it is important to distinguish failure of pattern scaling due to nonlinearity, in the usual sense of amplitude-dependence, from that due to different timescales of adjustment, which is a linear phenomenon but is manifest as nonlinearity with respect to GMST. They have quite different origins and thus would require different treatments. [Theodore Shepherd, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We distinguished between these different non-linearities in the discussion. Note however, that for some forms of fast adjustment the response is highly-nonlinear, as a function of time and as a function of the level of global warming.
35872	15	35	15	36	Pattern scaling is an approach used to make projections, not to detect nonlinearities, so it is not surprising that the approaches described cannot do this. Secondly the sentence implies that the inabilitity to predict internal variability is a limitation of pattern scaling. All the models used to make long-term projections in this chapter do not predict internal variability. Internal variability can only be predicted in initialized forecast systems, and even then only over a limited forecast window. Do the authors mean that pattern scaling cannot project changes in internal variability? If so, clarify this. [Nathan Gillett, Canada]	Taken into account. Pattern scaling is not used in the chapter to make predictions or projections. Rather, the chapter explores to what extent patterns of regional change scale as a function of the level of global warming. Within this context, nonlinear responses are important and are consequently discussed. In terms of internal variability, this section merely states that the traditional approach of simply applying a pattern of change of a single level of global warming to all other levels of global warming do not take into account potential changes in internal variability (for example patterns of ENSO impacting on patterns of change). We rephrased the text to make these aspects clearer in the discussion.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
32068	15	41	15	41	time-slice rather than time-slab? Please use consistent terminology. [Rowan Sutton, United Kingdom (of Great Britain and Northern Ireland)]	Noted.
37664	15	43			"pre-industrial levels" should be changed to "the early-industrial baseline" for consistency with the terminology established in Chapter 1. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Usage in Ch1 FOD is not the one agreed upon at LAM2, nor at LAM3.
9976	16	7	16	13	Authors should consider that there is a general framework for studying climate response to forcings (including for spatial patterns) 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)]	Taken into account. Thank you for pointing out these references, which we will consider when composing the FGD. Note however that in terms of radiative forcings of climate well-developed methodologies, in in the form of sensitivity experiments, exist to isolate the relative contributions of the different forcing mechanisms.
49364	16	16	15	16	Mention latest advancements assimilating in situ salinity obervations and the possibility of using independent satellite salinity mearements to study model uncertainty characterizations. For instance, assimilation of salinity showed to the improved ECMWF ocean reanalyses and its associated with spurious vertical convection following the Mediterranean Outflow water o See Hao Zuo et al., 2019 “ECMWF operational ememble reanalysis analysi system for ocean and sea ice: a description of the system and assessment” (thttps://doi.org/10.5194/os-2018-154) o Chapter needs to develope on latest advancements from including both salinity in situ and satellite observation o Reanalyses show fresh biases in salinity for the upper 100 m, with ORAS5 bias and these presented very similar patters as the temperature bises, which suggests common model or forcing errors. o Biases in salinity are larger than temperature ones o Include comparison of the models against independent datasets, for example for temperature model or reanalysis-analysis Vs CCI SST datasets (CCI: ESA Funded Programme Climate Change Initiative) o The contribution of the Climate Change Initiative aims to produced long time series using satellite only datasets. See link: http://cci.esa.int o Need to develop on the possibility of using Satellite retrieved Sea Surface Salinity to reduce model biases. See for example New Sea Surface Salinity data available (link CCI SSS: http://cci.esa.int/salinity). [Rafael Catany, United Kingdom (of	Rejected. This section provides a general discussion of sources of uncertainty and thus a detailed assessment of salinity is beyond the scope of this chapter

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
25618	16	16	17	16	It would seem that a para dealing with the consequence of uncertainty in model parameters is required. Uncertainty even within a single model in which parameters escribing physical processes (or biological) are varied within the range of their present knowledge. [Stephen E Schwartz, United States of America]	Rejected. Parametric uncertainties are already mentioned here. For a more detailed discussion we refer to chapter 1.
25620	16	16	17	16	To illustrate the effects of different sources of uncertainty a figure such as 4.1 should be presented for each source of uncertainty. First s figure showing for a given model the effects of ensemble uncertainty; perhaps more than one such model, separate panel for each model.. Then for a given model the effects of parameter uncertainty; again, perhaps more than one such model, separate panel for each model.. Then for a given scenario, the effects of model uncertainty, with an average trajectory for each of the models, multiple models in the fig . Finally a figure such as the present 4.1 that shows the effect of scenario uncertainty. That would be a very illuminating set of figures. One would hope that the uncertainty would grow with each successive figure. [Stephen E Schwartz, United States of America]	Taken into account. Due to the lack of existing analysis this figure is not yet implemented
8270	16	16	17	16	It should mention how to meature the contribution rates of various factors such as anthropogenic forcing, natural forcing, intersystem, urbanazation effects. [Zong Ci Zhao, China]	Rejected. The scenario uncertainty will here be represented by using different SSP-RCPs but the role of different forcing agents will not be separated here.
27334	16	16	19	35	A key source of model uncertainty is not discussed here. It is the aspect of tuning-calibration-parameteristaion, should be added to the chapter. Because nearly every model has been tuned-calibrated precisely to the 20th century climate records (Voosen, Held, Science 2016). Therefore the detection-attribution-projection process is "contaminated" bij the type of tuning-calibration-parameterisation used. This aspect, well known in the modelling community (Hourdin 2017), is not enough addressed . This report (The Physical Science Basis) should seriously discuss whether or not climate models are fit for the purpose of making climate projection-scenario's. A good fit with historical climate records is not a good basis for confidence in the future projections because the calibration-fit is often obtained with conflicting tuning-parameterisation. [ferdinand meeus, Belgium]	Rejected. Parameter calibration has been documented in documentations of several models and their implications discussed in the scientific literature. However, the resulting uncertainty contribution is nothing else than part of the parameter uncertainty that is discussed here and in chapter 1

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
35874	16	16			I would recommend that before a detailed discussion of the contributions of different sources of uncertainty to spread in model projections, the authors should consider uncertainties in real world climate projections and how these may be estimated or relate to model projections. Box 4.1 promises to 'produce a robust method to for quantifying the uncertainty in climate projections', so I would recommend moving Box 4.1 up to this point, so that the discussion starts with the paradigm used to relate real world projections to climate model projections, and then moves on to consider the contributions of different sources of uncertainty within models themselves. [Nathan Gillett, Canada]	Taken into account. The text of this paragraph is now better linked with box 4.1 and the discussion in chapter 1.
53386	16	18	16	22	Remember to cooridnate the terminology with ch1 authors who give a presentation of these uncertainties. [Jan Fuglestvedt, Norway]	Taken into account. This section is shortened and better linked and coordinated with the corresponding section in chapter 1
31684	16	18	16	33	Rowell (2012) also provides useful maps of the relative contributions of modelling and internal sources of uncertainty. Rowell, D.P., 2012: Sources of Uncertainty in Future Changes in Local Precipitation. Climate Dynamics, 39, 1929-1950 [Dave Rowell, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Reference added
35876	16	24	16	26	It is not only since the AR5 that 'it has become clear that internal variability manifests itself substantially also on the multi-decadal timescale'. For example, in section 10.3.1.1.3 of the AR5 WGI report 'Several studies that have aimed to separate forced surface temperature variations from those associated with internal variability have identified the North Atlantic as a dominant centre of multi-decadal internal variability, and in particular modes of variability related to the Atlantic Multi-Decadal Oscillation.' Chapter 14 of the AR5 has a whole section on Atlantic Multidecadal variability (14.7.6) which includes the statement 'Along with secular trends and Pacific variability, the AMO is one of the principal features of multidecadal variability in the instrumental record'. The role of internal multidecadal variability has been discussed in the literature in many much earlier literature as well, of which Delworth et al. (1997) (https://doi.org/10.1029/96GL03927) and Griffies et al. (1997; DOI: 10.1126/science.275.5297.181) are two examples. [Nathan Gillett, Canada]	Taken into account. The section is rephrased. The large role of internal variability received more attention in the scientific community due to the availability of initial condition large ensembles and can be more routinely quantified at various spatial and temporal scales

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
25622	16	27	16	27	"Large initial-condition ensembles" is hard to parse, especially if the reader doesn't know what is meant. Does "large" modify "initial condition" or "ensembles"? Clearly the latter,but hard to parse. Better "Studies with large ensembles using perturbed initial conditions but otherwise maintaining all model properties identical have shown"; that would be much more meaningful to the reader. [Stephen E Schwartz, United States of America]	Taken into account. The terminology is changed to single-model initial condition large ensembles
25624	16	27	16	33	It might be stated that because of the climate system being chaotic, the expectation that climate model runs will yield an long-term average change in state that is equivalent to the average of an ensemble with differing initial conditions is an unproved conjecture due to Lorenz. See "The Lorenz Attractor, A Paradigm for Chaos," E. Ghys, in Chaos (pp. 1–54), Editors: B. Duplantier, S. Nonnenmacher, and V. Rivasseau (Springer Basel, 2013). https://link.springer.com/chapter/10.1007/978-3- 0348-0697-8_1. [Stephen E Schwartz, United States of America]	Rejected. Since this section only briefly summarizes the sources of uncertainty, such a detailed theoretical discussion is beyond the scope of this section.
36536	16	29	16	31	You have rightly reminded us of model difficulties, etc. Why is it that large ensembles are "crucial"? They are certainly a more powerful tool but there will be important caveats on the results as models have not improved that much. You return to these broad questions in the following page, paragraph that starts in Line 6. Perhaps this paragraph that mentions AR5 should be moved up? [Carlos Mechoso, United States of America]	Taken into account. The benefits of large ensembles even with unperfect models is better motivated here. The paragraph at the end of the section is moved up to the second paragraph
9038	16	31			A good reference here might be: Sippel, S., N. Meinshausen, A. Merrifield, F. Lehner, A.G. Pendergrass, E. Fischer, and R. Knutti, 0: Uncovering the forced climate response from a single ensemble member using statistical learning. J. Climate, 0, https://doi.org/10.1175/JCLI-D-18-0882.1 [Anna Merrifield, Switzerland]	Accepted, Reference added
43898	16	32	16	33	"internal variability constitutes an uncertainty that is irreducible and that at best can be accurately quantified", this is a strong statement, can it be supported by a reference? [Michael Mugarura, Germany]	Accepted. Reference added
37666	16	32			See comment 196 regarding volcanic activity and internal variability. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. Unclear reference to other comment
53388	16	35	16	38	There are some recent developments on this and more recent publications e.g. van Vuuren et al., GEC, 2007; Christensen et al., 2018, PNAS,; 10.1073/pnas.1713628115). This is an issue for xWG cooridnation. I suggest you get in touch with the scenario team in WGIII, and also coordinate with what is written in ch1 about this. [Jan Fuglestvedt, Norway]	Taken into account. This sentence is shortened and refers to chapter 1, which provides a more detailed discussion of SSPs

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
37668	16	36			A word such as "reliable" should be inserted before "probabilities". [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Text revised accordingly
25626	16	40	16	41	The difference between emissions and concentration driven results is not a source of uncertainty. The only meaningful means of examining the consequence of a projected set of emissions is through emissions driven modeling. Concentraton driven results are useful for many purposes, but not for examining the consequences of projections of emissions. [Stephen E Schwartz, United States of America]	Rejected. The sentence does not say that the difference is an uncertainty. The emission driven runs account for additional sources of uncertainties in the carbon cycle.
25628	16	40	16	41	Carbon cycle feedbacks are but one source of such uncertainty. The residence times of atmospheric constituents, the response time of the climate system, the climate sensitivity, the ocean heat uptake rate all contriubte to such uncertainties. [Stephen E Schwartz, United States of America]	Taken into account. That's a valuable comment. The other sources of uncertainties are also referred to here but for a more detailed discussion we refer to chapter 1 and chapter 5.
25630	16	46	16	48	Again, this is unproved conjecture. See “The Lorenz Attractor, A Paradigm for Chaos,” E. Ghys, in Chaos (pp. 1–54), Editors: B. Duplantier, S. Nonnenmacher, and V. Rivasseau (Springer Basel, 2013). https://link.springer.com/chapter/10.1007/978-3- 0348-0697-8_1. [Stephen E Schwartz, United States of America]	Noted. The comment is unclear what it exactly refers to the statement is supported by the references added
13186	16	46	16	54	What about the predictability for seasons and seasonal differences? [Nora Richter, United States of America]	Not applicable. This section does not assess the predictability
9040	16	48	16	54	Could the seasonal differences in uncertainty due to internal variability also be mentioned here? [Anna Merrifield, Switzerland]	Taken into account. The role of seasonality differs between region and variable but is referred to here.
35878	16	49	16	50	It is not only new literature published since the AR5 that has shown that 'the relative contribution of internal variability is larger for short than long projection horizons'. See for example Hawkins and Sutton (2009), Figure 3 (https://doi.org/10.1175/2009BAMS2607.1). [Nathan Gillett, Canada]	Taken into account. Sentence rephrased.
32070	16		16		As indicated in my comments on Chapter 1, I strongly suggest using the term "forcing uncertainty" in place of "scenario uncertainty" and "response uncertainty" in place of "model response uncertainty". Neither the scenarios nor the models are in fact uncertain - the uncertainty lies in the future forcing and in the response of the climate. [Rowan Sutton, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. Difference between scenarios reflects our uncertainty about future decision by humankind. By contrast, forcing as in "radiative forcing" is a model outcome, not an input. We disagree with the reviewer's semantics here, which clashes unnecessarily with common usage and hence readers' expectations.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
52904	17	6	17	16	I would suggest to add an in-depth discussion about the underlying discussion in Chapter 1 and then referring to that somewhere in this paragraph. Also a link to Section 3.8.3.2 should be included, although that section requires still some work. [Douglas Maraun, Austria]	Taken into account. The paragraph is shortened and refers to chapter 1 for a more detailed discussion
35880	17	9	17	16	It is not always necessarily true that 'The true uncertainty is larger than the model spread'. This depends if there are additional observational constraints on the projection. Observational constraints may be able to constrain projections more narrowly than the full model spread. This section should also discuss the paradigm used to interpret the multi-model ensemble - see for example Sanderson and Knutti (2012, doi:10.1029/2012GL052665); Annan and Hargreaves (2010, doi:10.1029/2009GL041994). Are the authors using the 'models are indistinguishable from the truth' paradigm? [Nathan Gillett, Canada]	Taken into account. The corresponding section is substantially shortened and for a more fundamental discussion the reader is referred to chapter 1.
29986	17	12	17	14	This is a very rosy view of emergent constraints. Chapter 1 (section 1.4.5.2) takes a much more cautious view, which is (I would say) a more accurate reflection of the current literature. I suggest you follow their lead in this respect. (See Caldwell et al. 2018 J.Clim. doi: 10.1175/JCLI-D-17-0631.1 for a sobering assessment of the current state-of-the-art; also e.g. Pithan & Mauritsen 2013 J.Clim. 10.1175/JCLI-D-12-00331.1 and Simpson & Polvani 2016 GRL doi: 10.1002/2016GL067989 for examples of where previously published emergent constraints are shown to be wrong.) [Theodore Shepherd, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The discussion of emergent constraints is removed here and a reference is added to the corresponding box.
35884	17	12	17	14	This text seems dismissive of emergent constraints since 'the validity of the proxy results rest on untested connections established by the climate model', but according to Box 4.1, emergent constraints are component (vi) of the information which will be used by the chapter to 'produce a robust method for quantifying uncertainty in climate projections'. These two components of the assessment should be linked and made consistent. [Nathan Gillett, Canada]	Taken into account. The discussion of emergent constraints is removed here and a reference is added to the corresponding box.
15270	17	13	17	16	I completely agree with this discussion, but it is somewhat at odds with some more optimistic claims in Chapter 3. [Claudia Tebaldi, United States of America]	Noted. The discussion of emergent constraints is removed here and a reference is added to the corresponding box.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
35882	17	14	17	16	The text here ends on the note that 'obtaining a full assessment of the uncertainty in an ensemble of projections continues to pose a fundamental epistemological challenge' but seems not to recognise that of course this is what the chapter has to do. Moreover, Box 4.1 promises that the chapter 'will apply all availble information' to 'produce a robust method for quantifying the uncertainty in climate projections'. These two components of the discussion should be better linked, so that the reader is aware of the challenges, but also the way in which these are addressed in the chapter. [Nathan Gillett, Canada]	Taken into account. The discussion of emergent constraints is removed here and a reference is added to the corresponding box.
8272	17	19	20	10	Why did CMIP6 have a bigger spread than CMIP5? [Zong Ci Zhao, China]	Not applicable. The section does not claim that CMIP6 has a systematically larger spread than CMIP5
52906	17	19			How does this approach accommodate for the issues raised, e.g., by Zappa and Shepherd, J. Climate 2017? i.e., that in particular dynamical changes may be projected which span opposite signs, but where the MMM is not a physically plausible evolution? Could the approach be modified to account for such non-zero but non-robust signals? [Douglas Maraun, Austria]	Taken into account. We now refer to this challenge in this section. There have been suggestions for more comprehensive metrics that take this into account. However, the method used here is a compromise between simplicity and comprehensiveness. Multi-model mean maps are used to illustrate the response but are only one aspect used in the assessment. Where not applicable changes are now discussed as storylines or grouped in models with consistent responses. The chapter now refers to the papers by Zappa and Shepherd.
35888	17	37	17	39	Deriving projections based on a multi-model ensemble with different ensemble sizes for each model is not a fundamental difficulty. If the chapter decides to give equal weight to each model, then ensemble means, and cumulative distributions can still be calculated using all available ensemble members simply by weighting each ensemble member by the inverse of the ensemble size for that model - see for example equation 2.4 in Gillett (2015) - https://doi.org/10.1098/rsta.2014.0425. This will lead to better estimates of means and other statistics than just using one ensemble member from each model. The cited reference here (Olonscheck and Notz, 2017), does not seem to primarily be concerned with the question of deriving projections from inhomogeneous multi-model ensembles. [Nathan Gillett, Canada]	Taken into account. Only one member is used here for multi-model averages. Indeed different realizations per model could be combined but inconsistent set of ensemble members would make it challenging to quantify robustness and thus the chapter uses only one realisation per model

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
35890	17	50	17	51	Describe the method. [Nathan Gillett, Canada]	Take into account. Method is briefly summarized in revised version
47218	17	50	17	51	While I agree that a strong argument is needed to use a different methodology, I would at least emphasize that projected climate change less than two standard deviations of internal variability in 20-yr means may still be significant and harmful depending on its persistence (probably much longer than 20 years in case of low mitigation) and on the magnitude of internal variability. The AR5 stippling methodology is at odds with the risk assessement framing and this could be emphasized if not modified. [Hervé Douville, France]	Take into account. This is acknowledged in the revised paragraph and is also the reason why a signal is shown where no stippling is used
37670	17	50			I believe what is intended here is "The rest of this chapter uses" rather than "The following chapter wil be using". The following chapter is Chapter 5. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Sentence rephrased accordingly
30670	17	53	17	55	not clear, please rephrase [Annalisa Cherchi, Italy]	Take into account. Sentence is rephrased
46248	18	4	18	4	One of the main methods of increasing the validity of using GCM models is the weight of these models. This method is based on comparing the output of GCM models in past periods with weather observation data. It would seem useful to add explanations in this regard [sadegh zeyaeyan, Iran]	Noted. The box 4.1 discusses the potential and limitations of model weighting
8882	18	4	18	4	One of the main methods of increasing the validity of using GCM models is the weight of these models. This method is based on comparing the output of GCM models in past periods with weather observation data. It would seem useful to add explanations in this regard [Mohammad Javad Zareian, Iran]	Noted. The box 4.1 discusses the potential and limitations of model weighting
57524	18	4	18	4	One of the main methods of increasing the validity of using GCM models is the weight of these models. This method is based on comparing the output of GCM models in past periods with weather observation data. It would seem useful to add explanations in this regard [Sahar Tajbakhsh Mosalman, Iran]	Noted. The box 4.1 discusses the potential and limitations of model weighting
38968	18	8	18	9	A figure to asixt/enforce the text would be desirable. [Masahide Kimoto, Japan]	Rejected. Due to space limitation such a figure cannot be added

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
32072	18	12	20	8	I think there is a need to re-consider carefully the purpose of this box. In my view the key issue here is not "Ensemble Evaluation and Weighting" but rather "Methodological approach to assessment of future Global Mean Surface Air Temperature". It needs to be made clear that such assessments draw on multiple lines of evidence of which the CMIP6 ensemble is just one. This box must also address: 1) how the inconsistency between the CMIP6 ranges for ECS and TCR and the Chapter 7 assessments of the same quantities will be handled in this chapter; 2) more broadly, how evidence from different sources will be combined to provide an overall assessment; 3) whether the chapter will assess only the likely range for GSAT or also assess potential high impact scenarios associated with the upper end of the ECS/TCR possibilities. I suggest that an attractive approach would be to highlight 3 specific scenarios for GSAT, corresponding to: the lower and upper bounds of the likely range for ECS/TCR; and to the upper bound for the very likely range for ECS/TCR. [Rowan Sutton, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The points 1)--3) mentioned by the reviewer have already been addressed in the FOD, although not all in this box; 3) is covered in Section 4.8. Moreover, owing to the lack of CMIP6 output at FOD stage and the lack of literature, the treatment of 1) and 2) had to remain tentative at FOD stage. The SOD comprises a fuller account and is entirely focused on the assessment approach actually taken in Ch4.
9282	18	14	18	17	A synthesis is welcome; at the same time a large part of the discussion in CH01 page 64 is left out (see comment on this section above). Also, what is the added value in mentioning that some materials come from other chapters if the explicit references are not given? [philippe waldteufel, France]	Taken into account. Our reading of Section 1.4.5.3 in the FOD suggests compatibility in the two treatments. The beginning of the box has been modified to remove the noted inconsistency.
28810	18	14	220	6	I like the box a lot, especially the detailed specific plans to evaluate the model ensemble and possibly weight. [Piers Piers Forster, United Kingdom (of Great Britain and Northern Ireland)]	Noted, thank you.
44556	18	14			I like Box 4.1. We should ensure that it is consistent with the presentation in Chapter 1, section 1.4.5.3, where we are tasked with establishing how weighting is generally performed across the report. Perhaps some of the material you present could be moved up to Chapter 1, as we did for emergent constraints (originally in C3)? Or at least we (C1) have to link forward to your discussion. I'd be happy to coordinate with the responsible C4 author(s). [Bjorn Samset, Norway]	Taken into account. Our reading of Section 1.4.5.3 in the FOD suggests compatibility in the two treatments, although the Ch4 reviewer of Ch1 FOD felt that an overall assessment -- implying a procedural suggestion for the entire WGI -- was not given in Ch1. It may not be possible to do so in general terms, but Box 4.1 at a minimum provides the methodological underpinning for GSAT uncertainty assessment. Since this requires detailed exposition of GSAT projections, the Box needs to be placed in Ch4.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
53846	18	14			This box is useful and very much needed. It could also be placed in ch1, where model weigthing is already presented. [Jan Fuglestvedt, Norway]	Rejected. The box is placed in Ch4 because of its direct implications for the assessment of future changes. Thus the box, which has undergone substantial further development from FOD to SOD, is an integral part of the Ch4 assessment and is itself informed by the available projections. This level of detail would not be suitable for Ch1.
51910	18	16	18	17	I'm not sure what this stub sentence introduction adds and would suggest its removal [Peter Thorne, Ireland]	Accepted; text deleted.
35892	18	29	18	32	It was known at the time of the AR5 and long before that internal variability 'manifests itself substantially also on the multi-decadal timescale'. For example, in section 10.3.1.1.3 of the AR5 WGI report 'Several studies that have aimed to separate forced surface temperature variations from those associated with internal variability have identified the North Atlantic as a dominant centre of multi-decadal internal variability, and in particular modes of variability related to the Atlantic Multi-Decadal Oscillation.' Chapter 14 of the AR5 has a whole section on Atlantic Multidecadal variability (14.7.6) which includes the statement 'Along with secular trends and Pacific variability, the AMO is one of the principal features of multidecadal variability in the instrumental record'. The role of internal multidecadal variability has been discussed in the literature in many much earlier literature as well, of which Delworth et al. (1997) (https://doi.org/10.1029/96GL03927) and Griffies et al. (1997; DOI: 10.1126/science.275.5297.181) are two examples. [Nathan Gillett, Canada]	Accepted; the phrasing was inaccurate and has been corrected.
31948	18	30	18	30	is it interval or internal? [Marie-France Loutre, Switzerland]	Taken into account. Internal. Fixed, thank you.
6930	18	33	18	34	This passage is not entirely clear. For example, from the previous paragraph it is not obvious that this characterisation has to ignore the information from the observations in principle. Is it just the methods employed in the cited studies in the paragraph above that ignore observational information on the internal variability? [Olson Roman, Republic of Korea]	Taken into account. The list item is concerned not with estimates of the internal-variability level per se but instead with information provided by initialisation. The SOD makes this clearer.
9042	18	34			A good reference in response to observationally-constrained uncertainty might be: McKinnon, K.A., A. Poppick, E. Dunn-Sigouin, and C. Deser, 2017: An “Observational Large Ensemble” to Compare Observed and Modeled Temperature Trend Uncertainty due to Internal Variability. J. Climate, 30, 7585–7598, https://doi.org/10.1175/JCLI-D-16-0905.1 [Anna Merrifield, Switzerland]	Taken into account. However, the list item is concerned not with estimates of the internal-variability level per se but instead with information provided by initialisation.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
53390	18	42	18	44	the point is that "small uncertainties matter a great deal" for the ambious warming levels; e.g. 1.5. [Jan Fuglestvedt, Norway]	Accepted; point clarified.
6932	18	53			There are other recent studies which have attempted to incorporate model dependence into the weighting or multi-model projections, which could be referenced: 1. S. Steinschneider, R. McCrary, L. O. Mearns, and C. Brown (2015). The effects of climate model similarity on probabilistic climate projections and the implications for local, risk-based adaptation planning. Geophysical Research Letters, 42(12):5014-5022. 2. N. Haughton, G. Abramowitz, A. Pitman, and S. J. Phipps (2015). Weighting climate model ensembles for mean and variance estimates. Climate Dynamics, 45(11-12):3169-3181. 3. B. M. Sanderson, R. Knutti, and P. Caldwell (2015). Addressing interdependency in a multimodel ensemble by interpolation of model properties. Journal of Climate, 28(13):5150-5170. 4. R. Olson, S.-I. An, Y. Fan, W. Chang, and J. P. Evans (2019): A novel method to test non-exclusive hypotheses applied to Arctic ice projections from dependent models. Accepted to Nature Communications. [Olson Roman, Republic of Korea]	Taken into account. Thanks for the pointers. However, since the SOD version of Box 4.1 is more focused on what is actually being done in Ch.4, the references to methods per se has not been expanded.
49512	19	15	19	15	Beigging of the sentence "0" is probably for "Figure 1" ? [Zbigniew Klimont, Austria]	Accepted. Error introduced in production.
38434	19	15	19	23	The figure is referred to as „0“ in this paragraph. [Dirk Notz, Germany]	Accepted. Error introduced in production.
54218	19	15	19	23	what is the motivation of using MPI-GE not another large ensemble here? Or using more than one? [Nicola Maher, Germany]	Accepted; expanded in SOD. IV levels are quoted for several CMIP6 large ensembles.
9044	19	15	20	12	In terms of continuity, it might be nice to have a sentence addressing the change relative to the same base period in AR5 vs. just the change relative to the new 1995-2014 base period. Alternatively, when you discuss the years exceeding 1.5, 2.0, and 3.0˚C, you could comment on when these thresholds are reached in AR5. [Anna Merrifield, Switzerland]	Taken into account (comment is on 4.3, not Box 4.1). The SOD more systematically makes the connection to CMIP5/AR5 including the changing reference periods.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
35894	19	19	19	20	Only one sentence is given on the calculation of projected temperature change using an EBM, and no reference is provided either for the EBM itself, the radiative forcing, or the ocean diffusivity/ocean heat uptake used, all of which will affect the results. Some information is provided in the caption to Figure 4.8, but his deserves discussion in the text itself. This is important, as these EBM results are reported in the ES. Also, the text does not provide a justification for the use of an observational constrained on ECS to constrain projections, rather than TCR. TCR usually can be more closely constrained by observations, and is more closely correlated with projected warming than ECS is. See for example IPCC AR5 WGI Box 12.2 'For scenarios of increasing RF, TCR is a more informative indicator of future climate than ECS (Frame et al;, 2005; Held et al., 2010)'. See also Gillett (2015) - https://doi.org/10.1098/rsta.2014.0425. [Nathan Gillett, Canada]	Accepted. The description of the EBM has been expanded in the main text, and TCR as well as ECS are now used to inform likely warming ranges.
57460	19	19	19	29	I like this use of the 2-layer ocean model "emulator". I think Chapter 7 uses a different emulator (FAIR), so the chapters should be coordinated on this. Also, where is this emulator described? And is this information used later when assessing likely future warming ranges, or is that based solely on CMIP model range without regard to this calculation? [Kyle Armour, United States of America]	Accepted. The emulator is a subset of the one used in chapter 7, to the extent that the ERF is taken directly from Ch7 and not independently calculated here. . The box has been substantially expanded on how the emulator is defined and then used in the Ch4 assessment.
35896	19	27	19	29	The ECS of the CMIP6 models can be calculated directly based on DECK simulations - for the SOD this statement regarding whether the stronger-warming models have an ECS above the Chapter 7 assessed range can be changed to a statement of fact, rather than a medium confidence assessment. [Nathan Gillett, Canada]	Taken into account. At FOD stage, practically all information on CMIP6 ECS had been obtained informally and not through a peer-reviewed publication. Now the papers are available and have been cited.
35898	19	31	19	33	Why focus on just the uncertainty associated with ECS here, rather than the model uncertainty more generally? The ECS is not the only feature of the models which influences the warming rate - the rate of ocean heat uptake, as well as the simulated changes in ERF for projected GHG and aerosol changes are also important. [Nathan Gillett, Canada]	Taken into account. SOD includes TCR uncertainty information; we note that for a given ERF and fixed efficacy (as in Held et al. 2010), the information content of the pair ECS, TCR is equivalent to that of the pair climate feedback parameter, ocean heat uptake efficiency. All this is covered in SOD.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
42988	19	39	20	5	Box 4.1, Figure 1: Should use an average of interpolated observational series available back to 1850. Currently, that includes Berkeley Earth and Cowtan-Way, but not HadCRUT4. HadCRUT5 is expected to also be more spatially complete, according to Ch 02 FOD, so all three series could be used for observations average in Ch 04 in that case. [David Clarke, Canada]	Taken into account. SOD has aimed to use products consistent with Ch1 and Ch2.
37672	19	50			HadCRUT4 should not be referred to as observations. It is a datasets of gridded values based on analysing and blending observations. "observataions (HadCRUT4" could be replaced by "values from observations (HadCRUT4", "observationally-based values (HadCRUT4" or something similar. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. While the point is in principle correct, it would make the writing and communication unnecessarily complicated.
8274	20	11	59	3	Some CMIP6 projections should compare with CMIP5 results. [Zong Ci Zhao, China]	Noted. The figures in Subsection 4.3 show CMIP6 model results, while the text compares with CMIP5 results (where appropriate). Direct comparison between CMIP5 and CMIP6 results are limited because of the different forcing scenarios employed.
31954	20	11			It wuld be useful to make connection with what is reconstructed from the past in the different (previous or later) chapters [Marie-France Loutre, Switzerland]	Accepted. Chapter 4's mandate primarily concerns future projections and predictions. Where appropriate, the SOD makes connections to historical changes considered in the previous and later chapters.
30364	20	21	20	29	During the scenario cross-chapter and cross-WG coordination also the lowest scenario available in the ScenarioMIP set (SSP1-1.9) was recommended to be included as default scenario in plots (of course, if available from CMIP6 models as this is marked tier 2 because at the time of the ScenarioMIP prioritisation decision the Paris Agreement wasn't agreed upon yet). See Chapter 1 Box 1.6. [Joeri Rogelj, Austria]	Accepted.
30672	20	23	20	25	this has been already described in previous section. The need of repeating some information is understandable but it could be more useful to include a table to use as a reference for these information [Annalisa Cherchi, Italy]	Rejected. We have tried to minimize the repetition but will not add a table.
25632	20	24	20	25	It is essential that the forcings for the several SSPs be explicitly shown as function of time, either here or elsewhere in AR6, with reference to the location where they are presented. [Stephen E Schwartz, United States of America]	Noted. These forcings are shown elsewhere in the SOD and a reference provided.
30674	20	26	20	29	Same as comment above, information repeated: it would be better to have a table with all these information to refer to [Annalisa Cherchi, Italy]	Rejected. We have tried to minimize the repetition but will not add a table.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
9280	20	34	20	43	The assignations of subplots b) and c) should be reversed [philippe waldteufel, France]	Accepted..
45242	20	34	20	43	Figure 4.1. This is a nice summary figure, but it seems odd to me to just show a single component of future sea level change (i.e. thermosteric) in panel d). Perhaps for the final figure it would be good to show projections of total sea level change? An alternative woudl be to show total ocean heat content change, which essentially represents the total heat uptake by the climate system over the 21st century (and linkages in the text could be made to Chapter 7). A couple of aspects to highlight in this regard is the more monotonic nature of total OHC / thermal expansion compared to global surface temperature and the long-term commitment (both of these are discussed in Chapter 7). [Matthew Palmer, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Thermostatic sea level was only placeholder in the FOD. The SOD shows total sea level.
32954	20	34			Global sea level change' should be 'global mean sea-level change'. But if this only includes thermosteric, then it would be confusing to call it 'global mean sea level': it should be 'thermosteric sea level'. [Aimee Slangen, Netherlands]	Accepted. Thermostatic sea level was only placeholder in the FOD. The SOD shows total sea level.
44486	20		23		Future Projection of regions where prominent changes in temperature and precipitation is projected are missing. It is very necessary to discuss future projections with reference to different regions [Shaukat Ali, Pakistan]	Rejected. Other sections of this chapter, and subsequent chapters, will provide regional information.
9070	20				For MCB, do seedable marine low clouds cover 75% of the ocean surface? Does 75% of the ocean surface need to be covered in low clouds to achieve the 3.5 W/m2 reduction in solar insolation? [Anna Merrifield, Switzerland]	Noted. Refers to Table 4.6 on page 4-68 in FOD. Answer is "yes".
53392	21	12	21	23	This important para is hard to read. I sugges leaving out the info on CO2 concentration. [Jan Fuglestvedt, Norway]	Accepted.
36540	21	12	21	23	An underlying issue is whether the CMIP6 models have actually “improved” in reference to the CMIP5 models. Direct comparisons are difficult because scenarios are different. Nevertheless, similarity among CMIP5 and CMIP6 results may be a positive aspect of the results. After all, the progress from CMIP5 to CMIP6 may be quite model dependent so that some of the latter models are very close to the former ones. [Carlos Mechoso, United States of America]	Noted.
25634	21	15	21	15	" We will compare..." better "We compare..." but the important part of the sentence is not who is doing the comparing but what is being compared. So better still "The changes in GSAT across the three reference periods and the four SSPs are compared and contrasted..." [Stephen E Schwartz, United States of America]	Accepted.
30676	21	17	21	17	Table 4.2 not table 4.1 as reported [Annalisa Cherchi, Italy]	Accepted.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
57262	21	32	22	2	To be able to interpret the meaning of the entries correctly, it would be preferable not to give numbers compared to 1995-2014, but compared to pre or early-industrial (just as is shortly thereafter, on p. 22/l. 6-9). At the very least, the temperature difference between pre/early industrial and 1005-2014 should be given in the table caption [Oliver Geden, Germany]	Accepted.
27208	21	34	22	1	The difference of the projections towards the end of this century and the observations, 0.4°C since 1945, beginning of acceleration of CO2 emissions, are too large for comparable periods of time to be convincing. [François GERVAIS, France]	Noted.
16024	21	34	22	1	Suggest adding a table showing the projected temperature rise relative to pre-industrial levels under different SSPs [SAI MING LEE, China]	Rejected. Not enough space in the table.
35900	21	35	21	36	Just show the 5-95% range - you don't need to show the standard deviation as well, as this information is redundant. [Nathan Gillett, Canada]	Accepted.
37674	21	42	22	7	With regard to the 2021-2040 entries in Table 4.2, a rise of 0.8ºC relative to the 1995-2014 baseline implies a warming rate that is 70% higher than deduced from observations made over the past 40 years. As things stand now, these numbers do not engender any confidence in the models. It is understandable that it is perhaps too early to develop a critical discussion of the incomplete results, but if other models perform similarly such discussion must be given. Simply quoting 2025 (or another year earlier than the range indicated in SR1.5) for the year in which 1.5ºC is attained will not be enough. Even if the detailed discussion is given later in section 4.4.1.1 (which is far from complete at present) some summary of it will have to be given earlier. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. This apparent mismatch is discussed elsewhere in the SOD.
37676	22	6			"pre-industrial (1850-1900)" should be changed to "early-industrial baseline (1850-1900)" to be consistent with the terminology established in Chapter 1. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. For ease we use pre-industrial in the main text but explain it’s use in the chapter's introduction.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
27210	22	9	22	12	Among the 1°C of average temperature increase since the pre-industrial period, it is seen in Figure 2.12 that about 0.6°C has been achieved between 1910 and 1945 when the emissions were much lower than nowadays. As a result, Ring, M.J., Lindner, D., Cross, E.F., Schlesinger, M.E., 2012 (Causes of the global warming observed since the 19th century. Atmos. Clim. Sci. 2, 401–415) consider that this increase was mainly natural. This is confirmed in Fig. 1 of FAQ 9.2 with only 15 % of human driver in the period 1900-1950. Since the accelaration of emissions starting in 1945, the increase of temperature has been only about 0.4°C up to the plateau before (and after) the natural El Niño peak of 2016, among which one half might be anthropogenic. About +0.2°C anthropogenic during 3/4 of a century appears inconsistent with the conclusions based on CMIP6 models. [François GERVAIS, France]	Noted.
11526	22	22	23	22	It needs noted that while the precipitation over land increases, the demand for water (i.e. P-E) increases even faster as the climate warms. This means that most land areas in mid and low latitudes will be DRIER in spite of the increased P. [Roanld Stouffer, United States of America]	Accepted. This is noted elsewhere in the SOD.
46250	22	24	22	24	GCM weighting is associated with some problems in the dry months of the year when their precipitation is zero or near zero. This issue should be discussed in this chapter in general [sadegh zeyaeyan, Iran]	Rejected. This level of detail is beyond the mandate of Chapter 4.
8884	22	24	22	24	GCM weighting is associated with some problems in the dry months of the year when their precipitation is zero or near zero. This issue should be discussed in this chapter in general [Mohammad Javad Zareian, Iran]	Rejected. This level of detail is beyond the mandate of Chapter 4.
57526	22	24	22	24	GCM weighting is associated with some problems in the dry months of the year when their precipitation is zero or near zero. This issue should be discussed in this chapter in general [Sahar Tajbakhsh Mosalman, Iran]	Rejected. This level of detail is beyond the mandate of Chapter 4.
36542	22	31	22	32	Yes, it is good to focus on precipitation over land. [Carlos Mechoso, United States of America]	Thank you.
30678	22	34	22	34	better to include also the panel of interest in the figure [Annalisa Cherchi, Italy]	Rejected. Space limitations prevent this.
30680	22	48	23	3	increase measured in mm/day may have a small perception, it would maybe more interesting/useful to indicare increase in precipitation in terms of % with respect to reference period [Annalisa Cherchi, Italy]	Accepted.
30682	23	18	23	18	why only NAT subtropics? Tropics as stated in line 10 above would be more interesting to be assessed from a global or quasi-global perspective [Annalisa Cherchi, Italy]	Accepted. The table assesses the tropics.
48472	23	27	23	27	Please include assessment of Antarctic sea ice projections [Julie Arblaster, Australia]	Noted. Antarctic sea ice is covered in Chapter 9.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
45780	23	27	23	27	Remove biosphere from title as not assessed [Katja Mintenbeck, Germany]	Accepted. In the SOD we now assess land carbon uptake and therefore retain biosphere in the title.
35292	23	27	27	35	If Section 4.3.2 Is titled "Cryosphere, Ocean, and Biosphere", where is there nothing about the terrestrial part of the biosphere? Either delete biosphere from the title of this section or add projections on the terrestrial biosphere. While the later chapters cover aspects of the terrestrial biosphere response to climate change (carbon in ch. 5, water in ch. 8, etc) there is still room to make overall assessments about proijected biosphere change here. [Charles Koven, United States of America]	Accepted. In the SOD we now assess land carbon uptake.
49514	23	37	24	5	Not clear if the shortcomings of the models used in AR5 have been remedied or still remain? Do the current versions of the models suffer from the same problem and what does it mean for the CMIP6 results/their interprettion? [Zbigniew Klimont, Austria]	Noted. This question is outside the scope of Chapter 4.
31158	23	37	24	5	The case is made that model selection/subsetting or weighting is not appropriate for studying Arctic sea ice projections. Some recent literature where the opposite is advocated or done is not cited: Knutti et al., 2017 (https://onlinelibrary.wiley.com/doi/full/10.1002/2016GL072012), Overland et al., 2015 (https://www.sciencedirect.com/science/article/pii/S0079661115000038). The text would deserve more nuance in how to approach the question of reducing projection uncertainty. [François Massonnet, Belgium]	Accepted.
12806	24	7	24	15	When the Arctic loses ice, it allows extra RF that can further reduce ice; also, consider the timing (how soon) ice-free and the difference between lower emission scenarios that can severely limit the extent of ice loss and the likelihood of ice-free. (Note: this information is covered in below paragraph, so may be useful to comment that this paragraph just have a sign post to the information in the other paragraph.) May also be useful to mention proximity to ice-free Arctic. Overland and Wang (2013) When will the summer Arctic be nearly sea ice free?, GEOPHYSICAL RESEARCH LETTERS 40:2097–2101, 2097 (“Time horizons for a nearly sea ice-free summer for these three approaches [for estimating future ice loss covered in the study] are roughly 2020 or earlier, 2030 ± 10 years, and 2040 or later.”). [Durwood Zaelke, United States of America]	Noted. Chapter 9 has been mandated to go more extensively into these issues.
9046	24	13	24	14	"Permanently" suggests a completely ice-free Arctic all year.. The sentence reads well as: … the Arctic will become effectively ice-free (..) in September by the end of the 21st century. [Anna Merrifield, Switzerland]	Accepted.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
36546	24	23	24	33	These AMOC projections are quite interesting! [Carlos Mechoso, United States of America]	Noted. Thank you.
39396	24	32	24	35	This message is repeated inslightly different words p. 33 L.31-35, not sure if that is on purpose. [Clara Burgard, Germany]	Accepted.
38436	24	32			Is it intended to keep figure 4.3 as a single-model result? I'd find this difficult, as it gives too much weight to a single model. I expect that this will change as more CMIP6 results become available, but currently no indication is given that such change is intended. [Dirk Notz, Germany]	Accepted. This figure has been replaced with a multi-model figure.
32404	24	34	24	35	Regarding the impact of 2C warming vs 1.5C warming for a nearly-ice-free Arctic: As well as the 2 cited modelling studies here (CESM=Jahn et al.; CanESM=Sigmond et al.) we performed a 3rd modelling study performed at the same time using HadGEM2-ES: Ridley & Blockley (2018). The top-level conclusion is in agreement with the other stuides - that there is a very low (<1%) chance of a nearly-ice-free-Arctic with 1.5C warming and a much higher likelihood (~40%) at 2.0 C warming. Including this citation would strengthen this statement further. Ridley, J. K. and Blockley, E. W.: Brief communication: Solar radiation management not as effective as CO2 mitigation for Arctic sea ice loss in hitting the 1.5 and 2 °C COP climate targets, The Cryosphere, 12, 3355-3360, https://doi.org/10.5194/tc-12-3355-2018, 2018. [Ed Blockley, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. The selected number of references will suffice and the Ridley and Blockley study is not directly comparable.
15272	24	35	24	35	Sanderson et al. 2017 https://doi.org/10.3929/ethz-b-000191578 shows that too, based on 10-members ensembles with CESM1 in simulations stabilizing at 1.5C and 2C. [Claudia Tebaldi, United States of America]	Rejected. A more extensive reference list is provided in Chapter 9.
36544	24	37	24	38	Pointing to difficulties of models used in AR5 will bring the question about whether the difficulties have been overcome in AR6. Do we know the answer to this question? [Carlos Mechoso, United States of America]	Noted. No we do not know the answer to this question
35902	24	43	25	1	The Sigmond et al. (2018) simulations set aerosol emissions to zero at the start of the stabilisation period (see their Methods), so the effect of aerosol reductions is included in these simulations. [Nathan Gillett, Canada]	Rejected. This would be too much detail.
49516	24	43	25	1	Does is mean that the aerosol load is fixed at some level? Not clear. The SSPs have different aerosol trajectories that vary significantlky in terms of SO2, BC, etc.The quoted study refers to the CMIP5 experiments and RCPs indeed had a very narrow band of futures, i.e, all of them looked alike for aerosols - that is not the case for SSPs and so the CMIP6 runs shall provide a better insight. Additionally, analysis of the results for the SSP3-7.0 vs SSP3-NTCF would be interesting to look at here. [Zbigniew Klimont, Austria]	Rejected. This is beyond the scope of our Chapter.
30276	24		26		The tables are empty and the tables in entire chapter should be checked [Nazan An, Turkey]	Accepted.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
11528	25	17	25	17	The long time scales of sea level response should be noted here. One example could be the continued rise of sea level in the SSP1-2.6 scenario while the global temperature increase is near 0. [Roanld Stouffer, United States of America]	Rejected. A more detailed assessment can be found in Chapter 9.
32956	25	17	25	50	It is potentially confusing that the first half of the text talks about total sea level projections, while the second half is about thermosteric sea level change from CMIP6 models. For the lines 36-45, the use of 'GMSL' is not correct: this should be 'thermosteric sea-level change'. [Aimee Slangen, Netherlands]	Accepted.
55240	25	17	26	7	The fact that when CO2 levels rose from 307 ppmv (ninety years ago) to 410 ppmv (now), it caused no detectable acceleration in the rate of sea-level rise at any of the highest-quality long measurement sites, means that the projections from process-based models and SEMs are worthless. "Since the rate of sea level rise has not increased significantly in response to the last 3/4 century of CO2 emissions, there is no reason to expect that it will do so in response to the next 3/4 century of CO2 emissions. The best prediction for sea level in the future is simply a linear projection of the history of sea level at the same location in the past..." Burton (2012) doi:10.1007/s11069-012-0159-8 https://sealevel.info/1612340_Honolulu_Wismar_Stockholm_vs_CO2_annot3.png [David Burton, United States of America]	Noted.
8558	25	27	25	34	Kopp et al. (2014) is not emulation-based. It uses an approach similar to Slangen et al. (2014) and AR5. [Robert Kopp, United States of America]	Accepted. This text has been revised.
32116	25	31	25	32	A reference is missing for 'invalidate this assumption'. Does it depend on what is the past (century, millennia, geological timescale)? [Marie-France Loutre, Switzerland]	Accepted. This text has been revised.
32958	25	54	25	56	Happy to see that there will be additional periods for SL projections here, this will be a good match with the projections table in CH9 which will contain only the 2100 period. Perhaps only show the total change here (as other contributions are not discussed here anyway), while the breakdown into contributions happens in CH9? -> discuss with CH9 (Kopp/Slangen) [Aimee Slangen, Netherlands]	Accepted.
7884	26	18	26	19	At the moment Ch 9 looks like it is only showing one SSP and for AMOC at 26.5N. Please check consistency [Laura Jackson, United Kingdom (of Great Britain and Northern Ireland)]	Noted.
46646	26	19	26	19	Ch9 is referenced in general. More precise reference is 9.2.4.1 (Atlantic Meridional Overtuning Circulation) [WGI TSU, France]	Noted.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
7888	26	23	26	43	I don't understand why Fig 4.4 is shown rather than CMIP6 projections (unless this is a placeholder for projections). The advantage of the CanESM2 ensemble is that it can distinguish internal variability but the disadvantage is that it is only one model. Hence for projections you should be showing CMIP6 (when they are ready) and maybe discussing what the ensemble tells us in terms of forced changes vs internal variability. At the moment this section is all about the single model ensemble . If this is a placeholder then that should be made clear. [Laura Jackson, United Kingdom (of Great Britain and Northern Ireland)]	Accepted.
7890	26	23	26	43	It would seem sensible (given the chapter title) to discuss in more detail the specific SSP projections of the AMOC here, and use chapter 9 to discuss processes and details more [Laura Jackson, United Kingdom (of Great Britain and Northern Ireland)]	Accepted.
7886	26	31	26	33	The stabilisation could easily be model dependent. If you are discussing long term stabilisation then please also mention other studies [Laura Jackson, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. CMIP6 multimodel information now shown.
15274	26	33	26	33	Here is a case where I wonder if a discussion of the reasons for the behavior as it relates to GMST would be worth/warranted [Claudia Tebaldi, United States of America]	Rejected. This is the mandate of Chapter 9.
35294	27	4	27	35	Isn't this info more properly covered in chapter 5? [Charles Koven, United States of America]	Rejected. Chapter 4 was mandated to cover this.
39510	27	39	27	44	Notice that observed changes of both NAM and SAM are assessed in CH2 (sections 2.4.5.1 and 2.4.5.2) while the human influence on those changes is assessed in CH3 (sections 3.7.1 and 3.7.2). Check consistency and coherency across chapters regarding definitions and associated references. [Carolina Vera, Argentina]	Taken into account. This has been addressed by creating a technical annex on modes of variability across chapters 2, 3, 4 to ensure consistent definitions and uses. Cross-references to chapter 2 and 3 have been added.
28582	27	42			The paper recently published in Climate Dynamics, Watterson (2019) 52:2451-2466, uses these latitudes (40, 65) to assess pattern-scaled changes in all CMIP5 models. There is support for various assessments made regarding NAM and SAM, and uncertainty in their changes. [Ian Watterson, Australia]	Noted. Published literature on pattern scaling is assessed in Sections 4.2.4 and 4.6.1
45620	27	45			This section is somewhat redundant with section 4.4.3.1 later. [Julien Cattiaux, France]	Rejected. The chapter structure covers near-term and long-term changes separately. Hence the modes of variability sections 4.3.3 and 4.4.3 are both required. Overlap amongst the different subsections has been removed.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
35904	27	47	27	49	The statement that 'the SAM is likely to weaken under all of the priority SSPs through the mid-21st century' does not seem to be supported by Figure 4.6. Do the authors mean that the trend in the SAM will weaken? Also which season is the assessment for? This text does not specify a season, but Figure 4.6 is for boreal winter. [Nathan Gillett, Canada]	Taken into account. Figure 4.6 updated to show all seasons and core SSPs. The statement should have referred to weakening of the SAM trend. Corrected for SOD.
47220	27	51	27	52	This choice is at odds with the strong internal variabilityof the NAM. Note also that NAM may be defined using either SLP or Z500, which can make a significant difference in terms of projected anomalies (e.g., Cattiaux and Cassou, 2013) [Hervé Douville, France]	Noted. Most results in the chapter are based on one ensemble member per model. The definitions of the modes of variability follow the new technical annex on modes of variability and are consistent across chapters 2, 3, 4.
29614	27	60	27	60	There are a number of references to different sections of the Atlas which must be updated when needed (at the moment, they appear as “Atlas, Section X.Y” and refer to CMIP6 models, which are not included in the Atlas yet). [Rodrigo Manzanas, Spain]	Comment moved to Entire Chapter comments. These non-specific references to the Atlas have been removed in the SOD.
30684	28	1	28	3	not sure I would conclude this from Fig 4.6 (but this will need to be anyway updated with more models) [Annalisa Cherchi, Italy]	Noted. For the SOD stage we now have more CMIP6 models available and these have been included in the assessment of annular mode changes.
48464	28	2	28	2	Qualify that this statement is for the DJF SAM only [Julie Arblaster, Australia]	Taken into account. This has been qualified.
39514	28	2	28	3	It is confusing to talk about "SAM weakening" considering that it is a mode of variability with a phase and amplitude associated. I suggest to describe the SAM changes as positive or negative, like it is done for NAM, or clarify if the statement describes SAM positive trend. [Carolina Vera, Argentina]	Taken into account. The text has been amended to describe SAM trends with the appropriate sign.
30686	28	5	28	5	together with fig 4.6 I would suggest to include a table with mean values for short, medium and long term compared to present (reference period) [Annalisa Cherchi, Italy]	Noted. Updated figures 4.15 and 4.32 show the absolute SAM anomalies for the near-term and long-term for the SSP5-8.5 scenario.
30688	28	10	28	10	why averages are not relative to 1995-2014? Also the period 1995-2014 is subject to transiency in forcing, but it should be find a measure to identify if the modes are ïncreasing", "decreasing" or "unchanged" in the different futures [Annalisa Cherchi, Italy]	Taken into account. This was an error in the text and the reference period is 1995-2014. This is the present day reference period used throughout the chapter.
54214	28	18	28	42	Both ENSO precipitation and the central verses eastern Pacific events are important. Something on these should be included here. [Nicola Maher, Germany]	Taken into account. The SOD assesses literatures discussing changes in both central and eastern pacific ENSO. Changes in ENSO-related precipitation and temperature are discussed in subsections 4.4.3 and 4.5.3.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
35906	28	20	28	23	Refer to Chapter 2 for description and definition of ENSO. [Nathan Gillett, Canada]	Taken into account. A Technical annex on modes of variability are consistent definitions across chapters 2-4 is developing under the cross-chapter coordination.
54418	28	28	28	42	The discussion around Figure 4.7 should somehow take into account the natural variability of pre-industrial simulations before any claim can be made on the evolution of ENSO properties on the next decades. Most current assessments of ENSO in the next decades to century agree that no change is expected in mean ENSO properties. If AR6 decides to claim otherwise, this should be made from a balanced review of this litterature (Kim et al. 2014b is just one publication). [Eric Guilyardi, France]	Taken into account. Fig 4.7 and the assessment of ENSO projections have been updated in the SOD as more CMIP6 models are now available. The CMIP6 results are cast in the light of other literature. However, we don't use pre-industrial simulation to estimate the degree of natural variability which is addressed in Ch3 (section 3.7.3) for D&A of ENSO change.
54420	28	28	28	42	Also such an assessment should be based in CMIP5 results as well (not just CMIP6), even if the scenarii are not exactly comparable. [Eric Guilyardi, France]	Take into account. In the SOD more published literature from CMIP5 are included in the assessment of ENSO alongside the CMIP6 results.
36548	28	28	28	42	Anything to say about EP and CP Niños? [Carlos Mechoso, United States of America]	Take into account. The SOD assesses literatures discussing changes in both central and eastern pacific ENSO.
16026	28	29	28	30	The Nino 3.4 region is very far from the South American coast. Suggest revising the description of the Nino 3.4 region as appropriate. [SAI MING LEE, China]	Accepted. The description has been revised and the reference to the South American coast removed.
57462	28	33	28	35	What's the mechanism for the increase followed by decrease in ENSO variability? Perhaps this will go away with more models. [Kyle Armour, United States of America]	Take into account. Only a few models were available at the time of FOD and hence there was large sampling uncertainty. Fig 4.7 has been updated with more CMIP6 results and this behaviour is not present across the larger ensemble.
38970	28	34	28	34	Why does the ENSO amplitude goes up and down? [Masahide Kimoto, Japan]	Take into account. Only a few models were available at the time of FOD and hence there was large sampling uncertainty. Fig 4.7 has been updated with more CMIP6 results and this behaviour is not present across the larger ensemble.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
15276	28	36	28	36	Same as above, wouldn't it be good to attach some understanding of the changes presented? [Claudia Tebaldi, United States of America]	Take into account Only a few models were available at the time of FOD and hence there was large sampling uncertainty. Fig 4.7 has been updated with more CMIP6 results and this behaviour is not present across the larger ensemble. The assessment of projected ENSO changes has been updated.
38972	28	39	28	39	Please state why ENSO weakens. [Masahide Kimoto, Japan]	Take into account Only a few models were available at the time of FOD and hence there was large sampling uncertainty. Fig 4.7 has been updated with more CMIP6 results and this behaviour is not present across the larger ensemble. The assessment of projected ENSO changes has been updated.
15564	28				4.3.3.2 ENSO : The current version considered the evolution of the amplitude of ENSO variability projected by the five CMIP6 models over the 21st century. Based on this analysis, the authors can not conclude the changes in ENSO variability in near-term period. Simply put, it is not enough to conclude based on five CMIP6 climate models. [SANG-WOOK YEH, Republic of Korea]	Taken into account. We agree. For the SOD stage we now have more CMIP6 models available and these have been included in the assessment of ENSO variability.
11530	29	2	29	17	Need to define what time period is in view. What is "near-term? 1 year, 1 decade, what? [Roanld Stouffer, United States of America]	Taken into account. As introduced in Section 4.1, for the purposes of this chapter near-term is the period 2021-2040. This has been added to the opening paragraph of Section 4.4
9048	29	2			Near, mid, and long-term climate responses seem to be considered together in the previous section, would it be possible to consider them together throughout the chapter? If responses are largely monotonic (as described in the previous section), then describing them separately could result in repetition. [Anna Merrifield, Switzerland]	Rejected. The chapter structure is defined to consider all variables for each time period rather than all time periods for each variable. Both approaches have pros and cons but after much consideration this is the structure we have adopted. Efforts have been taken in the SOD to reduce repetition across sections 4.4 and 4.5 where possible.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
32074	29	15	30	2	There is a need to add a discussion of how CMIP6 multi-model projections are sensitive to reference period, particularly for the near term (e.g. Hawkins & Sutton, BAMS, 2016). Also, we found in AR5 Ch 11 that using trends rather than anomalies from the CMIP ensemble results provided an important constraint on GSAT for the near term. It would seem a good idea to consider this in AR6 also. [Rowan Sutton, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Thank you for the suggestion. For the purpose of the chapter we clearly define our reference periods (1850-1900 preindustrial and 1995-2014 present day). For consistency all projections are plotted relative the one of these periods. However, in the SOD we assess the literature on near-term predictions/projections, including where relevant the choice of reference periods and how this affects interpretation of the model results.
26546	29	17	3	2	This section is almost lacking explanations and comments. [Antonia Longobardi, Italy]	Noted. This was largely a placeholder section in the FOD and has been expanded for the SOD.
49518	29	18	29	21	Similar comment to the above in terms of differnece between RCP and SSP aerosol trajectories and wonder how these differences will play in showing robustness of AR5 assessment in this respect [Zbigniew Klimont, Austria]	Taken into account. We assess the available literature and the information from chapter 6 on the short-lived climate forcing in the SSP scenarios compared to the RCPs and its role for temperature projections in SOD.
37678	29	21			"GMST" should be "GSAT" here. GSAT/GMST differences pale into complete insigificance when compared with differences between current projected near-term warming rates from CMIP6 and the rate deduced from observations made over the past forty years. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Corrected to GSAT.
11532	29	27	29	27	The idea that the response patterns are very similar even when the forcing patterns are quite different is a very old result. Refernece Manabe, Syukuro, and Richard T Wetherald, 1980: On the distribution of climate change resulting from an increase in CO2 content of the atmosphere. Journal of the Atmospheric Sciences, 37(1), 99-118. See figs. 19 on to end. [Roanld Stouffer, United States of America]	Taken into account. Thank you for highlighting this study, which has been confirmed by more recent results. We account for this in the assessment of the consistency of warming patterns under different scenarios (section 4.6.1.1) in SOD.
42990	29	31	29	51	Figure 4.8: (Same as for Box 4.1 Fig 1) Should use an average of interpolated observational series available back to 1850. Currently, that includes Berkeley Earth and Cowtan-Way, but not HadCRUT4. HadCRUT5 is expected to also be more spatially complete, according to Ch 02 FOD, so all three series could be used for observations average in Ch 04 in that case. [David Clarke, Canada]	Taken into account. Thank you for this comment. In the SOD we take the recommendation from Chapter 2 about the use of multiple and/combined observation datasets to compare the models to.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
30690	29	33	29	33	in figure 4.8 it would be probably better to separate results from scenarios and results from DCPP: when all the models will be availble the figure would likely result too crowded [Annalisa Cherchi, Italy]	Rejected. The aim of Fig 4.8 is to update Fig 11.9 from AR5 which synthesises different sources of near-term information. Hence we prefer to keep the decadal predictions and scenarios together. However, we have improved the clarity of presentation for the SOD by showing the ensemble average predictions for the DCPP models rather than all members.
37680	29	41			See comment 207: HadCRUT4 should not be referred to as "observations". [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. "the observations (HadCRUT4" changed to "values derived from observations (HadCRUT4"
35908	29	56	30	2	Does the question of the probability of 'experiencing accelerated GSAT increase despite falling emissions' warrant particular attention? I would recommend folding this into a discussion of when we expect differences between scenario to emerge from internal variability, as a function of spatio-temporal averaging and variable. Undue focus on the probability of accelerated warming following a reduction in emissions may send a confusing message to policymakers. The report could equally focus on the probability of strong decreases in warming following reduced emissions, accelerated warming following increases in emissions etc. [Nathan Gillett, Canada]	Taken into account. This was a placeholder for the FOD. The discussion is developed further in the SOD and subsequent drafts capturing the broader question of signal-to-noise and forced temperature signals emerging against a background of internal variability on different timescales.
36550	30	24	30	25	“It has been found that, on average, the spatial patterns of near-surface warming are largely similar for different external drivers.” This is very remarkable. Can you perhaps highlight at least some locations in which this does not take place? [Carlos Mechoso, United States of America]	Noted. As noted in reviewer comment 11532 this idea goes back to work in the 1980s by Manabe and colleagues. The question of whether pattern scaling can be applied to the SSP scenarios (and hence whether path independence for temperature is an appropriate framing) is discussed in section 4.6.1.1
44504	30	52	30	54	It would be more valuable to indicate (specify) the regions located at the boarder of between wet and dry regions. [Shaukat Ali, Pakistan]	Taken into account. Examples of such regions are included in the SOD.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
36552	30				Section 4.4.1.3, This is a challenging section to write because the internal variability is so large and yet public interest is quite high. The text is not very informative in general. For precipitation in particular I would consider plotting results for all seasons. In South America, for example, anthropogenic influences seem to be detectable in the southern spring but not in the summer (monsoon) season when internal variability is very strong. See for example, "Barkhordarian, A., H. von Storch, A. Behrangi, P. C. Loikith, C. R. Mechoso, and J. Dexter, 2018: Simultaneous Regional Detection of Land-Use Change sand Elevated GHG Levels: The Case of Spring Precipitation in Tropical South America. Geophys. Res. Lett., 45." [Carlos Mechoso, United States of America]	Taken into account. We assess the precipitation patterns for summer and winter seasons in the SOD.
35910	31	2	31	4	What is the spatial and temporal averaging considered here? The fractional contribution of internal variability will be very different for a twenty year mean global average than for a daily mean grid cell average. [Nathan Gillett, Canada]	Taken into account. Thank you for pointing this out. The text is more specific in the SOD and refers to the specific temporal averaging to which these statements on relative uncertainty apply.
44506	31	2	31	4	High uncertanity in initial decades as compared to end century need to be varified as other studies (Ali et al., 2015) show increase in uncertanity with increase in time. [Shaukat Ali, Pakistan]	Taken into account. More studies are included in the assessment of precipitation uncertainty over the 21st century in the SOD
30692	31	21	31	55	this should be assessed in ch 8 [Annalisa Cherchi, Italy]	Noted. The mechanisms behind the global, regional and seasonal patterns of projected precipitation changes are assessed in much more detail in chapter 8 (section 8.4). Here in chapter 4 we consider only the large-scale precipitation patterns and a brief assessment of their uncertainty, as this is a quantity of interest for future global climate. For the SOD, we have worked with chapter 8 to improve consistency in the assessment and to reduce overlap where appropriate. This will require further work for the FGD.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
9050	31	24	31	26	To clarify, the dry-get-drier argument doesn't hold because the drying is happening outside the subtropics rather than in the subtropics? Wouldn't this mean instead that the wet-get-wetter argument doesn't hold (because a wet region is drying?)? [Anna Merrifield, Switzerland]	Noted. Recent studies have shown that dry-get-drier argument does not hold because reduced precipitation appears along the outer flanks of the subtropics, rather than in the subtropics. In the tropics, a weakening of the circulation can lead to a wet-gets-drier and dry-gets-wetter pattern (Chadwick et al., 2013). This has been clarified.
47222	31	28	31	30	You may also quote another recent relevant study: Chadwick R., H. Douville, C.B. Skinner (2017) Timeslice experiments for understanding regional climate projections: Applications to the tropical hydrological cycle and European winter circulation. Clim. Dyn., 49, 3011-3029, doi:10.1007/s00382-016-3488-6. [Hervé Douville, France]	Accepted Thank you for pointing out this paper. We account for this in the assessment of drivers of precipitation response to CO2,
37682	31	34	31	35	Precipitation depends on more than moisture flux and circulation. Temperature and the availability of condensation nuclei for example. Moreover, it is not clear what is meant by "the moisture flux" as a quantity separate from circulation. Moisture is a quantity carried by the circulation, and the moisture flux as I would interpret the term is a product of wind and humidity. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Thank you. The sentence is removed.
35912	31	34	31	36	The tropospheric energy budget exerts a strong control on mean precipitation - see e.g. Allen and Ingram (2002, https://www.nature.com/articles/nature01092). [Nathan Gillett, Canada]	Accepted. It is included in SOD
39516	31	34	31	38	These references have been already considered in the AR5. Isn't there any new literature updating this assessment? [Carolina Vera, Argentina]	Taken into account. Relevant references are included in SOD
43900	31	35	31	36	The sensitivity of global precipitation change…(to)…. is smaller (2% °C-1) as compared to the sensitivity of water vapour concentration change (7% °C-1). There is need to qualify sentivity to something [Michael Mugarura, Germany]	Taken into account. The sensitivity of global precipitation change to warming is smaller (2% °C-1) as compared to the sensitivity of water vapour concentration change (7% °C-1)
9052	31	38			A good reference here might be: Pfahl, S & A. O'Gorman, P & Fischer, Erich. (2017). Understanding the regional pattern of projected future changes in extreme precipitation. Nature Climate Change. 7. 10.1038/nclimate3287. [Anna Merrifield, Switzerland]	Not applicable. Thank you for pointing out this paper, however, changes to extreme precipitation are assessed in chapter 11.
35914	31	42	31	45	This text is unclear - clarify. [Nathan Gillett, Canada]	Taken into account. Text is modified.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
28584	31	44			The paper recently published in Climate Dynamics, Watterson (2019) 52:2451-2466, addresses the relationship between SST pattern uncertainty and other quantities. [Ian Watterson, Australia]	Noted. Thank you for pointing out this reference. The assessment of pattern scaling for different quantities is assessed in section 4.6
35916	31	49			Clarify that this is for a projected decrease in aerosols. [Nathan Gillett, Canada]	Taken into account. This is clarified in the SOD.
35918	31	53	31	54	This statement says that there is low to medium confidence in the impacts of aerosols on projected changes in precipitation 'because of the large uncertainty in the aerosol forcing'. Do the authors mean that there is are large differences in how aerosols will evolve in the different SSPs? Or large uncertainty in the magnitude of the aerosol forcing? Clarify. [Nathan Gillett, Canada]	Taken into account. There is larger uncertainty in the magnitude of aerosol forcing. The point is clarified in SOD.
9992	32	2	32	2	Authors should definitely refer to the recent HIMAP assessment report regarding thde South Asian Monsoon -Krishnan R. et al. (2019) Unravelling Climate Change in the Hindu Kush Himalaya: Rapid Warming in the Mountains and Increasing Extremes. In: Wester P., Mishra A., Mukherji A., Shrestha A. (eds) The Hindu Kush Himalaya Assessment. Springer, Cham - see https://link.springer.com/chapter/10.1007/978-3-319-92288-1_3 [Valerio Lucarini, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. Based on the policy of IPCC, only peer-reviewed papers should be cited. You are appreciated to provide some published paper which can be cited.
30694	32	2	32	2	in this section 4.4.1.4 I would use specific metrics for global monsoon: i.e. precipitation in global monsoon area, global monsoon area. Also I would add info on changes in the area and circulation in fig 4.11 [Annalisa Cherchi, Italy]	Accepted. We added the changes of global monsoon area in the historical simulation and four SSPs projections in figure 4.11.
39518	32	2	32	40	The following review paper could be useful for the assessment of global monsoon changes: Seth et al. 2019: Monsoon Responses to Climate Changes—Connecting Past, Present and Future. Current Climate Change Reports https://doi.org/10.1007/s40641-019-00125-y [Carolina Vera, Argentina]	Accepted. The following new references are now cited in section 4.4.1.4. Reference: Seth et al. 2019: Monsoon Responses to Climate Changes—Connecting Past, Present and Future. Current Climate Change Reports https://doi.org/10.1007/s40641-019-00125-y
46720	32	2	33	10	assessments on monsoon projection need coordination with Section 11.4.5 to avoid duplication [WGI TSU, France]	Noted. Based on coordination in LAM3, Chapter 4 assess global indices including global monsoon, while other chapters such as chapter 11 assess regional monsoon.
40522	32	5			Pedantic, but I would say "monsoons" (rather than singular monsoon) for Africa and American, since they cover different regions and different times of year. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Noted.

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29988	32	10	32	10	But you just said on the previous page that over land, the dry regions don't necessarily get drier! [Theodore Shepherd, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The precipitation changes roughly follow the concept of "wet-get-wetter" and "dry-get-drier" on the global scale. On the regional scale, the dry-get-drier argument might not hold indeed. But in this section, we focused on the global monsoon system. According to the monsoon-desert coupling mechanism (Rodwell and Hoskins 2001; Wang et al. 2012), the increased precipitation over monsoon region results in a monsoon heating-induced Rossby waves, which will cause the adjacent arid areas to become drier, namely the "dry-get-drier". The statement is revised as "While the basic pattern of global monsoon regions tending to get wetter and the adjacent dry regions tending to get drier is apparent".
30086	32	10	32	12	What is your justification for asserting that the spread in the projected response is due to internal variability? Surely some (unknown) component of it is due to systematic differences between models in the forced response? [Theodore Shepherd, United Kingdom (of Great Britain and Northern Ireland)]	Noted. According to Zhou et al. (2019 GRL, submitted), for mean precipitation over global land monsoon regions, the uncertainty of projected changes in the near term climate is mainly contributed by internal variability, while model uncertainty dominates the projected changes in middle and long term climate. In this part, we focus on the near term climate.
40524	32	17			Insert "the" before GM [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted.
39524	32	20	32	22	GM observed changes are assessed in chapter 2 and chapter 3 as well. Check consistency accross chapters. [Carolina Vera, Argentina]	Taken into account. We checked the FOD, the observed GM changes are consistent with Chapters 2 and 3.
40526	32	22	32	25	You might make a link to Chapter 10 here, since they discuss the "interplay" between decadal modes of variability and climate projections. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The statement is revised as "An assessment of the interplay between decadal modes of variability and climate projections is provided in section 10.4.3.1 of Chapter 10."

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40528	32	25			Change: global monsoon projection --> projection of the global monsoon [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted.
30696	32	29	32	40	this should be assessed in ch 8 and ch 10 [Annalisa Cherchi, Italy]	Taken into account.
40530	32	32			For the American monsoons please check the latest review of Salvatore Pascale: https://doi.org/10.1007/s40641-019-00135-w [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The following new references are now cited in section 4.4.1.4. Reference: Pascale, S., L. M. V. Carvalho, D. K. Adams, C. L. Castro, and I. F. A. Cavalcanti, 2019: Current and Future Variations of the Monsoons of the Americas in a Warming Climate. Current Climate Change Reports, doi: 10.1007/s40641-019-00135-w.
40532	32	48			Is 1995-2014 the standard baseline being used in AR6? [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Yes.
30698	33	2	33	8	I would combine with fig 4.11 as separate panel [Annalisa Cherchi, Italy]	Accepted.
45782	33	13	33	13	Remove biosphere from title as not assessed [Katja Mintenbeck, Germany]	Accepted. Land carbon uptake is also now assessed and so "biosphere" remains in the title.
15364	33	13	34	13	There is nothing about biosphere in "4.2.2. Cryosphere, Ocean, and Biosphere". Please add or delete "biosphere" from the subchapter's name [Oksana Lipka, Russian Federation]	Accepted. Land carbon uptake is also now assessed and so "biosphere" remains in the title.
35296	33	13	40	31	As with 4.3.2, the section title of "Cryosphere, Ocean, and Biosphere" is inaccurate if you don't discuss the terrestrial biosphere at all. Suggest that you either rename this section to delete the word biosphere or rescope it to also include the terrestrial biosphere. [Charles Koven, United States of America]	Accepted. Land carbon uptake is also now assessed and so "biosphere" remains in the title.
49520	33	17	33	43	A more general comment about the 4.4 vs 4.3. Current text has a number of repetitions and it would be good to review how much of repetition is needed. For example, section 4.4.2.1 -page 4-33, line 31-35 and page 4-24, line 32-43 have similar references, similar statement. All the points were made in 4.3 and there seem to be no additiaonl depth in 4.4 with this respect. Maybe 4.3 shoudl include less discussion of the period up to 2040? At the same time, it is interesting that here in 4.4.2.1 there is no mention of potential model biases or potential underestimation of sea-ice decline as in 4.3.2.1 (page 4-24, line 43-44). [Zbigniew Klimont, Austria]	Taken into account. As much as possible the repetitions have been reduced and the subsections reconciled.
35920	33	22	33	23	Poor phrasing - I suggest 'September Arctic sea-ice coverage is unlikely to decrease to below 1 million km^2 before 2040 under RCP2.6'. [Nathan Gillett, Canada]	Accepted.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
35922	33	37	33	43	The authors present the range of sea ice cover trends for all 10-year and 20-year periods ending in the period 2021-2040. While this may be of some scientific interest, I think this kind of analysis is less useful to the IPCC target audience. Given the large multi-model ensemble of CMIP6 simulations that will be available, I recommend that it would be better just to give the range of trends for the period 2021-2030 and 2021-2040. This will be more relevant to stakeholders who are interested in the range of possibilities of future Arctic sea ice trends, depending on projected future forcing changes. If there is skill in the initialised predictions, the predicted range from initialised simulations could be compared with the range in unitialised simulations for this period. [Nathan Gillett, Canada]	Rejected. We feel it is important to assess the sensitivity to trend length of all trends ending in the near-term. Initialised forecasts for Arctic sea ice area will not be utilized here.
35924	34	2	34	10	If retained I recommend that instead of discussing all possible 10-yr trends ending in the near-term, just show and assessed trends for the 2021-2030 period. Also, there is now litereature on decadal prediction of biogeochemical properties including ocean carbon uptake, which is not currently assessed in this section on near-term changes in ocean carbon uptake. If this section is retained, this should be assessed, and initialised predictions of carbon uptake also shown and assessed here. See for example Seferien et al. (2018; https://doi.org/10.1002/2017GL076092). [Nathan Gillett, Canada]	Rejected. We feel it is important to assess the sensitivity to trend length of all trends ending in the near-term. Initialized predictions of biogeochemistry is not in our mandate.
35926	34	7	34	8	Examine the reason why the trends in ocean carbon uptake differ so much under different scenarios. Is it the case that the differences are mainly explained just by the steeper increase in atmospheric CO2 in SSP5-8.5 versus SSP1-2.6? [Nathan Gillett, Canada]	Rejected. Assessing the physical mechanism responsible is the mandate of Chapter 9.
36556	34	20	34	25	Are you referring here to Rossby wave breaking? Woolings et al., JAS 2017, could be mentioned here. [Carlos Mechoso, United States of America]	Noted.
35928	34	24	34	43	There is no need to define or describe the NAO/NAM here - the chapter should refer to Chapter 2 for this. [Nathan Gillett, Canada]	Noted. The SOD includes a technical annex on modes of Variability with definitions. The text here is modified/summarised accordingly.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
45622	34	31	35	18	About the tropical high-tropospheric warming vs. Arctic amplification « tug of war » for driving future changes in Northern extratropical atmospheric dynamics, the authors could consider referencing Zappa, G. and T.G. Shepherd, 2017: Storylines of Atmospheric Circulation Change for European Regional Climate Impact Assessment. J. Climate, 30, 6561–6577,doi : 10.1175/JCLI-D-16-0807.1 ; Cattiaux, J., Y. Peings, D. Saint-Martin, N. Trou-Kechout and S.J. Vavrus (2016), Sinuosity of mid-latitude atmospheric flow in a warming world, Geophysical Research Letters, 43, 8259—8268. doi :10.1002/2016GL070309 ; Peings, Y., J. Cattiaux, S. Vavrus and G. Magnusdottir (2018), Projected squeezing of the wintertime North-Atlantic jet, Environmental Research Letters, 13(7), 074016. doi :10.1088/1748-9326/aacc79 . In particular the third paper introduces the idea that the tug-of-war could result in a squeezing of the westerly flow / jet stream. [Julien Cattiaux, France]	Taken into account. We consider these suggestions.
35938	34	31			Somewhat repetitive of 4.3.3.1. [Nathan Gillett, Canada]	Noted. For the SOD it is planned a technical Annex on the Modes of Variability. As a consequence the definitions in sections and sub-sections of Chapter 4 (and other Chapters) have been changed/summarised accordingly.
38974	34	35	34	39	Please define the polarity of NAM index. [Masahide Kimoto, Japan]	Noted. Definitions of the modes of variability and their indices is given in the Annex on Modes of Variability in SOD.
9054	34	35			This introduction to the NAM has been repeated several times in the chapter. Would it be sufficient to introduce the mode of variability just once? [Anna Merrifield, Switzerland]	Noted. A technical Annex on Modes of Variability is developed for the SOD. Definitions in the sections and sub-sections of the Chapters are changed/summarised accordingly.
35930	34	42	34	43	The NAM and the AO are the same thing. I recommend following AR5 and Chapter 2 and using only the term 'NAM'. The NAO and the NAM are related but not the same (the NAO is defined just over the N Atlantic, whereas the NAM is hemispheric). It doesn't make sense to write that 'we use the term NAM to refer also to the.. NAO'. [Nathan Gillett, Canada]	Taken into account.. Consistent definitions of the modes of variability are given in the Technical Annex on Modes of Variability in SOD. Your comments on the substantial differences between NAO and NAM/AO are taken into account.
35932	34	43	34	48	These lines discuss model evaluation and attribution for the NAM, with no reference to Chapter 3. These are assessed in Chapter 3 (Section 3.7.1), and that assessment could be summarised here if needed. [Nathan Gillett, Canada]	Noted. We have referred to Chapter 3 and summarise their assessment of the NAM
38976	34	45	34	46	Need a reference. [Masahide Kimoto, Japan]	Taken into account. We include the reference to Chapter 3

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35934	34	54	35	18	To the extent that this material is focussed on evaluation and understanding of past changes in the NAM, this should rely on the assessment of Chapter 3, which isn't currently referenced here. [Nathan Gillett, Canada]	Taken into account. We now refer to Chapter 3 and summarise their assessment
31478	34	54			The discussion of the impact of Arctic warming on the NAM and NH jet is also discussed in the Arctic box in Chapter 10. There is substanial overlap. The main message is, however, consistent. [Rein Haarsma, Netherlands]	Noted. We have shortened the text and refer to the Arctic-midlatitude Box in Chapter 10
13924	35	1	35	18	I agree with the focus taken here on the tug-of-war between upper and lower level temperature gradients. You could also note a broader range of ingredients entering the literature recently, for example the role of cloud and radiative changes, as discussed later on p52 of this chapter. [Tim Woollings, United Kingdom (of Great Britain and Northern Ireland)]	Noted. We have added there is new understanding of cloud radiative effects and include relevant references
51912	35	3	35	36	Should review this text for consistency with Box 10.1 and maybe cite that here one or more times? Perhaps this could be reduced, integrated into the box, and instead refer the reader to that box? [Peter Thorne, Ireland]	Taken into account. We have shortened the text and refer to the Arctic-midlatitude Box in Chapter 10
13922	35	15	35	15	While I am flattered by this reference to Harvey et al, I'm not sure it's the most useful here. Could even just refer to the more detailed discussion coming on p44. [Tim Woollings, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. Text has been removed in SOD to reduce overlap with Box 10.1
13926	35	20	35	23	For clarification, the increase in wave amplitude noted in Hoskins and Woollings was for one specific type of wave only, stationary waves forced by midlatitude heating, and does not necessarily apply to all waves. The decrease in wavelength should apply more generally, however. [Tim Woollings, United Kingdom (of Great Britain and Northern Ireland)]	No longer relevant. This text has been removed and incorporated into Box 10.1.
35936	35	20	35	30	This section should cross-reference Cross-chapter Box 10.1: Impact of the Arctic on midlatitude climate, and may be shortened/condensed to avoid duplication with that box. [Nathan Gillett, Canada]	Taken into account. We have shortened the text and refer to the Arctic-midlatitude Box in Chapter 10
46252	35	38	35	48	The concept of a Near-term projection in this section should be explained further. What is the convept of the near term? [sadegh zeyaeyan, Iran]	Noted. We define near-term for the purposes of the chapter as the period 2021-2040.
8886	35	38	35	48	The concept of a Near-term projection in this section should be explained further. What is the convept of the near term? [Mohammad Javad Zareian, Iran]	Noted. We define near-term for the purposes of the chapter as the period 2021-2040.
57528	35	38	35	48	The concept of a Near-term projection in this section should be explained further. What is the convept of the near term? [Sahar Tajbakhsh Mosalman, Iran]	Noted. We define near-term for the purposes of the chapter as the period 2021-2040.
30700	35	53	35	53	seasonality is shown in the figure but not assessed in the text [Annalisa Cherchi, Italy]	Taken into account. Text has been updated in the SOD

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39526	36	11	36	16	Notice that observed changes of SAM are assessed in CH2 (section 2.4.5.2) while the human influence on those changes is assessed in CH3 (section 3.7.2). SAM changes are also assessed in 4.3.3.1 . Check consistency and coherency across chapters regarding definitions and associated references. [Carolina Vera, Argentina]	Noted. A technical Annex on Modes of Variability is added in the SOD which improves consistency between chapters. We also now refer to the SAM assessment in Ch2 and ch3.
38978	36	13	36	13	Please define the polarity of SAM index. [Masahide Kimoto, Japan]	Noted. Definitions of the modes of variability and their indices are given in the Annex on Modes of Variability in the SOD
28586	36	15			The wave-3 pattern presented by Raphael does not seem to relate to SAM, rather to zonal asymmetry in the SH overall. The word canonical may need to be defined (or replaced). [Ian Watterson, Australia]	Noted. The text has been clarified in the SOD.
39538	36	20	36	58	The organization of the assessment by time ranges from near to mid and long terms (sections 4.3-4.5) in general works. But in the case of the modes of variability, the resulting assessment seems somewhat repetitive and fragmented. I wonder if the assessment of variability mode changes should not be removed from these three sections, and instead create a single section in which the assessment of each mode encompasses all time terms. This would facilitate a more consistent and coherent assessment of each mode of variability. [Carolina Vera, Argentina]	Noted. We have considered this comment regarding the chapter structure. However, we think the structure provides a logical progression based on time period. We have now ensured the modes of variability sections are better connected in the SOD and reduce overlap between them. This has been helped by the introduction of a Technical Annex on Modes of Variability in the SOD.
29990	36	23	36	39	Ceppi & Shepherd (2019 GRL doi: 10.1029/2019GL082883) show that a substantial component of model uncertainty in the summertime SAM response to GHG increases is associated with the stratospheric polar vortex response. [Theodore Shepherd, United Kingdom (of Great Britain and Northern Ireland)]	Noted.
35940	36	23	36	39	This paragraph should refer to Chapter 3 (Section 3.7.2) for assessment of the contributions of individual forcings to past SAM changes. [Nathan Gillett, Canada]	Taken into account. We refer to Chapter 3 and change the text accordingly.
39528	36	27	36	29	The SAM positive trend weakening is also assessed in 4.3.3.1 in very similar terms and it is even part of the ES. Check consistency between both sections [Carolina Vera, Argentina]	Noted. The text has been updated to reduce overlap in the SOD.
39532	36	29	36	29	What does "current scenarios" mean? [Carolina Vera, Argentina]	Noted. A reference to the WMO (2011) scenarios for ozone depleting substances used in the latest Chemistry Climate Model Initiative simulations that informed the 2018 WMO Ozone Assessment has been added

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39530	36	29	36	32	is there only one reference supporting the information about the period in which ozone would recover? what does it mean that there is a "period" in which "effect of ozone recovery on SH circulation" is the greatest? In this section about near-term changes of SAM, it is important to assess the new knowledge about both ozone depletion and ozone recovery on SH circulation during the next few years-decades. [Carolina Vera, Argentina]	Noted. We add earlier references for the timing of ozone hole recovery (the reference given is for the latest chemistry climate models). The wording is improved and any new literature that is published specifically focusing on SAM trends in the next few decades is be assessed. At the time of FOD production there were not many new papers since AR5 on this topic (see also WMO Ozone Assessment 2018 chapter 5)
37684	36	29			"ozone depletion recovers" is an awkward phrase, as it is not the depletion that recovers. Perhaps either "ozone depletion ceases" or "abundance of ozone recovers" would be better. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The text has been clarified in the SOD.
39534	36	34	36	34	when would be the increasing GHGs the dominant driver? Why only in winter? [Carolina Vera, Argentina]	Noted. The text has been clarified in the SOD.
35942	36	41	36	43	This text is discussing SAM changes by the end of the century, but this section is on near term climate change. There is another whole section on long-term changes in the SAM. Overall there is a lot of repetition between this section and 4.3.3.1 and 4.5.3.1. [Nathan Gillett, Canada]	Noted. The section now focuses more on the near-term SAM changes. The text across the modes of variability sub-sections has been revised to reduce overlap
39536	36	41	36	46	is this paragraph going to be updated with new literature published after AR5? [Carolina Vera, Argentina]	Noted. The text has been updated to include CMIP6 simulations for SSP scenarios.
37686	37	7	37	10	What is written in this paragraph is correct, but is only half the story. It needs to be stated what was used for the ozone dataset by those CMIP6 models that needed one. Was a dataset part of the CMIP6 specification? Did each model go its own way on this? [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Noted. A reference to the CMIP6 ozone dataset has been added in Section 4.3.3 where this text has been moved to.
35944	37	7	37	10	Given that this text describes the CMIP6 ozone forcing it should also cite the paper describing the CMIP6 historical ozone forcing (Checa-Garcia et al., 2018; https://doi.org/10.1002/2017GL076770). Note that a paper describing the future CMIP6 ozone forcing is in preparation and should be ready to cite for the SOD (Hegglin, M. I., D. Kinnison, D. Plummer, et al., Historical and future ozone database (1850-2100) in support of CMIP6, GMD, in preparation.). [Nathan Gillett, Canada]	Noted.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
36558	37	7	37	10	Do you mean “are prescribed”? What is the importance of this strategy in terms of results. This paragraph seems to be pretty isolated from the remainder of the subsection. [Carlos Mechoso, United States of America]	Noted. This text has been moved to section 4.3.3.1 where it fits better into the overall description of the SAM and its drivers including ozone.
30702	37	7	37	10	this is part of the CMIP protocol, as written here seems a recommendation for the future [Annalisa Cherchi, Italy]	Noted. Text has been clarified. Text moved to section 4.3.3.1
27264	37	8	37	8	suggest adding "with sufficient temporal resolution" in this sentence, as Neely et al 2014 demonstrates that daily ozone is required to capture the tropospheric effects, whereas monthly ozone is too coarse [Gabriel Chiodo, Switzerland]	Noted. Text has been amended
54212	37	13	37	13	This section has teleconnections in its title, but there is very little information here on these [Nicola Maher, Germany]	Noted, we intend to extend the discussion of ENSO teleconnections under climate change in the SOD.
30704	37	16	37	21	this part on teleconnections should be in ch 8 [Annalisa Cherchi, Italy]	Taken into account.
54210	37	23	37	45	This paper could be of interest here: C. Karamperidou, F.F. Jin, and J.L. Conroy: "The Importance of ENSO Nonlinearities in Tropical Pacic Response to External Forcing.”, Clim Dyn (2016). doi:10.1007/s00382-016-3475-y [Nicola Maher, Germany]	Noted.
9056	37	23			A good reference here might be: Deser, C., I. R. Simpson, A. S. Phillips and K. A. McKinnon, 2018: How well do we know ENSO's climate impacts over North America, and how do we evaluate models accordingly? J. Climate, 30, 4991-5014, doi: 10.1175/JCLI-D-17-0783.1. [Anna Merrifield, Switzerland]	Noted.
54216	37	38	37	45	This paper is of use here: https://journals.ametsoc.org/doi/full/10.1175/BAMS-D-13-00117.1 [Nicola Maher, Germany]	Noted.
36560	37	38	37	45	OK, I see that you mention the ENSO flavors here. Perhaps you could add a short sentence saying why distinction may be important? [Carlos Mechoso, United States of America]	Taken into account.
30706	37	49	37	49	PDV in this section should be treated as the other modes before: an index (or a spatial pattern) identified and shown [Annalisa Cherchi, Italy]	Taken into account. Refer the Technical Annex on the Modes of Variability in SOD.
35946	37	49	38	6	This description of PDV should be shortened and reference Chapter 2 where PDV is introduced. [Nathan Gillett, Canada]	Taken into account. The description has been shorten with reference to the Technical Annex on the Modes of Variability and Chapter 2.
39556	37	51	38	27	check consistency and coherency of process related explanations with those provided in section 4.5.3.3 [Carolina Vera, Argentina]	Taken into account.
38980	37	53	37	54	Better explain how IPO and PDV are defined. [Masahide Kimoto, Japan]	Taken into account. Refer the Technical Annex on the Modes of Variability in SOD.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
35948	38	16	38	27	This paragraph on mechanisms underlying PDV should reference Chapters 2 and 3 (3.7.6). [Nathan Gillett, Canada]	Taken into account. The description has been shorten with reference to the Technical Annex on the Modes of Variability and Chapter 2.
38982	38	17	38	17	The sentence requires supporting literature. [Masahide Kimoto, Japan]	Noted.
39540	38	17	38	17	An assessment shouldn't say "it seems accepted…". Or an assessment is actually made including more publications or the sentence is rewritten just saying that there evicendes showing that IPO represents the low-frequency component of ENSO [Carolina Vera, Argentina]	Taken into account.
39542	38	21	38	21	what does "better-defined variability" mean? [Carolina Vera, Argentina]	Noted. It's meaning is not clear. Thus, the text has been revised considerably.
39544	38	32	38	34	"current negative phase" is vague and unclear. Specify to which year periods the sentence refers to. [Carolina Vera, Argentina]	Accepted. We specifies the period
30708	38	42	38	42	same as for PDV: index and/or spatial pattern should be shown [Annalisa Cherchi, Italy]	Taken into account. The Technical Annex on the Modes of Variability in SOD shows definition and spatial pattern.
39776	38	44	38	45	IOD activity (trends and variability) also influences the storm-track activity in the Southern Hemisphere as well climate anomalies in South America and the Antarctica regions. Some useful references are (they might be more): Ashok et al. 2007 (https://doi.org/10.1175/JCLI4155.1), Reboita, M.S., da Rocha, R.P., Ambrizzi, T. et al. Clim Dyn (2015) 45: 1929. https://doi.org/10.1007/s00382-014-2447-3, Nuncio and Yan 2015 (https://journals.ametsoc.org/doi/abs/10.1175/JCLI-D-14-00390.1), IOBW influence on South America Taschetto and Ambrizzi 2012 (DOI 10.1007/s00382-011-1165-3) [Carolina Vera, Argentina]	Noted. The impact of IOD is discussed in Chapter 8 Section 8.4.2.
30710	39	4	39	40	so what will be assessed for the AMV in the SOD? [Annalisa Cherchi, Italy]	Accepted. The SOD includes AMV assessment.
35950	39	6	39	12	Cross reference Chapters 2 and 3 (3.7.7) here. [Nathan Gillett, Canada]	Noted. The SOD refers to Chapters 2 and 3
57464	39	6	39	40	Perhaps the place to discuss the mechanisms of AMV in more detail (Chapter 9 doesn't say much about this). In particular, natural AMV has contributions from ocean mixed layer integrating random atmospheric fluxes (Clement etc), but recent work shows pretty convincingly that AMOC driven by NAO variability plays a key role (e.g., O'Reilly et al. 2016, Wills et al. 2019 doi: 10.1175/JCLI-D-18-0269.1). Historical AMV has strong contributions from radiative forcing which might swamp this though (some very recent papers). [Kyle Armour, United States of America]	Taken into account. The modes of variability is better coordinated across Chapters in SOD with the Technical Annex on the Modes of Variability. Technical Annex includes detail mechanisms on AMV.

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7964	39	9	39	12	Will et al, 2019 doi: 10.1175/JCLI-D-18-0269.1 is a nice paper investigating this in CMIP5 models by doing a frequency dependent analysis. They find that the definition of AMV is very important for determining whether the ocean is forcing the atmosphere or the atmosphere forcing the ocean [Laura Jackson, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The modes of variability is better coordinated across Chapters in SOD with the Technical Annex on the Modes of Variability. Technical Annex includes detail mechanisms on AMV.
53026	39	9	39	12	Another relevant reference on AMV -- Wills et al. 2019, doi: https://doi.org/10.1175/JCLI-D-18-0269.1 [Anson Cheung, United States of America]	Taken into account. The modes of variability is better coordinated across Chapters in SOD with the Technical Annex on the Modes of Variability. Technical Annex includes detail mechanisms on AMV.
35952	39	22	39	23	Could the reduced skill for predictions over land compared to predictions of AMV not just be due to higher noise in regional land variables, compared to the AMV index? [Nathan Gillett, Canada]	Noted.
38984	39	36	39	36	Is it meant "argued against"? [Masahide Kimoto, Japan]	Rejected. "Argue about" does implicitly include argue against".
36562	39	46	39	47	The Atlantic Equatorial Mode is a.k.a. the “Atlantic Niño” and the nomenclature has stuck enough to be mentioned here. [Carlos Mechoso, United States of America]	Taken into account. The modes of variability is better coordinated across Chapters in SOD with the Technical Annex on the Modes of Variability.
36564	39	52	39	56	It may be appropriate to mention here that the models also have difficulties in simulations the mean climate of the tropical Atlantic. A possible reference here is: Mohino, E., B. Rodriguez-Fonseca, C. R. Mechoso, T. Losada, and I. Polo, 2019: Relationships Among Intermodel Spread and Biases in Tropical Atlantic Sea Surface Temperatures. J. Climate. https://doi.org/10.1175/JCLI-D-18-0846.1 [Carlos Mechoso, United States of America]	Noted.
35954	39	52	40	5	Reference Section 3.7.4 for model evaluation of Indian Ocean basin modes - don't need to repeat this here. [Nathan Gillett, Canada]	Taken into account.
35956	40	24	40	25	This is written as though a finding that there is 'a clear lack of studies' is an assessment conclusion. Focus on the assessment of skill of tropical Atlantic modes, and mention that lack of studies limits confidence in the assessmen if needed. [Nathan Gillett, Canada]	Taken into account.
53462	40	34	40	34	A paper by Stjern et al. In JGR 2017 (10.1002/2017JD027326) on temp responses to BC seems relevant for this section. [Jan Fuglestvedt, Norway]	Noted. Citation added
53396	40	34	42	48	Section 4.4.4 needs close coordination with chapter 6. Probably also ch7. [Jan Fuglestvedt, Norway]	Taken into account. It is better coordinated across Chapters in SOD.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
35960	40	36	41	23	While this discussion on the role of SLCFs is in the section on near-term cliamte change, it is mainly focussed on long-term effects - for example, discussion in the context of Paris targets for climate stablisation (pg 40, ln 37), reference to projections for the end fo the century (pg 40, ln 43), discussion of the effects at climate stablilisation (pg 41, ln 3-4) etc. I recommend either that this is moved out of the section on near-term climate projections, or better, put in a merged section on cliamte change over the 21st century. [Nathan Gillett, Canada]	Taken into account. The text has been further developed.
37688	40	39			"Phase out" is the wrong term when applied to naturally occuring SLCFs. Some play important roles in the climate system - dust aerosols from the Sahara supplying minerals to the Amazon, for example. Phasing out ozone would not be too good an idea. Perhaps "reductions" could be used in place of "phase outs". [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. It is specified that this refers to human-sourced SLCFs to distinguish from natural +K8+L2
53394	40	42	40	43	This statment needs to build on more than one reference [Jan Fuglestvedt, Norway]	Noted. This is further developed in the SOD and additional sources included in the assessment
43902	40	43	40	46	Tried to look into literature to quantify "several times lower" but did not find any real figures, I suggest the use of " much lower" in this statement instead of several times lower. [Michael Mugarura, Germany]	Noted. Several times lower replaced with much lower and reference to Smith and Mizrahi, 2013 added here
35958	40	44			Do the authors mean 'increasing methane and black carbon emissions'? [Nathan Gillett, Canada]	Noted. This is now specified as increasing methane and black carbon emissions
49522	40	46	40	46	I think the refernce to UNEP, 2011 can be replaced (or at least amended) by the Science paper Shindell et al. (2012) DOI: 10.1126/science.1210026 [Zbigniew Klimont, Austria]	Noted. Citation added
49524	40	48	41	1	I think the current text is biased towards low carbon scenarios, i.e., the arguments given hold to some extent for such futures but not necessarily for paths we are on now or even moderate climate mitigation paths. I feel it shall be rewritten to give a more balanced account of SLCF potential in medium to high CO2 paths, not just SSP1-1.9 or 2.6 since both aerosols and CH4 can be reduced even more efficiently and quicker (than in climate mitigation scenarios) for other purposes including air quality and development goals; largely included in the SDG agenda. Say, BC in transport can be (and has been in several places) reduced by 99% wihitn 10-15 years without need to wait for full electrification of vehicle fleet that will take much longer..and saving lives in the meantime. [Zbigniew Klimont, Austria]	Noted. This section reflected the available literature at the time the FOD was prepared. New literature submitted before the 31 December 2019 cut-off which considers SLCF effects in a pathway with higher CO2 emissions was added in the SOD. This will be further added to in the FGD if there are other relevant studies published before 31 January 2021.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
37690	40	48			In Chapters 2 and 5, methane is referred to as long-lived, but here (and in Chapter 6) it is classified as a short-lived climate forcer. A general comment (no. 2) on the entire report has been made concerning the need to come to an agreement on terminology. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Noted. It is better coordinated across Chapters in SOD.
49526	40	52	40	54	As above, the statement is about the 2 degree case and even then not all model realization of such scenario provide access to clean cooking energy, i.e, move away from solid biofuels for cooking to LPG for example, and these biofuel cooking emissions represent majority of anthropogenic BC so the statement is true only for specific model realization and not in general. [Zbigniew Klimont, Austria]	Rejected. We do not assess here the specific IAM scenarios and their underlying assumptions, only studies that have examined the relationship of scenarios to future climate.
36566	40				section 4.4.4. It may be appropriate to mention the possible links between volcanic eruptions and the Antarctic Ozone Hole (Solomon et al., Science 2016). This would be related to SAM's variability also. [Carlos Mechoso, United States of America]	Rejected. This section is concerned with the primary effects of volcanic eruptions on surface climate. The influence of the SAM on surface climate is already discussed in section 4.4.3.1 including the influence of the ozone hole on SAM.
41088	41	3	41	3	Change "are" to "is" [Alan Robock, United States of America]	Accepted.
49528	41	3	41	5	In fact the comment is probably to the whole paragraph as I wonder if the discussion of sensitiviety studies like (Samset 2018) shoud be separated from analysis of more realistic trajectories where SO2, BC, OC removal will be assocaited with rather significant reduction of CO, NMVOC, NOX and even CH4 afecting zone production, OH. [Zbigniew Klimont, Austria]	Noted. We have tried to be more explicit about which studies use idealised zero SLCF emissions cases and which use more realistic scenarios
53398	41	3	41	41	I suggest you insert "all" before "anthropogenic". This emphasis is needed since the experiments were very idealized. [Jan Fuglestvedt, Norway]	Accepted.
35962	41	6	41	8	Make clear that this more rapid reduction in Arctic sea ice area is relative to a baseline with no reductions in SLCFs. In fact, all the scenarios considered in this report include reductions in SLCFs. [Nathan Gillett, Canada]	Accepted.
41090	41	7	41	7	Change "are" to "is" [Alan Robock, United States of America]	Accepted.
7186	41	9	41	10	Figure 1c of Takahashi et al (2018) adds one more estimation for the response of global mean precipitation to the removal of anthropogenic aerosols, that is, an increase of roughly 2%. Takahashi, H. G., S. Watanabe, M. Nakata, and T. Takemura (2018), Response of the atmospheric hydrological cycle over the tropical Asian monsoon regions to anthropogenic aerosols and its seasonality, Progress in Earth and Planetary Science, 5:44, doi:10.1186/s40645-018-0197-2. [Shingo Watanabe, Japan]	Taken into account. Citation added

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
35964	41	10	41	12	The meaning of this text is vague. Given that it mentions Ozone Depleting Substances, it sounds as though the focus is on strastospheric ozone change - but ODSs are not SLCFs, and stratospheric ozone is not generally considered an SLCF. Ozone radiative forcing is mainly driven by changes in tropospheric ozone, for which ozone precursor emissions (which are SLCFs) are important - but this isn't mentioned in the text at present. I recommend adding explicit reference to tropospheric ozone changes and the influence of changes in ozone precursor emissions. Also, the radiative forcing of the ODSs is generally larger than that of the ozone changes they cause, so if discussion of ODS changes is included here, this should also be discussed. [Nathan Gillett, Canada]	Taken into account. The text is improved in the SOD to focus on tropospheric ozone and the factors that influence its forcing (including future changes in stratosphere-troposphere exchange which the Banerjee et al., 2018 paper quantifies)
8210	41	14	41	23	Another example of uncertainty of climate impact from SLCFs is in the response of lightning NOx to climate change. I recently showed that if lightning NOx emissions reduced in future (which is possible) then the ozone radiative forcing between 2000 and 2100 may be a third lower https://www.nature.com/articles/s41558-018-0072-6 I also show that an uncertainty on the order of +-5% is introduced into the equivalent methane radiative forcing. [Declan Finney, United Kingdom (of Great Britain and Northern Ireland)]	Noted. An assessment of the role of lightning NOx for projections of ozone ERF is added in the SOD
49530	41	16	41	16	What is the basis for the "(ii)" statement? I think that at a source or technology level the co-emission is not more uncertain than the absolute emissions of any single compound. [Zbigniew Klimont, Austria]	Accepted. Removed from text
53400	41	19	41	19	The findings of Etminan et al were later supported by Collins et al. [Jan Fuglestvedt, Norway]	Accepted. Cited
41092	41	23	41	23	I don't understand what "estimated additional 0.5 W m-2 to the Paris targets" means. I thought the Paris targets were in terms of temperature and not radiative forcing. And if you are going to give W m-2, is that RF or ERF or what? [Alan Robock, United States of America]	Taken into account. The wording was poor. The intended statement is that if the recent observed growth in methane emissions continues until 2100, it would add 0.5 Wm-2 radiative forcing compared to the RCP2.6 scenario, with the latter being consistent with the Paris Agreement target. The wording is changed in the SOD to be clearer

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
53402	41	23	41	23	Contributing 0.5 Wm-2 to the pris target sounds strange. Please clarify. [Jan Fuglestvedt, Norway]	Taken into account. The wording was poor. The intended statement is that if the recent observed growth in methane emissions continues until 2100, it would add 0.5 Wm-2 radiative forcing compared to the RCP2.6 scenario, with the latter being consistent with the Paris Agreement target. The wording is changed in the SOD to be clearer
53028	41	31	42	30	Stevenson et al. 2016 also used CESM-LME to quantify ENSO and its teleconnection change after volcanic explosion: https://doi.org/10.1175/JCLI-D-15-0239.1 [Anson Cheung, United States of America]	Accepted.
41094	41	35	41	35	The temperature and time ranges quoted here are very large and not clearly explained. It has to be pointed out that the largest cooling would be expected in the year after the eruption, and by 5 years, there would be a very small signal. Certainly there are more potential citations on what is to be expected. [Alan Robock, United States of America]	Accepted. We clarified that the largest cooling is occurred in the year after the eruption and the cooling lasts for 3-5 years.
41096	41	35	41	35	Why is only the NH addressed? What about all the people in the SH? [Alan Robock, United States of America]	Noted. Most paleoclimate records locate in the Northern Hemisphere (NH), there are large uncertainties in the reconstructions in the Southern Hemisphere regions, so most of the simulation-based work focused on the temperature response in the NH.
37692	41	35			AR5 used a value of 0.5ºC for the global cooling the year following a Pinatubo-like eruption in two places, but 0.1ºC to 0.3ºC in another. The global temperature record appears to show a value smaller than 0.5ºC, but there was an El Nino event in progress at the time, so what is seen in the observations is the net effect of a cool Pinatubo signal and a weaker warm El Nino signal. If the figures in this FOD are accepted, it would seem reasonable to round them to 0.1ºC to 0.4ºC. But 0.1ºC seems very low for the peak response to an eruption similar to Pinatubo. Either way, the differences within AR5 and from AR5 to AR6 need some discussion. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The PMIP3/CMIP5 models simulated a significant NH cooling in response to individual volcanic events (peaks between 0.1°C and 0.5°C depending on model) that lasts 3 to 5 years. But when large volcanic eruptions, such as that of Mt Pinatubo in 1991, inject aerosols into the atmosphere, they can cool the surface by around 0.3°C to 0.4°C. We clarified this in the revised version.

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39780	41	38	41	39	A recently published paper by Zuo et al. (2019) also revealed decreased global precipitation after volcanic eruptions, especially over monsoon regions, they also studied different precipitation responses to volcanic eruptions at different latitudes and the underlying mechanisms. I suggest adding this reference here. References:Zuo Meng, Tianjun Zhou*, Wenmin Man, 2019: Hydroclimate Responses over Global Monsoon Regions Following Volcanic Eruptions at Different Latitudes.Journal of Climate, 32, 4367-4385. DOI: 10.1175/JCLI-D-18-0707.1 [Meng Zuo, China]	Accepted. Cited.
41098	41	40	41	42	This last sentence in the paragraph should rather be the first sentence of the next paragraph. [Alan Robock, United States of America]	Taken into account
53404	41	47	41	47	It sounds obvious to write "that". I suggest to change to "how" [Jan Fuglestvedt, Norway]	Taken into account.
35970	42	1	42	2	Sutton et al. (2007) do not show that the heat capacity plays no role in the warming contrast, just that it is not the primary reason for the contrast. In fact they find that the warming ratio is lower at equilibrium than in transient climate change, suggesting that it does contribute. I suggest replacing 'is not caused' with 'is not primarily caused'. [Nathan Gillett, Canada]	Accepted.
38986	42	18	42	30	Have the simulation (?) results mentioned in this paragraph been verified by observation? [Masahide Kimoto, Japan]	Noted. Khodri et al.(2017) reveals an equatorial Pacific warming after the five largest tropical volcanic eruptions of the historical period based on observations. We cited this paper.
41100	42	18	42	30	You should also reference Khodiy et al. (2017) here, which explains the mechanism by which volcanic eruptions affect ENSO. Khodri, Myriam, Takeshi Izumo, Jérôme Vialard, Serge Janicot, Christophe Cassou, Matthieu Lengaigne, Juliette Mignot, Guillaume Gastineau, Eric Guilyardi, Nicolas Lebas, Alan Robock, and Michael J. McPhaden, 2017: Tropical explosive volcanic eruptions can trigger El Niño by cooling tropical Africa. Nature Communications, 8, 778, doi:10.1038/s41467-017-00755-6. [Alan Robock, United States of America]	Accepted. It is cited.
54206	42	18	42	30	There have been a lot of publications on this topic since the first paper by Maher et al 2015. I am unsure whether you want to go into more detail, however an overview will soon be available of all current literature by: McGregor, S., Khodri, M., Maher, N., Ohba, M., Pausata, F. and Stevenson, S. The effect of strong volcanic eruptions on ENSO. Submitted to AGU Books [Nicola Maher, Germany]	Rejected. Based on the policy of IPCC, only peer-reviewed papers should be cited. You are appreciated to provide a pdf copy of your book, we will see which published paper can be cited.

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54208	42	18	42	30	Worth looking at recent publications by Pausata, Stevenson, Khodri and Predybaylo on this topic all of which were published after Maher et al 2015 [Nicola Maher, Germany]	Accepted. It is cited.
13236	42	18	42	43	The first sentence "" Volcanic forcing can also influence modes of interannual variability such as ENSO"": While the relationship between volcanic and ENSO is important, this sentence is too simple and then it should be enhanced by previous papers. More specifically, this sentence can be followed by a sentence as like ""El Nino-like warming appears after large volcanic eruptions that is seen in both observation (Adams et al. 2003; McGregor et al. 2010) and climate model simulations (Ohba et al. 2013). "" Please consider my proposal to add the following reference. Adams, J., M. Mann, and C. Ammann, 2003: Proxy evidence for an El Nino-like response to volcanic forcing. Nature, 426, 274–278, doi:10.1038/nature02101. McGregor, S., Timmermann, A., and Timm, O. (2010). A unified proxy for ENSO and PDO variability since 1650. Clim. Past 6, 1–17. doi:10.5194/cp-6-1-2010. Ohba, M., H. Shiogama, T. Yokohata, and M. Watanabe, 2013: Impact of strong tropical volcanic eruptions on ENSO simulated in a coupled GCM. Journal of Climate, 26, 5169–5182, doi:10.1175/JCLI-D-12-00471.1. [Masamichi Ohba, Japan]	Accepted .Revised as suggested.
13238	42	18	42	43	Figure 4.18 is difficult for readers I think. It should be replaced by a composite of multi-model mean from the historical runs (during 4-5 years of large volcanic eruptions, ie., Krakatau (1883), Santa Maria (1902), Agung (1963), El Chichon (1982) and Pinatubo (1991)). [Masamichi Ohba, Japan]	Not applicable. The figure was replaced with a new figure (Figure 2) from Zuo et al. (2018), which is based on the volcanic-only forcing experiment from CESM-LME.
11534	42	53	42	53	Mid and long-term need explicitly defined. [Roanld Stouffer, United States of America]	Noted. The mid and long-term period are defined in section 4.1.

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52398	42	53	42	53	While this may seem minor quibbling over terms, suggest that from a policy standpoint it is time to change these terms from their origins which essentially date back to AR-1. "Mid-term global climate change," at the time this will be published is really closer to 2041-2100. Or better, if the model parameters can be shifted in this manner, though it may be too late already in the AR6 process, the truly policy-relevant divisions would be: "Near-term" as 2030-50, and "Mid-term" as 2051-2100. This keeps in mind the inter-generational aspect of climate change that is becoming more relevant to policy decisions: for a child born in 2020, Near-term so defined encompasses their life until age 30; and Mid-term their life from age 30-80. "Long-term" is more appropriate for 2101-2300. "Very long-term" is post-2300, when many impacts especially from ice sheets and commited SLR are still only beginning to emerge. [Pam Pearson, Sweden]	Rejected. Of course many different definitions of near, mid- and long term can be constructed, but the author team has decided to keep the original one. The near term refers to the 20-year period that has just started, and the mid-term to the 20-year period right after. That seems sensible, also given the timescales of the typical policy process.
30712	42	53	42	53	section 4.5 is not the exact mirror of sec 4.4, or the reverse [Annalisa Cherchi, Italy]	Rejected. The structure does aim at being as consistent as possible but not to systematically mirror the exact structure of 4.4
31480	43	6			A consistent way to estimate the forced response in relaltion to natural variability should be used in all chapters. This should be sorted out for the SOD [Rein Haarsma, Netherlands]	Not applicable. The multi-model mean forced response is consistently calculated using the first available member per model and averaging across models.
35328	43	11	43	27	Somewhere in here it should be mentioned that temperature extremes are projected to change and the ratio of daily record highs to daily record lows are projected keep increasing with mean warming such that, over the U.S. for example, for a 3 °C warming in US temperatures, the ratio of record highs to lows is projected to be ~15 ± 8 compared to the present average ratio of just over 2: Meehl, G.A., C. Tebaldi, and D. Adams-Smith, 2016: U.S. daily temperature records past, present and future. Proc. Nat. Acad. Sci., doi: 10.1073/pnas.1606117113. [Gerald Meehl, United States of America]	Not applicable. This is a valuable comment but temperature extremes are assessed in chapter 11.
35966	43	11	46	32	This is a well-written section, with thorough assessment of CMIP5 and CMIP6 results, and supporting assessment of physical mechanisms which goes beyond what was included in past assessments. [Nathan Gillett, Canada]	Noted. Thank you.
30714	43	13	43	14	and where is that shown? [Annalisa Cherchi, Italy]	Noted. This is shown in Figure 4.19. A reference to this figure has been added.
38988	43	19	43	19	"The temperature chamge pattern" in Figure 4.19? [Masahide Kimoto, Japan]	Noted. This is true for any temperature change pattern simulated by a model including the one shown in Fig.4.19.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
46254	43	19	43	27	The method for distinguishing between aerosol concentrations and land use is not well defined in Fig. 4-19 [sadegh zeyaeyan, Iran]	Not applicable. There is no separation of different forcing agents in Figure 4.19. The SSPs only represent a combination of all forcings.
8888	43	19	43	27	The method for distinguishing between aerosol concentrations and land use is not well defined in Fig. 4-19 [Mohammad Javad Zareian, Iran]	Not applicable. There is no separation of different forcing agents in Figure 4.19. The SSPs only represent a combination of all forcings.
57530	43	19	43	27	The method for distinguishing between aerosol concentrations and land use is not well defined in Fig. 4-19 [Sahar Tajbakhsh Mosalman, Iran]	Not applicable. There is no separation of different forcing agents in Figure 4.19. The SSPs only represent a combination of all forcings.
51914	43	42	44	17	Here it would seem worth alluding also to paleo which typically finds a ratio of 1.6. This is discussed a little in chapter 2 but the paleo task team should be able to provide some text and this would strengthen this assessment I assume. [Peter Thorne, Ireland]	Taken into account. The information on land-ocean warming contrast is coordinated with chapter 2
7206	43	43	43	43	The Byrne & Schneider (2018) reference is incorrect. Should be Byrne & O'Gorman (2018): "Trends in continental temperature and humidity directly linked to ocean warming." Proc. Natl. Acad. Sci. 115, 4863–4868. [Michael Byrne, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Reference corrected
7208	43	47	43	47	The Byrne & Schneider (2018) reference is incorrect. Should be Byrne & O'Gorman (2018): "Trends in continental temperature and humidity directly linked to ocean warming." Proc. Natl. Acad. Sci. 115, 4863–4868. [Michael Byrne, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Reference corrected
35968	43	51			In the equation given here, is the temperature of the ocean the global mean, the zonal mean, or the mean of the nearest ocean? The text refers to an amplification factor greater than one for 'dry subtropical continents' among other regions, but how is T_ocean defined for a dry subtropical continent? [Nathan Gillett, Canada]	Not applicable. The change of ocean temperature can be either global mean or zonal mean.
11536	44	1	44	2	This is a funny statement. It is made as if this argument is the common explanation. If kept, it needs a reference. From the 1970's it has been shown that the land warms faster than the adjacent oceans as the climate warms. Every IPCC notes this fact. The cause at that time (1970's) was attributed partly to the differences in heat capacity of the underlying surface. However, the fact that the land surface is usually drier than the ocean also leads to larger warming., more heat is available for sensible heat relative to latent heat. [Roanld Stouffer, United States of America]	Take into account. The sentence is revised accordingly to highlight that land-ocean contrast is not primarily caused by heat capacity differences.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
38990	44	1	44	17	The discussion here sounds new to the reviewer. It should also be included in the Executive Summary. [Masahide Kimoto, Japan]	Noted.
30716	44	3	44	3	"has" to be inserted between "contrast" and "been" [Annalisa Cherchi, Italy]	Taken into account. Sentence rephrased accordingly
47224	44	14	44	17	You may want to add that the projected decrease in near surface relative humidity might be underestimated by most CMIP5 models (low or medium confidence), at least in summer in the northern mid-latitudes (Douville and Plazzota, 2017). This paper could be also or alternatively quoted in section 4.5.1.3. Full reference : Douville H., M. Plazzotta (2017) Midlatitude summer drying: An underestimated threat in CMIP5 models? Geophys. Res. Lett., 44, 9967-9975, doi:10.1002/2017GL075353 [Hervé Douville, France]	Taken into account. The paper is now referred to in section 4.5.13
11538	44	20	44	20	Change "stronger" to "larger". Not sure what is stronger. [Roanld Stouffer, United States of America]	Taken into account. Rephrased accordingly
52400	44	20	44	20	Given the observational record, "virtually certain" is more accurate, pending contrary information in the SROCC. [Pam Pearson, Sweden]	Noted. The statement is qualified as very likely because internal variability is very large over the Arctic and depending on the SSP and period may temporarily mask the forced response
35972	44	20	44	21	Is this a statement about future warming or past warming? This is unclear from the phrasing. [Nathan Gillett, Canada]	Noted. While the statement is true for both past and future warming, it is rephrased to make clear that here it is about future warming
51916	44	20	44	21	Why only very likely? Where is the traceable underlying assessment? Perhaps this should be moved to the end of the assessment or if this is a summary of AR5 findings this needs to be made much clearer than is presently the case. [Peter Thorne, Ireland]	Noted. The statement is qualified as very likely because internal variability is very large over the Arctic and depending on the SSP and period may temporarily mask the forced response
36568	44	21	44	22	I agree that the understanding has increased in the sense of identifying responsible mechanism, but I think that quantifying relative contributions of individual mechanisms to observed Arctic Amplification is the major challenge at the present time. [Carlos Mechoso, United States of America]	Noted.
36554	44	22	44	23	A better appreciation of these figures may be given by mentioning the amount of loss. It is my understanding that the loss is ~3 million km2. [Carlos Mechoso, United States of America]	Not applicable. We assume that this statement refers to the loss of sea ice. This is assessed in the chapter but not in this section
55006	44	29	44	30	Reference to the research sythesis of (Serreze and Barry, 2011) is given while another recent reference with supporting empirical evidence may be considered as “Arctic warming by cloud radiation enhanced by moist air intrusion observed at Ny-Ålesund, Svalbard” by Yamanouchi in Polar Science (article in press) at <https://doi.org/10.1016/j.polar.2018.10.009>. [Kilkis Siir, Turkey]	Noted. This section is only assessing the broader context of the driving mechanisms of future changes, while the specific mechanisms are discussed in chapter 7.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
38992	44	34	44	48	Any mention on cloud feedbacks? [Masahide Kimoto, Japan]	Not applicable. Cloud feedbacks are mentioned in the paragraph below
35974	44	38			I suggest mentioning that the reduced efficiency of the Planck feedback at high latitudes is largely because of the lower temperatures there - a simple consequence of the Stefan-Boltzman law. [Nathan Gillett, Canada]	Take into account. The sentence is revised in coordination with Ch.7 and Cross-Chapter BOX 10.1.
35976	45	7			Polar amplified warming in the SH has not been observed in the 20th century. See figure 2.11. [Nathan Gillett, Canada]	Taken into account. This is coordinated with chapter 2
11540	45	8	45	8	Could include Stouffer, Ronald J., and Syukuro Manabe, March 2017: Assessing temperature pattern projections made in 1989. Nature Climate Change, 7(3), DOI:10.1038/nclimate3224. in the reference list. [Roanld Stouffer, United States of America]	Rejected. We did not find a direct reference to the Arctic amplification discussed here
39280	45	9	45	9	Citations from section 7.6 of paleoclimate proxies should also be cited here. [Daniel Ibarra, United States of America]	Rejected. The discussion here is intended to be shorter and draws from the assessment in chapter 7
37694	45	20			Is "under global warming" needed here? If something is needed would "climate change" be better than "global warming". Arctic warming is a contribution to global warming. I would not describe Arctic warming as being under global warming. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	We assume that this statement applies to the statement on the previous page. We prefer making the statement conditional on the warming.
30718	45	24	45	24	is "seasonal warming patterns" going to be tested in CMIP6? [Annalisa Cherchi, Italy]	Taken into account. Yes, the seasonal changes in CMIP6 are shown for the near-term section
38994	45	24	45	32	How much of the material discussed in this paragraph new relative to AR5? [Masahide Kimoto, Japan]	Noted. Most of the understanding is consistent with AR5 but there are some new aspects that deserve to be briefly assessed
35982	45	24	45	32	This section is about changes in surface temperature, but the studies cited to support the assessment that most models predict a decreased seasonal cycle over the mid-latitudes and subtropics are studies of changes in tropospheric temperature (Dohohoe and Battisti, 2013; Santer et al., 2018). Comparing Figure AI.4 an AI.5 showing projected changes in DJF and JJA SAT in the CMIP5 models, it appears to me that the seasonal cycle decreases rather than increases in much of the Northern Hemisphere mid-latitudes over land (for example in much of the region between ~40N and 60N over N America and Asia). [Nathan Gillett, Canada]	Noted. This is correct and we specify that the statement is mostly robust for the northern mid-latitudes in Europe.
35980	45	30			The study cited here (Sanchez and Simon, 2018) is about kidney cancer, not changes in the seasonal cycle. [Nathan Gillett, Canada]	Accepted. Thank you this citation has been removed

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
35984	45	45	45	46	This text says that it is hard to assess changes in global temperature variability 'due to the interplay of unforced internal variability and forced changes'. As discussed elsewhere in this chapter, large ensembles allow changes in variability to be robustly diagnosed, and separated from forced changes, in the context of a particular model. [Nathan Gillett, Canada]	Noted. That is correct but that statement was meant to imply that it is hard to separate to separate it in observations. However, this does not necessarily belong here and thus the statement has been removed.
36570	46	1	46	2	It looks like “Over the extratropics” at the beginning of the page can be deleted because it does not quite fit with what follows. [Carlos Mechoso, United States of America]	Revised accordingly
28588	46	4			Is the standard deviation (in Fig. 4.20) calculated in a way that avoids its augmentation by the trend within the period? If not, the trend in 2081-2100 could explain some of the increase. [Ian Watterson, Australia]	Noted. Yes, the standard deviation was calculated across members and not in time to avoid trend-inflation of variance. The method used to calculate standard deviation is now better explained in the caption
36572	46	14	46	21	Do we have information about changes in maximum and minimum daily temperatures? [Carlos Mechoso, United States of America]	Not applicable. Changes in annual maximum and minimum temperatures are assessed in chapter 11.
30720	46	26	46	26	"DJF" instead of "DFJ" [Annalisa Cherchi, Italy]	Accepted. Corrected
30722	46	26	46	32	and what about changes in variability for the other periods considered (short and long term)? Is it appropriate to measure this (changes in variability) in 30 years for data? [Annalisa Cherchi, Italy]	Rejected. Not all changes are assessed at all time scales. Since large ensembles are here used to calculate much more than 30 years of data is used to quantify changes in variability.
48790	46	39	46	41	Several papers have focused on the rate of future climate change using CMIP5. Considering the rate of change is crucial for ecosystems but also for adaptation. A sub paragraph could be considered. See papers: "Near-term acceleration in the rate of temperature change", Smith et al. 2015 (Nature CC) - "Pace of shifts in climate regions increases with global temperature", Mahlstein et al. 2013 (Nature CC) - "The velocity of climate change" Loarie et al. 2009 (Nature) - Chavaillaz Y., S. Joussaume, A. Dehecq, P. Braconnot, R. Vautard, Investigating the pace of temperature change and its implications over the twenty-first century, Climatic Change, 137: 187-200, 2016. doi:10.1007/s10584-016-1659-4 [Sylvie JOUSSAUME, France]	Noted. This section is not about the rate of change but changes in tropospheric temperatures.
51918	46	55	47	38	It feels strange for this discussion not to make reference to the reductions seen in surface RH over land since 2000 and as assessed in chapter 2 which would, presumably, strengthen this segment? [Peter Thorne, Ireland]	Accepted. Cross-reference has been made to Chapter 2.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
9058	47	1			Could a subsection heading like "Relative Humidity" be included here? [Anna Merrifield, Switzerland]	Accepted. Subsection heading "Relative Humidity" added.
35986	47	5	47	7	Explain the mechanism driving RH decreases over land briefly here. Also give the reference to a particular chapter in a particular working group report of the AR5, not the whole AR5. [Nathan Gillett, Canada]	Accepted. We have further explained the mechanism and referred to AR5 properly.
7210	47	15	47	15	The Byrne & O'Gorman (2015) reference is incorrect. Should be Byrne & O'Gorman (2016): "Understanding decreases in land relative humidity with global warming: conceptual model and GCM simulations". J. Clim.29, 9045–9061. [Michael Byrne, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Reference has been corrected.
35988	47	23	47	26	This sentence is very hard to follow. Please clarify. [Nathan Gillett, Canada]	Accepted. We have modified the statements to further clarify.
9060	47	28			It might be good to also discuss the vegetation effects that may mitigate increasing aridity, as discussed in: Abigail L. S. Swann, Forrest M. Hoffman, Charles D. Koven, James T. Randerson, 2016: Plant responses to CO2 reduce estimates of drought. PNAS.113 (36) 10019-10024; DOI: 10.1073/pnas.1604581113 [Anna Merrifield, Switzerland]	Accepted. The following statement and new citation have been added as suggested: "However, vegetation effects may mitigate increasing aridity in response to increasing CO2 via feedbacks on surface relative humidity (Swann et al. 2016)."
36574	47	32	47	38	The lack of references in this paragraph contrasts with the style in the others. [Carlos Mechoso, United States of America]	Taken into account. This paragraph describes projected RH changes in CMIP6. We added the comparison between CMIP6 and CMIP5.
9062	47	50			Could a subsection heading like "Heat Stress" be included here? [Anna Merrifield, Switzerland]	Accepted. Subsection heading "Heat Stress" added.
51920	47	54	48	40	This segment feels to me to be missing a number of relevant heat stress papers e.g. https://www.pnas.org/content/114/15/3861 and papers by Steve Sherwood would be relevant here. [Peter Thorne, Ireland]	Accepted. More recent publications have been added, including the recommended papers.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
35990	48	2	48	5	Assessment of attribution should be cross-referenced with Chapter 3. [Nathan Gillett, Canada]	Taken into account. The current Chapter 3 does not include attribution results on heat stress. Suggest Chapter 3 (3.3.1 or 3.3.2) to include the following references. Then cross-reference with Chapter 3 can be made here. Knutson, T. R., & Ploshay, J. J. (2016). Detection of anthropogenic influence on a summertime heat stress index. Climatic Change, 138(1-2), 25-39. Li, C., Zhang, X., Zwiers, F., Fang, Y., & Michalak, A. M. (2017). Recent very hot summers in northern hemispheric land areas measured by wet bulb globe temperature will be the norm within 20 years. Earth's Future, 5(12), 1203-1216.
53406	48	7	48	14	Check if there are some links to ch12 that should be established here on indicators [Jan Fuglestvedt, Norway]	Accepted. Cross-reference has been added to Chapter 12.
35992	48	7	48	33	Especially to the extent that this relates to extremes in heat stress, and regional changes in heat extremes, this should cross-reference Chapter 11, and summarise underlying assessment in Chapter 11 (section 11.8.3). [Nathan Gillett, Canada]	Accepted. Cross-reference has been added to Chapter 11 (section 11.8.3) with a brief summary.
48792	48	7	48	40	We have just submitted a paper to GRL considering extremes of temperature and wet bulb global temperatures. "investigating the role of relative humidity in the co-occurrence of temperature and heat stress extremes i, CMIP5 projections", Brouillet and Joussaume, GRL, submitted June 2019. We show that both extremes co-occur during the year in mid-latitudes, but extremes of heat stress occur later than temperature extremes within the tropics, when the relative humidity is annually higher. We also show in future climate simulations that the global projected drying that strengthens the increase in temperature extremes, weakens the intensification of heat stress extremes. We can provide the paper of you consider it interesting to cite. [Sylvie JOUSSAUME, France]	Taken into account. Please provide the manuscript and we can refer to the paper properly.
11542	48	27	48	28	Delworth was the first to note this. Delworth, Thomas L., Jerry D Mahlman, and Thomas R Knutson, 1999: Changes in heat index associated with CO2 -induced global warming. Climatic Change, 43(2), 369-386. [Roanld Stouffer, United States of America]	Accepted. Citation added.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
39278	48	27	48	31	Citations needed here from paleoclimate literature even if no new results on AMV since AR5. [Daniel Ibarra, United States of America]	Rejected. AMV is not mentioned in section 4.5.1.3. Possibly the reviewer refers to other sections in the chapter. However we will take into account the suggestion. If there is a new result on AMV since AR5, we will assess it in the FGD.
35994	48	29			Is this a gloal mean change? [Nathan Gillett, Canada]	Not applicable. Clarified that it is the global land average.
35996	49	1			Replace 'global precipitation' with 'global mean precipitation'. 'Global precipitation' could be read as precipitation throughout the globe, whereas here the statement is true for the global mean, but not each location individually, since some locations are projected to get drier. [Nathan Gillett, Canada]	Accepted.
35998	49	6	49	7	Replace 'precipitation' with 'Global mean precipitation'. Some locations are projected to get drier. [Nathan Gillett, Canada]	Accepted. It is corrected.
54432	49	8	49	10	Did Huang et al. (2013) really state that seasonal precipitation anomalies can provide a robust signal than annual mean? [Reynold Stone, Trinidad and Tobago]	Not applicable. Yes, they have suggested that projections of tropical seasonal mean rainfall are more reliable than the annual mean.
54434	49	8	49	10	Huang et al. (2013) actually stated "the wet-gets-wetter mechanism contributes more to the projected seasonal rainfall changes, whereas the warmer-gets-warmer mechanism to the mean annual rainfall changes." [Reynold Stone, Trinidad and Tobago]	Not applicable. Huang et al (2013) have shown that the wet-gets-wetter mechanism contributes more to the projected seasonal rainfall changes. They have also shown that projections of tropical seasonal mean rainfall are more reliable than the annual mean.
36000	49	24	49	28	As written the text implies that regional variations in projected precipitation changes are only caused by changes in atmospheric circulation, with the thermodynamic component causing an increase everywhere - this is not the case. Held and Soden (2006, https://journals.ametsoc.org/doi/pdf/10.1175/JCLI3990.1) show how if lower tropospheric relative humidity remains constant and the atmospheric circulation stays the same, the climatological pattern of P-E will be enhanced - the wet-gets-wetter, dry-gets-drier paradigm referred to elsewhere in the chapter. Before the effects of circulation changes are discussed, which are second order, the authors should discuss the regional characteristics of the thermodynamic component. [Nathan Gillett, Canada]	Taken into account. Thank you for the suggestion. Assessment of thermodynamic component as well as dynamic component on precipitation changes are included in the text.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
48794	49	24	49	33	"This paper can be of interest to this paragraph, showing a stabilization of precipitation patterns with the increasing role of thermodyncamic ocnriobution. Chavaillaz Y., S. Joussaume, S. Bony, P. Braconnot, Spatial stabilization and intensification of moistening and drying rate patterns under future climate change, Climate Dynamics, 2015. DOI 10.1007/s00382-015-2882-9 from abstract: As we move fur- ther over the twenty-first century, more regions exhibit a significant rate of precipitation change, while the patterns become geographically stationary and the trends persis- tent. The stabilization of the geographical rate patterns that occurs despite the acceleration of global warming can be physically explained: it results from the increasing contri- bution of thermodynamic processes compared to dynamic processes in the control of precipitation change. " [Sylvie JOUSSAUME, France]	Accepted. It is included.
36004	49	24	49	53	There is an extensive discussion of the mechanisms controlling projected precipitation changes, including their regional character, in Section 8.2 which isn't cited here. This chapter should avoid replicating assessment in Chapter 8, and just summarise the main Chapter 8 assessment conclusions. [Nathan Gillett, Canada]	Taken into account. Thank you for the suggestion. The mechanisms contributing to precipitation changes discussed in Section 8.2 of Chapter-8 is referred in the text. Removed the discussion of precipitation processes which are discussed in Chapter 8.
7212	49	30	49	33	I found the discussion of tropical precipitation/circulation changes to be rather cursory. Although what is stated is generally correct, lots of important results are omitted. For example, CMIP5 simulations indeed show an overall slowdown of the tropical circulation but this is a small residual between weakening ascent at the edges of the ITCZ and strengthening in the ITCZ core. These same models predict a strengthening and narrowing of the ITCZ, but no robust "shift" of the zonal-mean ITCZ [see Byrne et al (2018), Current Climate Change Reports]. So it seems strange to mention an ITCZ shift (which is expected to be small [e.g. Donohoe & Voigt (2017)] rather than the much more robust ITCZ narrowing and strengthening. Finally, in the last sentence circulation and precipitation changes are linked to weaker net radiative cooling. This is partly true, but lots of other processes are important too like cloud radiative effects, gross moist stability, moist static energy gradients etc. Although a detailed discussion of the patterns and mechanisms for tropical precipitation changes is maybe not appropriate here, I would suggest at least mentioning the more robust changes and expand the discussion of the physical processes contributing to these changes (weaker net radiative cooling is far from the full story). [Michael Byrne, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Thank you. Discussion about the processes contributing to precipitation changes are removed from the section, since it is discussed in detail in Section 8.2. The Section 8.2 also provide assessment of ITCZ changes, hence is removed from this section

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
38996	49	37	49	37	A related study: Watanabe, M., Y. Kamae, H. Shiogama, A. DeAngelis, and K. Suzuki, 2018: Low clouds link equilibrium climate sensitivity to hydrological sensitivity. Nature Climate Change, 8, 901-906, doi:10.1038/s41558-018-0272-0. [Masahide Kimoto, Japan]	Noted.
36002	49	37	49	39	First this sentence is unclear. Second, it says that dynamical uncertainty unrelated to climate sensitivity dominates precipitation uncertainty 'across the globe', but only cites a study which examines the spread in projected seasonal mean precipitation in four relatively small regions of the tropics. If retained, please cite the studies which support this assessment for the rest of the globe. [Nathan Gillett, Canada]	Taken into account. Thank you for pointing out this. Since the study addresses the uncertainties in tropical rainfall projections, not providing a global perspective, the sentence is removed from the section.
36006	49	41	49	46	It has long been known that the precipitation response to sulphate aerosol has opposed the response to greenhouse gases - see for example Allen and Ingram (2002; https://www.nature.com/articles/nature01092). This isn't a new result from PDRMIP. See also the discussion in 8.2.1.1.1. I recommend citing the underlying assessment in Chapter 8 and briefly summarising here. [Nathan Gillett, Canada]	Taken into account.
36008	49	44			Why is volcanic forcing listed here? There was not an overall increase in volcanic forcing over the 20th century (or if there was it will depend very sensitively on the exact period over which the trend is computed). [Nathan Gillett, Canada]	Take into account. The sentence is revised accordingly.
36010	49	48	49	53	Rather than just writing 'The processes that govern large-scale changes in precipitation are discussed in Chapter 8', better to summarise their assessment and cite the corresponding sections of Chapter 8. [Nathan Gillett, Canada]	Taken into account. Relevant section of Chapter-8 is referred in the text.
39546	50	5	50	45	check consistency and coherency in terms of process related explanations with those provided in section 4.4.1.4 and chapters 2 and 3 assessing global monsoon observed changes [Carolina Vera, Argentina]	Take into account. There is a thematic focus team on monsoon to avoid unnecessary overlap and make more consistency across chapters.
40534	50	8			Remove the words "that there is" as they are not needed for the sentence. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. It is removed.
30724	50	11	50	11	remove "aggregated over all monsoon systems" [Annalisa Cherchi, Italy]	Take into account. the sentence is revised properly.
37696	50	12			Monsoon "intensity" is stated to increase, but monsoon circulation weakens. It would be helpful to indicate what parameter determines intensity, as it clearly is not circulation. Is it precipitation? [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Take into account. The monsoon "intensity" indicates global monsoon precipitation "intensity". It is more clearly stated in the SOD.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
38540	50	23	50	24	It is not clear the meaning of "significant enhancement of the Hadley circulation". [Masaki Satoh, Japan]	Noted. The sentence is based on Lee and Wang (2014) paper. Based on CMIP5 models, the paper shows increase in cross-equatorial wind from the SH to NH associated with the Hadley circulation which would enhance the NH monsoon precipitation in response to global warming.
40536	50	24			Since both northern and southern hemispheres are discussed, which Hadley Cell is referred to here? [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. In the paper, the assessment was done in terms of annual mean. However, the enhancement of Hadley circulation is larger during boreal summer with more moisture transport into the NH from SH.
30726	50	27	50	28	I would remove the sentence and keep only global view [Annalisa Cherchi, Italy]	Take into account. There is more coordination with Ch8 regarding regional monsoon change assessment.
40538	50	30	50	32	The sentence here is rather confusing. The moisture convergence is increasing, apparently offset by a signal from convergence. Doesn't this mean there is negative convergence, or in other words, divergence? Maybe the sentence could be written more carefully. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Take into account. The GMP increase is mainly attributable to thermodynamics contribution associated with increase in atmospheric moisture. However, the moisture convergence due to circulation change many be weakened. The sentence is revised.
36576	50	36	50	37	In the previous lines you have given enough reasons on the different behavior of the monsoons under global warming to make me caution me on the usefulness of the “global monsoon” precipitation changes. You may want to think about the need to point out such caveats. [Carlos Mechoso, United States of America]	Take into account. Although there are large uncertainties in regional monsoon change and circulation contribution, the global monsoon mean precipitation change will increase significantly by the end of 21st century due to several aspects described in the text. Anyway, the sentence is revised clearly in the SOD.
30728	50	37	50	37	remove "aggregated over all monsoon systems" [Annalisa Cherchi, Italy]	Take into account. the sentence is revised properly.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
38998	50	39	50	39	Why precipitation increase is greater for SSP1-2.6 than for SSP5-8.5? [Masahide Kimoto, Japan]	Not applicable. The increase is given one degree warming, not the actual value at the end of 21st century. In AR5 and several papers indicate that the higher forcing scenario may have larger inefficiency in increase precipitation given one degree warming. One of biggest contribution may be weakening of tropical circulation associated with atmospheric stabilization.
36012	51	12	51	14	This text on possible mechanisms underlying the relative influences of ozone recovery and GHGs on SLP could be supported in the SOD by analysis of the ssp245-GHG and ssp245-stratO3 DAMIP simulations (at least for SSP2-4.5). [Nathan Gillett, Canada]	Noted. It will be considered in the FGD.
36578	51	32	51	43	In general, climate models have strong systematic errors over the Southern Ocean. A word of caution may be added about how this can add uncertainty to latitudinal shifts in the jet streams. [Carlos Mechoso, United States of America]	Taken into account.
39548	52	9	53	49	check consistency and coherency of process related explanations with those provided in section 2.3.1.3.3 of chapter 2 assessing observed changes. [Carolina Vera, Argentina]	Take into account. It is better coordinated with Chapter 2.
27284	52	12	52	12	Suggest adding reference to Barnes & Polvani 2013 here [Gabriel Chiodo, Switzerland]	Accepted. Reference is included.
46510	52	16	52	16	Add here a sentence: "Simulations indicate that most of the changes in winter storminess over the Euro-Atlantic regional will occur only after exceeding the 1.5 degree warming level (Barcikowska et al. 2018)", Reference: Barcikowska et al. 2018: Euro-Atlantic winter storminess and precipitation extremes under 1.5 °C vs. 2 °C warming scenarios. Earth System Dynamics 9(2):679-699, DOI: 10.5194/esd-9-679-2018 [Frederik Schenk, Sweden]	Take into account.
29992	52	19	52	19	Zappa et al. (2018 GRL doi: 10.1002/2017GL076096) show that the equatorward North Atlantic wintertime jet shift in response to sea-ice loss is robust across the CMIP5 models. [Theodore Shepherd, United Kingdom (of Great Britain and Northern Ireland)]	Take into account.
51924	52	28	52	31	It is unclear whether this discussion is solely to the NH storm tracks or extra-tropical storm tracks as a whole. If the latter presumably the allusion to the southern edge isn't appropriate as in the SH it will be the northern edge? [Peter Thorne, Ireland]	Take into account.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
7182	52	36	52	36	At the end of this paragraph, please briefly mention that the anthropogenic changes in the upper tropospheric jet and the transient eddy kinetic energy distribution likely affects aviation-relevant turbulence distributions (Watanabe et al. 2019). Watanabe, S., M. Fujita, S. Kawazoe, S. Sugimito, Y. Okada, R. Mizuta, and M. Ishii (2019), Frequency change of clear-air turbulence over the North Pacific under 2 K global warming – Ensemble projections using a 60-km atmospheric general circulation model, J. Meteorol. Soc. Jpn., 97, doi:10.2151/jmsj. 2019-038. [Shingo Watanabe, Japan]	Take into account.
36014	52	38	53	12	This section overlaps extensively with Chapter 11 (Section 11.7.2.4), which assesses changes in extreme ETCs. I recommend leaving the primary assessment of changes in extreme ETCs to Chapter 11, and just summarising that assessment here. [Nathan Gillett, Canada]	Take into account. We refer to Chapter 11 in the FGD and will give a shorter account here.
39000	52	41	52	42	The meaning o the sentence is not very clear to the reviewer. [Masahide Kimoto, Japan]	Noted.
31498	52	50			HighresMIP could contribute to the assessment of NH ETCs. [Rein Haarsma, Netherlands]	Noted.
39550	53	3	53	5	from where does the conclusion about that ozone recovery would compesate the GHG increasing signal in low scenarios come from? Check consistency in the explanations of the processes related with the competing effects of ozone depletion/recovery and GHG increasing with sections about the SAM changes (4.3.3.1, 4.4.3.1) [Carolina Vera, Argentina]	Take into account. The SOD has better consistency on that.
36016	53	4	53	5	Is this for the annual mean or DJF only? [Nathan Gillett, Canada]	Noted. It is DJF only. We specify this in the SOD.
13928	53	24	53	49	As a caveat, could cross-ref to section 3.3.3.3 where blocking biases in models are discussed. [Tim Woollings, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Cross reference included in the SOD.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
46406	53	34	53	35	This statement appears to be inconsistent with observations which show a clear increase in quasi-stationary weather extremes in recent decades over the mid-latitudes of the northern hemisphere. E.g. Duchez et al. (2016) has clearly linked unusually cold North Atlantic SST with persistent atmospheric blocking and major European heatwaves since the 1980s. The very cold SST are linked with the slowdown of the AMOC which is projected to continue under future warming. As shown for the CMIP5 model mean summer change under RCP4.5 (2071–2100)–(1971–2000) (Haarsma et al. 2015), there is a tendency to increased sea-level pressure during boreal summer over the NE-Atlantic consistent with an oceanic cooling caused by a weakening AMOC. For the last major partial AMOC collapse around 12,000 years ago, a similar increase in summer blocking and warm European temperatures has been found in climate model simulations and geological evidence (Schenk et al. 2018). The uncertainty of future climate simulations has be to be more thoroughly discussed in the light of recent observations and paleoclimate evidence as blocking events are behind the most extreme weather and climate anomalies with major impacts on the society. I would therefore argue that our confidence in the simulation of blocking is very low for present and future climate states. References: Duchez et al. 2016: Drivers of exceptionally cold North Atlantic Ocean temperatures and their link to the 2015 European heat wave. Environ. Res. Lett. 11, 074004, doi:10.1088/1748-9326/11/7/074004 Haarsma et al. 2015: Decelerating Atlantic meridional overturning	Take into account.
36580	53	37	53	39	In general, climate models have strong systematic errors over the Southern Ocean. A word of caution may be added about how this can add uncertainty to latitudinal shifts in the jet streams. [Carlos Mechoso, United States of America]	Taken into account.
31500	53	44			Here the Arctic box in Ch10 could be mentioned [Rein Haarsma, Netherlands]	Take into account.. We refer to the box in Chapter 10
36582	53	48	53	49	Perhaps one could me more precise on the types of shifts expected. [Carlos Mechoso, United States of America]	Taken into account. The text is revised.
37698	54	7			"Observationally-based values" would be better than "observations", as the data are drawn from reanalyses. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. The figure is deleted in SOD.
12808	54	12	55	21	Section does not contain information about the cryosphere, focusing only on ocean. Items to be added into this section to cover cryosphere in the mid-term to long-term include the increased loss of Arctic sea ice, the continued melt of glaciers and ice sheets, and thawing permafrost. [Durwood Zaelke, United States of America]	Taken into account. We focus on the discussion of ocean in this section, and thus removed the word 'cryosphere' from the subheading.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
12652	54	12	55	21	Section does not contain information about the cryosphere, focusing only on ocean. Items to be added into this section to cover cryosphere in the mid-term to long-term include the increased loss of Arctic sea ice, the continued melt of glaciers and ice sheets, and thawing permafrost. [Kristin Campbell, United States of America]	Taken into account. We focus on the discussion of ocean in this section, and thus removed the word 'cryosphere' from the subheading.
35298	54	12	59	3	As with 4.3.2 and 4.4.2, the section title of "Cryosphere, Ocean, and Biosphere" is inaccurate if you don't discuss the terrestrial biosphere at all. Suggest that you either rename this section to delete the word biosphere or rescope it to also include the terrestrial biosphere. [Charles Koven, United States of America]	Taken into account. We deleted the word 'biosphere' here.
46994	54	16	54	47	One of the robust features of the structure of projected ocean warming is a surface intensification, and hence an increase in the time- and spatial-mean stratification of the upper ocean. As discussed in SROCC Chapter 5, this increased upper ocean stratification is one of the primary pathways by which physical ocean changes impact open-ocean biogeochemistry and ecosystesms. The impact of this section would be improved by at least some mention of increasing upper ocean stratification. [Robert Hallberg, United States of America]	Taken into account. We have added discussion of upper ocean stratification.
36018	54	18	54	19	Assessment of forced changes in ocean heat content in the past belongs in Chapter 3 (3.5.1.2) and Chapter 9 (9.2.3.1). [Nathan Gillett, Canada]	Taken into account. We have summarised main assessment for ocean heat content from Section 2.3.3.1, 3.5.1.2, 7.2.2.2. and 9.2.3.1.
39002	54	21	54	29	What is the major difference between old and new data sets? [Masahide Kimoto, Japan]	Taken into account. We have revised ocean heat content discussion by summarising relevant assessment from Section 2.3.3.1, 3.5.1.2, 7.2.2.2. and 9.2.3.1.
45244	54	21	54	29	I don't think we should single-out the Cheng et al product - this is one of many estimates and we have no real objective means of saying whether this is an improvement over other estimates. The argument about consistency with satellite estimates of radiative imbalance is somewhat circular - the satellite estimates have to be "anchored" to long-term rates of OHC change (as discussed in Chapter 7). I would suggest that discussion of the observation-based estimates of OHC change belongs in Chapter 2 and/or Chapter 7. My view is that we either want to show an observational ensemble, or choose a representative product or products for comparison with model simulations. [Matthew Palmer, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We have revised ocean heat content discussion and summarising relevant assessment from Section 2.3.3.1, 3.5.1.2, 7.2.2.2. and 9.2.3.1.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
51926	54	21	54	29	This paragraph should make reference to relevants ections in chapters 2, 3 and 9 where a substantive assessment of these issues is performed. [Peter Thorne, Ireland]	Taken into account. We have revised ocean heat content discussion by summarising relevant assessment from Section 2.3.3.1, 3.5.1.2, 7.2.2.2. and 9.2.3.1.
36020	54	21		24	Chapter 4 should not independently assess observed changes in ocean heat content, but should summarise the assessment in Chapter 2 and Chapter 9 (9.2.3.1) on this topic. [Nathan Gillett, Canada]	Taken into account. We have summarised main assessment for ocean heat content from Section 2.3.3.1, 3.5.1.2, 7.2.2.2. and 9.2.3.1.
9064	54	21			A good reference here might be: Durack, P. J., Gleckler, P. J., Landerer, F. W. & Taylor, K. E. Quantifying underestimates of long-term upper-ocean warming. Nature Clim. Change 4, 999–1005 (2014). [Anna Merrifield, Switzerland]	Taken into account. We have revised the discussion on ocean heat content by summarising relevant assessment from Section 2.3.3.1, 3.5.1.2, 7.2.2.2. and 9.2.3.1.
36022	54	23		25	Chapter 4 should not independently assess the closure of earth's energy budget, but if relevant, summarise the assessment of Chapter 7 on this topic. [Nathan Gillett, Canada]	Taken into account. We have summarised main assessment for ocean heat content from Section 2.3.3.1, 3.5.1.2, 7.2.2.2. and 9.2.3.1.
36024	54	25	54	27	Chapter 4 should not independently assess model observations consistency for past ocean heat content, but summarise Chapter 3 assessment on this topic (3.5.1.2). [Nathan Gillett, Canada]	Taken into account. We have revised ocean heat content discussion by summarising relevant assessment from Section 2.3.3.1, 3.5.1.2, 7.2.2.2. and 9.2.3.1.
38058	54	27	54	29	Those are not consistent with Chap 9 (almost but not exactely). Coordination is needed. Chap 9: "It is very likely that the ocean will take up another 2000±350 ZJ (2080-2100 CMIP5 mean±std) by the end of the century relative to year 2000 under RCP8.5, compared to 1000ZJ under RCP2.6, translating into 0.8±0.1K versus 0.4±0.1K warming [Jean baptiste SALLEE, France]	Taken into account. We have revised ocean heat content discussion by summarising relevant assessment from Section 2.3.3.1, 3.5.1.2, 7.2.2.2. and 9.2.3.1.
57466	54	31	54	40	Check for consistency with Chapter 9's discussion of heat uptake and its redistribution. This makes it sound pretty complicated, when in fact much of heat uptake appears to track what one would expect from the uptake of a passive tracer (e.g., doi: 10.1007/s00382-014-2308-0, doi: 10.1073/pnas.1808838115, doi: 10.1038/NGEO2731) [Kyle Armour, United States of America]	Taken into account. We have added the corresponding discussion and references.
36026	54	35	54	39	Attribution of past changes in ocean heat content in the Southern Ocean belongs in Chapter 9 (9.2.3.1) and Chapter 3 (3.5.1.2). Summarise those assessments here if relevant. [Nathan Gillett, Canada]	Taken into account. We have referenced Section 3.5.1.2 and 9.2.3.1
38060	54	39	54	40	This is consistent with Chap 9 in spirit, but that is not stated in chap 9 with this confidence level. Coordination needed. [Jean baptiste SALLEE, France]	Taken into account. We changed the wording accordingly

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
46990	54	45	54	45	Please avoid using the word "observed" when describing model projections. [Robert Hallberg, United States of America]	Taken into account. Text revised.
45246	54	50	54	55	Figure 4.30: I like this figure and it will be interesting to see how it looks for CMIP6. However, please cross-check against what will appear in Chapter 9 - I think they have something very similar planned. [Matthew Palmer, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. After coordination with chap.9, this figure will be retained here. We will update the figure using CMIP6 in FGD.
36028	55	4	55	7	No references are given to support the assessment on future ocean acidification. [Nathan Gillett, Canada]	Taken into account. We update this figure using CMIP6 results and add corresponding references.
36030	55	24	59	3	This section is very repetitive of 4.4.3 and somewhat repetitive of Section 4.3.3. I suggest merging these sections. [Nathan Gillett, Canada]	Taken into account.
46256	55	31	55	35	"Observed in the past" in this section is not clear [sadegh zeyaeyan, Iran]	Taken into account. Thank you for the comment. "Observed in the historical period". As assessed in AR5. This is specified in the SOD.
8890	55	31	55	35	"Observed in the past" in this section is not clear [Mohammad Javad Zareian, Iran]	Taken into account. Thank you for the comment. "Observed in the historical period". As assessed in AR5. This is specified in the SOD.
57532	55	31	55	35	"Observed in the past" in this section is not clear [Sahar Tajbakhsh Mosalman, Iran]	Taken into account. Thank you for the comment. "Observed in the historical period". As assessed in AR5. This is specified in the SOD.
36034	55	37	55	39	This does not appear to reflect the assessment of AR5. As mentioned earlier in the paragraph, AR5 Chapter 14 assessed that the NAM 'is likely to become slightly more positive in the future' i.e. it is part of the forced response. Chapter 14 also noted that "Some studies have even considered NAO to be a source of natural variability that needs to be removed before detection and attribution of anthropogenic changes (e.g. Zhang et al., 2006).'. Zhang et al. (2006) is a regional temperature attribution study, which found that detection of anthropogenic influence on continental mean temperatures continued to occur even after NAO-congruent temperature changes were regressed out. So AR5 did not assess that there is debate in the literature over whether or not the NAM is part of the forced response, just that the NAO is a driver of variability in surface temperatures - I suggest deleting this text. [Nathan Gillett, Canada]	Taken into account. The text has been removed as suggested.

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27268	55	44	55	45	First off, the paper by Wang discusses the NAO, not the NAM. The two metrics are not always well correlated. Also, this statement about an underestimation of low-frequency NAO variability is very debatable. It may also well be that the low-frequency variability in the OBS is not well constrained either. The observational record is one single realization, and is also quite short in time for the analysis of low-frequency variability in a noisy parameter such as the NAO. For instance, in Chiodo et al., 2019 (DOI:10.1038/s41561-018-0293-3), we found that the NAO decadal periodicity in the HadSLP data is highly time-dependent, as it mostly arises only in the 1960-2000 period (see wavelet analysis in Fig 5a of Chiodo et al., 2019). Hence, it would be good to add an additional statement noting the caveat about the short observational time-series, and that we thus need longer data before making definitive conclusions about a systematic model error. Something like: ""Although longer observational time-series are needed to better constrain low frequency variability in the NAO and the NAM". [Gabriel Chiodo, Switzerland]	Taken into account. Text has been removed. Assessment of model performance for NAM is in chapter 3. This is now summarised in Section 4.3.3.1 where the NAM is introduced and defined.
13930	55	45	55	45	Evidence related to underestimation of decadal NAM variability has also been given by (Simpson et al 2018; https://doi.org/10.1175/JCLI-D-18-0168.1 and Bracegirdle et al 2018, https://doi.org/10.1029/2018GL078965) . [Tim Woollings, United Kingdom (of Great Britain and Northern Ireland)]	Noted. the suggested literature is assessed. Model performance for NAM variability now summarised in Section 4.3.3.1 from chapter 3.
27270	55	47	55	55	Again, considering the previous comment, this paragraph sounds a bit too bold. It's fair to say that models are imperfect, but it's a bit premature to say that the models are fundamentally wrong in their representation of NAM variability. [Gabriel Chiodo, Switzerland]	Noted. The statement has been deleted.
36036	55	47			Replace 'CanESM6' with 'CanESM5'. [Nathan Gillett, Canada]	Not applicable. Figure replaced with CMIP6 results
36038	55	49	55	50	It is not clear what 'In boreal winter the central estimate for the NAM is more than four times higher than the current one' means. Figure 4.32 shows that the projected NAM index in DJF under SSP5-8.5 is more than 4 hPa higher by 2081-2100 compared to 1995-2014, but this isn't the same as 'four times higher'. [Nathan Gillett, Canada]	Not applicable. Text removed in SOD.
36042	55	50	55	52	The meaning of 'where it has been shown a considerable uncertainty by 2100' is unclear. [Nathan Gillett, Canada]	Not applicable. Text removed in SOD.
36040	55	51			Replace 'CanESM6' with 'CanESM5'. [Nathan Gillett, Canada]	Not applicable. Figure replaced with CMIP6 results
36046	55	51			In what way is the CanESM5 DJF NAM change 'in partial contrast with the CMIP5 multi-model ensemble'? Based on Figure 1 of Gillett and Fyfe (2013), a ~4hPa DJF NAM change between 1995-2014 and 2081-2100 appears to be within the range of the CMIP5 ensemble. [Nathan Gillett, Canada]	Noted. No longer applicable as Figure updated with CMIP6 results.

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30730	55	52	55	55	it could be, but for the moment it is based on a comparison with one model and one scenario from CMIP6 [Annalisa Cherchi, Italy]	Noted. The sentence has been deleted.
36044	55	52	55	55	This argument concerning the uncertainty in NAM projections is very unclear. First it is not clear what 'this uncertainty' is referring to. Second, assuming it is referring to uncertainty in the NAM projections, why does this provide 'further evidence that our inability to make more precise projections of changes to the NAM is primarily by a lack of physical understanding and imperfect models, rather than by internal climate variability'? None of the references cited specifically discuss the implications of the impacts of physical understanding and imperfect models on NAM projections. I think this is saying that model uncertainty is more important than internal variability for projections of the NAM on the long term. But this isn't demonstrated in the cited references or in the chapter. And moreover, since this conclusion is also valid for long-term projections of GSAT and many other variables, why is this conclusion highlighted for the NAM in particular? [Nathan Gillett, Canada]	Noted. The statement has been deleted.
48466	56	2	56	10	Ideally these SAM figures/assessment will highlight both model and scenario dependence but also how significant the changes are with respect to internal variability. At present one gets a very different sense of the significance of the change in Figure 4.6 and 4.32, [Julie Arblaster, Australia]	Taken into account. Fig 4.32 was a placeholder figure based on one model (CanESM5). The figure for the SOD is updated and the uncertainty ranges provided in a way that reflects the spread of simulations as evident in Fig 4.6. Additional scenarios is also be added to Fig 4.32 and it is updated based on CMIP6 models.
39552	56	3	56	10	Besides the fact that this section focuses more on the seasonal variations of SAM changes, while section 4.3.3.1 discusses more the scenario-based dispersion of the SAM changes, it is not that clear yet why we need two separate sections to discuss SAM long-term changes. [Carolina Vera, Argentina]	Noted. The sections on the SAM are more integrated in the SOD. Section 4.3.3.1 is intended to give a centennial overview of global climate indicators (including SAM) which section 4.5.3.1 goes into more detail on drivers, scenario dependence and seasonal variations in trends
36048	56	4			Replace 'CanESM6' with 'CanESM5'. [Nathan Gillett, Canada]	Not applicable. Figure replaced with CMIP6 results
30732	56	8	56	10	this will have to be confirmed with more scenarios and more models from CMIP6 [Annalisa Cherchi, Italy]	Noted. the results are updated based on CMIP6 models for the SOD.
30734	56	20	56	20	in the text CanESM6, here CanESM5 [Annalisa Cherchi, Italy]	Not applicable. No longer applicable as the Figure is updated with CMIP6 results.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
36050	56	26	57	1	This section is repetitive of Sections 4.3.3.2 and 4.4.3.2. [Nathan Gillett, Canada]	Taken into account.
38520	56	28	56	29	Please refer to Chapter 11, Internal Box 11-3, in the context of "The El Niño Sourthern Oscillation (ENSO) influences global climate and is the dominant source of seasonal climate predictability". [Masaki Satoh, Japan]	Accepted. We refer to Box 11-3 in Chapter 11 in Section 4.3.3.2 since the sentence is moved to the subsection.
39554	56	28	56	56	which are the main key findings of this section that distinguishes it from 4.4.3.2? I understand that the difference between both sections is mainly that the two assess ENSO changes in different terms, but that distinction is not clear yet. [Carolina Vera, Argentina]	Noted. This section is focussed on medium to long term change, as opposed to near-term changes discussed in 4.4.3. It may be expected that at least under low mitigation changes in ENSO would manifest more strongly in the mid to long term. These aspects is discussed with more clarity in the SOD.
38438	56	29	56	29	The term „very likely“ is possibly used without implying reference to IPCC terminology. [Dirk Notz, Germany]	Taken into account.
39004	56	40	56	41	Caution should be excercised to say that the warming pattern IS El Nino like. [Masahide Kimoto, Japan]	Taken into account.
36584	56	47	56	48	Can you tell us whether equatorial and coastal tropical upwelling will also be affected? [Carlos Mechoso, United States of America]	Not applicable. Changes in upwelling are discussed in Chapter 9 "Ocean, cryosphere and sea-level change", but such changes depend of course on more than ENSO forcing.
53030	56	47	57	1	Fasullo et al. 2018 used multiple Large Ensembles to show extremes associated with ENSO will increase regardless of how ENSO would change, doi: https://doi.org/10.1029/2018GL079022 [Anson Cheung, United States of America]	Noted.
39006	56	49	56	49	Another related study: Watanabe, M., Y. Kamae, and M. Kimoto, 2014: Robust increase of the equatorial Pacific rainfall and its variability in a warmed climate. Geophys. Res. Lett., 41, 3227-3232, doi:10.1002/2014GL059692. [Masahide Kimoto, Japan]	Noted.
38522	56	49	56	52	Chand et al. (2017) is based on CMIP5 coarse resolution models, and not reliable for TC frequency projection. The sentence is not an assessment but merely introducing a single literrature, and not balanced. This sentence should be more neutralized. "Moreover, there is an indication that tropical cyclones will become more frequent during future El Niño events (and less frequent during future La Niña events) by the end of the 21st century (Chand et al., 2017), thus contributing to the projected increase in ENSO-associated hydroclimate impacts." [Masaki Satoh, Japan]	Taken into account. Agreed that this is a single study, therefore it is referred to as providing an "indication" and no confidence language is assigned to the statement.
36052	56	49	56	52	Projected changes in tropical cyclones should be cross-referenced with Chapter 11. [Nathan Gillett, Canada]	Not applicable. TC statement is removed in SOD.

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15566	56				4.5.3.2 ENSO and its teleconnection Many parts of this section is redundant compared to previous chapter (chapter 2, chapter 3). In addition, the current version is missing the latest literature on the ENSO and its teleconnection. In particular, it is necessary to describe the changes in ENSO and its teleconnection largely depend on which metrics are used to define ENSO and its teleconnections. [SANG-WOOK YEH, Republic of Korea]	Taken into account. There has been better coordination on mode of variability across chapters including its Technical Annex.
30736	57	8	57	9	better to have it in ch 8 [Annalisa Cherchi, Italy]	Chapter 4 deals with projected changes in global climate, and it would be fitting to show a Figure on the global hydrological impacts of ENSO.
39558	57	16	57	45	check consistency and coherency of process related explanations with those provided in section 4.4.3.3 [Carolina Vera, Argentina]	Taken into account.
36054	58	16	58	17	This assessment that ENSO events in general and extreme El Nino events in particular are projected to increase is not consistent with the assessment in the previous section - 4.5.3.2 - 'there is no strong model consensus on how these mean state changes affect ENSO SST variability.' (pg 56, ln 47-48). [Nathan Gillett, Canada]	Noted. The assessment of changes in ENSO attributes (4.5.3) is something different that analysing the underlying causes of these changes, so there is not necessarily an inconsistency here. Nevertheless, this section as well as 4.4.3 is revised in the SOD given new literature on ENSO, including some CMIP6 based studies.
36586	58	17	58	18	It is my understanding that there is an ongoing debate on the possible association between IOD and ENSO. It seems that strong El Niño events can influence the strength of the eastern pole of the IOD through evaporative cooling associated with anomalous easterlies during the co-occurring ENSO, but the general connection between IOD and ENSO is weak (e.g., Saji, N. H., 2018: The Indian Ocean Dipole Mode, Climate Systems and Climate Dynamics, History of Climate Science, Climate Impact: Extreme Events. Oxford Research Encyclopedias). In this case, the last sentence in the paragraph may be not informative. [Carlos Mechoso, United States of America]	Taken into account.
57468	58	27	58	27	Some contradictory statements about whether AMV can and will change, and on what timescale. [Kyle Armour, United States of America]	Noted. You are right. We revise the text to avoid time-scales mismatch.

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47208	58	27	58	31	Just as an illustration on my former comment about the need to focus on low-probability-high-impact changes and about the potential harm of emphasizing the most likely range : how policy-relevant was the AR5 assessment about the fact that « AMV is unlikely to change its behaviour in the future » as long as the same paragraph recognize that there is a clearly « knowledge gap » on this issue ? If we don't know much about something, why should we bet on a « bright future » ? [Hervé Douville, France]	Noted. The AR5 assessment represents the state-of-the art of our knowledge. Since AR5 no new results/studies (to our knowledge) have become available. Therefore we can just repeat what was assessed by AR5.
31950	58	27	58	31	A reference would be welcome here. [Marie-France Loutre, Switzerland]	Taken into account.
36060	58	29	58	31	Elsewhere in the Chapter a projected reduction in AMOC over the 21st century is discussed (e.g. pg 44, ln 46-48), but this isn't mentioned in this section on AMV. Won't this have an influence on AMV? [Nathan Gillett, Canada]	Taken into account.
36056	58	36	58	37	Reference the assessment of Chapter 3 for model evaluation of Tropical Atlantic Modes here - Section 3.7.5. [Nathan Gillett, Canada]	Taken into account.
7966	58	51	58	54	I don't understand what these are references for (and one of them is mine!). Neither addresses that the AMOC is expected to weaken from global warming. I don't think either addresses the AMOC having an influence on the tropical Atlantic. For the latter point better references would be: DOI:10.1007/s00382-017-3756-0 , 10.1175/jcli-d-16-0127.1 , 10.1175/jcli3460.1 [Laura Jackson, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account.
53408	59	6	59	6	the title is in my view, too broad, but I understand that "climate policies" is chosen to cover both emission reductions and SRM and CDR. Perhaps you could use "Effects of climate policies" ? [Jan Fuglestvedt, Norway]	Noted. The title of this section should reflect a broad range of mitigation options and their implications. Replacing "implications" with "effects" is not a meaningful improvement of the text.
30366	59	10	59	10	It would be useful to clarify that these global mean temperature increases are expressed in GSAT (I assume). [Joeri Rogelj, Austria]	Taken into account.. In the SOD, the term GSAT is clearly defined in the text and consistently applied throughout Chapter 4. Section 3.6.1 clearly states that the analysis is GSAT based.
53410	59	33	59	33	this scenario is used to capture the levels you need, I guess. You could mention that. [Jan Fuglestvedt, Norway]	Taken into account In the SOD we are assessing how consistent the spatial patterns of change scale for a given level of warming across different mitigation scenarios.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
15278	59	41	59	41	This has been supeseded by Tebaldi and Knutti 2018 https://doi.org/10.1088/1748-9326/aabef2 that does not find particular challenges in scaling RCP2.6 on the basis of CMIP5 projections of average temperature and precip. For lower stailizaton (~1.5C) results in that paper (also showing good performance of pattern scaling techniques) only the single model (CESM1) ensembles are used though [Claudia Tebaldi, United States of America]	Taken into account.
57470	59	43	59	43	This is the first place pattern scaling is discussed or used since the Methodology section, I think. Could you move that discussion to to here? [Kyle Armour, United States of America]	Rejected. We prefer to consistently discuss all the methodologies applied in Chapter 4 in Section 4.2.
36072	59	54	63	29	As written this text is repetitive of previous sections, and re-assesses the same primary literature, variable-by-variable as the previous sections on near-term and long-term changes. I suggest that the text is shortened, that the only studies directly assessed in this section are publications on climate change at 1.5C, 2C etc, and that conclusions on general findings such as Arctic amplification, hydrological sensitivity, shifts in storm tracks etc, are either omitted, or if essential to the discussion on changes at different warming levels that the findings of 4.4 and 4.5 on these topics are simply summarised here. This should be a short section, updating the assessment of SR1.5 with any relevant new findings, showing key figures based on CMIP6, but not going through a detailed description and literature assessment of the changes variable-by-variable overlapping with that already done. [Nathan Gillett, Canada]	Noted. It is important from the perspective of informing policy-making to have a self-consistent section discussion changes a function of the level of global warming. We have, however, revised this section to minimize repetition of previous sections. Note that SR1.5 only assessed changes at 1.5 vs 2 degrees C of global warming, whilst this section also discuss changed at higher levels of warming. The section has been updated with the latest references and statistics of change for given levels of global warming, using the latest findings from CMIP6, amongst other publications.
37700	60	1			Change "pre-industrial levels" to "the early-industrial baseline" to be consistent with terminology introduced in Chapter 1. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. The term "pre-industrial" is clearly defined (also in the SOD) and is used consistently across Chapter 4 and the WGI report, which will also be consistent with the use of this term in SR1.5.
53412	60	9	60	9	check consistency with what is written elsewhere in the report about the elements of the polar amplification (and which are dominating) [Jan Fuglestvedt, Norway]	Taken into account. We have revised the text to largely refer the discussion of the mechanisms of polar amplification to the discussion in Section 4.5. This avoids repetition and in this way also ensure consistency of this component of the Chapter 4 discussion.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
15280	60	9	60	9	Maybe it could be noted here that the polar region is providing one of the main challenges to pattern scaling exactly because of the sensitivity of this feedback to level of warming when a threshold is exceeded (with for example the disappearance of ice/snow at high warming levels). I think any paper about pattern scaling contains some reflections on this challenge. Probably Tebaldi and Arblaster mentions that... [Claudia Tebaldi, United States of America]	Noted. Our discussion in this section is focussed on polar amplification, that is, on the relatively high rate of temperature changes in the polar regions. Polar amplification as such, at least for the Arctic, is in fact a phenomena that does scale well with increases in GSAT. We have elaborated on these aspects in the revised discussion.
36058	60	9	60	10	The statement that the largest warming is found in the NH high latitudes 'due to substantial ice-snow-abedo-temperature feedbacks' differs from the assessment on pg 44, ln 36-37 that 'it is now recognized that temperature (lapse-rate and Planck) feedbacks also contribute substantially to Arctic amplification'. [Nathan Gillett, Canada]	Taken into account. We have revised the text to largely refer the discussion of the mechanisms of polar amplification to the discussion in Section 4.5. This avoids repetition and in this way also ensure consistency of this component of the Chapter 4 discussion.
37702	60	14	60	15	What is meant by "the recent global warming of 0.5ºC"? The peak temperature anomaly in February 2016 was indeed around 0.5ºC higher than anomalies in 2014 and some preceding years, but temperature anomalies then decreased. They have now started to rise again, but are still well below values for late 2015 and early 2016. The text needs to be clearer here. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. This statement refers to approximations of 0.5 degrees C of global warming as obtained from the historical record. For example, there is a 0.5°C difference in GMST warming between 1991–2010 and 1960–197 (Schleussner et al., 2017), also, the warming since 1950 is approximately 0.5 degrees C (see SR1.5). We add this clarification to the text.
48060	60	27	60	27	There are a number of references to different sections of the Atlas which must be updated when needed (at the moment, they appear as ?Atlas, Section X.Y? and refer to CMIP6 models, which are not included in the Atlas yet). [WGI TSU, France]	Noted. The SOD has been composed to refer very specifically to other sections and chapters in the report that are relevant to Chapter 4, and this also holds for section 4.6.1.
15576	60	35	60	35	Are the authors referring to global mean precipitation or the sentence means that precipitation in all land areas will increase with increased global mean surface temperature? [Izidine Pinto, South Africa]	Noted. The sentence refers to global mean precipitation. Corrected in the text.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
15282	60	35	60	49	Is there anything to be said about uncertainties from dynamics/circulation changes that are less well understood, and probably diferent from model to model? Maybe something from the literature distinguishing thermodynamics and dynamics effects (probably Trenberth's work, or Ted Shepherd's)? [Claudia Tebaldi, United States of America]	Noted. Thank you for the comment. Section 4.5 addresses the thermodynamic and dynamic effects on precipitation changes. In Section 4.6.1 we so point out, in section 4.6.1.3, that at least in the high latitudes circulation changes scale considerably less well as a function of the level of global warming. We have extended this discussion compared to the info available in the FOD, siting some of the most recent and relevant literature.
36064	60	35	60	49	This section should reference and summarise the assessments of 8.2.1.1.1 and 8.2.1.1.2. [Nathan Gillett, Canada]	Taken into account.
44508	60	36	60	36	The word 'increases' should be corrected as 'increase' [Shaukat Ali, Pakistan]	Accepted. Thank you. Corrected.
36062	60	39	60	40	As written the text implies that there is a nonlinear relationship between precipitation changes and global mean temperature, but I think what the authors mean is that factors other than global mean temperature also influence precipitation, such as the mix of forcings, and whether the warming is transient or approaching equilibrium. Clarify this. [Nathan Gillett, Canada]	Noted. Yes, Section 4.2.4 discusses the pattern scaling on precipitation pattern and show that temperature change patterns conform better to pattern scaling approximation than precipitation patterns (Tebaldi and Arblaster, 2014). Precipitation response can be considered as fast response which are forcing-dependent and the slow response which are forcing-independent and scale with the change in GSAT. These are included in the text.
39008	60	40	60	40	The reviewer wonders if a figure could be produced to show the degree of linear scaling for temperature and precipitation patterns against GMST [Masahide Kimoto, Japan]	Noted.
15578	61	11	11	61	Is extreme precipitation here including heavy precipitation and lack of rainfall ("droughts")? If referring only to heavy precipitation the confidence language in chapter 11 is 'likely' while in here is 'very likely' [Izidine Pinto, South Africa]	Not applicable. Thank you for pointing out this. Since extreme precipitation is assessed in Chapter-11, we are excluding assessment of extreme precipitation in this section.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
55008	61	11	61	11	As a cross-cutting issue between working groups in the scope of extreme precipitation events, the intensity of precipitation is also increasing in urban areas with multiple drivers. For example, Song et al. (2019) recently report on “Changes in precipitation extremes in the Beijing metropolitan area during 1960–2012” that is published in Atmospheric Research 222: 134-153 at <https://doi.org/10.1016/j.atmosres.2019.02.006>. In the study, the multiple drivers of "global climate indices, urban expansion and topography on the change in precipitation extremes are also considered.” [Kilkis Siir, Turkey]	Not applicable. Thank you. Extreme precipitation assessment is excluded in this section as it is addressed in Chapter-11.
37704	61	11	61	12	See comment 192, on the copy of this text that appears in the opening summary. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account.
36066	61	11	61	19	This section should be consistent with and cross-reference Chapter 11. [Nathan Gillett, Canada]	Taken into account.
44510	61	13	61	13	The word 'increases' should be corrected as 'increase' [Shaukat Ali, Pakistan]	Accepted. Thanks. Corrected.
29994	61	21	61	23	The AR5 statement (at least in the SPM) included the important caveat "although there may be regional exceptions". We must get away from repeating the 'wet get wetter, dry get drier' meme when we know it doesn't work over land in the lower latitudes (as is said elsewhere in this chapter). [Theodore Shepherd, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Thank you. Sentence is removed.
40540	61	23	61	24	The sentence here on 1.5C and 2C monsoons appears to be unreferenced. One of many examples could be Chevuturi et al. https://doi.org/10.1007/s00382-018-4536-1 [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Noted.
44512	61	31	61	31	The word 'decreases' should be corrected as 'decrease' [Shaukat Ali, Pakistan]	Accepted. Thanks, corrected
29996	61	32	61	32	Do you not mean "statistically detectable" rather than "statistically significant"? [Theodore Shepherd, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Tebaldi et al., 2015 have shown that 2.5°C–3°C change is required for a statistically significant change in regional annual average precipitation
36068	61	34	61	35	Provide references to support the assessment on changes in precipitation variability in response to warming. [Nathan Gillett, Canada]	Taken into account. Thanks, reference included.
36070	61	35	61	37	Cross reference with Chapter 11. [Nathan Gillett, Canada]	Taken into account
37706	61	41	61	42	Likewise see comment 192, on the similar text that appears in the opening summary. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account
15580	61	41	61	42	Repetition. Is extreme precipitation here including heavy precipitation and lack of rainfall ("droughts")? If referring only to heavy precipitation the confidence language in chapter 11 is 'likely' while in here is 'very likely' [Izidine Pinto, South Africa]	Assessment of extreme precipitation is excluded from this section.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
39010	62	15			Presenting in terms of zonal mean may better visualize the degree of scaling? [Masahide Kimoto, Japan]	Rejected. In this section, we are interested in the spatial patterns of change, rather than in zonal means.
30738	62	29	62	35	this is a repetition, sect 4.6.1.4 could likely start from line 35 [Annalisa Cherchi, Italy]	Taken into account.
29998	62	29	62	41	It's very confusing (as well as repetitive) to discuss mean changes in the NAM and SAM in a different section to the midlatitude jet shifts, since they are effectively the same thing. This comment applies throughout Chapter 4. [Theodore Shepherd, United Kingdom (of Great Britain and Northern Ireland)]	Noted. These aspects are not exactly the same. Showing the spatial patterns of changes in wind speed also reveal changes in the easterlies, which are not directly related to SAM/NAM. We therefore discuss the spatial patterns of change separately and link the discussion to the subsections with the large-scale modes of variability (Including SAM and NAM).
30740	62	43	62	44	same as aboev, I would simply start the paragraph from the word "Despite" [Annalisa Cherchi, Italy]	Taken into account
39560	62	49	62	51	Leaving aside that in principle the conclusion sounds logical, and in order to contribute to integrate conclusions across sections related with SAM, from where does the "likely" category come from? How does this conclusion relate with that resulted from section 4.3.3.1 based on SSPs? [Carolina Vera, Argentina]	Noted. The entire section has been removed from the SOD. It has been decided of not applying epoch analysis to assess midlatitude atmospheric circulation changes and related annular modes of variability. The reason why has been clearly explained in section 4.6.1.3 of the SOD.
53414	63	36	63	37	The first sentence is unclear. Please improve and clarify. [Jan Fuglestvedt, Norway]	Taken into account
41682	63	36	64	6	There may be an overshoot in surface temperature after emissions cease as illustrated by a 1000 year experiments with an Earth system model (Froelicher et al., 2014). This peak in surface warming after carbon emissions cease is due to a decline in ocean heat uptake, which increases the proportion of radiative forcing used to increase surface temperature (Williams et al., 2017, GRL). Frölicher, T. L., Winton, M., & Sarmiento, J. L. Continued global warming after CO2 emissions stoppage. Nature Climate Change, 4, 40–44 (2014). Williams, R. G., Roussenov, V., Frölicher, T. L., & Goodwin, P.. Drivers of continued surface warming after cessation of carbon emissions. Geophysical Research Letters, 44 (2017). [Ric Williams, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. there may also be an undershoot. The Zero Emissions Commitment on decade-century timescales is now able to be assessed in multi-model simulations. These results have been used to inform the assessment in the SOD (section 4.7.2)

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
56352	63	44	63	44	It is important to make very clear that here you are talking about the relationship between climate respone and atmospheric CO2 concentrations. The relationship between CO2 emissions and several climate variables is linear. [Kirsten Zickfeld, Canada]	Taken into account
36074	63	44	63	46	The statement that 'There is strong evidence that climate response to CO2 forcing is not linear (high confidence)' is more general than the supporting evidence cited. Good et al. (2016) just examine regional precipitation changes under the first and second 2K of global warming and find differences. I suggest focussing this assessment on particular variables for which departures from linearity have been evaluated. [Nathan Gillett, Canada]	Noted. we cite wider literature and also draw on CMIP6 results to confirm or update this assessment
11544	63	44	63	51	The statements made here are very surprising to me. I disagree with most of them. First of all, many old studies found that the second 2C of warming looks a lot like the first 2C of warming on global and continental scales if the forcing is constant (i.e as in a 1% CO2 increase run). The notable exception are the large changes seen near the sea ice edge. This is the case for SAT and P. One has to be careful to eliminate or account for variability.The response-forcing relationship in 1% runs is remarkably linear, locally and globally for SAT - again if variability is accounted properly. "Climate sensitivity" which one? If equil. climate sensitivity is in view, Spelman and Manabe found this in 1975. Albedo feedback is state dependent as are the other feedbacks (but less so). [Roanld Stouffer, United States of America]	Taken into account. Thank you for the helpful comment. we assess CMIP6 results, especially from non-lin-MIP under CFMIP to determine the extent to which this assessment holds.
56358	63	44	63	51	From a policy perspective it would be important to include a paragraph assessing the linearity and path-indendence of the climate response to CO2 emissions. This is done to some extent in Chapter 5 section 5.5.1 (TCRE) but not in an exhaustive way. E.g. the regional climate response to CO2 emissions is not discussed there. [Kirsten Zickfeld, Canada]	accepted. Aspects of TCRE beyond global temperature are assessed
36076	63	44	63	51	This section should cross-reference 4.2.4 on pattern scaling. [Nathan Gillett, Canada]	Accepted.
56354	63	48	63	48	I assume you mean state independent? [Kirsten Zickfeld, Canada]	Accepted. Text is corrected
53416	63	48	63	49	You may add a ref to ch7 and what they write about state dependence [Jan Fuglestvedt, Norway]	Accepted.
56356	63	50	63	50	Again, it needs to be clarified that you are referring to the relationship between regional climate response and CO2 forcing. The relationship between regional warming and CO2 emissions has been shown to be largely path independent (Leduc et al., 2016, Nature Climate Change, volume 6, pages 474–478). [Kirsten Zickfeld, Canada]	Taken into account. here, we mean here CO2 concentration not emissions (and also non-CO2 concentrations)
39012	63	50	63	50	Can one notice the difference on maps? [Masahide Kimoto, Japan]	Taken into account.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
11546	63	50	63	51	This statement is true because the rate of change in the forcing does influence the pattern and magnitude of the warming. Stouffer, Ronald J., and Syukuro Manabe, 1999: Response of a coupled ocean-atmosphere model to increasing atmospheric carbon dioxide: Sensitivity to the rate of increase. Journal of Climate, 12(8), 2224-2237. [Roanld Stouffer, United States of America]	Taken into account. The text is revised in SOD.
15284	63	50	63	51	should this be more general about patterns at various levels, not just 2C? [Claudia Tebaldi, United States of America]	Taken into account. 2 degrees is shown because both high and low emissions scenarios reach this level of warming in most models
56360	63	51	63	51	I think it wpuld be worth unpacking a bit more why the climate response at a given level of warming is dependent on the combination of forcings. [Kirsten Zickfeld, Canada]	Taken into account. Results, e.g. from PDRMIP can be used to inform this assessment
12810	64	5	64	8	Overshooting the 1.5C goal risks offsetting feedbacks and tipping points—a large cluster of which exist between 1.5 and 2ºC of warming (Drijfhout et al 2015)—that could amplify warming and jeopardize successfully limiting warming to 1.5C. Furthermore, when the uncertainty of climate sensitivity and climate feedbacks like released carbon from permafrost thaw are considered, the "fat tail" risk of warming extends into the catastrophic range (Xu and Ramanathan 2017). Further, it can take millennia for CO2 levels to go down. Solomon S., et al. (2009) Irreversible climate change due to carbon dioxide emissions, PNAS 106: 1704–1709; Cheng et al (2019) How fast are the oceans warming?, Science (Perspectives), 363(6423):128–129. [Durwood Zaelke, United States of America]	Noted. Thank you for the literature suggestions. Tipping points are assessed in section 4.7.3
42328	64	5	64	8	Overshooting the 1.5C goal risks offsetting feedbacks and tipping points—a large cluster of which exist between 1.5 and 2ºC of warming (Drijfhout et al 2015)—that could amplify warming and jeopardize successfully limiting warming to 1.5C. Furthermore, when the uncertainty of climate sensitivity and climate feedbacks like released carbon from permafrost thaw are considered, the "fat tail" risk of warming extends into the catastrophic range (Xu and Ramanathan 2017). Further, it can take millennia for CO2 levels to go down. Solomon S., et al. (2009) Irreversible climate change due to carbon dioxide emissions, PNAS 106: 1704–1709; Cheng et al (2019) How fast are the oceans warming?, Science (Perspectives), 363(6423):128–129. [Gabrielle Dreyfus, United States of America]	Noted. Thank you for the literature suggestions. Tipping points are assessed in section 4.7.3

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
12654	64	5	64	8	Overshooting the 1.5C goal risks offsetting feedbacks and tipping points—a large cluster of which exist between 1.5 and 2ºC of warming (Drijfhout et al 2015)—that could amplify warming and jeopardize successfully limiting warming to 1.5C. Furthermore, when the uncertainty of climate sensitivity and climate feedbacks like released carbon from permafrost thaw are considered, the "fat tail" risk of warming extends into the catastrophic range (Xu and Ramanathan 2017). Further, it can take millennia for CO2 levels to go down. Solomon S., et al. (2009) Irreversible climate change due to carbon dioxide emissions, PNAS 106: 1704–1709; Cheng et al (2019) How fast are the oceans warming?, Science (Perspectives), 363(6423):128–129. [Kristin Campbell, United States of America]	Noted. Thank you for the literature suggestions. Tipping points are assessed in section 4.7.3
30368	64	5	64	8	This is also in part discussed in Chapter 5, but it would be useful to define geophysically what "overshoot" means. In the context of temperature, this emitting more cumulative CO2 than consistent with capping warming to a specific level, and then relying on actively removing CO2 from the atmosphere at a global scale to accelerate the reduction in radiative forcing so that temperatures come down again. A connection to the zero emissions commitment would be also of interest here. [Joeri Rogelj, Austria]	Taken into account. Relevant terms such as overshoot is defined here and also where relevant in the Glossary
57264	64	11	67	51	ch4 should decide - together with ch5 (and maybe even with WGIII) - on a consistent terminology around "carbon dioxide removal" and "negative emissions", which is able to clear distinguish between gross and net (CDR or negative emissions). Ch4 seems to favor CDR, ch5 seems to favor negative emissions. In my view, CDR is better to represent the process of removing CO2, while negative emissions might work better when describing global aggregates, first and foremost the state of net negative emissions [Oliver Geden, Germany]	Taken into account. We provide clarity in SOD in the introduction in section 4.6.3.2. We have also coordinated with Ch5 on the usage of 'carbon dioxide removal' and 'negative emissions'.
57266	64	13	64	14	True that this is "sometimes referred to as geoengineering", but since SR1.5 not by the IPCC anymore [Oliver Geden, Germany]	Taken into account. The term "geoengineering" has been used in the literature in the past (e.g. Royal Society Report 2009). We now discuss the past usage of this term but refrain from using this term.
55554	64	13	64	18	This definition or description of potential measures to reducing climate risk is incomplete and lacks appropriate reference to the diversity of ways through which sinks could be maintained or enhanced - leading to removals of CO2 alongside other potential forms of removal often referred to as Carbon Dioxide Removal (CDR) or Negative Emissions Technologies (NETs). [Matthisa Honegger, Germany]	Taken into account. CDR is discussed in detail in Chapter 5 where more references are cited. In this chapter, we discuss only the climate system response to SRM and CDR options. Text is revised

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
30370	64	13	64	25	I think this paragraph gets itself into trouble by trying to distinguish mitigation from "CDR", whereas the text below does not speak to "CDR" but to "net CDR". Of importance to the discussion here is the response to the net global removal of CO2. This can hence be quit easily described as one single and independent concept that is an extension of mitigation (i.e. it is a case of deep mitigation). The IPCC SR1.5 defined CDR as mitigation. Changing this definition again based on no new information is not very user-friendly. [Joeri Rogelj, Austria]	Taken into account. CDR is distinguished from mitigation in SR1.5. The revised text clearly discusses that CDR is included in some strong mitigation scenarios.
56362	64	14	64	14	My understanding of the outcome of the cross-chapter session on geoengineering is that we would avoid the term "geonengineering" in the report, as well as any other term that refers collectively to SRM and CDR (e.g. "climate intervention"). [Kirsten Zickfeld, Canada]	Taken into account. In the revised text, we refrain from using the term "geoengineering". The terms "climate engineering" or "climate intervention" are used only in this paragraph to provide a background to SRM and CDR.
53418	64	15	64	15	I suggest you insert "fundamentally different" or something like that before "categories" - since these two are often lumped together in various contexts [Jan Fuglestvedt, Norway]	Accepted. "fundamentally different" is inserted now.
35380	64	15	64	18	Solar Radiation Mangement (SRM) is not the approriate umbrella term, and accordinlg abbreviation, for all proposed radiation management measures. As expalined in section 4.6.3.3, cirrus cloud thining is targeting long wave radiation, making the 'solar' inapproriate. I think that the IPCC report should make an effort to correct the predominant abreviation of SRM as an umbrella and introduce a more appropriate term & abriviation, such as Radiation Managment (RM), or RMM (radiation management measure), which is used later on (page 70 line 10) already. [Nadine Mengis, Canada]	Taken into account. We continue to use the term SRM in this report as SRM is the term widely used in the literature. In this chapter SRM refers to Solar Radiation Modification (not Management). As in SR1.5, the terminology of SRM is used in this report to refer to all direct interventions on the planetary radiation budget, including both shortwave and longwave modification.
56364	64	17	64	17	Definition of SRM, CDR, mitgation needs to be consistent with the rest of report. Chapter 5 adopted definitions form SR1.5 glossary. [Kirsten Zickfeld, Canada]	Accepted. We use the SR1.5 definitions of SRM and CDR in the revised draft
16182	64	21	64	25	This is an incorrect and misleading portrayal, and the opposite of what is stated here is true: Strong and early mitigation reduces – or even eliminates – the need for so-called „negative emissions“. Large-scale CDR is deployed in weak-mitigation and high fossil fuel consumption scenarios. [Linda Schneider, Germany]	Taken into account. Text is revised.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
30372	64	24	64	25	The precise reference for the SSPx-1.9 scenarios is: Rogelj, J., A. Popp, K. V. Calvin, G. Luderer, J. Emmerling, D. Gernaat, S. Fujimori, J. Strefler, T. Hasegawa, G. Marangoni, V. Krey, E. Kriegler, K. Riahi, D. P. van Vuuren, J. Doelman, L. Drouet, J. Edmonds, O. Fricko, M. Harmsen, P. Havlík, F. Humpenöder, E. Stehfest and M. Tavoni (2018). "Scenarios towards limiting global mean temperature increase below 1.5 °C." Nature Climate Change 8(4): 325-332. [Joeri Rogelj, Austria]	Accepted. It is included now.
35382	64	27	64	29	Studies showed that the interaction between different Earth system processes are perturbed by both climate change (Mengis et al., 2018) and climate engineering (Mengis et al., 2019). This has consequences for the assessment of future climatic responses of these measures. This should/could be mentioned here. Further research is needed in this area. Citations: Mengis, N., Keller, D. P., and Oschlies, A.: Systematic Correlation Matrix Evaluation (SCoMaE) – a bottom–up, science-led approach to identifying indicators, Earth Syst. Dynam., 9, 15-31, https://doi.org/10.5194/esd-9-15-2018, 2018.; Mengis, N., Keller, D. P., Rickels, W., Quaas, M., & Oschlies, A. (2019). Climate engineering–induced changes in correlations between Earth system variables—implications for appropriate indicator selection. Climatic Change, 1-18. [Nadine Mengis, Canada]	Taken into account. It is a general statement that earth system processes are affected by climate change and climate engineering. This is already implied in the first paragraph of section 4.6.3 and hence we do not cite the references. On the other hand, we added the suggested references in the appropriate place in section 4.6.3.2 on the discussion of CDR.
53420	64	28	64	29	Please check if there should be a link to WGII also on this. [Jan Fuglestvedt, Norway]	Taken into account. The outline of WG2 report does not include SRM or CDR.
6337	64	32	71	18	Scenario/Regional based climate policy/adaptation constraints can be considered and integrated to better understand factors derailing full realization of climate policies [Isaac Sarfo, Ghana]	Noted, but we believe that the scenarios used in the model based assessment of mitigation, CDR and SRM do integrate climate policy and adaptation issues in the scenario development. Considerations of climate policy are beyond the scope here.
56366	64	36	64	37	Include references to Matthews et al., 2009, Nature 459, 829-832; Zickfeld et al., 2009, PNAS 106, 16 129-16134. [Kirsten Zickfeld, Canada]	Accepted. These two references are cited now.
30374	64	37	64	38	This sentence is confusing. One expects peak temperature around the time of net zero CO2 emissions. 2100 is an arbitrary year here. The sentence only speaks to "mitigation" without indicating that it is only mitigation that reaches net zero CO2 emissions before 2100 at the very latest that will result in warming peaking by 2100. Please edit. [Joeri Rogelj, Austria]	Accepted. Sentence is edited as per the suggestion.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
35384	64	38	64	38	The term 'overshoot' implies a following reduction of temperatures, which is not a given under mitigation-only scenarios. 'Exceedance' would be better. [Nadine Mengis, Canada]	Accepted. "overshoot" changed to "exceedance".
32812	64	40	64	43	If one is going to include the rough magnitude of warming from eliminating suphate aerosols (something that would be hard to do without eliminating emissions from all coal-fired power plants), then I'd urge including an estimate of the amount of warming offset that could be achieved by elimination of short-lived climate forcing agents, which is likely several times larger. While full elimination would be a challenge, significant reductions of not only black carbon, but methane, tropospheric aerosols, nd HFCs (which should all be mentioned) would also likely more than offset the warming that would be expected from any plausible path toward reduction of all sulphate aerosols. In that reductions of emissions of short-lived species is essentially the only way to have a significant moderating effect on the rate of temperature rise, I'd urge a much more positive description and coverage here. [Michael MacCracken, United States of America]	Taken into account. There is an entire chapter (Chapter 6) devoted to the SLCF which we have referred here. The text is revised and in SOD, SLCF is discussed in section 4.4
49532	64	40	64	43	These two sentence are correct in itself but appearing one after the other give a false picture of reality of mitigation. The GHG mitigation will remove more effectively SO2 than BC (since coal power sector and industry are primary goals....and these have mostly very small BC emission) and in fact lots of other species too. It is very difficult to cherry pick even if SO2 mitigation (in flue gas) is one example where principally SO2 only is removed (and gas has to be cleaned form particulate matter). I am not sure what the message is here, maybe that accelerated SO2 removel due to the ambitious air quality policies (on top of the climate straegy) will reduce temporarily the positive effect of GHG mitigation, although to be fair GHG policy itself also removes cooling species like SO2, NOx, OC...but slower indeed. [Zbigniew Klimont, Austria]	Taken into account. The text is revised and in SOD, SLCF is discussed in section 4.4. The main message is that the effects of GHG mitigation may be enhanced or reduced in the short term by SLCF.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
12812	64	40	64	47	Add that SLCPs reduce the rate of warming, and SLCP mitigation has powerful benefits in the near-term, even with the unmasked warming. UNEP (2017) The Emissions Gap Report, xv ("The report also covers an assessment of the potential contribution from reductions in short-lived climate pollutants (SLCPs), although they are not directly comparable with reductions in long-lived greenhouse gases. Reductions of SLCPs limit the rate of short-term warming, and when sustained and combined with CO2 reductions, these reductions also help to limit long-term warming, which is the ultimate aim of closing the emissions gap."); Xu et al 2013 ("This estimate is consistent with RX10, which would also yield 0.5 C avoided warming if only CH4, O3, and BC were mitigated. All three studies calculated that full implementation of mitigation measures for these three SLCPs can reduce the rate of global warming during the next several decades by nearly 50%. Furthermore, Arctic warming can be reduced by two-thirds over the next 30 yr compared to business as usual (BAU) scenarios (UNEP and WMO, 2011)."). [Durwood Zaelke, United States of America]	Taken into account. The text is revised and in SOD, SLCF is discussed in section 4.4. The revised text clearly indicates that there would be short term enhancement or reduction of temperatures from mitigation of SLCF emissions.
42330	64	40	64	47	Expand discussion to include that SLCPs reduce the rate of warming, and SLCP mitigation has powerful benefits in the near-term, even with the unmasked warming. UNEP (2017) The Emissions Gap Report, xv ("The report also covers an assessment of the potential contribution from reductions in short-lived climate pollutants (SLCPs), although they are not directly comparable with reductions in long-lived greenhouse gases. Reductions of SLCPs limit the rate of short-term warming, and when sustained and combined with CO2 reductions, these reductions also help to limit long-term warming, which is the ultimate aim of closing the emissions gap."); Xu et al 2013 ("This estimate is consistent with RX10, which would also yield 0.5 C avoided warming if only CH4, O3, and BC were mitigated. All three studies calculated that full implementation of mitigation measures for these three SLCPs can reduce the rate of global warming during the next several decades by nearly 50%. Furthermore, Arctic warming can be reduced by two-thirds over the next 30 yr compared to business as usual (BAU) scenarios (UNEP and WMO, 2011)."). [Gabrielle Dreyfus, United States of America]	Taken into account. The text is revised and in SOD, SLCF is discussed in section 4.4. The revised text clearly indicates that there would be short term enhancement or reduction of temperatures from mitigation of SLCF emissions. The revised paragraph provides a balanced view that there would be clear short term effects but very little long-term effects from mitigating SLCF.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
12656	64	40	64	47	Add that SLCPs reduce the rate of warming, and SLCP mitigation has powerful benefits in the near-term, even with the unmasked warming. UNEP (2017) The Emissions Gap Report, xv ("The report also covers an assessment of the potential contribution from reductions in short-lived climate pollutants (SLCPs), although they are not directly comparable with reductions in long-lived greenhouse gases. Reductions of SLCPs limit the rate of short-term warming, and when sustained and combined with CO2 reductions, these reductions also help to limit long-term warming, which is the ultimate aim of closing the emissions gap."); Xu et al 2013 ("This estimate is consistent with RX10, which would also yield 0.5 C avoided warming if only CH4, O3, and BC were mitigated. All three studies calculated that full implementation of mitigation measures for these three SLCPs can reduce the rate of global warming during the next several decades by nearly 50%. Furthermore, Arctic warming can be reduced by two-thirds over the next 30 yr compared to business as usual (BAU) scenarios (UNEP and WMO, 2011)."). [Kristin Campbell, United States of America]	Taken into account. The text is revised and in SOD, SLCF is discussed in section 4.4The revised text clearly indicates that there would be short term enhancement or reduction of temperatures from mitigation of SLCF emissions.
53422	64	41	64	41	More refernce should be added re mitigation of SLCF. Please consult ch6 authors [Jan Fuglestvedt, Norway]	Taken into account. The text is revised and in SOD, SLCF is discussed in section 4.4.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
32814	64	41	64	43	That sulphate aerosols are exerting a cooling influence on the climate is a very clear example of what SRM would be attempting to do, just in an advertent way that causes less health and ecological damage than the present mechanisms and patterns of emission of sulphate aerosol precursors. Were such precursors being emitted out over remote ocean areas and in a more dispersed way, it would likely make sense to keep this cooling influence as an offset to global warming, at least until the excess atmospheric GHG concentrations could be brought down. That the IMO (International Maritime Organization) is calling for the desulfurization of fuels for long-distance shipping is a step that, while needed to reduce air pollution and health damages in the world's ports, is actually in effect, going to exert a warming influence. Marine Cloud Brightening (MCB) using sea salt aerosols would, potentially, be one way to offset this warming component as would shifting global emissions of sulphur dioxide to a distribution of sites over vast ocean areas like the Pacific Ocean, creating a thin clear sky haze that, being over a dark albedo surface and also likely brightening clouds, seem to be a climate intervention approach with net climatic benefits (this notion is mentioned in MacCracken, M. C., 2016: The rationale for accelerating regionally focused climate intervention research, Earth's Future 4, 649-657, doi:10.1002/2016EF000450. It seems to me that cooupling the mention of the effects in this sentence to SRM would help in making clear that the world is already doing climate intervention, just not intentionally, and so doing so with foresight and intention would not be some radically different undertaking and merits consideration as a possible policy	Taken into account. Excellent comment. This regional SRM method is now mentioned in the first paragraph of section 4.6.3.3
36080	64	41	64	44	The warming caused by eliminating short-lived negative forcings from sulphate aerosols is not temporary - it will last indefinitely. It would only be temporary if sulphate aerosols were to increase again in the future. [Nathan Gillett, Canada]	Accepted good point. "temporary" is deleted in the revision
56368	64	42	64	43	This is only the case if the aerosol forcing is removed abruptly [Kirsten Zickfeld, Canada]	Taken into account. The sentence is modified in response to this comment.
56370	64	43	64	43	Correct reference for Matthews is Nature Climate Change, 2, 338-341, 2012. [Kirsten Zickfeld, Canada]	Accepted. The reference is changed.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
32816	64	43	64	47	It is interesting how it is suggested here that emissions of short-lived species do have regional influences on the climate and yet in considering intentional intervention via SRM there is resistance to suggestions that the technologies be used to induce regionally specific outcomes, such as focused cooling of the Arctic, as suggested in MacCracken, M. C., 2016: The rationale for accelerating regionally focused climate intervention research, Earth's Future 4, 649-657, doi:10.1002/2016EF000450 and earlier publications on this aspect of SRM. An approach to really inquiring about the potential for regional influences would be to investigate the extent to which the shift of the centroid of sulphate aerosol precursors (and so aerosols) from the countries bordering the North Atlantic (so US, Canada, Europe) to the countries of East and South Asia (so mainly China and India) had regional influences; if such analyses have been done, it would seem appropriate to provide references to the point about there likely to be regional effects from elimination of sulphate aerosol forcings. [Michael MacCracken, United States of America]	Taken into account. In response to this comment and a similar comment, these regional schemes and the reference suggested by the reviewer are cited.
56372	64	49	64	50	Explain relationship to CO2 sinks; i.e. atmospheric CO2 will continue to increase as long as emissions exceed removals by sinks. [Kirsten Zickfeld, Canada]	Taken into account. Thanks for suggesting us to put this in a simpler message. The suggested revision is done.
32818	64	49	65	1	While I tend to agree with the statement, there are quite a number of those who suggest that the sink rates for CO2 would stay elevated into the future; indeed, Chapter 5 has some loose statements suggesting that the sink amounts are determined by the total burden of human-induced CO2 excess rather than by the gradient created by the ongoing emissions of CO2 with respect to the adjustments to the terrestrial biosphere and pCO2 in the ocena mixed layer that have been occurring. I think it essential for the text here and elsewhere to be very clear on what is expected to happen to the atmospheric sinks of CO2 as emissions drop as there are leading scientists on both sides of the issue (e.g., Steve Pacala and Steve Hamburg gave a recent joint presentation suggesting the absolute sink rates would remain high as emissions went down and the CO2 excess would clear the atmosphere in order of 50-100 years, so quite different than stated here. It is essential to be very clear on what is expected and why. [Michael MacCracken, United States of America]	Taken into account. The paragraph is revised now.
9066	64	50			Is there a specific level in PgC per year that can be cited hererather than "a few"? [Anna Merrifield, Switzerland]	Taken into account. The amount is ~ 1-2 PgC and it would depend on the CO2 stabilization levels. The text is revised accordingly.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
30376	64	51	64	53	This sentence confounds stabilizing temperatures and stabilizing concentrations. Even with stabilzed concentrations temperatures would continue to increase for decades to centuries. Making this clearer would be important, unless there is any new evidence that net zero CO2 emissions is not anymore considered a requirement for temperature stabilisation. [Joeri Rogelj, Austria]	Taken into account. Thanks for this comment. Text is revised.
36082	64	52	64	53	AR5 assessed that 'stabilization of global temperatures…. requires decreasing emissions to near-zero' (12.4.5.4.2), but here the authors assess only that 'This implies a continued increase in surface temperature… until emission rates fall below the threshold level of a few PgC per year (high confidence)' i.e. that global mean temperature will stabilise with CO2 emissions of a few PgC per year. Is there new litereature or other evidence to support this? This also seems to contradict the assessment within this chapter on pg 73 lines 31-33 that even under a complete cessation of emissions 'some models continue the warming by up to 0.5 C, while others simulate little to no additional warming'. I suggest replacing with 'until emission rates fall to near-zero'. [Nathan Gillett, Canada]	Taken into account. This sentence is now revised.
35386	64	53	65	1	This statement should be validated and updated with the findings of the Zero-Emissions-Comittment MIP. There is substantial uncertainty about the sign and magnitude of the Earth system reaction to halted emissions. You could point to section 4.7.2 [Nadine Mengis, Canada]	Taken into account. The sentence is now revised so that the message is clearer. We also added discussion of latest finding on Zero-Emissions-Commitment and cited the section 4.7.2.
56374	64	55	64	55	In most studies surface air temperature does not decrease after emissions of long-loved GHGs are halted. [Kirsten Zickfeld, Canada]	Taken into account. The sentence is revised now.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
36084	65	3	65	4	This description of why temperatures and CO2 concentrations peak after emissions peak is inaccurate. The text attributes this to 'intertia and internal variability in the physical climate system and the global carbon cycle'. A lag between a peak in CO2 emissions and a peak in CO2 concentration in the atmosphere arises because CO2 has a non-zero lifetime in the atmosphere - not because of 'intertia and internal variability'. Moreover, CO2 concentration will only peak if the emissions rate is reduced very substantially, not just in response to any peak in emissions, as the text currently suggests. I suggest 'Because of the long atmospheric lifetime of CO2, under scenarios of decreasing emissions there would be a substantial lag betweewn a peak in CO2 emissions and any peak in atmospheric CO2 concentration, and because of the thermal inertia of the climate system, there would be a further possibly substantial lag in any peak in GSAT (see section 4.7.2).'. A peak in emissions rates does not by itself imply either a peak in concentration or a peak in temperture, as is currently implied. Moreover, internal variability is not the reason for these lags. [Nathan Gillett, Canada]	Taken into account. We agree with the reviewer and revised the text as per the reviewer's suggestion.
30378	65	3	65	5	This is explored for very high pulse emissions, but is there also evidence that there is a significant lag for policy-relevent trajectories of emissions? Ultimately, this is the "policy relevant" section. [Joeri Rogelj, Austria]	Taken into account. It is also true for policy relevant scenarios. We have cited a reference that uses a policy relevant emission scenario (Matthews Carbon Management 2010)
53424	65	3	65	15	This discusison on committment should be expanded - and linked to their parts of the report where this is treated. [Jan Fuglestvedt, Norway]	Taken into account. Thanks for the comment. The role of internal variability is large in decadal prediction, recent hiatus and detection and attribution in addition to its role in detection of mitigation benefits. We have linked this section to other parts of the report.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
36086	65	5	65	15	I think there is too much emphasis on the relative influence of internal variability here. If retained, I think this comparison of climate response in RCP2.6 and RCP4.5 would be more useful if it first compared the emissions, then the ensemble mean climate response, and finally commented on the role of internal variability. Just commenting on the internal variability part doesn't give the reader much context. e.g. 'For example, while CO2 emissions in RCP2.6 peak in xxxx and are x% lower over the 2021-2035 period than RCP4.5, the ensemble mean GSAT trend over this period is only x K/decade lower in RCP 2.6 than RCP 4.5 based on a large ensemble of simulations (Marotzke, 2019), and approximately a third of RCP 2.6 simulations simulate a larger trend over this period than over 2006-2020, as compared to a half of the RCP 4.5 simulations. Larger differences in the climate responses are, however, projected to occur in the mid to long-term (Section 4.5)'. This could in part be accomplished by merging this paragraph with the paragraph on lines 47-55. [Nathan Gillett, Canada]	Taken into account. In response, the text is revised in SOD.
36088	65	7	65	10	Do aerosol reductions play a role here? Aerosol reductions in all the RCPs will tend to contribute enhanced near-term warming in all the RCPs. See for example Gillett and Von Salzen (2013; https://iopscience.iop.org/article/10.1088/1748-9326/8/3/034008). [Nathan Gillett, Canada]	Taken into account. reduction in sulphate aerosols would have a small warming effect as discussed in section 4.4 in SOD. However, here the discussion is focussed on the role of internal variability in the near term benefits of mitigation. The important message is that although the ensemble mean warming is less in RCP2.6, there is still finite probability that the warming rate would be larger in RCP 2.6 than in RCP 4.5 because of internal variability. The suggested reference is cited now
36508	65	10	65	11	It is incorrect to describe RCP 4.5 as a 'no-mitigation scenario'. RCP 4.5 has strongly declining CO2 emissions from ~2040 onwards. See e.g. IPCC AR5 Figure 6.25. [Nathan Gillett, Canada]	Accepted. We remove the word "no-mitigation"
56376	65	14	65	14	It is important to emphasize that the faster rate of warming in the near-term would also occur in non-mitigation scenarios. [Kirsten Zickfeld, Canada]	Accepted. We changed the word "despite" to "with or without" to include both RCP 2.6 and 4.5 scenarios.
35388	65	14	65	15	This is true as a result of our current CO2 emissions. This should be made clear. [Nadine Mengis, Canada]	Noted, but the main message here is on the role of internal variability on the near term trends.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
56378	65	30	65	30	Repeats information provided in section 4.6.1 [Kirsten Zickfeld, Canada]	Taken into account. We review the findings in SR1.5 that are relevant here. We now cite this also section 4.6.1
32820	65	30	65	45	Is the effects of different stabilization levels on sea level intentionally being left out here? In that paleoclimatic considerations suggest an equilibrium sea level sensitivity of something like 20 meters per degree C, the amounts of ultimate sea level rise also differ significantly across the various scenarios, although what is clear is that the near-term influence of scenario differences is very small (IPCC 1.5 report cited a 0.06 m difference in SL at 2100, implying this was significant even though the rate of SL rise in 2100 would be of order 0.1 m/decade at that time, which is well within any reasonable uncertainty analysis. I'm just suggesting here that the implications for SL rise need to be mentioned here, in that the amount of eventual SL rise will likely be far harder to adjust to than the eventual rise in global average temperature. [Michael MacCracken, United States of America]	Noted, but there is no literature on the probabilistic assessment of the "near term (next 2 decades)" benefits of mitigation on sea level rise. Hence such as assessment for sea level rise is not discussed here.
36090	65	30	65	45	This paragraph mostly concerns projected changes in climate extremes, but it does not refer to Chapter 11. This material should mainly be assessed in Chapter 11, and summarised here if relevant. [Nathan Gillett, Canada]	Noted, we do discuss the projected changes in extremes. However, here the main emphasis is not on projections per se. Rather, the focus is clearly distinguishing "climate system response" to mitigation by comparing two scenarios. In the revision, we now refer to chapter 11 for a detailed discussion on changes in extremes.
41102	65	35	65	35	Delete "an" [Alan Robock, United States of America]	Accepted. Deleted "an"
36510	65	40			I recommend not naming individual projects - just cite and assess the publications arising from them. [Nathan Gillett, Canada]	Accepted. We have replaced project name with "several studies"
53426	65	47	65	47	The text on detection time for mitigation is very relevant. Are there similar studies for other components that could be referred to and used here? (CH4, aerosols etc) [Jan Fuglestvedt, Norway]	Taken into account. There is one study (that is submitted now) that looks at this issue for various forcing agents. We discuss this in SOD now.
36092	65	47	65	55	I suggest merging this paragraph with lines 3-15, since they both concern the emergence of the response to mitigation in emissions, CO2 concentration and GSAT. [Nathan Gillett, Canada]	Taken into account. In the revision, we moved the discussion of the benefits of mitigation before discussing the role of internal variability in detection of mitigation benefits and delayed benefits of mitigation

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
36094	65	50	65	51	This sentence refers to time of detection of differences in CO2 concentration - is this for emission driven simulations? Mention this and cite the underlying studies. [Nathan Gillett, Canada]	Taken into account. The text is revised.
36096	65	51	65	52	Say what averaging period this applies to. The detection time for 5-yr mean GMST will be different than the detection time for monthly mean GMST. [Nathan Gillett, Canada]	Taken into account. The method is briefly explained in the revision.
32822	65	51	65	53	Isn't this all theoretical given that actual emissions have been following a high emissions scenario over the past couple of decades, so the time for actual detection of the temperature difference will be of order 35 years after the world actually gets shifted to a different scenario than it has been on. It seems to me suggesting detection in 2035 is thus far earlier than will be the actual case. [Michael MacCracken, United States of America]	Taken into account, it is hypothetical. Thus, the high emission scenarios are called counterfactual scenario. In the revised text, we use the term "counterfactual" to describe the RCP 4.5 and RCP 8.5 scenarios as in the original paper Tebaldi and Friedlingstein 2013 PNAS.
30380	65	52	65	52	It might be useful to indicate that global mean temperature change in the original study was expressed in GSAT, if I remember correctly. [Joeri Rogelj, Austria]	Taken into account. There is big confusion in the literature on the usage of GMAT and GSAT. Several paper simply use the terms "global mean temperature", "global temperature", "surface temperature, "temperature". In the revised text, we have used uniformly used the term "global mean temperature" in the section 4.6.3
37708	65	52			"GMST" appears on this line, but GSAT appears twice in the rest of the paragraph. Can "GMST" be changed to "GSAT"? [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Both GMST and GSAT are used to refer to the temperature in Tebaldi and Friedlingstein. Hence, it is not clear what the authors really used. We use the term “global mean temperature” in the revision
32824	65	53	63	55	Yes, signal-to-noise is the issue, but the reason is the thermal lag in the climate system creates a long time constant to a new equilibrium, or even getting part way to a response, especially given the time constants of the oceans and cryosphere. [While this is pointed out in subsequent paragraphs, it would really help reader to mention the main issue right at the start of the consideration of the issue when reader is hungry for the explanation.] [Michael MacCracken, United States of America]	Accepted. We modified the sentence as per the reviewer's suggestion.
36098	66	1	66	10	Clarify that the detection time will depend on the averaging period considered, as well as the spatial averaging. [Nathan Gillett, Canada]	Taken into account. The method is briefly explained in the revision.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
36100	66	9	66	10	Although it is the case that uncertainties in climate sensitivity may cause overlap of projected changes in some variables/averaging periods for different scenarios by the end of the century, if the question is by when will be able to detect the benefits of mitigation measures, observational constraints could/should be considered. For example, if emissions were to follow RCP 2.6, by 2060 we would have very strong observational constraints on climate evolution under RCP 2.6, and we would also be able to use these constraints to limit uncertainties in simulated climate change under RCP 8.5. So we would be better able to separate climate evolution under the two scenarios using observational constraints, than by comparing e.g. unconstrained RCP 2.6 and RCP 8.5 CMIP5 simulations relative to preindustrial. I'm not sure if there is literature on this yet, but the text could acknowledge that future observations may make the response to mitigation more detectable than is implied by an unconstrained multi-model ensemble. [Nathan Gillett, Canada]	Taken into account. We agree that observationally constrained would help to narrow down the uncertainty in detection time. As per the suggestion, the text is revised in SOD
15286	66	17	66	17	Also Aerenson et al surveyed a suite of ETCCDI indices and their behavior under 1.5C and 2.0C https://doi.org/10.1088/1748-9326/aaafd6 [Claudia Tebaldi, United States of America]	Taken into account. The text is revised.
36102	66	28	66	31	First - the meaning of the sentence is not clear. Is it saying that the range of regional climate states in RCP8.5 is further removed from today's climate than RCP 4.5 is removed from today's climate (not surprising)? Or that RCP8.5 is further removed from today's climate than it is from RCP 4.5 (also not surprising)? Finally, to be meaningful, information on the variables and averaging periods needs to be added here. The distribution of decadal mean continental mean temperature may be well-separated in RCP 8.5 from the distribution in RCP 4.5, but the distribution of grid point daily precipitation may be very similar. [Nathan Gillett, Canada]	Taken into account. The text is modified to indicate that the range of regional climate states in RCP8.5 is further removed from today's climate than RCP 4.5 is removed from today's climate. Text is revised in SOD
55562	66	36	66	63	This introduction is a bit confusing with regard to the potential contribution of CDR: CDR by itself does not lead to a reduction in atmospheric conentrations, this only occurs when residual emissions (after deep emissions cuts) are smaller than the amounts of CO2 removed via CDR and natural sinks (meaning global net-negative CO2-emissions) have been achieved. Please refer to the appropriate section in the IPCC SR15 on this to be consistent. In elaborating this, it would seem helpful to also introduce and explain the notion of overshoot-and-return (CO2-concentration overshoot). Also it would be helpful in this context to explain that CDR plays a different, but nonetheless important role in the period before reaching net-zero emissions. [Matthisa Honegger, Germany]	Taken into account. The comments are appreciated. The original text clearly states that "mere deployment of CDR would not cause reduction in atmospheric CO2 levels, and CO2 removed by CDR should exceed emissions for CO2 levels in the atmosphere to decrease". Further, in response to the last part of the comments, we have revised the text.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
57268	66	40	66	42	It's unclear what the term "scheme" refers to here (and throughout the section). Is it a certain program of deployment, or an individual CDR option (like BECCS), or both? I think "scheme" works better for describing a program for deployment [Oliver Geden, Germany]	Accepted. "scheme" changed to "option"
16174	66	40	66	46	The statement presupposes that „all CDR schemes“ are actually capable of removing CO2 from the atmosphere. However, none of these technologies are proven to work, especially at scale, and their overall lifecycle emission balance raise serious doubts about their carbon negativity. These substantial uncertainties should be reflected here rather than making it sounds as if CDR schemes were a reliable instrument for limiting global warming. [Linda Schneider, Germany]	Taken into account. We replace the terms "schemes" and "methods" with "options" or "approaches" to make it clear these are just proposals. We also add a sentence "However, it should be cautioned that none of these proposals are proven to work, especially at scale, and their overall lifecycle emission balance raises questions about their carbon negativity."
30384	66	40	66	46	These couple of sentences are very confusing. The first sentence says that all CDR schemes would result in a reduction in atmospheric CO2 concentration, and a following sentence says the opposite. Editing for consistency would be useful here. [Joeri Rogelj, Austria]	Taken into account. The text is revised.
30386	66	40	66	46	This seems to be wrong. CO2 removed does not have to exceed CO2 emitted for CO2 levels in the atmosphere to decrease. Simply bringing the net emissions below the threshold referred to in 4.6.3.1 would be sufficient. Please correct. [Joeri Rogelj, Austria]	Taken into account. In response to several comments, Text is revised accordingly.
30382	66	41	66	41	Maybe include "compared to a situation without CDR". [Joeri Rogelj, Austria]	Taken into account. This sentence is revised in response.
32826	66	42	66	46	This needs to be revised to make it a bit clearer to average reader--at present it seems quite confusingly phrased. [Michael MacCracken, United States of America]	Taken into account. The text is revised in response to this and other comments
36104	66	45	66	46	Net negative CO2 emissions are expected to cause a cooling not just a 'reduction in rate of warming'. [Nathan Gillett, Canada]	Taken into account. We revised the text to convey the cooling that follows the net negative emissions
16176	66	48	66	49	That is a false statement. There are IAM scenarios even for 1.5°C that do not rely on CDR, or only to a very limited extent. Even if there is a majority of IAM scenarios for 2°C that rely on large-scale CDR, that has primarily to do with the way these models have been run prior to SR1.5 and the low priority that has been placed on avoiding speculative and risky CDR schemes. It clearly does not justify the statement that limiting global warming to 2°C above pre-industrial levels by 2100 is „difficult“ without the use of CDR schemes. [Linda Schneider, Germany]	Taken into account. We modified the text.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
30388	66	48	66	49	This statement can also refer to 1.5°C and the IPCC Special Report on Global Warming of 1.5°C, where this is discussed explicitly in Chapter 2, Section 2.3.4 "CDR in 1.5°C Pathways" [Joeri Rogelj, Austria]	Taken into account. Text is revised to include 1.5 deg warming scenario. SR1.5 report is also cited in this sentence.
56028	66	48	66	49	Rickels is meant to 2c°C,how about1.5°C?The technological options considered are just a few,what does it mean it is "difficult" [Roque Pedace, Argentina]	Taken into account. The revised text addresses the comment by this reviewer as well as others
12814	66	48	67	3	Nature-based solutions should be a priority as they can provide more than a third of cost-effective CO2 mitigation needed before 2030. Brandon W. Griscom et al., Natural Climate Solutions, Proceedings of the National Academy of Sciences (2017). Direct air capture removal technology should be able to scale up to play important role in achieving carbon neutrality, with non-severe foreseeable side effects, especially in forms of land use or competition with other uses. The key limitation is cost – but this is not represented as written (severe side effects). See Sabine Fuss et al., Negative emissions—Part 2: Costs, potentials and side effects, Environ. Res. Lett. (May 2018). [Durwood Zaelke, United States of America]	Noted. Appreciate the comments. Our scope here is the assessment of the science of the climate system response to CDR. We do not make cost assessments which are likely made in WG3 report
53428	66	49	66	49	Seem strange to ony refer to this. At least you could add a ref to SR1.5 [Jan Fuglestvedt, Norway]	Taken into account. The SR1.5 report is now cited.
57270	66	49	66	49	Unclear what is meant here by "Use of CDR schemes". As a matter of fact, all scenarios with low stabilization need to deploy some CDR to offset residual CO2 and GHG emissions (e.g. from agriculture) to get at least close to net zero GHG. Under the the new carbon budget, scenarios might not necessarily need net negative emisions [Oliver Geden, Germany]	Taken into account. Text is now revised.
16178	66	49	67	3	These are important new insights and findings and should be emphasized. Add an assessment of to what extent rebound effect and outgassing are reflected in IAM modelling that relies on large-scale CDR? What are the consequences that should be drawn from this? [Linda Schneider, Germany]	Noted. Our scope here is the assessment of the science of the climate system response to CDR. A detailed assessment of the carbon cycle response to CDR is made in Chapter 5 as indicated in the first paragraph of this section.
56380	66	52	66	52	Include references to Tokarska & Zickfeld, 2015, ERL 10, 094013; Vichi et al., 2013, Climatic Change 118, 105–118 [Kirsten Zickfeld, Canada]	Accepted. They are cited in the revision
30390	66	53	66	53	This could best be supported by a couple of references that use the "rebound effect" term in this context. [Joeri Rogelj, Austria]	Taken into account. The text is revised in SOD with no discussion on the rebound effect. The rebound effect is discussed in Chapter 5 as it is a biogeochemical response

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
32828	66	53	66	55	The key issue regarding this point, however, is the timing of all of this. CO2 goes into the ocean and into the terrestrial biosphere through processes that are not directly reversible and the processes governing return to the atmosphere would seem to have much longer time constants. For example, CO2 goes into the deep and interior ocean by downwelling and the biological pump, but comes back to the surface through upwelling--and the deep ocean component has a time constant of something like 1000 years (even before overturning rate is reduced by climate change), so the rebound effect could be spread over a long time, making CDR easier. Similarly, storage of C in roots, etc. can take a relatively long time to rebound. So, while eventually one might have to remove virtually all (an increased amount goes into sediments, and probably don't have to deal with that), eventually is a long time--and it would be of great help if this point was made and information given regarding this. I do agree that for the ocean mixed layer, the rebound effect would be virtually instantaneous. [Michael MacCracken, United States of America]	Taken into account. The text is revised in SOD with no discussion on the rebound effect. The rebound effect is discussed in Chapter 5 as it is a biogeochemical response.
35390	66	53	66	55	This statement needs to be set into a time scale context. On shorter time scales (e.g., until 2100) the ocean carbon that has entered into the deep ocean or even reacted with ocean sediments, will not have to be removed. [Nadine Mengis, Canada]	Taken into account. The text is revised in SOD with no discussion on the rebound effect. The rebound effect is discussed in Chapter 5 as it is a biogeochemical response.
30392	66	53	66	55	This is too simplistic unless time horizons are provided.On timescales of relevance to society (a couple of centuries) this would not be correct: Mathesius, S., M. Hofmann, K. Caldeira and H. J. Schellnhuber (2015). "Long-term response of oceans to CO2 removal from the atmosphere." Nature Clim. Change 5(12): 1107-1113. [Joeri Rogelj, Austria]	Taken into account. The text is revised in SOD with no discussion on the rebound effect. The rebound effect is discussed in Chapter 5 as it is a biogeochemical response.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
36106	66	55	67	3	There are a number of issues with this assessment of the effects of CDR. First, the text refers to CDR 'at scales as large as currently represented in the RCP8.5 scenario'. But RCP 8.5 doesn't require CDR - it still has large positive CO2 emissions in 2100 (see e.g. IPCC AR5 WGIII Figure SPM.4). Second the statement on the effectiveness and side effects of CDR, which is taken verbatim from Keller et al. (2014), refers to 'all CDR methods', but Keller et al. only consider a subset of CDR methods. In particular, Keller et al. (2014) do not consider BECCS, which is the CDR method most assessed in AR5 (e.g. SPM4.1 of IPCC AR5 WGIII). Third, according to page 66, line 37, this chapter only assess the climate response to CDR, and not the biogeochemical response, which is assessed in Chapter 5. But the potentially severe side effects referred to in Keller et al. (2014), from which this text is taken, are mainly biogeochemical. This assessment should be replaced with one focussed on the climate response to CDR, based on all relevant literature, and also considering BECCS and other CDR approaches not considered by Keller et al. (2014). Biogeochemical side effects of CDR are already assessed in Chapter 5, and should not be assessed here. [Nathan Gillett, Canada]	Taken into account. Text is now revised.
32830	66	55	67	3	Indeed, a key point to be made about both CDR and SRM is that their capacity is limited, and, critically, they are not at all a substitute for mitigation. The world has surpassed the levels of CO2 and other GHGs that are consistent with meeting the Objective of the UN Framework Convention and so mitigation is essential to stop making the problem worse, CDR to get back to where the Objective requires, and SRM is very likely needed to shave off at least some of the warming that is resulting from our earlier failures to cut emissions to zero. [Michael MacCracken, United States of America]	Noted. The scope here is only to assess the climate response to CDR and SRM. Also, we are mandated to only assess policy relevant science and do not make policy prescriptive statements.
48130	66		67		Both Sections 4.6.3.2 and Section 5.6 cover ocean and terrestrial out-gassing of CO2 after implications of CDR. [WGI TSU, France]	Taken into account. While section 4.6.3.2 summaries the key aspects of CDR, section 5.6 provides an in-depth discussion of CDR. In section, 4.6.3.2, the main focus in the climate system response to CDR. We cite chapter 5 in several instances in the revised text.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
35392	67	1	67	3	Sonntag et al., 2018, assesses the potential of afforestation, and ocean alkalinity enhancement under the RCP8.5 scenario to reach the RCP4.5 climate state with respect to predefined climate metrics in the MPI-ESM_LR. Mengis et al., 2019, performed the same simulations with the UVic ESCM and performs a comprehensive assessment of possible side effects. Sonntag et al., 2018, show higher potential for the ocean alkalinity enhancment. Mengis et al., comprehensivley assess the Earths system variables that are perturbed by these intervention. These references should be added here. Sonntag, Sebastian, et al. "Quantifying and comparing effects of climate engineering methods on the Earth system." Earth's Future 6.2 (2018): 149-168.; Mengis, N., Keller, D. P., Rickels, W., Quaas, M., & Oschlies, A. (2019). Climate engineering–induced changes in correlations between Earth system variables—implications for appropriate indicator selection. Climatic Change, 1-18. [Nadine Mengis, Canada]	Accepted. The references are cited in the revised text.
56032	67	1	67	3	Keller is considering just a few CDR methods,warming reduction could be higher than 8% and side effects are still to be studied [Roque Pedace, Argentina]	Taken into account. Text is now revised.
56382	67	2	67	2	Need to explain that CDR methods are applied at their maxium potential. [Kirsten Zickfeld, Canada]	Taken into account. Text is now revised.
53430	67	2	67	2	8% of what? How calculated? [Jan Fuglestvedt, Norway]	Taken into account. Compared to the case where no CDR option was employed. Text is revised
9068	67	2	67	3	Could these "potential severe side effects" be briefly detailed here? [Anna Merrifield, Switzerland]	Taken into account. The text is revised.
56386	67	5	67	5	This is confusing as stated. You explained the relationship between CDR and atmospheric CO2 earlier and in my mind this paragraph should be framed in terms of the lag relative to the decline in atmospheric CO2. [Kirsten Zickfeld, Canada]	Taken into account. We revised the sentence to also include the lag in atmospheric CO2 response
55564	67	5	67	6	This sentence would be clearer if it made explicit that the climate system exhbits inertia with regard to the CO2 concentration, and that therefore the inertia applies as much to emissions reductions as it does to CDR. [Matthisa Honegger, Germany]	Taken into account. The text is revised to discuss the lag in atmospheric CO2 response too.
56388	67	6	67	6	It needs to be mentioned that the 1% rampdown scenarios discussed in this paragraph and shown in Fig. 4.41 requires very large and likely infeasible amounts of CDR. The authors may consider moving the discussion of the 1% ramp-up, ramp-down experiments to section 4.7.2 and focus the discussion in this section on results from studies with more plausible CDR scenarios (e.g. Tokarska & Zickfeld, ERL, 2015; Jones et al.,ERL, 2016). [Kirsten Zickfeld, Canada]	Noted. The 1% ramp up and down scenarios are unrealistic. However, where possible, we need to assess the robustness of the science discussed here. Therefore, we have decided to use the CDRMIP results in this section.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
16180	67	10	67	12	Given that 2100 is the timeframe that climate policy operates in, should a time lag of over 1000 years not qualify as virtually „irreversible“? Given that sea level rise is of utmost important for many countries and communities, it should be emphasized that sea level rise will essentially not be reversed by CDR and that land lost to sea level rise will not be able to be retrieved. [Linda Schneider, Germany]	Taken into account. The text is revised.
8560	67	10	67	12	Cross-check with ch 9 (9.6.3.5) [Robert Kopp, United States of America]	Taken into account.
56390	67	12	67	12	Include reference to Tokarska & Zickfeld, ERL, 2015 [Kirsten Zickfeld, Canada]	Accepted. The suggested reference is now cited
32960	67	17	67	25	For consistency, it would be best to use the same models for thermosteric sea level here and in Ch9 [Aimee Slangen, Netherlands]	Noted. However, there are very limited CDR studies. Further, it is important to assess the robustness of model produced science. Therefore, the results from CDRMIP study are illustrated
26872	67	22			The definition of Risk = likelyhood x impact is fundamental. This definition is missing in the introduction Chapter 1 (page 24). By interpreting this definition you can show that risk can be mitigated by either reducing the likelyhood or the impact. [Thomas Ackermann, Germany]	Rejected. Risk assessment is not within the scope.
7968	67	24	67	25	There are also similar experiments for the AMOC showing diverse results in doi: 10.1007/s00382-013-1842-5 and 10.1007/s00382-014-2391-2 [Laura Jackson, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The text is revised.
39014	67	31	67	31	Is the explanation of "fast adjustment" provided anywhere in the report? [Masahide Kimoto, Japan]	Taken into account. fast adjustment is discussed in Chapter 7. We cite this chapter in the revised text
16184	67	37	67	41	what does that mean exactly? Explain in more detail the relationship between „negative emissions“ and the lagged response of the deep ocean. [Linda Schneider, Germany]	Taken into account. We changed "negative emissions" to "net negative emissions". "lagged" is changed to "delayed" for clarity
32834	67	43	67	44	This is confusing. If CDR is working, then the temperature would be dropping, so how would its termination cause increasing warming trends--perhaps a return to warming or less cooling (as the system would still be adjusting to the lowered CO2 concentration), but "increasing warming trends"--huh? [Michael MacCracken, United States of America]	Taken into account. The text is revised.
35394	67	43	67	44	This statement is only true under high emission 'background' scenarios, and large scale deployment. There was no substantial termination effect found in Keller et al., 2014. This statement should be extended to match the tone of the respective SRM statement (p. 71 l. 17-18). [Nadine Mengis, Canada]	Taken into account. The text is revised.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
57272	67	43	67	44	Unclear again what is meant by "scheme" here, all the more the sentence makes only sense if referring to net negative emissions (otherwise there would be no outgassing, see also ch5) [Oliver Geden, Germany]	Taken into account. The text is revised.
30394	67	43	67	45	This is incorrect, because it would only be true if substantial global net CDR is achieved. As this section does not distinguish net global CDR from CDR deployment in general, it is really hard to avoid such fundamental confusion. [Joeri Rogelj, Austria]	Taken into account. The text is revised.
12816	67	43	67	51	Recommend including more examples for this broad point. Ocean sequestration is hardly representative of CDR, and is not the preferred deployment method of most policymakers. For example, nature-based solutions should be put first, as natural climate solutions can provide more than a third of cost-effective CO2 mitigation needed before 2030. Brandon W. Griscom et al., Natural Climate Solutions, Proceedings of the National Academy of Sciences (2017). Direct air capture removal technology should be able to scale up to play important role in achieving carbon neutrality, with non-severe foreseeable side effects, especially in forms of land use or competition with other uses. The key limitation is cost – but this is not represented as written (severe side effects). See Sabine Fuss et al., Negative emissions—Part 2: Costs, potentials and side effects, Environ. Res. Lett. (May 2018). [Durwood Zaelke, United States of America]	Noted, but the various CDR options and their maximum potential are discussed in Chapter 5, as mentioned in the first paragraph of this section. Cost estimates are beyond the scope of the assessment here.
16186	67	43	67	51	The „termination effect“ is usually associated only with SRM. It would be useful to highlight here that recent scientific findings that the same termination effect applies to CDR. [Linda Schneider, Germany]	Taken into account. The last paragraph of this section that discusses the termination effect is now completely revised. We note that the effect is there but it is not as large as in SRM
12658	67	43	67	51	Recommend including more examples for this broad point. Ocean sequestration is hardly representative of CDR, and is not the preferred deployment method of most policymakers. For example, nature-based solutions should be put first, as natural climate solutions can provide more than a third of cost-effective CO2 mitigation needed before 2030. Brandon W. Griscom et al., Natural Climate Solutions, Proceedings of the National Academy of Sciences (2017). Direct air capture removal technology should be able to scale up to play important role in achieving carbon neutrality, with non-severe foreseeable side effects, especially in forms of land use or competition with other uses. The key limitation is cost – but this is not represented as written (severe side effects). See Sabine Fuss et al., Negative emissions—Part 2: Costs, potentials and side effects, Environ. Res. Lett. (May 2018). [Kristin Campbell, United States of America]	Noted, but the various CDR options and their maximum potential are discussed in Chapter 5, as mentioned in the first paragraph of this section. Cost estimates are beyond the scope of the assessment here.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
30396	67	43	67	51	This paragraph gives only partial information and doesn't mention the CDR measures mostly considered in mitigation pathways: bioenergy combined with carbon capture and storage (BECCS), afforestation, or direct air capture with sequestration (DACS). A balanced discussion of the implications of a sudden stop of these measures would be useful. [Joeri Rogelj, Austria]	Taken into account. The text is revised in SOD. We make assessments based on only published literature.
56034	67	43	67	51	seaweed cultivation and other managemnt of oceans could be considered as CDR ,not just upwelling or fertilization.what would be the consequence of termination? [Roque Pedace, Argentina]	Taken into account. The CDR options are extensively discussed in Chapter 5 as mentioned in the first paragraph of section 4.6.3.2. The text is revised. We make assessments based on only published literature. Therefore, we do not assess the termination effects of seaweed cultivation and other management of oceans.
32832	67	45	67	48	Please explain the mechanism for this rather significant change. Indeed, is the significance of this being exaggerated because this is apparently an effect over a limited region and is being compared then to the rate of change of a global scale response. Some further explanation is needed. [Michael MacCracken, United States of America]	Taken into account. The text is revised to clearly indicate that the global scale termination effects are rather small for CDR
32836	67	46	67	48	I would think there would be a reference here to where this issue is discussed. And this notion that SRM might be ended when it is relatively low cost and far less expensive than adapting to the warming conditions is a bit strange to be featuring. Yes, the global political system is irrational, but it seems a quite low risk, and this needs to be mentioned. [Michael MacCracken, United States of America]	Taken into account. Our scope is only the assessment of the climate system response to SRM. Cost and risk is beyond the scope of WG1 assessment. Therefore we do not make any statement regarding cost, risk or political system.
36108	67	54	71	18	Ocean acidification is not assessed in the context of SRM, but should be. [Nathan Gillett, Canada]	Taken into account. This is discussed in Chapter 5 as indicated by our statement that Chapter 5 assesses the biogeochemical implications. In the revision, we mention the ocean acidification issue briefly.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
32080	67	54	71	18	I find the framing of these assessments extremely misleading and potentially very dangerous. This is another area where a risk assessment framing is essential. One of the most fundamental issues for any geo-engineering proposal is not what is the most likey response, but rather what could go wrong and what are the potential very high impact side effects. Rather than concluding that various goals "... can be met simultaneously (low confidence)" an appropriate assessment should be that there is huge uncertainty and risks associated with all such strategies. And specific risks should be described and assessed carefully and quantitatively. [Rowan Sutton, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. In the revised version, we address the framing issue by discussing the large uncertainties associated with SRM. However, risk assessment is beyond the scope of this report.
53432	67	54	71	18	When I read this section I get the feeling that this is seen a quite straight forward. I would expect a bit more focus and assessment of various effects that may follow. [Jan Fuglestvedt, Norway]	Taken into account. In response to this and other similar comments, the text is substantially revised. We address the framing issue by discussing the large uncertainties associated with SRM.
55528	67	54	71	18	This subsection speaks of "CO2-induced warming" etc. This shoud be "GHG-induced" [Matthisa Honegger, Germany]	Taken into account, We changed "CO2-induced" to "GHG-induced" where appropriate.
7184	67	54			In section 4.6.3.3, current understanding for the uncertainty in modeling of SAI should be discussed. For example: A multi-ESM assessment of SAI simulations exhibits large inter-model variability both in the direct SRM forcing and the shortwave rapid adjustment from change in the cloud amount, and imply a high uncertainty in modelled processes of sulfate aerosols and clouds (Kashimura et al. 2017). Kashimura, H., Abe, M., Watanabe, S., Sekiya, T., Ji, D., Moore, J. C., Cole, J. N. S., and Kravitz, B.: Shortwave radiative forcing, rapid adjustment, and feedback to the surface by sulfate geoengineering: analysis of the Geoengineering Model Intercomparison Project G4 scenario, Atmos. Chem. Phys., 17, 3339-3356, https://doi.org/10.5194/acp-17-3339-2017, 2017. [Shingo Watanabe, Japan]	Taken into account. It is cited in the revised text. The revised text discusses the uncertainties.
55530	67	56	68	1	This definition is not consistent with elsewhere in the FOD and with the SR15, where it is "the intentional modification of the Earth's shortwave radiative budget with the aim of reducing warming." Related, cirrus cloud thinning (CCT) is appropriately considered part of SRM but does not reduce incoming sunflight. [Matthisa Honegger, Germany]	Taken into account. The definition is changed as per the suggestion in the revision

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
32838	67	56	68	2	If this is the definition of SRM, namely that it has to exert a global influence, then why are rooftop whitening and crop whitening mentioned as examples of SRM approaches as they could not come close to causing a 1-2% reduction in solar radiation? The problem here is that it is not at all clear to me that SRM should be limited to thinking about global scale influences of a magnitude of 1-2% of incoming solar radiaiton to the exclusion of regional scale and smaller magnitude applications (some of which are considered in a paper I've done--see MacCracken, M. C., 2016: The rationale for accelerating regionally focused climate intervention research, Earth's Future 4, 649-657, doi:10.1002/2016EF000450. After all, the present sulphate forcing is regional and presumably has a regional response pattern, and one can use SRM, even stratospheric aerosols, to have greater effects in some regions than others. It is my personal view, which I have published, that indeed the initial focus might be regional or seasonal, etc. and I'd urge allowing for this more clearly in this sentence. [Michael MacCracken, United States of America]	Taken into account. In response to this and other comments, the definition is now changed. This new definition is the same as in SR1.5 Glossary. The regional approaches are mentioned in the revision.
48122	67		71		Response to CDR/SRM: the FOD does not cover the potential RF of SRM. Should this be covered? [WGI TSU, France]	Taken into account. The RF from SRM are discussed in the table in a brief manner in Table 4.6 of FOD
15414	68	1	68	1	The "1-2%" depends on how much cooling is being done; better to word something like "...planet; reflecting ~2% would be sufficient to offset all of the warming from a doubling of CO2" [Douglas MacMartin, United States of America]	Taken into account. The sentence is reworded now
55532	68	1	68	1	SRM could alter net solar radiation by amounts other than 1-2% [Matthisa Honegger, Germany]	Taken into account. In response to this and other similar comments, the sentence is reworded now.
16188	68	6	68	7	This is not a scientific or neutral statement. Some pro-geoengineering researchers are of the opinion that this should be considered as part of the overall strategy to limit global warming; international civil society, however, as well as the international community of states do not consider geoengineering a responsible and legitimate response strategy, as evidenced by the 2010 moratorium on all climate-related geoengineering agreed unanimously under the UN Convention on Biological Diversity (CBD). [Linda Schneider, Germany]	Taken into account. We agree and appreciate the comment. This sentence is now removed. Also, we include a paragraph that discusses the assessment in SR1.5 on governance, ethics and institutional feasibility.
32840	68	6	68	7	The words "below 1.5 C" need to be deleted. While 1.5 C is the lower Paris goal, it is not at all clear that such a value would meet the Objective of the UNFCCC, with respect to any of the three suggested components and particularly with respect to limiting sea level rise to a suffficient level. Virtually all of the SRM studies actually look to reducing global warming down to well below 1.5 C, even to preindustrial, so this sentence as written is far too limited with respect to what could be done (and needs to be done). [Michael MacCracken, United States of America]	Accepted. "below 1.5 C" is deleted in the revised text.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
48874	68	8	68	8	Please include reference: Tilmes, S., B. M. Sanderson, and B. C. O'Neill, Climate impacts of geoengineering in a delayed mitigation scenario, Geophys. Res. Lett., 43, doi:10.1002/2016GL070122, 2016 [Alan Robock, United States of America]	Accepted. The suggested reference is cited now
32842	68	11	68	11	Please change "schemes", which sounds pejorative, to "approaches". Same comment on heading in Table 4.6 and across the section as a whole--they are technological approaches, and no more a scheme than is mitigation or CDR. [Michael MacCracken, United States of America]	Accepted. We replaced "schemes" with either "approaches" or "options"
16190	68	13	68	15	This is a misleading statement that ignores the context in which this research has been conducted. The SRM research community almost exclusively focusses on idealized deployment scenarios that are highly unlikely under real-world political conditions. Hence it is misleading to only state that research has been carried out that is designed to meet different climate goals. Delete sentence or put in context of the wealth of studies analysing non-idealized deployment scenarios and all the ways in which this could go terribly wrong that is not being conducted by the SRM research community. [Linda Schneider, Germany]	Taken into account. The sentence is revised to address the reviewer's concern.
57274	68	18	69	2	Here"scheme" clearly seems to mean option/method, and it would be better to use one of the latter terms [Oliver Geden, Germany]	Taken into account. There were similar comments. In response, we have revised the term to "approaches" or "options"
12818	68	20	69	1	In Table 4.6, for stratospheric aerosol injections (SAI) – this section implies that all forms of SAI are sulphate injections, when in fact a key experiment is using calcium carbonate. Either include other forms of SAI in that part of the table, or add a new row on non-sulphate SAI. See Jeff Tollefson, The Sun Dimmers, Nature (November 2018). [Durwood Zaelke, United States of America]	Taken into account. We have now included calcium carbonate and tin oxide in the table.
12820	68	20	69	1	In Table 4.6, for key side effects of SAI, include potential for diminished crop yields, See Jonathan Proctor et al., Estimating global agricultural effects of geoengineering using volcanic eruptions, Nature (August 2018), potential to delay the recovery of the Antarctic ozone hole, World Meteorological Organization (WMO) (2018) Scientific Assessment of Ozone Depletion: 2018, Executive Summary, Global Ozone Research and Monitoring Project-Report No. 58, ES.32, and potential disruption to monsoon precipitation, World Meteorological Organization (WMO) (2018) Scientific Assessment of Ozone Depletion: 2018, Global Ozone Research and Monitoring Project-Report No. 58, 6.18. [Durwood Zaelke, United States of America]	Taken into account. Thanks for the comments on the potential side effects of SAI. The table is modified to address this comment. The suggested references are also cited now.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
12822	68	20	69	1	Add to Table 4.6 other forms of SRM, including A) removing the subglacial stream under Antarctica that acts as a lubricant to speed up flow of the ice into the ocean, B) blocking warm water from reaching glaciers by constructing 100-meter-high walls with sloping sides at its edge, and C) artificially pinning ice shelves that hold back glaciers by constructing berms and islands. See Moore et al., Geoengineer polar glaciers to slow sea-level rise (Comment), Nature (14 March 2018). Desch et al., Arctic Ice Management, Earth's Future (19 December 2016). ("Here we investigate a means for enhancing Arctic sea ice production by using wind power during the Arctic winter to pump water to the surface, where it will freeze more rapidly. We show that where appropriate devices are employed, it is possible to increase ice thickness above natural levels, by about 1 m over the course of the winter. We examine the effects this has in the Arctic climate, concluding that deployment over 10% of the Arctic, especially where ice survival is marginal, could more than reverse current trends of ice loss in the Arctic, using existing industrial capacity.") This team calculates that adding about 1 meter of thickness over 10% of the Arctic Ocean, at an estimated cost of $50 billion/year, would offset decreases in ice thickness observed since 2000. "Our analysis so far shows that artificial thickening of the ice can counteract a roughly 1°C temperature increase across the Arctic."). L. Field, et al., Increasing Arctic Sea Ice Albedo Using Localized Reversible Geoengineering (2018) https://doi.org/10.1029/2018EF000820 ("In this paper, a novel localized surface albedo modification technique is presented that shows promise as a method to increase multiyear ice using	Taken into account. We believe that the 3 approaches indicated by the reviewer are certainly large scale engineering schemes to prevent the ice sheet collapse but they are not part of SRM approaches. They are not designed to directly increase the solar reflection. However, the approach indicated in the paper "L. Field, et al., Increasing Arctic Sea Ice Albedo Using Localized Reversible Geoengineering (2018) https://doi.org/10.1029/2018EF000820" belongs to regional SRM approach. In the revised draft, we cite this paper where we discuss the regional SRM options.
41112	68	21	68	21	It is not correct to title the column "How does the SRM scheme work?" This implies that the schemes actually will work, and there is no scheme today that exists. If you wanted to do SRM today you could not. There are substatial questions about whether the technology can be built to do what is proposed. Therefore, I suggest you change this to "Proposed mechamism of SRM scheme" [Alan Robock, United States of America]	Accepted. We changed the title of the column as per the suggestion
41114	68	21	68	21	The 10 Mt S per year to achieve -3.5 W/m2 radiative forcing is absolutely wrong. If you look at Fig. 1 of Niemeier and Timmreck (2015), the correct number is 22 Mt S per year. The aerosols grow with increased sulfur dioxide injection. [Alan Robock, United States of America]	Taken into account. Thanks for indicating the correct number. The value is changed now . The indicated reference is cited in the table now
41116	68	21	68	21	It is also incorrect to call 10 Mt S "equivalent to Mt. Pinatubo eruption." First of all, Pinatubo has erupted many times, so you have be specific that you are referring to the 1991 Pinatubo eruption. Second, the size of the eruption is not well known, as some suggest the emission was less than this. [Alan Robock, United States of America]	Taken into account. The value is corrected in response to this and other comments.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
41118	68	21	68	21	Under Whitening roofs, Lightening color of crops, and ocean albedo increase, the potential RF is NEGATIVE, not positive, and should be so indicated. [Alan Robock, United States of America]	Accepted. The sign is changed to negative in the revision
48878	68	21	68	21	Table 4.6 first line: 10TgSO2 injection can only counter 1C with most efficient approaches. Where did the value 10Tg for 2xCO2 come from?) [Alan Robock, United States of America]	Taken into account. In response to this other similar comments, the number is now changed to 20-25 TgS.
48880	68	21	68	21	Side effects and references are not complete, the table seems to be not the right place to fit all those. [Alan Robock, United States of America]	Taken into account. As the approaches were discussed in both AR5 and SR1.5, it was decided that an extensive discussion of the various SRM approaches was not needed, and a table which summaries the approaches and provides some latest references would be sufficient in AR6.
16192	68		68		Table: SAI: achievable with 10 Mt S per year injection – but injection quantities surely must increase to hold down temperatures against rising CO2 concentrations? [Linda Schneider, Germany]	Taken into account. This message is now included in the table.
41120	69	0	69	0	It needs to made clear that space sunshades are a dream, and using the word "achievable" implies that this can some day actaully be done. It is so expensive and so dangerous that nobody takes this seriously. [Alan Robock, United States of America]	Taken into account. We remove the word "achievable". Instead we write "Blocking of about 2 % of the incoming solar radiation"
32844	69	5	69	6	While there will be some impacts, they need to be considered in the context of not undertaking SRM and allowing GHG-induced warming to continue. There would be tremendous benefits to "offsetting large amounts to climate change" and I would think that this first sentence would devote considerable wordage to explaining these rather than using the second phrase to raise rather hypothetical side effects and termination aspects (and note that this would only apply to sudden termination as opposed to terminationas CDR builds up in ways that reduce the need for SRM. So, this sentence needs a lot of revision. [Michael MacCracken, United States of America]	Taken into account. We have modified these two sentences in the revision.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
16194	69	5	69	11	This is paragraph is highly misleading in that it ignores the context in which this research has been conducted. The SRM research community almost exclusively focusses on idealized deployment scenarios that are highly unlikely under real-world political conditions. Hence it is misleading to only state that research has been carried out that is designed to meet different climate goals. Delete sentence or put in context of the wealth of studies analysing non-idealized deployment scenarios and all the ways in which this could go terribly wrong that is not being conducted by the SRM research community. [Linda Schneider, Germany]	Taken into account. This paragraph is revised to address the reviewer's concern.
48876	69	5	69	11	The content of Tilmes et al., 2016, could be also discussed in the text, Maybe Figure could be used from that paper, since it was run with a real GMT, and could be updated with an updated figure from the new paper that is going to be based on SSP5-34-OA. [Alan Robock, United States of America]	Taken into account. This content is now revised and is now discussed . Tilmes et al. 2016 is cited
57472	69	8	69	9	A point made in McCusker et al. 2014 as welll. [Kyle Armour, United States of America]	Accepted. The reference is cited in this section
32846	69	8	69	11	While the papers focused on staying below 1.5 C, in that that amount of sustained warming would have very significant, even what might be called "dangerous anthropogenic interference", it should be noted here that using SRM for peak shaving of the temperature increase could be used to cause the temperature to be less than values lower than 1.5 C, even lower than 0.5 C, etc. that Hansen et al. suggested was the value at which "dangeroous anthropogenic interference" was triggered. A broader statement is needed here. [Michael MacCracken, United States of America]	Taken into account. This sentence is substantially modified in the revision. It should be noted that our assessment is based on what is available in the literature. Also, risk assessments are beyond the scope in this section.
15416	69	13	69	13	missing word "to" [Douglas MacMartin, United States of America]	Accepted.
55534	69	13	69	13	missing "to" [Matthisa Honegger, Germany]	Accepted.
44514	69	13	69	14	The sentence is not understandable need to be improved. [Shaukat Ali, Pakistan]	Taken into account. The sentence is revised.
16196	69	13	69	15	This paragraph is trying to convey a false certainty around SRM and downplays the serious risks and uncertainties associated with the fact that only modelling studies have come to this conclusion. That is far from being „confirmed“ under real-world conditions. Also, the statement that one of the key features of SRM is that in principle it can cool the planet rapidly is not only unclear (what is „in principle“ supposed to mean here? What principle?), it is also extremely biased. One could also say that one of the key features of SRM is that it is extremely dangerous, has the potential to wreck havoc rapidly and can be militarized and used or deployed as a weapon. [Linda Schneider, Germany]	Taken into account. Thanks for the critical comments. We have moderated the messages in the revision.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
32848	69	14	69	17	While rapid cooling is possible, it also needs to be said that waiting until some tipping point, like thawing rate of the ice sheets, and then suddenly exerting a cooling influence would neither be likely to reverse the tipping situation nor would such a rapid cooling be without its own adverse impacts. A much more rational policy approach would be a slow onset of SRM seeking to offset further warming (and some past warming) and/or to start by seeking to offset some of the worst impacts (e.g., amplified Arctic warming)--for the arguments on this, see MacCracken, M. C., 2016: The rationale for accelerating regionally focused climate intervention research, Earth's Future 4, 649-657, doi:10.1002/2016EF000450. [Michael MacCracken, United States of America]	Taken into account. Text is revised in SOD.
57474	69	18	69	23	Inconsistent with statements elsewhere and in Chap 7 that efficacy of non-CO2 forcings is not different from one, and that pattern of warming/cooling is similar to that of CO2. Need to resolve what is thought about this. [Kyle Armour, United States of America]	Noted. Chapter 7 finds the feedback parameter is "approximately" the same for various forcings. However, it does discuss the variations across the various forcing agents.
27266	69	21	69	21	another important factor reducing the efficacy of solar forcing is stratospheric ozone (via its response to increased irradiance): this has been shown in Chiodo and Polvani, 2016 "Reduced climate sensitivity to solar forcing due to stratospheric ozone feedback". Note that this work was based on a more realistic model & set-up than Modak et al., 2016, since we used a chemistry climate model (instead of a GCM) and we also imposed a spectrally-dependent reduction, rather than a uniform scaling of the irradiance. Note that interactive chemistry is generally not considered in many SRM experiments. Hence, this study at least be considered in this discussion. [Gabriel Chiodo, Switzerland]	Taken into account. Thank for informing us this citation on the efficacy of solar forcing. The text is revised and the reference is cited.
41122	69	25	69	25	Change "pattern" to "patterns" [Alan Robock, United States of America]	Accepted.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
15418	69	33	69	33	There is no such thing as "the" spatial pattern of changes to stratospheric sulfate aerosols, or MCB, or CCT. The spatial pattern depends on choices such as the latitude of aerosols to inject (and similar choices for MCB or CCT). So you can include a figure, but only if you make this point extremely clear. (Reading below I see you point this out later, but it needs to be said in the figure caption as well.) Relevant references include Kravitz et al 2016 "Geoengineering as a Design Problem" in ESD, MacMartin et al 2017 "The climate response to stratospheric aerosol geoengineering can be tailored using multiple injection locations" J. Geophys. Res. A., 122, 12,574–12,590, 2017. doi: 10.1002/2017JD026868, Kravitz et al 2017, "First simulations of designing stratospheric sulfate aerosol geoengineering to meet multiple simultaneous climate objectives", J. Geophys. Res. A., 122, 12,616–12,634, 2017. doi:10.1002/2017JD026874, Kravitz et al 2019, "Comparing surface and stratospheric impacts of geoengineering with different SO2 injection strategies", to appear, J. Geophysical Research A, 2019, and MacMartin and Kravitz, "The engineering of climate engineering", Annual Reviews of Control, Robotics, and Autonomous Systems, 2:445-67, 2019. doi:annurev-control-053018-023725 (which summarizes everything else, so if you only want one reference you could use that). Similar research hasn't been conducted yet for MCB or CCT but in principle could. [Douglas MacMartin, United States of America]	Taken into account. The patterns of changes depends how one designs SAI, MCB and CCT. We indicate now that these scenarios are highly idealized. In the revision, the figure is removed.
41124	69	35	69	35	Change "pattern" to "patterns" [Alan Robock, United States of America]	Accepted.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
32850	69	47	69	52	There needs to be greater elaboration of what is being duscussed here. The comments here seem to be stated with respect to the return to preindustrial conditions, which is one way to evaluate what is accomplished. An alternative, and more appropriate comparison is what SRM does with respect to the conditions that would exist without SRM--and in such comparisons, SRM gets conditions back relatively close to preindustial. Also, in this regard, for some reason the analyses are presenting only differences between mean values rather than whether the offset is leading to significant overlap between the natural variability in the preindustrial (or even early 20th century) state and the SRM-induced state. Of course the return will not be perfect, but is it relatively close and much less impactful than the uncontrolled enhanced GHG situation. The situation we face is the relative risk of GHG induced changes with and without SRM, preindustrial to mid-20th century conditions being the supposed situation that would have the lowest impacts. This is a relative risk and not an absolute risk situation, and this just has to be clarified. [Michael MacCracken, United States of America]	Taken into account. In SOD, we provide a balanced discussion that compares SRM simulations to the control climate as well as to climate with high CO2 but without SRM. Further, for the final draft, we will consider to show in the figures if the changes are above the internal variability. We refrain from using the term "risk" as we do not assess risk in this section as it is beyond the scope of assessment.
55536	69	47	69	52	The word "trade-off" and the sentence at line 51-52 imply that any use of SRM to compensate changes in temperature would result in greater precipitation anomalies, and vice versa. Instead, we recommend that there is a trade-off beyond a certain amount of temperature compensation. Likewise, line 52 should say is "changes in global mean surface air temperature (GSAT) are fully or offset" [Matthisa Honegger, Germany]	Taken into account. The sentence is now reworded to address the reviewer's comment. We also now write that "if changes in global mean temperature are fully offset"
41126	69	49	69	49	Add reference to this paper also, which pointed this out before any of the papers referenced here: Robock, Alan, Luke Oman, and Georgiy Stenchikov, 2008: Regional climate responses to geoengineering with tropical and Arctic SO2 injections. J. Geophys. Res., 113, D16101, doi:10.1029/2008JD010050. [Alan Robock, United States of America]	Accepted. It is cited in the revision
15420	69	50	69	52	This sentence should be reworded; one could take the identical sentence but substitute "mitigation" for "SRM" and it would still be true. Given that, this is misleading. Need to say that while CO2 increases precipitation, if SRM were used to completely offset all changes in GSAT, it would over-compensate the changes in precipitation. [Douglas MacMartin, United States of America]	Accepted. The sentence is now reworded to include the term "over-compensate"
57276	69	52	69	52	Here "scheme" seems to mean a certain deployment program, not a certain method [Oliver Geden, Germany]	Taken into account. In response to this comment and other similar comments, we have changed the term to either "options" or "approaches"

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
15422	69	52	70	2	This could be misleading without adding that the effect is easily remedied by simultaneously cooling the southern hemisphere (e.g., Kravitz et al 2016, "Geoengineering as a Design Problem", ESD.) [Douglas MacMartin, United States of America]	Taken into account. This is now mentioned and the suggested reference is cited
32852	69	52	70	2	As a reference, I would also suggest citing MacCracken, M. C., H-J. Shin, K. Caldeira, and G. Ban-Weiss, 2013: Climate response to solar insolation reductions in high latitudes, Earth Systems Dynamics, 4, 301-315, 2013; www.earth-syst-dynam.net/4/301/2013/; doi:10.5194/esd-4-301-2013. In this paper we did identify and investigate response to the ITCZ shift and how that shift might be offset, namely to simultaneously apply SRM over the Southern Ocean, thus tending to balance the SRM-induced energy reduction between the two hemispheres. I'd also note that that paper made the point that application of SRM is comparatively more effective than global SRM because it is being concentrated directly over the region where sea-ice albedo feedback is active and would respond. So, I'd suggest this sentence is inappropriately dismissive of Arctic SRM, and in any case, in evaluating such a possibility one would want to do a comparative risk analysis of the benefits of cooling the Arctic to any impacts elsewhere, considering, for example, the likely slow down in the rate of sea level rise, the likely better sustaining of the ocean overturning circulation due to greater brine rejection due to more sea ice formation, the likely beneficial effect on mid-latitude weather and an associated reduction in extreme weather, the likely beneficial effect of reduced biodiversity loss (including migrating species from lower latitudes), and so on. Overall, ths single sentence and reference here is a totally inadequate summation of the situation (I do agree that figuring out how one would do the comparative benefit analysis would be a challenge, one that would likely best start in the Arctic Council to clarify what the benefits and costs in the region would be--see MacCracken,	Taken into account. In response to this comment and others, we add a sentence that the ITCZ shift can be prevented by performing both Arctic and Antarctic SRM. The suggested reference is also cited.
40542	70	1			Remove "the" before "tropical monsoon precipitation". [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
32854	70	6	70	7	Yes, this is possible, but this is not always a negative outcome as GHG-induced warming is causing very significant shifts in precipitation systems and a primary use of marine cloud brightening (MCB) might well be in seeking to offset some of the detrimental precipitation changes being cause particularly by expansion of the subtropics and subsequent aridification (often misleadingly called droughts even though there is no chance of early and natural fluctation back to some previous normal). Thus the southward shift of the Southern Jet so storms are now south of the traditional Australian agricultural area might be a situation that could possibly be influenced by a smart MCB implementation. Or the aridification of southwestern North America might be another, based on early studies by Namias regarding the path of what is now called the atmospheric river that sometimes flows to the west coast of the continent (previously called the pineapple express). This possiblity has been mentioned in a couple of my papers. I agree that MCB as an approach to offsetting global climate change may well be problematic, but thoughtful regional application may be useful in offsetting severe impacts (e.g., slightly cooling the marine areas where tropical cyclones are tending to significantly intensify such as up-track of tropical cyclones striking the Philippines and the coasts of the Gulf of Mexico and Caribbean Sea. Thus (possibilities also mentioned in my paper [MacCracken, M. C., 2016: The rationale for accelerating regionally focused climate intervention research, Earth's Future 4, 649-657, doi:10.1002/2016EF000450]. I think this sentence thus needs a good bit more elaboration [Stephen Salter also has papers on the potential regional application of MCB.] [Michael	Taken into account. In response to this comment, we now discuss the regional approach and the associated uncertainties and side effects
32858	70	8	70	9	The potential application of cirrus-cloud thinning merits a bit more explanation, namely that it would be used in the polar regions during the winter in order to increase wintertime cooling of these regions, enhancing formation of sea ice and increasing the fraction of precipitation that would fall as snow in high latitudes. At least as explained to this point, as for example in the table of approaches, seems to imply this approach would be used globally instead of regionally, which is about all that makes potential sense. [Michael MacCracken, United States of America]	Noted. Thanks for this comment on how CCT in polar region would help to cool the climate system. The text is revised substantially in SOD.
16198	70	9	70	12	This is an unscientific and dangerous statement. Each SRM technology is dangerous and risky in its own right, and the feedback dynamics of simultaneously deploying two or more SRM technologies are entirely unclear. This statement should be deleted and replaced by a sentence flagging the risks and uncertainties of combining several geoengineering technologies. In any case, the IPCC would do well not recommending „cocktail geoengineering" if it has an interest in maintaining its scientific credibility and authority. [Linda Schneider, Germany]	Taken into account. In the revision, we discuss the large uncertainties involved in aerosol forcing and cirrus microphysical processes. The text is revised so that it is policy relevant but not policy prescriptive.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
35396	70	10	70	10	The abbreviation RMM is not introduced. I am assuming it stands for Radiation Management Measures? [Nadine Mengis, Canada]	Accepted. In the revision RMM is deleted
32856	70	10	70	10	Should RMM be SRM? [Michael MacCracken, United States of America]	Accepted. In the revision RMM is deleted
55538	70	10	70	10	The abbreviation "RMM" is not defined. [Matthisa Honegger, Germany]	Accepted. In the revision RMM is deleted
15424	70	11	70	12	Simulations of CCT are way more uncertain than simulations of aerosols, so should say owing to the large uncertainty in simulating the response to both aerosol forcing and cirrus thinning. [Douglas MacMartin, United States of America]	Taken into account. We have now included a couple of sentences on the uncertainty in CCT potential and the complex microphysical processes
16200	70	14	70	14	This is paragraph is highly misleading in that it ignores the context in which this research has been conducted. The SRM research community almost exclusively focusses on idealized deployment scenarios that are highly unlikely under real-world political conditions. Hence it is misleading to only state that research has been carried out that is designed to meet different climate goals. Delete sentence or put in context of the wealth of studies analysing non-idealized deployment scenarios and all the ways in which this could go terribly wrong that is not being conducted by the SRM research community. [Linda Schneider, Germany]	Taken into account, we write now "Highly idealized model simulations…..". The risks associated with SRM are discussed in Chapter 14 of the WG3 report.
47226	70	14	70	20	The discussion may also derserve an assessment about the relative efficiency of different SRM techniques and the possible overestimation of such an efficiency by CMIP5 models (e.g., Plazzotta et al., 2018). Full reference : Plazzotta M., R. Séférian, H. Douville, B. Kravitz, S. Tilmes, J. Tjiputra (2018) Land surface temperature response to stratospheric aerosol injection constrained by major volcanic eruptions. Geophys. Res. Lett., doi:10.1029/2018GL077583. [Hervé Douville, France]	Taken into account. We discuss the use of emergent constraints in the estimation of efficacy of SAI approach. The suggested paper is cited now
32860	70	15	70	15	The phrase "sulphate aerosol injection at different locations" needs revision. First, the injections being discussed are presumably into the stratosphere, and the word "locations" needs to be changed to "latitudes". It would also be useful to indicate that there could also be seasonal variations with respect to injections, and that, while volcanic eruptions are not perfect analogs, they do provide solid indiations of the types of responses that might be achieved by adjusting the pattern of injections. [Michael MacCracken, United States of America]	Taken into account. "locations" changed to "latitudes", and text is now modified accordingly.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
16202	70	15	70	18	The authors should be very clear here and not use terms that make their research more palatable to an unexpected audience. What happened in the 2017 Kravitz et al. papers is that a computer algorithm, i.e. an Artificial Intelligence, was designed to „deploy“ SRM. That has nothing to do with „interactive“, which is associated with involving humans/communities into decision-making processes – the opposite is true here. Are the authors suggesting we hand over planetary control over the climate system over to a computer algorithm? [Linda Schneider, Germany]	Taken into account. We write "Highly idealized model simulations" to characterize the model simulations that are discussed here.
32862	70	18	70	18	The hope would be that the world is no longer on the RCP 8.5 scenario--the problem being that if that is the emissions scenario that results, SRM would need to be involked for many centuries whereas if the world can get on a much lower emissions scenario that would likely need SRM for decades to centuries until CDR can pull out enough CO2 to slowly phase out SRM. [Michael MacCracken, United States of America]	Taken into account. This message is indicated and conveyed in this section
41128	70	18	70	18	Add this reference: Tilmes et al. (2018), https://doi.org/10.1175/BAMS-D-17-0267.1 [Alan Robock, United States of America]	Accepted. This paper is now cited.
35398	70	18	70	20	Studies have shown, that such an assessment of broad-scale temperture feautures does not provide a comprehensive enough assessment for these climate interventions, as ad-hoc relationships between Earth system processes are perturbed (Mengis et al., 2019). This has to be taken into account for the assessment of future climatic responses of Climate Engineering measures and radiation management in particular. Mengis, N., Keller, D. P., Rickels, W., Quaas, M., & Oschlies, A. (2019). Climate engineering–induced changes in correlations between Earth system variables—implications for appropriate indicator selection. Climatic Change, 1-18. [Nadine Mengis, Canada]	Noted. The message suggested by the reviewer is not relevant for the discussion on meeting multiple goals. The suggested reference is cited in section4.6.3.2
48884	70	22	70	22	Could add references: Tilmes at al., 2016 (above) and Tilmes, S., A. Jahn, J. E. Kay, M. Holland, and J.-F. Lamarque (2014), Can regional climate engineering save the summer Arctic sea ice?, Geophys. Res. Lett. , 41, doi:10.1002/2013GL058731. [Alan Robock, United States of America]	Accepted. The Tilmes et al. (2014) is a suitable citation here and hence it is cited in the revision
16204	70	22	70	28	This is paragraph is highly misleading in that it ignores the context in which this research has been conducted. The SRM research community almost exclusively focusses on idealized deployment scenarios that are highly unlikely under real-world political conditions. Hence it is misleading to quote these studies and portray their findings as reliable and scientific facts. Must be put in context of the wealth of studies analysing non-idealized deployment scenarios and all the ways in which this could go terribly wrong that is not being conducted by the SRM research community. [Linda Schneider, Germany]	Taken into account. In response to this comment, we write now "Highly idealized model simulations…..". The risks associated with SRM are discussed in Chapter 14 of the WG3 report.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
35400	70	22	70	29	Cvijanovic et al., 2015, found that Arctic ocean albedo modification has the potential to recover arctic sea ice to 40% of its preindustrial value. However, imposed albedo changes alter climate outside the Arctic region too, affecting precipitation distribution over parts of the continental United States and Northeastern Pacific. Mengis et al., 2015 extended these findings into a scenario-based setting, and identified a one time offsetting effect , delaying the reduction of sea ice extent and permafrost areas. While Arctic Ocean albedo modification initially dampens the decline of the Atlantic Meridional Overturning, this causes a subsurface warming singal entering the Arctic basin, which potentially acts to further destabilize Arctic marine gas hydrates. These studies should be added to Table 4.6as well. Cvijanovic, I., Caldeira, K., & MacMartin, D. G. (2015). Impacts of ocean albedo alteration on Arctic sea ice restoration and Northern Hemisphere climate. Environmental Research Letters, 10(4), 044020.; Mengis, N., Martin, T., Keller, D. P., & Oschlies, A. (2016). Assessing climate impacts and risks of ocean albedo modification in the Arctic. Journal of Geophysical Research: Oceans, 121(5), 3044-3057. [Nadine Mengis, Canada]	Accepted. The suggested references are cited now in the table
32864	70	47	70	53	While there are stratospheric responses, I'd note that this would also be offsetting some of the strong cooling that is being induced by the GHG-induced cooling in this region of the atmosphere. In addition, what is needed is an overall comparative risk analysis rather than isolated picking out various changes and citing them. [Michael MacCracken, United States of America]	Taken into account. This is now discussed in the revision. The risks associated with SRM are discussed in Chapter 14 of the WG3 report.
32866	70	50	70	53	It needs to be mentioned that strengthening the jet is likely to reduce the occurrence of extreme weather in mid-latitudes as the Rossby wave pattens are likely to be less exaggerated. So, rather than just mention the broad response, the sentence needs to mention the important expected consequence for those living in the mid-latitudes, noting that GHG warming has weakened the jet and so this is a positive effect of SRM and not just of interest to meteorologists. [Michael MacCracken, United States of America]	Taken into account. This is now discussed in the revision

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
32868	70	55	50	56	While Robock has indeed made this point, that such a full-scale test is needed is just not clear (as just an example, the world is now committed to completely change its global energy system based on model simulations of projected CO2 increases without there being any full scale test this would be the case (unless one wants to accept long ago paleoclimatic simulations lacking very extensive detail). The models and other supporting information we have have dealt with the types of perturbations being proposed and there is good reason to have reasonable confidence in their simulations. Indeed, in that the optimal implementation plan would be a gradual increase in stratospheric aerosol injection, so periods with a series of small eruptions would provide a useful test of the models, and in addition it would be possible to learn as the aerosol loading is gradually built up. Thus, I think some context needs to be provided to this sentence, making clear that there is quite high confidence in the projected SRM results. After all, the intent of SRM is to keep the global average temperature in the range of conditions for which the last several decades provide a good basis for testing the models, whereas the simulation of RCP8.5 well off into the future takes us to conditions for which we have no recent experience at all and so, I would argue, involve larger uncertaintities about the rseults than would be associated with model-simulated GHG plus SRM scenarios. I just do not understand the argument that uncertainties would be greater for the SRM situation than for the case of ongoing GHG emissions, and suggesting otherwise would seem to me to undermine the case the scientific community has made for going to zero GHG emissions. Again, I think context is	Taken into account. Agreed that the uncertainties are similar for climate change and SRM. Text is revised in SOD.
53434	70	55	70	55	Does "a full test" mean "real life"? Perhaps add that. [Jan Fuglestvedt, Norway]	Taken into account. Text is revised
31952	71	1	71	1	Studies on the local/regional impacts of pas volcanic eruption can inform on the potentila impact of stratospheric aerosol injection. Such studies could be included here in addition to the general reference to mount Pinatubo eruption. [Marie-France Loutre, Switzerland]	Taken into account. This is now discussed in the revision
32870	71	4	71	10	All well and good, but the likelihood that we will get to a 1.5 C world in the near-term via mitigation is becoming vanishinly small, so the latter analysis really applies. In addition, it is not at all clear that 1.5 C meets the criterion laid out in the Objective of the UNFCCC. To meet that UNFCCC objective, the increase in global average temperature likely needs to be brought down to less than 0.5 C (as Hansen et al. have suggested is the threshold for "dangerous" or at least very impactful), and if that were the goal, the effect of an early start at SRM would become readily detectable within a few decades. [Michael MacCracken, United States of America]	Noted. However, the scope of this section is a discussion on the broad theme of the climate system response to SRM. An assessment and discussion on the level of "dangerous warming" and the "risks" are beyond the scope.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
48882	71	5	71	10	In addition to GLENS, the DECIMALS project needs to be included be described, since some papers will come out soon as well. [Alan Robock, United States of America]	Noted. Discussion of funding & projects is beyond the scope of the report. Glens project is discussed in the context of its scientific outcomes that are related to the climate system response to SRM which is within the scope of this section. However, the DECIMALS project is funding mechanism for assessing the impacts of SRM in developing countries. A discussion on this project is beyond the scope here.
48886	71	11	71	11	Could add comment on ocean circulation changes and refer to: Fasullo J., S. Tilmes, J. H. Richter, M. J. Mills, B. Kravitz, D. MacMartin, I. Simpson, Persistent Polar Ocean Warming in a Strategically Geoengineered Climate, Nature Geoscience [Alan Robock, United States of America]	Accepted. This paper is now cited in the revision
48888	71	11	71	11	For SRM, one could also give a range of studies showing different radiative forcing effects and efficiency [Alan Robock, United States of America]	Taken into account. The efficacy of SRM forcing is discussed in the revised text.
45786	71	12	71	12	incorrect use of risk terminology, "The potential for adverse consequences for human or ecological systems" [Katja Mintenbeck, Germany]	Taken into account. We do not make an assessment of risk to human and ecosystems here. Hence we avoid the use of term here.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
32874	71	12	71	13	In that the adverse consequences of termination would adversely affect virtually everyone on Earth, this situation seems a relatively unlikely outcome, sort of like the risk that was taken of nuclear war by adoption of the Mutual Assured Destruction policy during the Cold War. Indeed, it would be nice not to have to be invoking SRM, but the alternative, given the current pace of emissions reductions around the world, is much more frightening. So, if this point is going to be made here instead of in the WG III report, some context is needed--basically, there is no such thing as a free lunch, but there are better lunches than others. So, overall, SRM offers some very large potential benefits, but also comes with downsides--the more serious one being that it would likely need to be continued for centuries, which would be a legacy the current generation is imposing far into the future. So, what is needed here is a call for a relative risk analysis that includes the many aspects, both positive and negative, setting up the issue for further discussion in WG III. [Also, just a note saying something is one of the most discussed risks and then not having any references giving the range of views--and the views are quite varied--seems quite strange.] [Michael MacCracken, United States of America]	Taken into account. The scope here is the assessment of climate system response to SRM. We discuss the termination of SRM in this context. The benefit and risk analysis is covered in Chapter 14 of WG3. In the revision, we now discuss the lifetime of CO2 and stratospheric aerosols and the implied long-term commitment of SRM if ever implemented.
39684	71	12	71	18	Avoid the use of term "risk" when its use is not consistent with the definition provided in section 1.2.4.1 of Chapter 1 [Carolina Vera, Argentina]	Taken into account. We do not make an assessment of risk to human and ecosystems here. Hence we avoid the use of term here.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
7770	71	12	71	18	"One of the most discussed risks of SRM is the sudden termination of the deployment of SRM because of…." Presumably this and other risks will be covered by Working Group III. But since this risk has been mentioned, you must also include other risks, even if only briefly. I suggest these: (1) What will occur should a Pinatubo- or El Chichón-scale eruption occur after Pinatubo-scale artificial cooling aerosols (SO2, carbonates, etc.) have been injected into the lower stratosphere? Pinatubo caused significant reductions in total column ozone and serious increases in UV-B at the surface. (I measured both in 1991-93. The UV-B levels were the highest I've measured other than during annual calibrations since 1992 at the high-altitude (3.4 km MSL) Mauna Loa Observatory.) See Stephen Self, Jing-Xia Zhao, Rick E. Holasek, Ronnie C. Torres and Alan J. King, The Atmospheric Impact of the 1991 Mount Pinatubo Eruption in FIRE and MUD: Eruptions and Lahars of Mount Pinatubo, Philippines, PHIVOLCS, U.S.Geological Survey (https://pubs.usgs.gov/pinatubo/). (2) How will an artificial, long-term aerosol blanket impact regional and global precipitation, weather systems, crop growth and many other natural weather and climate parameters? (3) How will AERONET, satellite sensors and those of us who measure sunlight and the atmosphere know how to remove the contamination caused by an artificial aerosol blanket? (My 30-year record will certainly be seriously compromised.) (4) Finally, will the scientists and organizations that propose global engineering on a scale sufficiently large enough to reduce or reverse global warming be liable for unforeseen and potentially	Taken into account. We discuss the termination of SRM in the context of climate system response (large warming rates in short period) which is within the scope of the assessment in this section. Yes, large warming rates could imply risks. However, risk analysis is beyond the scope here. (1) and (2) are within the scope of this section and are discussed in the Table as SAI side effects. (3) is related to the increase in diffuse radiation which is also discussed in the table. (4) is beyond the scope this section and hence are not discussed.
32876	71	12	71	18	This paragraph also needs to make clear that this issue of termination risk only applies if SRM has been used for a substantial period during which there have been very large amounts of emissions, so mitigation has failed and so has CDR--and if this turns out to be what happens, we are cooked anyway, so this issue is really quite stangely posed. Given SRM would be far less expensive than mitigation, international governance would really be in trouble for SRM to be terminated. My view is that this risk is being over-emphasized and really does not make much sense. [Michael MacCracken, United States of America]	Taken into account. We now discuss that SRM had to be implemented for a substantial period. However, a discussion of the risk, the cost and governance are beyond the scope of the assessment in WG1 report. Therefore, we do not discuss those issues.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
55540	71	12	71	18	Although sudden termination of SRM could cause "a rapid increase in global temperature and precipitation", it would also need to be both sustained termination, as well as occuring after SRM had been used for a long period of time at a relatively high intensity. Furthermore, given that SRM appears to be relatively inexpensive and technologically simple, and that the risks of termination are widely known, it is unclear how such sudden and sustained termination would occur. After all, another state or other actor could simple assume the responsibility. Although this paragraph need not go into detail, this dynamic should be mentioned. See Parker, Andy, and Peter J. Irvine. "The risk of termination shock from solar geoengineering." Earth's Future 6, no. 3 (2018): 456-467; Rabitz, Florian. "Governing the termination problem in solar radiation management." Environmental Politics 28, no. 3 (2019): 502-522. [Matthisa Honegger, Germany]	Taken into account. We now use the term "sustained" along with "sudden termination". However, A discussion on the cost and governance are beyond the scope of the assessment in WG1 report. Therefore, we do not discuss those issues. We now cite the paper: Parker, Andy, and Peter J. Irvine. "The risk of termination shock from solar geoengineering." Earth's Future 6, no. 3 (2018)
33272	71	12	71	18	When talking about risk of SRM, please also mention the risk of not keeping within 1.5C and the urgent need to increase knowledge about the risks involved with SRM. [Henry Neufeldt, Denmark]	Noted, but a discussion on the "risk" of breaching 1.5 deg warming is beyond the scope of this section
9072	71	12			As ocean biogeochemistry is addressed in this chapter, it may be good to mention the issue of increased ocean acidification in a SRM without GHG emission reduction scenario. [Anna Merrifield, Switzerland]	Taken into account. We discuss this in the first paragraph of the section now. However, the detailed biogeochemistry is discussed in chapter 5 as indicated in this section.
41130	71	14	71	14	Add reference to this paper also, which pointed this out before any of the papers referenced here: Robock, Alan, Luke Oman, and Georgiy Stenchikov, 2008: Regional climate responses to geoengineering with tropical and Arctic SO2 injections. J. Geophys. Res., 113, D16101, doi:10.1029/2008JD010050. [Alan Robock, United States of America]	Accepted. The reference is now cited
32872	71	15	71	17	There needs to be an explanation of why this would occur--that is, why would the changes be greater. It is understandable that the rate of change would be greater, but why the changes themselves. This is such an important point that just giving a reference is not sufficient. [Michael MacCracken, United States of America]	Taken into account. In the revision, we add a sentence to discuss the cause for the large warming rates when SRM is abruptly terminated
16206	71	17	71	18	Again, this is presupposed idealized conditions of deployment that are highly unlikely in the real world. This is the place to emphasize the tremendous risk of the termination effect that is known to make adaptation for human communities and other species virtually impossible. It is not the place to downplay these risks by invoking highly unlikely und unrealistic deployment and phase-out scenarios. [Linda Schneider, Germany]	Taken into account. This comment is appreciated. The text is revised. However, we refrain from using the word "risk" as a risk analysis is beyond the scope in this section.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
8564	71	21	71	55	See Clark et al. (2016, doi:10.1038/NCLIMATE2923) [Robert Kopp, United States of America]	Noted. Thanks for the suggestion
56156	71	21	72	56	Missing from this very terse overview of simulations is a set of fully coupled concentration-forced ESM simulations following ECP-8.5 conducted with CESM1-BGC to year 2300. Mention is made of Randerson et al. (2015) in Chapter 5. While focused on evolving BGC feedback strengths for a single model, it describes projected temperature changes and increasing influence of ocean heat uptake and consequent feedbacks. It further quantifies reductions in AMOC. Subsequent papers by Mahowald et al. (2017) and Moore et al. (2018) describe effects of land use change and responses of ocean biogeochemistry to this forcing. [Moore, J. Keith, Weiwei Fu, François Primeau, Gregory L. Britten, Keith Lindsay, Matthew Long, Scott C. Doney, Natalie Mahowald, Forrest M. Hoffman, and James T. Randerson (2018), Sustained climate warming drives declining marine biological productivity, Science, 359(6380):1139–1143, doi:10.1126/science.aao6379.] [Mahowald, Natalie M., James T. Randerson, Keith Lindsay, Ernesto Muñoz, Scott C. Doney, Peter Lawrence, Sarah Schlunegger, Daniel S. Ward, David M. Lawrence, and Forrest M. Hoffman (2017), Interactions between land use change and carbon cycle feedbacks, Global Biogeochem. Cycles, 31(1):96–113, doi:10.1002/2016GB005374.] [Randerson, James T., Keith Lindsay, Ernesto Muñoz, Weiwei Fu, J. Keith Moore, Forrest M. Hoffman, Natalie M. Mahowald, and Scott C. Doney (2015), Multicentury changes in ocean and land contributions to the climate–carbon feedback, Global Biogeochem. Cycles, 29(6):744–759, doi:10.1002/2014GB005079.] [Forrest Hoffman, United States of America]	Accepted. Results in SOD have been updated using available literature and new models, including ESM simulations of the extended RCPs
36110	71	21			I understand the need to distinguish the content of this section from Section 4.6, but I am concerned that the title 'Very-Long-Term Climate Changes' may give the impression to policymakers and other readers that that changes discussed in this section are in the far distant future, such that we don't need to be too concerned about them. The period considered in this section begins in 2100, which is about 78 years from the publication date of this report, and within the expected lifetime of many babies born today. Or to compare with the description of past climate changes, this would be equivalent to referring to climate changes in the period before 1940 as very-long-term past climate changes. I suggest 'Climate change beyond 2100'. [Nathan Gillett, Canada]	Accepted. sub-section heading is revised to say explicitly it is post 2100
26550	71	23	72	44	Better balance comments and results description for each subparagarph. [Antonia Longobardi, Italy]	Accepted. results have been updated with available literature and models and balanced accordingly

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
30742	71	25	71	25	why only ESMs? [Annalisa Cherchi, Italy]	Not applicable. The text does not say "only ESMs" it clearly says that both ESMs and EMICs will be used. In the SOD all models with available data were considered
36112	71	25	71	32	This text should be updated to also describe the SSP extension scenarios for the SOD. [Nathan Gillett, Canada]	Accepted. As of the SOD, not enough data available - 1 model only - for SSPs/CMIP6 to be included. This may be revised as data becomes available.
46258	71	50	71	55	All GCM models used to obtain this result should be introduced [sadegh zeyaeyan, Iran]	Accepted. All models used have been listed in figure 4.42 caption
8892	71	50	71	55	All GCM models used to obtain this result should be introduced [Mohammad Javad Zareian, Iran]	Accepted. All models used have been listed in figure 4.42 caption
57534	71	50	71	55	All GCM models used to obtain this result should be introduced [Sahar Tajbakhsh Mosalman, Iran]	Accepted. All models used have been listed in figure 4.42 caption
30398	71	51	71	51	The "1.5°C-2°C target" are not really a target Why not just saying: will exceed 1.5°C or 2°C of global warming? [Joeri Rogelj, Austria]	Accepted. Text is revised.
53436	71	52	71	52	Good that you go beynd 2100. But please add some info about what is assumed about the emisisons. That helps the reader in undetanding the behaviors and inertia etc. [Jan Fuglestvedt, Norway]	Accepted. Extensions are described
36114	71	53	71	55	The range quoted for RCP8.5 extension of 4 - 9 K is for EMICs only. The four CMIP5 ESMs which ran this scenario, and published data on the ESGF, show GSAT warming of 9-13 K in 2300 relative to preindustrial. See Tokarska et al. (2016) Figure 1a (https://www.nature.com/articles/nclimate3036/figures/1). caesar et al. (2013) (already cited) show that the warming for HadGEM2-ES was approx 11.5 under this scenario. Add discussion of the warming in the CMIP5 ESMs. [Nathan Gillett, Canada]	Accepted. Results have been updated using available literature and new models including ESMs for the RCP extensions
37710	71	54			Another appearance of "GMST", in a section headed "Global Surface Air Temperature". [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Terminology is corrected
27744	72	1	72	1	correct the bibliographic citation [Poot Delgado Carlos Antonio, Mexico]	Noted

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
36116	72	3	72	7	The authors should also briefly assess regional precipitation changes under these scenarios. Caesar et al. (2013) show maps of precipitation changes in their Figure 3a for the end of the RCP8.5E simulation, with local increases in mean precipitation exceeding a factor of five and decreases exceeding a factor two (their colour scales saturate, so it impossible to read the maxima and minima). Tokarska et al. (2016) show four-model ensemble-mean regional precipitation changes at 5000 PgC emissions (between 2050 and 2300) diagnosed from CMIP5 RCP8.5 extension simulations (scaled down by the ratio of CO2 RF to total RF). Local multi-model mean precipitation increases and decreases exceed a factor of four. These are very large changes in mean regional precipitation which would presumably be very hard for human and natural systems to adapt to. Just reporting the change in global land mean precipitation of 7% underplays these profound regional changes. While the RCP8.5E scenario might be considered unrealistic, it could be mentioned that this scenario may approximate a non-mitigation scenario in which all economically extractable fossil fuel reserves were used (Tokarska et al., 2016), thus representing an approximate upper limit on long-term climate change in the event that emissions are not mitigated. [Nathan Gillett, Canada]	Taken into account to the extent that Ch.4 covers regional change. We keep this section consistent with the rest of chapter 4 in terms of the hand-over of metrics assessed or left to other chapters. Hence we only show here global land precipitations.
32880	72	5	72	7	Fine to say precipitation will go up, but it needs to be said that (a) the patterns of change will be such that some regions see significant increases and others see significant decreases as the boundaries of the subtropics shift, and (b) that evaporation will also significantly increase, and in that available soil moisture is critical to agriculture, this will be very significant for large-scale growing of key grain crops (most vegetables by then will likely be grown indoors under LED light, etc.--or under highly controlled conditions in that one does not need large acreage). [Michael MacCracken, United States of America]	Rejected. In this section we only show global projections. Regional changes are covered in other chapters. We cannot speculate in this report about the conditions for growing vegetables beyond the year 2100.
32882	72	12	72	15	It might also be stated that observed changes are exceeding model projected changes, so that the statements here might well be understating the extent of change. [Michael MacCracken, United States of America]	Taken into account. This is an important point which requires coordination with other chapters too (esp. ch.3 on evaluation and ch.2 on observations). In the FGD we will ensure consistency with any assessment Ch.3 makes regarding rates of change in observations compared with models
52402	72	18	72	18	Suggest adding a new paragraph here on mountain glaciers based on the SROCC and work by Marzeion (2012) and Glacier MIP. [Pam Pearson, Sweden]	Taken into account. GlacierMIP can be assessed as results allow. We check consistency with SROCC

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
56392	72	20	71	24	Long-term sea level rise was laso investigated in Zickfeld et al., J. Clim., 2013. [Kirsten Zickfeld, Canada]	Noted. thank you - we extend the literature coverage accordingly
32878	72	20	72	24	All well and good, but it also needs to be said what the estimated sea level rise will be out at 2300 due to the non-steric component caused by loss of mass of the Greenland and Antarctic Ice Sheets. And, given that the paleoclimatic estimate of equilibrium sensitivity for sea level rise is something like 20 meters per degree C in global average temperature and that the last deglaciation involved rates of sea level rise of 1-2 meters per century when the global average temperature was rising 1-2 C per thousand years (so far slower than present warming), sea level rise is likely to be of order 10 meters by then, which would be an astoundingly impactful rise given the objective stated in the UNFCCC, this much larger contribution just has to be mentioned--and not just the rise at that point, but the ongoing rate of rise. It would be totally irresponsible not to discuss the glacial melt component. [Michael MacCracken, United States of America]	Taken into account - to ensure consistency with SLR across our chapter we show here only steric SLR from GCMs
8562	72	20	72	24	Cross-check with ch 9 (9.6.3.5) [Robert Kopp, United States of America]	Taken into account - we coordinate with cryosphere and sea level chapter on this
32884	72	27	72	44	It needs to be explained why the AMOC is important enough to have so much more space devoted to its likely condition as compared to the very brief presentations of the preceding impacts. So, why should the reader care about AMOC? [Michael MacCracken, United States of America]	Noted. Space for assessment depend on available results and literature. This is balanced better in the SOD
56394	72	29	72	32	The long-erm AMOC response in CMIP5 ESMs amd EMICs is investigated in Weaver et al., GRL 39, L20709, 2012. [Kirsten Zickfeld, Canada]	Noted. thank you.
8566	72	29	72	44	Cross check with ch 9 [Robert Kopp, United States of America]	Taken into account - We coordinate with cryosphere and sea level chapter on this
37712	72	52			Again, should "GMST" be "GSAT"? Also, should "pre-industrial" be replaced by "the early-industrial baseline" for consistency with the terminology introduced in Chapter 1? [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. terminology is corrected
6934	72				It seems that some information on AMOC here is presented twice, in this chapter, and in chapter 9. I am wondering whether this is a redundancy. It may be a good idea to present some more quantitative information on the pdf of future AMOC, and on the likelihood of AMOC collapse, under each scenario. I understand that this may not be possible with CMIP6 model output, but there have been several relevant studies since the AR5 on this topic. [Olson Roman, Republic of Korea]	Taken into account. We coordinate with Ch.9

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36588	72				Section 4.7. In principle it is good to look at these long term horizons. Models used for the required long term simulations may include important simplifications. For example, I believe that models used in ARS did not include the melting of the Greenland Ice sheet. It is also my understanding that CMIP6 goes beyond this simplification in some cases. There is even an Ice Sheet Model Intercomparison (ISMIP6) (Nowicki et al 2016) that includes fully coupled climate-ice-sheet modelling. This may already be mentioned elsewhere in this vast report. However, it may be good not to miss an opportunity to highlight that much research is necessary to address important issues in climate projections. [Carlos Mechoso, United States of America]	Taken into account. We agree model complexity is important to describe. In the FGD we will discuss limitations of projections due to limited inclusion in ESMs of key processes.
8698	73	1	75	14	This sections is very interesting and the authors expect to give more insight based on ongoing projects (CDR-MIP). From a WG2 perspective, I would like to suggest considering delivering a statement here about the following questions: (1) what would be the stabilization of global temperatures if all proven fossil fuels reserves are burnt? (NB: Clark et al. (2016) suggest temperature at arround 12°C above pre-industrial means, but this holds for one third of proven fossil fuels only); (2) Is there any possiblility for the onset of a new glacial era in the coming millenia? (AR5WG1Ch5 suggests it is not possible at least for 50000 years). These questions may seem very simple but a WG1 statement would be useful for WG2 and adaptation stakeholders to understand long term implications of anthropogenic climate change. [Goneri Le Cozannet, France]	Rejected. This is an interesting idea but insufficient literature is available of WG1 remit on physical climate science. The amount of remaining and accessible fossil fuel is best covered in WG3
53446	73	4	73	4	The paper by Mauritsen & Pincus, NCC 2017, could also be used and put in the context here [Jan Fuglestvedt, Norway]	Noted. thank you for the reference
53438	73	6	73	6	The first sentence is uncelar. Do you mean "for continued GHG emisison " or "for rising GHG emissions"… please be more clear. [Jan Fuglestvedt, Norway]	Taken into account. We mean the former - continued, but not necessarily increasing, emissions. Text is clarified
56396	73	6	73	9	Make TCRE definition consistent with rest of AR6 report. TCRE is usually defined as the transient global average surface temperature change per unit cumulative CO2 emissions, usually 1000 GtC (see e.g. SR1.5 glossary). [Kirsten Zickfeld, Canada]	Accepted. Text is clarified
36118	73	6	73	27	Since TCRE is assessed in Chapter 5, I recommend not discussing it in this section (it is mentioned on lines 6-7 and 25), since the concept is not needed just to describe the ZEC. If the mention of TCRE is retained it needs to be clearly defined as the ratio of warming to cumulative emissions, which is approximately constant, with reference to Chapter 5. As written, unless readers already know what TCRE is, the discussion of it won't make much sense. [Nathan Gillett, Canada]	Taken into account. To enable chapter to read stand-alone some mention is required here, but we coordinate with ch.5 to remain consistent and avoid unnecessary duplication

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
53440	73	11	73	11	This goes beyond only GHGs, i think. It applies for all emissions. [Jan Fuglestvedt, Norway]	Noted. Here we mean CO2 only. Text is clarified
32886	73	11	73	16	What abou ocean acidifciation--does ongoing ocean overturning mix the excess CO2 and so actually slightly reduce ocean acidification, or is it just that there is less outgassing in low latitudes, so ocean acidification stays the same? [Michael MacCracken, United States of America]	Taken into account. Ocean BGC is also covered in ch.5. we can assess here if there is available literature
57476	73	11	73	16	Is this zero emisssion commitment assuming that emissions of aerosols also cease, or that somehow aerosol emissoins continue without GHG emissions (a geoengineering scenario)? If the former, then there is a collection of papers discussing the respones after emissions cease showing potenial for a temporary rise in temperature for the first decade as aersols are quickly washed out and before methane decreases (e.g., doi:10.1029/2011GL048739 and others). Perhaps discuss this scenario as well, and clarify which you intend here. [Kyle Armour, United States of America]	Taken into account - Here we mean CO2 only. Text is clarified
36120	73	11			Replace 'global climate' with 'GSAT'. [Nathan Gillett, Canada]	Accepted
30400	73	13	73	13	The ZEC can be zero, at which point it is still called the ZEC (I think). Can't you simply write: "The evolution of global warming after a complete cessation of emissions is called the ZEC." (but this is already written two lines earlier) [Joeri Rogelj, Austria]	Taken into account.
37714	73	13			What is meant by a "constant climate"? Many climate variables continue to change, as stated later in the paragraph. So it would be better to spell out which variables are approximately constant. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. It means constant GSAT. We clarify in SOD.
53442	73	14	73	14	please add a ref to ch 5 [Jan Fuglestvedt, Norway]	Taken into account.
56398	73	18	73	20	I have not seen a "constant temperature" commitment discussed in the literature. Forms of commitmens that are usually discussed in reviews (see e.g. AR5, SR1.5) are the constant composition and constant emission commitment. [Kirsten Zickfeld, Canada]	Taken into account - Assessment is guided by available literature.
32888	73	19	73	19	Only for "centuries"--would such rise not continue for millennia, or is it the case that near equilibirum will be reached in centuries. If so, this would seem to imply that the rates of sea level rise must be quite high given equilibrium is likely something like 20 meters per degree C. [Michael MacCracken, United States of America]	Accepted. "centuries" here means many and does not preclude millennia. Text is clarified
32962	73	19			insert cross-reference to CH9 section 9.6.3.5 [Aimee Slangen, Netherlands]	Accepted.
53444	73	21	73	21	You may mention and explain the "constant concentration commitment" in order to clarify earlier misconceptions [Jan Fuglestvedt, Norway]	Taken into account.
30402	73	23	73	24	Maybe cite AR5 Chapter 12 here? [Joeri Rogelj, Austria]	Taken into account.
36122	73	25			Replace 'constraining the robustness' with 'constraining'. The meaning of 'constraining the robustness of TCRE' is not clear. [Nathan Gillett, Canada]	Taken into account.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
56400	73	27	73	27	Herringtonand Zickfeld, ESD, 2015 also examine the scenario dependence of TCRE. [Kirsten Zickfeld, Canada]	Noted. Thank you for the reference
56402	73	29	73	29	There is a significant body of literature on the ZEC.The probelm is that studies use difference scenarios and different combinations of forcings, which makes the results difficult to compare. Also, most of the literature focuses on the long-term (century to millennial timescale) ZEC whereas what matters for carbon budgets is the ZEC on a decadal timescale (up to the time of peak warming). [Kirsten Zickfeld, Canada]	Taken into account.
30404	73	29	73	33	This paragraph would benefit from also indicating the time horizons over which this warming is projected to occur. [Joeri Rogelj, Austria]	Taken into account.
56406	73	29	73	39	It needs to be made clear that this paragraph discusses the ZEC after elimination of CO2 emissions only. It may be worth adding a few sentences assesing the ZEC after elimination of all emissions (see e.g. Matthews and Zickfeld, Nat. CC, 2012; Smith et al., Nat. Comms., 10 (101), 2019). [Kirsten Zickfeld, Canada]	Taken into account.
13258	73	29	73	39	In additon to Nohara et al. 2015, Nohara et al 2013 also discuss about the zero emission future senario. Please consider my proposal to add the following reference. Nohara, D., Y. Yoshida, K. Misumi, and M. Ohba, 2013: Dependency of Climate Change and Carbon Cycle on CO2 Emission Pathways. Environ. Res. Lett., 8 014047 doi:10.1088/1748-9326/8/1/014047. [Masamichi Ohba, Japan]	Noted. Thank you for the reference
56404	73	34	71	34	The ZEC was assessed in Chapter 1 of the SR1.5 report (Allen et al., 2018). This assessment was then used for the quantification of remaining carbon budgets uncertainty (Rogelj et al., 2018). [Kirsten Zickfeld, Canada]	Taken into account.
13240	73	55	74	19	Reversibility/irreversibility of ENSO is discussed in Ohba et al. (2014) by using CESM1 (CDR simulation). ENSO is the most dominant climate variation mode in our planet that cause the climate extreme events. I think the reversibility of interannual variability should be mentioned/discussed in here and be included it in Table 4.7, because of the importance of irreversibility for the climate extremes. Please consider my proposal to add the following reference. Ohba, M., J. Tsutsui, and D. Nohara, 2014: Statistical parameterization expressing ENSO variability and reversibility in response to CO2 concentration changes, Journal of Climate, 27, 398-410, doi:10.1175/JCLI-D-13-00279.1. [Masamichi Ohba, Japan]	Noted. modes of variability can be discussed if literature is available.

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56410	74	2	74	3	Changes in ocean physical and biogeochemical propoerties also continue on long timescales. See e.g. Mathesius et al., Nat CC, 2015). [Kirsten Zickfeld, Canada]	Accepted. And can be assessed if literature is available
56408	74	3	74	3	Add reference to Ehlert & Zickfeld, ESD 9, 197–210, 2018. [Kirsten Zickfeld, Canada]	Noted - thank you for the reference
7976	74	3	74	6	Please include a clear definition of (ir)reversibility. The addition of a definition of abrupt (in 4.7.3) is a very useful addition, but the definition of irreversibility is more vague. For instance over what timescale should this be considered? Many impacts are likely to lag forcing changes - over what time scale is this considered reversible? [Laura Jackson, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Text and/or glossary are clarified
56412	74	5	74	6	Studies that examines climate change reversibiity with respect to other forcers are Solomom et al., PNAS, 2010; Zickfeld et al., PNAS, 2017. [Kirsten Zickfeld, Canada]	Noted. Thank you for the reference
9074	74	7			Do TCR and TCRE refer to the same thing? [Anna Merrifield, Switzerland]	Noted. No. Definitions are clarified.
56414	74	8	74	10	The CDR-MIP 1% ramp-down simulations are discussed in section 4.6.3.2, but I think that discussion would fit better here. [Kirsten Zickfeld, Canada]	Noted. CDRMIP is highly relevant both here and 4.6.3. we coordinate to avoid duplication
56416	74	19	74	20	Table 4.7: Additional references: GMSAT: MacDougall et al., GRL, 2013; Tokarska & Zickfeld, 2015. Global SST: Mathesius et al., Nat CC, 2015. Ocean heat content: Mathesius et al., Nat CC, 2015; Ehlert and Zickfeld, ESD, 2018. Land and ocean carbon store: Tokarska et al., ERL 2015. Suggest to add column on ocean biogeochemistry (pH, oxygen); see Mathesius et al., 2015. [Kirsten Zickfeld, Canada]	Noted. Thank you for the references
8568	74	19	75	2	Should this be a cross-chapter box? [Robert Kopp, United States of America]	Rejected. We have coordinated with other chapters to fill this synthesis table, but it fits within this section in chapter 4
30406	74	19	75	2	Excellent table! [Joeri Rogelj, Austria]	Noted. Thank you.
7970	74	20	74	20	Table 4.7 AMOC, Reversible with lag: other references doi: 10.1007/s00382-013-1842-5 and 10.1007/s00382-014-2391-2 [Laura Jackson, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Thank you for the references
7972	74	20	74	20	Table 4.7 AMOC, Overshoots: wrong reference. Assume should be Jackson et al 2013, doi: 10.1007/s00382-013-1842-5 [Laura Jackson, United Kingdom (of Great Britain and Northern Ireland)]	Noted- References are checked
7974	74	20	74	20	Table 4.7 AMOC, irreversible: See Jackson + Wood, 2018 doi: 10.1029/2018GL078104 [Laura Jackson, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Thank you for the references
6940	74		75		I am wondering whether the table may be better presented with a single column on reversibility, where the answers can be color-coded depending on whether the variable is reversible or not. The overshoot can be its own column. [Olson Roman, Republic of Korea]	Taken into account - table format is re-visited once content is complete to enable optimal communication of content

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36124	74				In Table 4.7, explain that 'Inconsistent behaviour' means 'Inconsistent behaviour in different models'. This isn't clear at present. [Nathan Gillett, Canada]	Taken into account.
36126	75	5	75	11	This paragraph discussing mutually reinforcing climate feedbacks appears to be based mainly on one study. I recommend assessing a range of literature on this topic if available, or if there is no new literature since AR5, then draw in some information from the AR5 to examine how the new literature changes the AR5 assessments on this topic. Secondly, the paragraph really just says that Steffen et al. (2015) reviewed a range of feedbacks, without saying what their results were, or what the resulting assessment in this chapter is. [Nathan Gillett, Canada]	Accepted. We agree this reference is overly cited here. Paragraph is revised
16172	75	5	75	11	the term „strawman“ that is used here makes this paragraph sound unnecessarily perogative; given that climate change impacts have so far tended to occur earlier and at faster pace than foreseen by previous IPCC assessments, a portrayal of mutually reinforcing climate feedbacks – even if qualitative – seems very valid and important and should not be discarded as „speculative“. [Linda Schneider, Germany]	Accepted. We agree this reference is overly cited here. Paragraph is revised
27466	75	5	75	11	the term „strawman“ that is used here makes this paragraph sound unnecessarily pejorative; given that climate change impacts have so far tended to occur earlier and at faster pace than foreseen by previous IPCC assessments, a portrayal of mutually reinforcing climate feedbacks – even if qualitative – seems very valid and important and should not be discarded as „speculative“. [Linda Schneider, Germany]	Accepted. We agree this reference is overly cited here. Paragraph is revised
56418	75	5	75	11	This paragraph seems out of place here. Move to section 4.7.3? [Kirsten Zickfeld, Canada]	Taken into account. Paragraph is revised
30408	75	5	75	11	Para seems to be in wrong section. [Joeri Rogelj, Austria]	taken into account. Paragraph removed
8570	75	14	75	28	Should abrupt commitment (with long-term realization) be considered here? See discussion in Kopp et al (2016, doi:10.1002/2016EF000362) [Robert Kopp, United States of America]	Noted.
36128	75	16	75	22	As written this paragraph says that the AR6 is adopting a different definition of abrupt climate change than the AR5, without saying what the AR5 definition was, or why a new definition is being adopted. [Nathan Gillett, Canada]	Accepted. Terms are defined here and in the glossary and used consistently through the chapters.
56420	75	16	75	22	From this paragraph it is unclear how the definition adopted in AR6 differs from that in AR5. [Kirsten Zickfeld, Canada]	Accepted. Terms are defined here and in the glossary and used consistently through the chapters.
47228	75	16	75	22	This new definition may need to be even more specific about how the rate of change of forcing is defined. For instance, if climate change is ultimately driven by future concentrations of GHG, should the strongly time-dependent radiative forcing due to anthropogenic aerosols be included ? [Hervé Douville, France]	Accepted. Terms are defined here and in the glossary and used consistently through the chapters.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
6936	75	17	75	19	What is meant by the nonlinearity of the climate system? I recommend defining terms in this section in a more precise and a mathematically-oriented manner. Specifically, how do you quantify a faster rate of change with respect to forcing for variables with different units? Does the relevant quantity relate to equilibrium or a transient response? [Olson Roman, Republic of Korea]	Accepted. Terms are defined here and in the glossary and used consistently through the chapters.
53448	76	11	76	40	Please check consistency with the definition of risk that has been developed in Ar6. [Jan Fuglestvedt, Norway]	Accepted. Text now refers to risk definition in chapter 1
32076	76	11	77	2	I am glad to see discussion here of a specific PPHIS. I suggest it would be appropriate to add some more discussion (including of the mechanisms) of other potential high impact scenarios which could have global or near global importance. For example: high TCRE could occur for many different reasons (e.g. high TCR vs rapid release of carbon from melting permafrost; an abrupt change to global monsoon circulations or to the AMOC are examples of sub-global but still very important physically plausible high impact scenarios. In each case it would be necessary to quantify some of the key impacts, as is done already for the specific case considered. Lastly, there is currently no mention of PPHIS in the Executive Summary - I suggest this needs to be corrected since they are very policy relevant. [Rowan Sutton, United Kingdom (of Great Britain and Northern Ireland)]	This comment has been partly taken into account. The section on PPHIS is extended and also includes precipitation. Changes in carbon cycle feedbacks are covered in chapter 5 and abrupt changes are assessed in section 4.7. High warming storylines for sea level rise are assessed in chapter 9. The low-probability high-warming scenarios are now highlighted in the executive summary

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
8700	76	11	77	32	Storylines for low-probability/high warming scenarios (or "high-end scenarios") are extremely relevant for WG2 and adaptation stakeholders and relying on the 90th quantile of model projections is a straightforward approach. However, other approaches could be considered as well, if WG1 authors feel they are appropriate for temperature changes: as an analogue, in the area of sea-level rise, two other approaches toward high ends can be mentionned: one consists in decreasing progressibly upper bounds (i.e. SLR scenarios which are impossible based on our understanding of ice-melting dynamics); the other consists in increasing progressivly high-ends (starting e.g. from the upper bound of the likely range) and assess whether they might be reached with known mechanisms (e.g., Marine Ice Sheets Instability). I suppose that applying similar approaches to other climate variables (e.g. temperatures) could lead to different "high-ends" and "upper bounds" than those based on the 90th percentile, and may lead to considering some positive climate feedbacks not included in current models (high hydrocarbon realease triggered by climate change...). The two approaches toward high ends are discussed in Stammer et al. (forth, Earth Future), "framework for high-end estimates for stakeholders applications", but I suppose there are other papers specifically on temperatures which I am not aware of. [Goneri Le Cozannet, France]	Noted. The section on low-probability high-warming storylines primarily addresses temperature and precipitation changes. For many other aspects targeted storylines need to be developed at regional scale. Storylines of high and low SLR are addressed in Box 9.3, which is now referred to here.
36134	76	15	77	26	Chapter 1 has a section on risk framing (1.2.4.1) and a cross-chapter box (1.2) on risk framing in the AR6, which are relevant here, but not currently referenced. These should be cross-referenced, to ensure consistency. [Nathan Gillett, Canada]	Accepted. Text now refers to risk definition in chapter 1
55024	76	16	76	16	Do you really really mean Box 1.1 ? [Rojas Maisa, Chile]	Accepted. Reference to box 4.1. corrected
55026	76	22	76	24	please check the risk refinition for AR6 provided in Chapter 1. [Rojas Maisa, Chile]	Text now refers to risk definition in chapter 1
8572	76	22	76	26	The risk analysis literature has long recognized that "likelihood x impact" is a very partial measure of risk -- see Garrick and Kaplan (1981) for the quantitative risk analysis literature [Robert Kopp, United States of America]	Accepted. Text now refers to risk definition in chapter 1
45788	76	22	76	26	not the ipcc definition of risk, please be consistent with IPCC definitions [Katja Mintenbeck, Germany]	Accepted. Text now refers to risk definition in chapter 1
31482	76	22			Risk also involves for vulnerability. Check for consistency of definition of risk in the entire report. [Rein Haarsma, Netherlands]	Accepted. Text now refers to risk definition in chapter 1
6938	76				I like the presentation of the reversibility of thresholds in Table 4.8. Some of the boxes appear to be blank, I am wondering whether they would be eventually filled with some text? Also, it may be useful to color the positive answers in the second and third columns in red. [Olson Roman, Republic of Korea]	Taken into account. Boxes are filled as much as possible depending on literature and results or assessment in other chapters. We do not use coloured text as this is not usual in IPCC reports

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
36132	76				Table 4.8. In my view, wind-driven changes in the S Ocean carbon sink are not abrupt based on either the definition used here or the AR5 definition. Changes in the S Ocean carbon sink in response to changes in winds are simulated in CMIP5/6 models, and based on the results shown in the Chapter (e.g. Figure 4.5) there is no evidence for abrupt change. Admittedly, Section 5.4.8.4, cited in the table, does suggest that a 'tipping point' is possible, but none of the references cited in Section 5.4.8.4 characterise the observed or projected ocean carbon sink in this way - they just characterise and describe the observed variability in the sink. I recommend removing this item from the table. [Nathan Gillett, Canada]	Taken into account. We coordinate with ch.5 to see if this should be kept or not as a defensible abrupt change
12824	77	2	77	8	Even if they are less likely to occur, these high impact possibilities are still important to the climate change discussion, especially when considering policies that should be implemented to limit the risk. Xu and Ramanathan 2017 show that median temperatures staying well below 2ºC can keep warming to less than 1.5ºC, but the fat tail—the extension of the curve to the right—continues into the dangerous and catastrophic range, highlighting that even the best solutions still face some risk of excessive warming though far less risk than baseline scenarios that fail to include faster and much more aggressive mitigation. Xu and Ramanathan (2017) Well below 2 °C: Mitigation strategies for avoiding dangerous to catastrophic climate changes, Proc. Natl. Acad. Sci., doi: 10.1073/pnas.1618481114; 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; Ramanathan and Feng (2008) On avoiding dangerous anthropogenic interference with the climate system: Formidable challenges ahead, Proc. Natl. Acad. Sci., doi: 10.1073/pnas.0803838105; see also Spratt D. & Dunlop I. (2019) Existential climate-related security risk: A scenario approach, Policy Paper, Breakthrough – National Centre for Climate Restoration; Weitzman M. (2011). Fat-Tailed Uncertainty in the Economics of Catastrophic Climate Change. Review of Environmental Economics and Policy 5(2):275-292 [Durwood Zaelke, United States of America]	Taken into account. This is exactly the motivation for assessing such low-probability high-warming storylines. We included the references in the discussion.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
42332	77	2	77	8	Even if they are less likely to occur, these high impact possibilities are still important to the climate change discussion, especially when considering policies that should be implemented to limit the risk. Xu and Ramanathan 2017 show that the median staying well below 2ºC can keep warming to less than 1.5ºC, but the fat tail—the extension of the curve to the right—continues into the dangerous and catastrophic range, highlighting that even the best solutions still face some risk of excessive warming though far less risk than baseline scenarios that fail to include faster and much more aggressive mitigation. Xu and Ramanathan (2017) Well below 2 °C: Mitigation strategies for avoiding dangerous to catastrophic climate changes, Proc. Natl. Acad. Sci., doi: 10.1073/pnas.1618481114; 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; Ramanathan and Feng (2008) On avoiding dangerous anthropogenic interference with the climate system: Formidable challenges ahead, Proc. Natl. Acad. Sci., doi: 10.1073/pnas.0803838105; see also Spratt D. & Dunlop I. (2019) Existential climate-related security risk: A scenario approach, Policy Paper, Breakthrough – National Centre for Climate Restoration; Weitzman M. (2011). Fat-Tailed Uncertainty in the Economics of Catastrophic Climate Change. Review of Environmental Economics and Policy 5(2):275-292 [Gabrielle Dreyfus, United States of America]	Taken into account. This is exactly the motivation for assessing such low-probability high-warming storylines. We included the references in the discussion.
12660	77	2	77	8	Even if they are less likely to occur, these high impact possibilities are still important to the climate change discussion, especially when considering policies that should be implemented to limit the risk. Xu and Ramanathan (2017) Well below 2 °C: Mitigation strategies for avoiding dangerous to catastrophic climate changes, Proc. Natl. Acad. Sci., doi: 10.1073/pnas.1618481114. Ramanathan, Molina, and Zaelke (2017) Well Under 2 Degrees Celsius: Fast Action Policies to Protect People and the Planet from Extreme Climate Change. Spratt D. & Dunlop I. (2019) Existential climate-related security risk: A scenario approach, Policy Paper, Breakthrough – National Centre for Climate Restoration; Weitzman M. (2011). Fat-Tailed Uncertainty in the Economics of Catastrophic Climate Change. Review of Environmental Economics and Policy 5(2):275-292. [Kristin Campbell, United States of America]	Taken into account. This is exactly the motivation for assessing such low-probability high-warming storylines. We included the references in the discussion.
36136	77	4	77	8	Should reference Section 1.2.4.3 Narratives and Storylines. [Nathan Gillett, Canada]	Accepted. Reference added

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
47230	77	10	77	26	As suggested in a former comment, I would argue that Rowan Sutton advocated for a systematic risk assessment framing rather than just an illustration of this approach in the AR6 and I would also favour such a systematic approach throughout chapter 4 rather than just at the end. [Hervé Douville, France]	Noted. Due to the lack of relevant literature this approach cannot yet be applied to all different variables. Note that chapter 12 expands on these aspects.
53450	77	10	77	26	I expected more from this. What does this imply? [Jan Fuglestvedt, Norway]	Noted. The section has been extended to discuss the implications also for the hydrological cycle. The intention is to assess plausible manifestations for such high warming storylines. The impacts of such high-warming story lines will be assessed in WG2.
56422	77	12	77	12	I assume you are discussing concentrations-driven SSP5-8.5 simulations? If that's the case it would be more appropraite to choose the model with the highest TCR, not TCRE, as the carbon cycle response is irrelevent in this case. [Kirsten Zickfeld, Canada]	Taken into account. Correct, it should be high TCR and in addition storylines on carbon cycle feedbacks is coordinated between chapter 4 and chapter 5.
36140	77	12	77	13	While I think the approach of using a high-TCR model to illustrate a storyline of strong climate change is reasonable, I think the justification here is lacking. On line 8 the text says that storylines can be used to assess the consequences of changes outside the 'likely model range'. In AR5 at least, projected changes were assessed by taking the 5-95% range of simulated changes, and interpreting this as the 'likely' range. But here the text says that CanESM5 was picked because it nearest to the upper bound of the assessed 'very likely' range, then it describes this as the '90% quantile'. So the overall sense of the text seemed to be that we shoudl consider storylines above the model 5-95% range for risk assessment, but then the chapter picks a model at the top of this range to do this. Also, based on the calibrated uncertainty language definitions 'very likely' means P>=90%, so the corresponding two-sided range would be 5-95%. The 90the percentile would only be relevant for a one-side confidence interval. Finally, the text here would only make sense as written if 'likely model range' is assessed as the 17-83% range - is that what is meant? [Nathan Gillett, Canada]	This is a very relevant comment. The selection of the model and the wording here is adjusted with the assessment of the likely and very likely range in GSAT.
36138	77	12			Given that you are showing projected climate change in prescribed concentration scenarios, the TCR of the models is more relevant than their TCREs. Differences in carbon cycle response across models will not affect their climate response under a given prescribed concentration scenario, at least not to first order. [Nathan Gillett, Canada]	Taken into account. Correct, it should be high TCR and in addition storylines on carbon cycle feedbacks is coordinated between chapter 4 and chapter 5.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
32890	77	15	77	15	Why are these values not with respect to preindustrial, as has typically been the convention? Having a common baseline seems a very important consistency to maintain. It would also be worth mentioning that this is the warming roughly consistent with the Cretaceous, when tropical plants were growing in polar latitudes, etc.--so a very different climate. [Michael MacCracken, United States of America]	Noted. The changes are here shown relative to present-day conditions in order to be consistent with section 4.5. Some of the values will also be given relative to pre-industrial conditions
39686	77	22	77	26	Check consistency with the risk-related discussion provided in section 1.2.4.1. of Chapter 1 that includes the risk definition agreed for AR6 across WGs [Carolina Vera, Argentina]	Taken into account. Text now refers to risk definition in chapter 1
36142	77	23	77	24	Since precipitation is projected to increase in some regions and decrease in others it is inevitable that there will be some regions in between where little change is projected - this is true for projected precipitation changes from all models, not just this particular one, so I don't think needs to be highlighted here. [Nathan Gillett, Canada]	Taken into account. The sentence is rephrased but it is important to highlight that the pattern associated with the high warming is not necessarily the one that shows the most pronounced changes in regional precipitation everywhere.
36144	77	28	77	30	Evaluating model performance in the historical period is done in Chapter 3 - Chapter 4 should report/summarise the assessment from Chapter 3 on this topic. [Nathan Gillett, Canada]	Noted. The statement is now better linked to the assessment of the historical model performance in chapter 3.
39016	77	43			Some of the "knowledge gap" items would be elaborated in SOD? [Masahide Kimoto, Japan]	Accepted, indeed the ones retained have been elaborated whereas most have been dropped.
8574	77	46	77	46	Not a WG1 issue [Robert Kopp, United States of America]	Taken into account. But uncertainty assessment of expected future climate change is fundamentally limited by the complete formal absence of knowledge of which scenarios are more likely to unfold than others. Therefore, all statements in Ch4 can only be made conditional on a particular scenario, but not in a holistic sense. This is a very serious limit to our assessment. The revised strategic role of this section -- now called "limits to the assessment" -- makes this point clearer and also gives a clear path forward for characterizing this limit without being policy-prescriptive.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
53452	77	46	77	46	As commented on earier in the chapter; there may be new knowledge withinh the "WGIII community" on this. I suggest you contact WGIII authors. [Jan Fuglestvedt, Norway]	Taken into account. Since this chapter has to rely on the SSPs, though, for which no probability information exists, the statement is correct as in FOD. The point has been expanded, though, in the SOD.
36146	77	46			Lack of probabilities for scenarios is not a knowledge gap. This is the framework in which IPCC works. IPCC reports are intended to inform policy, but not be policy prescriptive. Which scenario is followed depends on future decisions made by the parties to the UNFCCC. Assigning probabilities to the scenarios would be akin to telling the parties what decisions they are going to make, at least in a probabilistic sense. [Nathan Gillett, Canada]	Taken into account. But uncertainty assessment of expected future climate change is fundamentally limited by the complete formal absence of knowledge of which scenarios are more likely to unfold than others. Therefore, all statements in Ch4 can only be made conditional on a particular scenario, but not in a holistic sense. This is a very serious limit to our assessment. The revised strategic role of this section -- now called "limits to the assessment" -- makes this point clearer and also gives a clear path forward for characterizing this limit without being policy-prescriptive.
36148	77	48	77	49	There is literature on this, some of which is already assessed in the Chapter (see Box 4.1). [Nathan Gillett, Canada]	Accepted, indeed they have been. The revised strategic role of this section -- now called "limits to the assessment" -- implies that this point be dropped.
36152	77	51	77	52	This description is somewhat vague and open-ended. Steffen et al. (2018) is already assessed in the section on abrupt change. If retained, I suggest that more detail is added to this knowledge gap description, and the authors describe how this knowledge gap limits confidence in the chapter assessments on abrupt climate change, and how it is accounted for in the uncertainty framework used for projections (for example in the AR5 approach, is this one of the factors that leads to the model 5-95% range being interpreted as a 'likely' range rather than a 'very likely' range?' [Nathan Gillett, Canada]	Taken into account. Abrupt change is being assessed, therefore its mention has been removed from this section. The discussion of "unknown unknowns" has been broadened and generalized instead.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
32892	77	51	77	52	It might be indicated here that we are already experiencing some types of impacts that were not projected, such as the increase in waviness of the jet stream as the Arctic has warmed, leading to greater extremes. Also, Arctic sea ice is melting back at near or above the upper end of most model simulations done before it started happening. In fact, most uncertainties have tended to resolve on the more extreme end of the range. [Michael MacCracken, United States of America]	Noted. The revised strategic role of this section -- now called "limits to the assessment" -- implies, however, that these points are not taken up here, because they are covered elsewhere in Ch4.
8576	77	51	77	52	Somewhere discuss how the transition of 'unknown unknowns' to 'known unknowns' can lead to an expansion of quantified uncertainty? [Robert Kopp, United States of America]	Taken into account. The discussion of "unknown unknowns" has been broadened and generalized in the SOD.
36590	77				Knowledge Gaps. There is overwhelming evidence that climate variations in one ocean basin can significantly modulate the variability in other ocean basins. The recent review paper by Cai et al (Science 2019) is a start in the direction of assessing the importance of such interactions in the tropics. These interactions among ocean basins have affected the oceanic influence on continental climates, with associated impacts on societies. This is another example of questions that can benefit from large initial conditions ensembles. [Carlos Mechoso, United States of America]	Noted. The existence of literature implies that an assessment is possible, although not necessarily in this chapter.
32078	78	5	78	13	This is really the same issue as Knowledge Gap 3 on page 77: the models are imperfect and miss-out many processes which could be important, so we have significant ignorance of the true climate response to forcing. See my comment 22. [Rowan Sutton, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The discussion of "unknown unknowns" has been broadened and generalized in the SOD, and model imperfections have been merged with theses considerations.
26960	78	15	78	21	This paragraph is not clear. Please explain what is meant by "scenario robustness" and what "budgeting" means in this context. [Joachim Rock, Germany]	Accepted. The revised strategic role of this section -- now called "limits to the assessment" -- implies that this point be dropped.
30000	78	23	78	26	You could elaborate on this point a bit more. One basic knowledge gap is that we don't have a good estimate of what the range of forced circulation responses is in climate models, since the ensemble sizes are generally not large enough. One cannot begin to understand and reduce uncertainty until one quantifies it! [Theodore Shepherd, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account The revised strategic role of this section -- now called "limits to the assessment" -- implies that this point be dropped, because there is assessment elsewhere in the chapter.
36154	78	23	78	26	This knowledge gap on detection and attribution is out of scope for Chapter 4 - it is more relevant to Chapter 3. Secondly, the statement that 'more work needs to be done' is a research recommendation - research recommendations are not allowed in IPCC assessments. [Nathan Gillett, Canada]	Accepted.. The revised strategic role of this section -- now called "limits to the assessment" -- implies that this point be dropped.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
9076	78	26			There is a rich body of literature on "dynamical adjustment" methods aimed at addressing the multidecadal variability vs. forced response question. I recommend the following references (no need to cite all): Deser, C., L. Terray, and A. S. Phillips, 2016: Forced and internal components of winter air temperature trends over north america during the past 50 years: Mechanisms and implications. Journal of Climate, 29 (6), 2237–2258 /// Ruixia Guo, Clara Deser, Laurent Terray, and Flavio Lehner. (2019) Human Influence on Winter Precipitation Trends (1921–2015) over North America and Eurasia Revealed by Dynamical Adjustment. Geophysical Research Letters 46:6, 3426-3434 /// Flavio Lehner, Clara Deser, and Laurent Terray. (2017) Toward a New Estimate of "Time of Emergence" of Anthropogenic Warming: Insights from Dynamical Adjustment and a Large Initial-Condition Model Ensemble. Journal of Climate 30:19, 7739-7756 /// Claudio Saffioti, Erich M. Fischer, and Reto Knutti. (2017) Improved Consistency of Climate Projections over Europe after Accounting for Atmospheric Circulation Variability. Journal of Climate 30:18, 7271-729. /// Smoliak, B. V., J. M. Wallace, P. Lin, and Q. Fu, 2015: Dynamical adjustment of the Northern Hemisphere surface air temperature field: Methodology and application to observations. J. Climate, 28, 1613–1629. /// Thompson, D. W. J., E. A. Barnes, C. Deser, W. E. Foust, and A. S. Phillips, 2015: Quantifying the role of internal climate variability in future climate trends. J. Climate, 28, 6443–6456 /// Wallace, J. M., Y. Zhang, and J. A. Renwick, 1995: Dynamic contribution to hemispheric mean temperature trends. Science, 270, 780–782 /// Wallace, J. M., Q. Fu, B. V. Smoliak, P. Lin, and C. M. Johanson, 2012: Simulated versus observed patterns	Accepted, indeed there is. The revised strategic role of this section -- now called "limits to the assessment" -- implies that this point be dropped.
16334	79	1	79	55	This FAQ could be a bit confusing - on one hand it appears to say it is too difficult to provide information about climate change in the next twenty years due to natural internal variability, but then at the end states that it is possible and that globally averaged surface temperature is expected to continue to rise. The summary in italics is a lot clearer and provides a nice answer to the question. [Renee van Diemen, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. SOD text tries to follow chapeau in logic without repetition.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
32894	79	25	79	31	It seems to me it would be helpful to explain that whatwe hope that climate models are projecting is the change in the statistical distribution of weather conditions and that these are then averaged to get the average temperature change. As such, it is important to understand that what is generally happening is a reduciion in the likelihood of cool weather and an increase in the likelihood and intensity of warm weather systems, etc. I think it important in presenting such answers to FAQs that it be made clear that the models do not project the change in climate directly (such that every day is so much warmer, etc.), but that they are designed to generate the statistical distribution of the weather under different boundary/externally changed conditions. Otherwise, people will say, if you can't predict the weather, how can you predict (really, project) the climate? Because it is the statistical distribution that is being simulated (and that shows up so well in the shifting bell curves in the Hansen et al. analysis of past summertime conditions), the models can be used to get a sense of changes in extreme weather conditions as well--and the percentage increase in extremes (as compared to a past baseline) can be disproportionately large as the distributions shift. [Michael MacCracken, United States of America]	Taken into account. The FOD text already builds the contrast to weather prediction; however, reference to extremes might overburden this FAQ and should be covered in Ch11.
36156	79	30	79	31	Specify the timescale over which these predictions can be made. [Nathan Gillett, Canada]	Accepted and implemented.
36158	79	44			Calibrated uncertainty language is not allowed in FAQs. [Nathan Gillett, Canada]	Accepted and implemented.
37716	79	45	79	46	This sentence seems to be a reasonable one, as was the statement in SR1.5. But it is at odds with the 2025 date that is first seen very early in the chapter. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Noted. This poses a challenge for the chapter, though less so for this FAQ.
36160	79	48			Calibrated uncertainty language is not allowed in FAQs. [Nathan Gillett, Canada]	Accepted and implemented.
36162	79	49	79	51	First - why focus on global land average precipitation, when this is composed of increased in some regions and decreases in others? And when regional changes are more relevant to readers? Based on Section 4.4.1.3 and Figure 4.10, there are regions in which robust changes in precipitation are projected for the next 20 years. So rather than saying that we cannot say much about how precipitation averaged globally will change, I suggest something like 'Precipitation is expected to increase in much of the polar regions and parts of the mid-latitudes, and to decrease in much of the dry subtropical regions. Away from these regions, natural variability dominates, and there is substantial uncertainty about how precipitation over the next twenty years.' [Nathan Gillett, Canada]	Taken into account. Precipitation has been dropped from this FAQ, to make it more accessible for the target audience.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
32964	80	3			For panel d) use sea-level projections from Ch9 (Kopp/Slangen) [Aimee Slangen, Netherlands]	Taken into account. SLR has been dropped from this FAQ, to make it more accessible for the target audience.
16336	81	1	81	6	It might be helpful to define 'climate inertia' [Renee van Diemen, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Rephrased.
9078	81	1			Perhaps "are reduced", "decrease", or "subside"? [Anna Merrifield, Switzerland]	Noted. "reduce" was suggested by editor.
53456	81	3	81	9	useful! [Jan Fuglestvedt, Norway]	Noted, thank you.
53454	81	11	81	11	Unlogical sentence. Reductions in GHGs may not reduce CO2 (if non-CO2 are reduced...) [Jan Fuglestvedt, Norway]	Taken into account. Rephrased.
36166	81	12	81	13	Replace 'one fundamental element of inertia in the climate system' with 'the very long lifetime of carbon dioxide in the atmosphere'. [Nathan Gillett, Canada]	Taken into account. Rephrased.
36164	81	12			Replace 'but will over the first few decades not yet lead to a decrease in concentrations' with 'but will only lead to a decrease in CO2 concentrations if CO2 emissions approach zero'. [Nathan Gillett, Canada]	Accepted and implemented.
36168	81	21	81	35	This discussion may be too technical for a FAQ. [Nathan Gillett, Canada]	Taken into account. Text rewritten to make it less technical and more focused on core message. .
16338	81	33	81	35	This FAQ might be strenghtened by explaining how scientists know that a response will emerge in the second half of the century (i.e. if it is so difficult, and there is no broad quantitative consensus, where does the high confidence come from?) [Renee van Diemen, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Text rewritten to make it less technical and more focused on core message. .
37718	81	43			"the model's simulation of" could be inserted before "natural". The model may not simulate natural variability very well, in which case what is shown is not a manifestation of true natural variability. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. "Simulated" added before "natural".
36170	82	1	81	44	This question is about future regional climate change, but the answer focusses on pattern scaling. Pattern scaling is just one tool to assess regional climate change. General readers will be more interested in robust projections of actual regional climate changes than discussion on the strengths and weaknesses of pattern scaling, which will be mainly of interest to a scientific audience. I suggest that the discussion of pattern scaling in the first paragraph is sufficient, and then the rest of the answer is replaced with text discussing actual regional climate changes. For example, rather than saying that pattern scaling is not robust for sea-ice and snow cover, just describe projected regional changes in these variables, perhaps for low and high emissions scenarios. [Nathan Gillett, Canada]	Taken into account. This FAQ aims to explain why knowledge of a warming level can suffice to provide much spatial information, irrespective of scenario and time at which this warming level is reached. It is then important also to state for which quantities this might not work. That said, the change in title, which now omits the word "regions", hopefully sets readers' expectations straight.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
53458	82	1	82	56	Is FAQ 4.3 more relevant for chapter 10? [Jan Fuglestvedt, Norway]	Taken into account. Change in title, which now omits the word "regions", hopefully sets readers' expectations straight.
30002	82	8	82	11	Precipitation changes are also highly uncertain in many regions (outside of the highest latitudes) because of the role of the forced atmospheric circulation response, which is itself highly uncertain. [Theodore Shepherd, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Explanation added.
30744	82	15	82	25	you discuss here more about time than about regions (in the title) [Annalisa Cherchi, Italy]	Taken into account. Change in title, which now omits the word "regions", hopefully sets readers' expectations straight.
30004	82	34	82	36	This comment appears to be confusing two different things. The term "pattern of climate change" is usually understood as the forced response, because internal variability is not climate change. Of course, the specific manifestation of precipitation over a limited time period at a given warming level is subject to internal variability, but that is just sampling error, not a systematic effect on the pattern of climate change. [Theodore Shepherd, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. "Pattern" sent the wrong signal in line 36. Rephrased.
30746	82	39	82	44	you discuss here more about time than about regions (in the title) [Annalisa Cherchi, Italy]	Taken into account. Change in title, which now omits the word "regions", hopefully sets readers' expectations straight.
46260	82	39	82	44	Reference must be provided for this item. Or be cited to the previous sections [sadegh zeyaeyan, Iran]	Rejected. FAQs must not contain references to the original literature. Pattern scaling is assessed, including references, in Section 4.2.4, on which this FAQ text is based.
8894	82	39	82	44	Reference must be provided for this item. Or be cited to the previous sections [Mohammad Javad Zareian, Iran]	Rejected. FAQs must not contain references to the original literature. Pattern scaling is assessed, including references, in Section 4.2.4, on which this FAQ text is based.
57536	82	39	82	44	Reference must be provided for this item. Or be cited to the previous sections [Sahar Tajbakhsh Mosalman, Iran]	Rejected. FAQs must not contain references to the original literature. Pattern scaling is assessed, including references, in Section 4.2.4, on which this FAQ text is based.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
32966	82	39	82	44	Not completely true, pattern scaling for sea level can work for individual components of sea-level change (see for instance Bilbao et al 2015: Analysis of the regional pattern of sea level change due to ocean dynamics and density change for 1993-2099 in observations and CMIP5 AOGCMs) [Aimee Slangen, Netherlands]	Accepted. Sentence removed.
26876	82				Can we add a FAQ which addresses the tipping points? "What are "tipping points". Why are they relevant for climate change?" [Thomas Ackermann, Germany]	Rejected. Ch4 and WGI FAQ author teams agreed to keep the current Ch4 FAQ list.
54226	113	1	113	28	More large ensemble results should be included here [Nicola Maher, Germany]	Accepted. Included in SOD.
25636	113	2	113	2	In addition to showing anomalies, show actual temperatures as reported by the models, and also observations. [Stephen E Schwartz, United States of America]	Rejected. The objective is to analyse temperature changes that occur du to changes in the external forcing and internal variability. AR5 already showed lack of correlation between simulated absolute temperature and ECS.
30274	113		163		Most of the figures' resolution is very low [Nazan An, Turkey]	Taken into account. This is a production issue outside of the control of Ch4 authors. Apparently, there was a problem with file size of full-resolution pdf files. Hopefully improved in SOD.
32968	114	1	114	13	I would suggest to change 'global sea level change' to 'thermosteric sea level change' in the figure and in the caption, for clarity [Aimee Slangen, Netherlands]	Accepted.
30410	114	1	114	13	During the scenario cross-chapter and cross-WG coordination also the lowest scenario available in the ScenarioMIP set (SSP1-1.9) was recommended to be included as default scenario in plots (of course, if available from CMIP6 models as this is marked tier 2 because at the time of the ScenarioMIP prioritisation decision the Paris Agreement wasn't agreed upon yet). See Chapter 1 Box 1.6. [Joeri Rogelj, Austria]	Accepted.
56258	114	1			Figure 4.1, top left: Since definition of global warming compared to pre-industrial time frame is most relevant in public discourse, I strongly recommend setting the y-axis displaying temperature compared to pre-industrial on the left (where it is generally expected) and the one showing warming compared to present on the right. [Sonia Seneviratne, Switzerland]	Rejected. Following the proposal would destroy the internal consistency of the display across panels a, b, and d. Note that in b and d no ready observations-based conversion to anomalies relative to 1850--1900 can be performed.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
56260	114	1			It seems that chapter 4 is using GSAT as the definition of global warming. However, for consistency with observations, and also for consistency with the IPCC SR15 report, it would likely be better to use GMST (i.e. surface warming on the oceans). [Sonia Seneviratne, Switzerland]	Rejected. GSAT is the quantity provided by the modelling centres, and for this reason we employ GSAT here.
25638	114	2	114	2	It would seem essential to show results for individual models, necessarily it would seem, in multiple panels, perhaps in supplemental. . [Stephen E Schwartz, United States of America]	Rejected. This comment is better directed to the Atlas authors. The Atlas may or may not be providing individual model results. Chapter 4 will not.
25640	114	2	114	2	A useful addition to the figure would be panel(s) showing time series of transient sensitivity, estimated as DeltaT/Forcing, both with reference to the same time frame. Examination of this intensive variable across models and as a function of time for a given model run would highlight differences among the models and over time much better than plots of the extensive variable DeltaT [Stephen E Schwartz, United States of America]	Rejected. This is closer to the mandate of Chapter 7.
46262	114	4	114	14	The pattern shown in Figure 4.1 should be changed. It's very cluttered and inaudible [sadegh zeyaeyan, Iran]	Accepted.
8896	114	4	114	14	The pattern shown in Figure 4.1 should be changed. It's very cluttered and inaudible [Mohammad Javad Zareian, Iran]	Accepted.
57538	114	4	114	14	The pattern shown in Figure 4.1 should be changed. It's very cluttered and inaudible [Sahar Tajbakhsh Mosalman, Iran]	Accepted.
13188	116	1	116	13	What are the uncertainites for these model simulations? Include this in the figure and/or figure caption. [Nora Richter, United States of America]	Accepted.
31956	116	1	116	13	What are the grey boxes?Near term, Mid term and Long term as in Figure 4.1 It should be repeated for all the figures. [Marie-France Loutre, Switzerland]	Accepted.
32896	116	5	116	5	Why does this plot not include observations? My sense is that observed sea ice loss is greater than the models are showing and, unpleasant at it may be, this needs to be shown in the plot. [My hypothesis for the explanation is that the models got generally calibrated at a time when sulphate aerosols were brightening clouds in the Arctic, and the reduction of pollution in the Arctic as North America and Europe (which has not until recently been captured in model simulations) reduced their SO2 emissions has led to less bright clouds in spring and so earlier melting of snow cover and so a longer period of ice thinning, etc. In any case, observations need to be added to this figure, which seems quite optimistic with respect to September sea ice cover. [It would also help to make clear that this polot is of September average sea ice cover and not minimum September sea ice cover that is often how observations are presented.] [Michael MacCracken, United States of America]	Rejected. The mandate of Chapter 4 is mainly on future projections. Chapter 3 compares observed and simulated changes over the historical period.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
9154	116	5	116	12	It is noteworthy that Fig 4.3 shows Arctic sea ice growing up until 1979, which coincidentally is when satellite monitoring started. [Jim O'Brien, Ireland]	Noted.
42748	116		117		Fig 4.3 and 4.4 - add some division or distinction between historical and RCP8.5 model-projected data (both), and perhaps in-graph label/text for the 2100 individual model value circles (4.3) [Stephanie Courtney, United States of America]	Accepted.
13190	117	1	117	12	What are the uncertainties for these projections? Include this in the figure and/or in the figure caption. [Nora Richter, United States of America]	Accepted.
54228	117	1	117	12	This has been done by Maher et al 2019 (MPI-GE) and Maroon et al 2018 (CESM-LE), these can be compared to in the results. [Nicola Maher, Germany]	Rejected. This figure now shows CMIP6 transient simulations.
25642	118	1	118	1	Suggest that when final figure is presented, there be a single panel for each model, showing the time series for each of the scenarios (perhaps also a measure of spread if there is an ensemble of runs). Putting results for a dozen or so models in a single panel would perhaps be too complicated. [Stephen E Schwartz, United States of America]	Rejected. Individual model curves are better relegated to the Atlas.
25644	118	1	118	1	I am surprised at the great difference over the historical era between the two models shown in panel a; it would seem that this would require some discussion once the results from all the models are available. [Stephen E Schwartz, United States of America]	Accepted. Multimodel results now shown and assessed.
27212	118	5	118	9	The findings of https://doi.org/10.1038/s41467-019-08633-z should significantly modify the projections of Figure 4.5 [François GERVAIS, France]	Rejected. This reference is more relevant to Chapter 9 than Chapter 4.
13192	119	1	119	13	It might be easier to interpret and/or look at these figures if they were displayed with a smoothing average on top of the "annual" variations shown. [Nora Richter, United States of America]	Taken into account. The multimodel ensemble averages are now plotted which by construction have smaller variance.
48470	119	4	119	4	suggest decadal means are used instead of yearly [Julie Arblaster, Australia]	Taken into account. The multimodel ensemble averages are now plotted which by construction have smaller variance.
36172	119				Figure 4.6. Show the multi-model ensemble mean. [Nathan Gillett, Canada]	Accepted. These have been added.
36174	120	6			Clarify whether this 'climatological monthly mean' is for the whole time period, or for each 50-year period separately. I think it's the former, otherwise long-term changes in the mean climate would give apparent changes in variability. [Nathan Gillett, Canada]	Not applicable. The method for computing ENSO variance has been updated in SOD.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
25646	121	1	121	1	In addition to showing anomalies, show actual temperatures as reported by the models, and also observations. [Stephen E Schwartz, United States of America]	Rejected. GMST observed timeseries are constructed based on anomalies not absolute temperature. Furthermore, there is little evidence for a dependence of key climate response measures, such as ECS, on background state in models. See e.g. Stolpe et al., 2019 doi: https://doi.org/10.1007/s00382-019-04849-3
25648	121	1	121	1	I am pleased to see results of emulator being given prominence here. It would seem that a citationis required to the primary literature in which the emulator work and results are reported. [Stephen E Schwartz, United States of America]	Taken into account. The emulator results are updated in the SOD to also include the FAIR model (Smith et al., 2018) and the relevant literatures are added to the SOD.
36176	121	9	121	13	The emulator results sample assessed uncertainty in ECS, but not assessed uncertainty in ocean heat uptake efficiency (only one value is used), or in historical forcing (only one timeseries is used). Both ocean heat uptake efficiency and historical radiative forcing changes are uncertain, and thus the results of the emulator should not be interpreted as an overall 'very likely' range in projected temperatures. The authors should sample over uncertainties in radiative forcings and ocean heat uptake efficiency as well. [Nathan Gillett, Canada]	Taken into account.. The emulator results are updated for the SOD using the FAIR model which is also applied in chapter 7 (Smith et al., 2018). FAIR allows for other uncertain physical parameters to be sampled (ERF, ocean heat update) and hence a more representative uncertainty range can be reached. See e.g.. Figure 7.13 in chapter 7.
29862	122	1	122	9	Very low resolution [Mustafa Tufan Turp, Turkey]	Taken into account. The resolution of the figures is improved in the SOD
13194	122	1	122	12	In figure caption, mention what is the difference between stippling and hatchmarks in the figure. [Nora Richter, United States of America]	Noted. This explanation is added to the captions in the SOD.
15588	122	1	163	1	General comment on the figures: would like to suggest the authors to make sure of the use of the same color bar (color palette) for temperature/rainfall for the all plots and a uniform plot projection (e.g. figure 4.47 vs 4.42 vs 4.39 vs 4.36) [Izidine Pinto, South Africa]	Taken into account. The chapter team worked towards more consistency across figures for the same quantities.
29864	123	1	123	10	Very low resolution [Mustafa Tufan Turp, Turkey]	Taken into account. The resolution of the figures is improved in the SOD
29866	124	1	124	10	Very low resolution [Mustafa Tufan Turp, Turkey]	Taken into account. The resolution of the figures is improved in the SOD

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
42750	126				Fig 4.13 and 4.14 are the examples I noticed but good to watch throughout the report - for the most part a particular color-coding is used for the SSPs, I think that coding is useful, so it would be helpful to adjust color coding in 4.13-14 to match the colors used in other graphs. [Stephanie Courtney, United States of America]	Accepted.
36178	127				Figure 4.14. First - PgC per decade is a unit of flux, not a trend in fluxes. Should this be PgC/yr per decade? Second - I suggest replacing with projected trends just for the periods 2021-2030 and 2021-2040 and comparing unitialised and DCPP initialiseds predictions. [Nathan Gillett, Canada]	Noted. The units are corrected in the SOD. Initialised forecasts will not be presented here.
36180	128	9	128	10	Most of the chapter shows the sample range across individual simulations e.g. 5-95% spread across individual simulations - this is the relevant quantity if we want to know about the uncertainty in future changes in the real world. Why show uncertainty in the ensemble mean in this figure? [Nathan Gillett, Canada]	Taken into account The Figure is revised.
29868	129	1	129	7	Very low resolution [Mustafa Tufan Turp, Turkey]	Taken into account. The figure quality is improved in SOD.
54230	131	1	131	18	Decide which Nino indicie to use and be consistent [Nicola Maher, Germany]	Taken into account. Using Technical Annex on the Modes of Variability, definition of mode of variability including ENSO is consistent across Chapters.
25650	132	1	138	1	Suggest that supplementary figures be made available to show results from individual models, as in previous reports. [Stephen E Schwartz, United States of America]	Not applicable. Atlas will show individual models' results.
25652	134	1	134	1	Suggest that plots of quantities vs latitude be plotted on sine (latitude) scale, labeled in latitude, so as not to overly weight high latitudes in visual image. [Stephen E Schwartz, United States of America]	Rejected.
27214	134	5	134	7	Temperature measurements of UAH MSU tropical at 200-300 hPa show no significant trend and do not validate, therefore, the projections of the models in Fig. 4.21. [François GERVAIS, France]	Rejected. No significant trend in recent observation doesn't rule out possibility for future change.
36184	134				Figure 4.2.1. A cooling is shown near 1000 hPa over the Antarctic in SSP5-8.5. Is this just an artefact of extrapolating temperature changes below the surface of Antarctica? I suggest masking out areas below the surface, if this has not already been done. [Nathan Gillett, Canada]	Taken into account. Area is now blanked out

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
56002	136	1	136	10	An increase in WBG-Temerature matters much more when WBGT already is high. Therefore, I suggest to show absolute WBGT instead of WBGT changes; have color-coding such that values > 28 °C are distinctly noticable. However, with this you need to show an additional row at top with present day reference values (also averaged over a 20 yr period). For each line of figures: in the figure caption please specify the average annual temperature increase vs. the pre-industrial era which corresponds to the time slice shown (for reference), i.e. for the present day reference, for 2041-2060, and for 2081-2100. [Urs Ruth, Germany]	Take into account.
51930	136	1			I am very heartened to see both the discussion of WBGT and its presentation here. However, the impact of WBGT is on exceedance of absolute thresholds. Therefore a more appropriate way of visualizing this may be exceedance of absolute threshold values rather than changes. A change in WBGT of several degrees in Siberia is of no consequence whereas it is in SE Asia. Could these maps instead show changes in areas where WBGT exceeds 30/35C? Or perhaps use internationally recognized health impact thresholds? [Peter Thorne, Ireland]	Take into account.
46992	143	1	143	1	In the future equivalent of Figure 4.30, please consider separating out the signals of ocean warming between the Atlantic Ocean and the combined Indian and Pacific Oceans.Doing so would greatly help illustrate AMOC-related changes compared with those changes in other watermasses or in the wind-driven gyres, and will hopefully make the text more readily comprehensible. [Robert Hallberg, United States of America]	Taken into account. We update this figure to show temperature change for different ocean basins using CMIP6 results.
36186	145				Figure 4.32. Why only show SSP5-8.5, especially given the sensitivity of projected SAM changes to the scenario, as discussed in the text. I suggest also showing SSP1-2.5. [Nathan Gillett, Canada]	Taken into account. The Figure is revised.
36188	151				Figure 4.38. This figure lacks a y-axis label, and the x-axis label does not specify the variable. I suggest showing GSAT on the x-axis and Nino3.4 on the y-axis, and starting both axes from zero. A 1:1 line could also be included. If I understand this figure correctly, the message seems to be that Nino3.4 warms slightly less than the global mean for warming levels up to 4 C. The figure doesn't show anything about changes in ENSO variability. [Nathan Gillett, Canada]	Not applicable. The figure is removed.
36190	152	6	152	8	The caption does not explain clearly what is shown in the figure. [Nathan Gillett, Canada]	Taken into account.
9156	153	4	153	9	See previous comment 6 above about AR5 models being "overheated" compared to observations. [Jim O'Brien, Ireland]	Taken into account. This figure and caption have been revised in SOD

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
32898	155	3	155	3	What is not conveyed in this type of plot is whether the return is within the natural variability (perhaps the 1 sigma extent) of the unperturbed case. Showing just centroid values without any consideration of ranges is really not conveying the significance or not of the differences that remain. [Michael MacCracken, United States of America]	Taken into account. We have now updated the figure using GeoMIP results. We will consider to show if the changes are above the internal variability in the final draft.
55542	155		155		If the purpose is to show that these SRM would compensate for climate change "disparities in spatial pattern of temperature and precipitation change," images that show the difference between each method and CO2 reduction would be more informative. [Matthisa Honegger, Germany]	Taken into account. The current figure indeed shows the pattern of changes for a CO2 reduction and an equivalent SRM forcing. We have now updated the figure using GeoMIP results.
36192	155				Figure 4.42. The figure shows the differences between geoengineered climates designed to offset global warming from doubled CO2, and the 2xCO2 climate. This presentation makes all the geoengineering approaches look about the same, and makes it hard to identify where the geoengineering approaches do and do not offset the CO2 doubling. Instead plot the temperature and precipitation in the geoengineered climate minus the preindustrial control - SOL - 1xCO2, SAI - 1xCO2 etc. [Nathan Gillett, Canada]	Taken into account. For SOD, we have now updated the figure using GeoMIP results. The point here is to show that for the same amount of cooling achieved by different SRM methods, pattern of temperature and precipitation change is different. Text is revised for the SOD.
56004	156	1	156	14	This figure is much too positive. If viewed alone, it could suggest that everything could be done by SO2 incection. You have to include in additional panels: (i) ocean acidification continues to increase, (ii) pH of tropospheric rain, (iii) adverse precipitation changes. [Urs Ruth, Germany]	Taken into account. We have now updated the figure using GeoMIP results. However, the carbon and other biogeochemical cycle changes are discussed in Chapter 5 as discussed in the text. We are not aware of literature that has assessed the pH change in rainwater for SRM. However, some studies have shown that stratospheric sulphate aerosol SRM would not significantly cause an increase in acid rain. A discussion here on this issue is beyond the scope.
36194	160	4	160	5	Explain the figure better. First SSP1-2.6 is mentioned in the caption, but isn't in the figure. Second, explain that this figure just shows results from one model which warms strongly. [Nathan Gillett, Canada]	Taken into account. The figure and caption are updated accordingly.
36196	161	3	161	5	I'm not convinced that global land-average precipitation is 'an icon of global change'. It represents an average across regions of increase and regions of decrease, and it is these regional changes which exhibit a higher signal to noise ratio, and are more relevant to impacts. The authors could consider instead showing precipitation averaged over two regions - such as a high latitude region and a subtropical region. [Nathan Gillett, Canada]	Taken into account. Revised figure only shows GSAT and Arctic sea-ice area.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
36198	162	10			The two periods described have different lengths - why compare trends over two periods of different lengths? [Nathan Gillett, Canada]	Taken into account. Figure is a placeholder. Typo; it should have read 2006--2020, as correctly stated in figure label.
35840					The Chapter and ES overall reach a very positive assessment of SRM. On SRM, AR5 SPM assessed 'Modelling indicates that SRM methods, if realizable, have the potential to substantially offset a global temperature rise, but they would also modify the global water cycle, and would not reduce ocean acifidication'. SR1.5 assessed 'Although some SRM measures may be theoretically effective in reducing an overshoot, they face large uncertainties and knowledge gaps as well as substantial risks and institutional and social constraints to deployment related to governance, ethics, and impacts on sustainable development. They also do not mitigate ocean acidification. (medium confidence).' The assessment in the Chapter ES here is 'Modelling studies have consistently suggested that SRM can markedly dimish global and regional climate change... a combination of stratospheric aerosol injection and cirrus cloud thinning is expected to offset global temperature changes and precipitation changes simultaneously. Model simulations suggest that by injecting aerosols into the stratosphere at multiple locations and by adjusting the annual rate of injections, multiple temperature targets, such as GSAT, equator-to-pole temperature gradient and inter-hemospheric temperature gradient can be met simultaneously (low confidence).. There is high confidence that sudden termination of SRM would cause a rapid increase in temperature, but a gradual phase-out of SRM combined with mitigation and CDR could avoid the risk from sudden SRM termination.' seems to suggest that all the major technical problems associated with SRM have been solved. Does the new literature since AR5/SR1.5 really support such a change in the assessment? Moreover, even though ocean	Taken into account. The SRM sections have been re-written, using as both framing and starting point the assessments from AR5 and SR1. A cross-chapter coordination across WGI has been also organized for the rigorous assessment of SRM.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
37634					It is hard to review this chapter due to paucity of results to date from CMIP6, but what is there at present is disturbing. Early on we read that the available CMIP6 results indicate that 1.5ºC of warming since 1850-1900 is projected to occur around 2025 on average, a year that is five years earlier than the beginning of the 2030-2052 likely range identified in SR1.5, and a year that appears unlikely from the observational record as it stands in mid-2019. We then learn that predictions initialized using recent observations, for one particular model, gives warming more consistent with recent observations and the SR1.5 projection. But not long after we are presented with a table showing an average 26-year temperature increase from 1995-2014 to 2021-2040 of 0.8ºC, which is larger than the 39-year change from 1980-2018 that is estimated from observations. 2025 begins only about five and a half years from now. So if forthcoming CMIP6 results tell the same story there is some explaining to do, and it would need to be done up front, where reference to the year 2025 first appears. Is it the design of this CMIP exercise? Are the models not fit for the purpose of short-term projections? Are we in for an unexpected rise in the rate of global warming, having been lulled into a false sense of security by natural variability? If the forthcoming CMIP6 results do not change the picture, consideration should be given to showing the initialised predictions first. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The FOD was based on a small subset of CMIP6 results; the SOD contains a full uncertainty assessment including the effects of high ECS and a comparison to SR1.5 For the full uncertainty assessment, Chapter 4 is using not only the CMIP6 models, as an essential input, but also CMIP5 models, large initial-condition ensembles and emulators.
6918					The discussion of geoengineering focuses on its climatic effects, but enumeration of risks is limited and the risks are not mentioned in the executive summary. I think it is also problematic to discuss geoengineering while leaving out relevant ethical questions (see Tuana et al., 2012). In my view, the major risk is that the geoengineering may, qualitatively speaking, push the climate system into a state which it may have not experienced in a very long time, or which it may have never experienced. This raises a prospect of so-called "unknown unknowns". Not considering this and other, more specific risks in depth may give an impression to policy-makers that following a high-emissions scenario is OK, because we can fix any problems using geoengineering if needed. Considering this, I believe the question of geoengineering may be better placed in a report of another working group, or into a special report of its own, rather then in this chapter. Reference: Tuana, N., R. Sriver, T. Svoboda, R. Olson, P. Irvine, J. Haqq-Misra, and K. Keller (2012): Towards integrated ethical and scientific analysis of geoengineering: A research agenda. Ethics, Policy & Environment. 15(2), pp. 136-155. [Olson Roman, Republic of Korea]	Taken into account. The SRM sections have been re-written, using as both framing and starting point the assessments from AR5 and SR1.5. We largely agree with the reviewer's comment. However, the expected climate response to geoengineering should be assessed in WGI. A cross-chapter coordination across WGI has been also organized for the rigorous assessment of SRM.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
38426					The term „inferred“ requires somewhat more explanation [Dirk Notz, Germany]	Taken into account. Presumably the comment refers to page 4-5 line 38. An attempt was made to be more explicit.
56860					Figure General comments chapter 4: ideally, the figure should be a bit more independent from the caption => some crucial information in the caption should be included directly in the figurel (e.g. color legend) //Figures and caption should be more independent from the main text => spell out acronyms in figure and/or caption wherever possible, see AMOC in figure 4.4 as an exemple (model acronyms are not expected to be spelled out). // you can add titles to your figure to enhance the understanding at first glance (e.g. in figure 4.1)// what Stippling and hatching represent should be explained in caption // 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. SOD figures follow the style guide more faithfully.
56862					Figure 4.1: explaining also in the figure what the numbers on the top right corners are would be less confusing (i.e. models used: 5). This annotation could be added in smaller font under the plot title [WGI TSU, France]	Accepted.
56864					Figure 4.2: explaining also in the figure what the numbers on the top right corners are would be less confusing (i.e. models used: 5). This annotation could be added in smaller font under the plot title [WGI TSU, France]	Accepted.
36130					The glossary of the AR5 Synthesis Report defines abrupt climate change in this way: Abrupt change refers to a change that is substantially faster than the rate of change in the recent history of the affected components of a system. Abrupt climate change refers to a large-scale change in the climate systemthat takes place over a few decades or less, persists (or is anticipated to persist) for at least a few decades and causes substan-tial disruptions in human and natural systems. {WGI, II, III}. This is the same as the definition using in Chapter 12 of the WGI AR5 report (see Section 12.5.5.1). Note that the glossary definition refers to all three working groups implying that this definition is used across all three working groups. Have the authors agreed on the new proposed definition with WGII and WGIII? While I don't have a strong opinion on the actual definition used, I recommend that unless the authors get agreement from the other WGs on the new definition of abrupt climate change, or otherwise continue to use the AR5 definition. Otherwise there will be an inconsistency across the three WG reports. [Nathan Gillett, Canada]	Taken into account. The definitions have been harmonised.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
56866					Figure 4.3: shaded areas should be annotated (like in previous figures) // green-red together should be avoided for colorblind vision // the color for RCP 8.5 should be RGB 185 0 2 [WGI TSU, France]	Accepted.
56868					Figure 4.7: the legend for the thick line (mean) and thin line (single simulations) could be added in the figure [WGI TSU, France]	Rejected. In the SOD, Figure 4.7 does not show individual models curves. Rather, it shows the ensemble mean (over many more model simulations) and 5-95% confidence intervals. Identifying these elements in a legend will be unnecessary.
56870					Figure 4.9: What the stippling and hatching represent should be explained in the caption. [WGI TSU, France]	The caption has been updated to describe the meaning of stippling and hatching.
56872					Figure 4.11: What the stippling and hatching represent should be explained in the caption. [WGI TSU, France]	The caption has been updated to describe the meaning of stippling and hatching.
56874					Figure 4.17: this figure does not stand by itself without the main text, the context is not understood clearly. It would help to spell out GSATin caption and add a title that is refering to Global surface air temperature and volcanoes // color legend is missing in figure [WGI TSU, France]	Taken into account. The figure is revised with higher quality.
56876					Figure 4.20: This figure should have a title like the previous one // (% of…) should be added close to the color bar [WGI TSU, France]	Accepted.
38958					Despite unavailability of the majority of CMIP6 data, the chapter structure and (tentative) list of executive summary are much better defined than the reviewer has expected. The IPCC assessment goes far more than simply reviewing the existing literature, and the writing team's efforts are much appreciated. [Masahide Kimoto, Japan]	Noted, thank you.
56878					Figure 4.23: the color bar is slightly different from the one in the Visual Style Guide for temperature change. This should be updated according to the Visual Style Guide RGB codes for consistency throughout the report. [WGI TSU, France]	Accepted.
56880					Figure 4.27: This figure would benefit from a title on top of the pannels "Multi-model mean change in winter" (similar to Figure 2.25 [WGI TSU, France]	Taken into account.
43314					This chapter looks great except the labels in Fig. 4.16 page 129; Fig. 3.34 page 149; Fig. 4.44 page 157; FAQ 4.1 Fig. 1 page 161 [Onema Adojoh, United States of America]	Taken into account. Figures have been improved for the SOD.
56882					Figure 4.28: Caption should rather say "Multi-model mean change of extratropical storm track density in winter" [WGI TSU, France]	Taken into account.

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56884					Figure 4.29: A title would help putting the figure in context without refering to the caption (e.g." atmospheric blocking frequencies"). Also it should be clearer which data correspond to "present-day" (is it "historical" ? if so, it is confusing to call present day in caption what is presented as historical in legend) and which one to future climate in the caption [WGI TSU, France]	Taken into account.
56886					Figure 4.30: This figure would benefit from a title on top of the pannels (e.g "mean ocean temperature"), see Figure 2.25 for visual example // the color bar is slightly different from the one in the Visual Style Guide for temperature change. This should be updated according to the Visual Style Guide RGB codes for consistency throughout the report. // it is unclear what stippling and hatching represent [WGI TSU, France]	Taken into account.
56888					Figure 4.31: legend is missing in the figure (dotted line, colorbar) [WGI TSU, France]	Taken into account.
56890					Figure 4.34: this figure is hard to read // label/units are missing // latitude/longitude are not necessary according to other map figures in the chapter // "tas" should be spelled out [WGI TSU, France]	Taken into account.
56892					Figure 4.35: color palette is not properly sequential. [WGI TSU, France]	Accepted. The colour palette is revised.
56894					Figure 4.36: this figure is hard to read // label/units are missing // latitude/longitude are not necessary according to other map figures in the chapter // "ugs" should be spelled out [WGI TSU, France]	Accepted. The graphics of this Figure is revised as suggested.
56896					Figure 4.39: it take some time to understand what is (a) and what is (b) as this is not explained in the caption // color palette has to be revised - consult the IPCC Visual Style Guide or contact the graphic officer at the TSU [WGI TSU, France]	Taken into account - the figure is revised
56898					Figure 4.40: acronyms GSAT and GSMT should be spelled out // the colors chosen for RCP should be the ones from the IPCC Visual style guide // Units should be °C for consistancy throughout the chapter and report. [WGI TSU, France]	Taken into account. The figure and the caption are changed in SOD
56900					FAQ 4.1 figure: this figure should be redisgned for lay out audience [WGI TSU, France]	Accepted.
56902					FAQ 4.2 figure: this figure as it is is not suitable for a lay audience [WGI TSU, France]	Accepted. SOD uses redesigned figure.
56904					FAQ 4.3 figure: this figure should be redisgned for lay out audience [WGI TSU, France]	Accepted.

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16208					It is evident from Chapter 4 that the parts on CDR were written from a position of an honest scientific interest and balance that seeks to adequately reflect the science on CDR, including the risks, uncertainties and inefficacies, whereas the SRM parts are clearly written from a pro-SRM standpoints that consistently and systematically downplays the profound risks and potential for climate disaster arising from such disruptive technologies. [Linda Schneider, Germany]	Taken into account. The SRM sections have been re-written, using as both framing and starting point the assessments from AR5 and SR1.5.
36182					The Atlas is only referenced in 4.6.1 currently, but the projections shown there presumably including percentiles of projected changes will be relevant to other sections too. I suggest cross-referencing the Atlas more in the SOD, especially for discusion of uncertainties in projected changes. [Nathan Gillett, Canada]	Taken into account. Cross-referencing to other chapters, including the Atlas, is more extensive in the SOD.
46678					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]	Taken into account. MoVs have been harmonised across WGI. To help the harmonization and avoid unnecessary overlaps, the technical annex on MoVs has been worked out in coordination with Chapters 2, 3, and 8. However, some overlaps may be inevitable for each chapter to be stand-alone.
46680					projection changes of ENSO, IOD, NAM, SAM, NAO,PDO show overlap with Section 8.4.2.5 [WGI TSU, France]	Taken into account. MoVs have been harmonised across WGI. To help the harmonization and avoid unnecessary overlaps, the technical annex on MoVs has been worked out in coordination with Chapters 2, 3, and 8. However, some overlaps may be inevitable for each chapter to be stand-alone.
35978					This chapter does not contain projections of seasonal mean temperature. It shows changes in seasonal mean temperature variability in Figure 4.20, but not in seasonal mean temperature itself, even though there is a section on seasonal warming patterns (4.5.1). I suggest adding maps of DJF and JJA temperature changes, or cite the relevant figures in the Atlas. [Nathan Gillett, Canada]	Taken into account. The SOD explains selection choices more explicitly.

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15258					The structure of the chapter seems to enhance the potential for repetition/redundancies. In many cases I asked myself "didn't I read this before?" I think in particular the section looking at global quantities across the 21st century (4.3) is going to see repetitions of its content in the following two...Arctic sea ice is one of the prime examples. Not sure if that section is needed at all, could it be subsumed in the following "short-term" and "mid to long-term" sections (4.4, 4.5)? Otherwise I found the chapter in good shape.Let's hope CMIP6 comes about! I do think in some parts the writing is uneven in terms of the attention given to "explaining" the changes or just "describing" them. I will point out a couple of specific examples. [Claudia Tebaldi, United States of America]	Taken into account. MoVs have been harmonised across WGI. The SOD avoids unnecessary overlaps across sections.
36032					Replace references to 'the AR5' with references to a specific chapter in the AR5. The AR5 consists of three WG reports over several thousand pages - readers may not be able to find the relevant part of the assessment without a specific chapter reference, moreover, references to individual chapters are the recommended way of citing the contents of IPCC reports as indicated in the reports themselves. [Nathan Gillett, Canada]	Accepted.