

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
25654	0	0	0	0	Throughout: avoid v in ppmv; the quantity is molar mixing ratio relative to dry air; the unit is ppm. It has nothing to do with volume. [Stephen E Schwartz, United States of America]	Accepted - text revised
25656	0	0	0	0	I am surprised not to see any impulse response function for CO2 emissions. This would seem essential given the finding (stated at page 6, line 48, regarding the rates of uptake of CO2 by the ocean and land. In principle one would like to see a set of IRFs as a function of time subsequent to emission indicating the fraction of a pulse emission that is present in the atmosphere, the ocean, the terrestrial biosphere, summing to unity. Because of the increases in oceanic and terrestrial sinks that have been evidenced over time, such IRFs would be very different from those presented by Joos et al., 2013. [Stephen E Schwartz, United States of America]	Noted. We are exploring such an addition
56124	0	0	0	0	The discussion of N2O and CH4 changes in this chapter need to be consistent with the N2O and CH4 discussion in Chapter 4. Are the trend estimates quantitatively the same? And are they based on the same assumptions/models? [Rolf Müller, Germany]	Accepted. Yes, quantities are consistent between chapters.
47960	0	0	0	0	How will the CMIP6 updated ECS values (Chapter 7 section 7.5) be incorporated into Ch5's assessment? E.g. the impact on potentially higher ECS on carbon budgets? [WGI TSU, France]	Accepted. Yes, there have taken up for the construction of the remaining carbon budget
47962	0	0	0	0	How will chapter 8's assessments on future water cycle changes, e.g. to precipitation relate to ch5's assessment on the climate response to CDR and SRM. [WGI TSU, France]	Noted. We are now coordinating the content.
27746	0	0	0	0	et al in italics, bibliographical citations in chronological order. [Poot Delgado Carlos Antonio, Mexico]	Taken into account (TSU is in charge of putting citations in chronological order)
27748	0	0	0	0	Unify the use of "and" in bibliographic citations [Poot Delgado Carlos Antonio, Mexico]	Rejected - this aspect is taken care by TSU when using their specific bibliography format.
27750	0	0	0	0	delete the two points and followed by the legends of figures (Figure 4.19:) [Poot Delgado Carlos Antonio, Mexico]	Rejected - this is the display provided by the AR6 word template.
22418	0	0	0	0	Many of the references in this Chapter have errors in formatting, spelling, incomplete info, etc. in the reference list. I did not correct them all per the instructions, but wanted to bring this to attention since there were so many. [Gwenaelle GREMION, Canada]	Noted. More attention was paid for the SOD, and attempted in correcting for spelling, incomplete, and duplicate citations.

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28822	0	0	0	0	Several feedbacks are not covered here. They are discussed in chapter 6 but should be cross referenced here. e.g. Fire effects on ozone and aerosol through diffuse radiation on vegetation NPP & the carbon cycle (Yue & Unger, Rap et al. Pacifo et al.etc. going back to Sitch et al. 2007). This chapter should also cover aerosol effects in general on diffuse radiation and the carbon cycle? Sitch, S., Cox, P. M., Collins, W. J. & Huntingford, C. Indirect radiative forcing of climate change through ozone effects on the land-carbon sink. Nature 448, 791–794 (2007) .Rap, A. et al. Fires increase Amazon forest productivity through increases in diffuse radiation. Geophys. Res. Lett. 42, 4654–4662 (2015). Pacifco, F. et al. Biomass burning related ozone damage on vegetation over the Amazon forest: a model sensitivity study. Atmos. Chem. Phys. 15, 2791–2804 (2015). Yue and Unger, 2019, Nature Communications volume 9, Article number: 5413 (2018) [Piers Piers Forster, United Kingdom (of Great Britain and Northern Ireland)]	Noted. We are covering the diffuse light component in the solar radiation management section. Accepted to note the issue of ozone.
48022	0	0	0	0	Scoping Outline Check: All bullets from approved outline are covered in the first order draft but please note there is less focus on the near-term (as stated in bullet 4 of the approved outline). [WGI TSU, France]	Accepted. We have now a new subsection on near term future projections.
28824	0	0	0	0	Chapter is nice and short [Piers Piers Forster, United Kingdom (of Great Britain and Northern Ireland)]	Thank you.
48048	0	0	0	0	Please check the correct use of IPCC Confidence/Uncertainty language. In some cases some assessment arguments are provided with uncertainty language without a discussion of the results of cited papers. Please refer to the IPCC guidance note on uncertainty: <a href="https://wg1.ipcc.ch/SR/documents/ar5_uncertainty-guidance-note.pdf">https://wg1.ipcc.ch/SR/documents/ar5_uncertainty-guidance-note.pdf</a> [WGI TSU, France]	Noted.
28868	0	0	0	0	FAQ 5.1 better framed as " how much is nature and plant growth compensating for our emissions" to deal with skeptical argument? FAQ 5.2 "Will" would be a better question than can - even if you can answer can but not will! FAQ 5.3 This is not a question asked. How much can negative emissions help and are the dangerous may be better? FAQ 5.4 : nice one - it should cover all uses though - i.e. the UK CCC have a carbon budget that is quite different [Piers Piers Forster, United Kingdom (of Great Britain and Northern Ireland)]	Noted.
47812	0	0	0	0	A call out to chapter 3 could be appropriate here. Please check the consistent use of uncertainty language. [WGI TSU, France]	Noted
47838	0	0	0	0	The AR5 WGI report briefly covered Phosphorous, as well as CO2, N2O and CH4. Should this also be covered here? Should any other sources of anthropogenic emissions be covered in this chapter? [WGI TSU, France]	Noted. There is mentioned in section 5.2.3

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43768	0	0	0	0	Different units are used interchangeably for carbon amount. For example, "PgC" is used for, among others, p.8, ll.34-39, p.62, l.33, and p.66, l.19 while "GtCO2" is used for p.67, l.45. Units should be unified throughout the chapter, or, if different units are used depending on the context, a certain standard should be established on the usage and clearly stated. [Michio Kawamiya, Japan]	Noted. We use PgC and PgC and GtCO2 for the remaining C budget.
16150	0				In each section, results of paleo-oceanography or -climatology come first. I agree to that reconstruction of past environment is important in knowing possible environment changes. But they include large uncertainty inherently. I do not know why it comes first. It is better to place them at the last parts. In addition, it is stated repeatedly that rates of changes in the modern time are faster than those ever experienced in the past. I think it is necessary to state distinctly the importance of reconstruction of past environment in spite of the different rates of changes. [AKIHIKO MURATA, Japan]	Rejected, the move of the section to last. Accepted to clarify the rates at different spatial scales.
53788	0				This chapter addresses scientific topics that are essential for Article 4.1 of the Paris Agreement. I think the authors could go a bit further on these topics and address more directly what 'net zero' and 'greenhouse gas balance' mean and how this can be understood from national to global scale. The chapter is in a very good position to do this and it would strengthen its policy relevance. [Jan Fuglestedt, Norway]	Noted. We further develop last subsection in 5.2 to address the issue.
50800	0				Please include relevant information about ongoing emissions of CFC-11 and carbon tetrachlorid (CTC) that could stem from illegal production. These substances is prohibited to use, except as feedstock, and regulated under the Montreal Protocol due to their Ozone depleting potential. But, since they also have very high GWP values it is also relevant when it comes to climate Change. For CFC-11 Montzka et al. (2018) and Bixby et al (2019) are relevant references, while for CTC Lunt et al. (2018) is relevant. [Ole-Kristian Kvissel, Norway]	Rejected. Outside of the mandate of the chapter.
44926	0				I very much enjoyed reading CH5 FOD! It's truly an excellent contribution to the WG1 report. My comments focus on my expertise in paleoclimatology. [Darrell Kaufman, United States of America]	Thank you.
44928	0				Generally, while I understand the need for each chapter to be relatively self-contained, in my opinion, there's currently too much direct overlap with CH2 and possibly CH9, specifically the sections on GHG concentrations, paleo temperatures, and ocean acidification and de-oxygenation. I suggest that the CLAs decide which chapter should serve as the primary account of these topics, or how to subdivide the topics so that the report makes overall better use of space. For example, maybe CH2 could take the lead on GHG concentrations through time (changing state of the climate system) and CH5 could take the lead on rates of GHG changes (key metric for carbon cycle dynamics). This will free up space in CH5 to significantly expand on its assessment of key information, including the natural causes of GHG fluctuations over multiple time scales, a topic that is not touched in CH2. [Darrell Kaufman, United States of America]	Noted. We increased the coordination among chapters to ensure consistency and minimum overlap.

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47318	0				authors are to be congratulated on a really nice draft -clearly much work and coordination since the initial draft [chris jones, United Kingdom (of Great Britain and Northern Ireland)]	Thank you.
51942	0				Another very good draft chapter. Its hard to ascertain a direct trace of the ES statements to the underlying assessment. Several other chapters are closing each section with their new assessment finding. This increases traceability of the assessment finding to underlying text. Could each sub-section end with a new assessment finding couched in confidence / likelihood language to increase traceability of teh assessment findings to the underlying text? In some sections the chapter opens with the substantive assessment but this is opposite to other chapters' approach. The chapter also only fitfully provides a synopsis of what the relevant findings were from either the AR5 or prior SRs in the present cycle. Doing this more consistently would help to highlight new knowledge. [Peter Thorne, Ireland]	Accepted. We have now implemented a structure with more clear conclusions and traceability of them.
47084	0				Short outlook/summary of each main sub-chapter that contains more sub-chapters (e.g. 5.3.1) would be helpful at the beginning of each of those sub-chapter in order to know what to expect in this sub-chapter. Similar as it has been done for the sub-chapter 5.1,5.2,... [Sophie von Fromm, Germany]	Noted, implemented.
47086	0				Always use the same color code for the three major gases (CO2, CH4, N2O) in the graphs, This makes it easier to recognize the individual gases in the different graphs. Same is true for differentiation between anthropogenic and natural fluxes/stocks in the graphs. [Sophie von Fromm, Germany]	Noted, implemented to the extent it was possible.
53486	0				The chapters has some nice overview figures for CO2, CH4 and N2O. [Jan Fuglestedt, Norway]	Thank you.
47090	0				Some of the sub-chapter/sections explain everthing very detailed and show and report a lot of data, others just have make a statement without much explanation - this should be more equally through the entire chapter. [Sophie von Fromm, Germany]	Noted. We have improved balance.
51956	0				There is a general paucity of cross-referencing to remaining chaptersin this FOD. This firstly leads to a degree of avoidable repetition, and secondly does not help a reader to refer elsewhere where a more substantive assessment is undertaken. Better cross-referencing in future drafts would, on balance, be helpful I think. [Peter Thorne, Ireland]	Noted. Added many new cross references.
25658	1	1	1	1	Better title: Global biogeochemical cycles of CO2 and other climate influencing substances. As presently phrased it is hard to recognize that "Carbon" is an adjective modifying cycles and feedbacks. [Stephen E Schwartz, United States of America]	Rejected. The authors don't think compounds is the best representation to represent what the chapter focuses, co2, ch4, n2o.
9286	1	1	1	1	Surely it was not easy to find a convenient title for this chapter, which deals with several rather different topics. I suggest however to replace "Global Carbon" by "Carbon budgets". "Carbon budgets" gives indeed better visibility to the topic adressed in section 5.5. Besides, as far as I can say, no ambiguity is introduced inasmuch as everytime "carbon budget" is mentioned in the FOD, it does refer to the global carbon budget as considered in section 5.5 [philippe waldteufel, France]	Noted.

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15366	1	1	176	1	Too many overlapping with chapter 2 [Oksana Lipka, Russian Federation]	Noted. Reduced overlap.
31760	1	1	176	29	Clarify the relationship between “thresholds” and “tipping points”. If they mean the same thing, which, a priori, looks likely, use a one of them. If the intention is that “tipping point” should refer to a specific kind of “threshold” with large and dramatic consequences, then the nature of the distinction needs to be explained. [Martin Jukes, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Well defined now.
57236	1	1	176	29	Chapter 5 seems unbalanced towards a thorough and detailed examination of the carbon cycle, but much less so for other biogeochemical changes, e.g. changes in nutrient cycling, oxygen, and oceanic N2O changes. Ocean deoxygenation especially could benefit from more discussion and description of drivers, including on topics of attribution, role of internal variability, observations, and projections, as outlined above. The review papers of Levin et al 2018, and Oschlies et al 2019 provide excellent references for recent observations, gaps in understanding, and advances in projections and attribution. A figure or two on Oxygen changes can help balance out the discussion of biogeochemical cycles in this chapter. [Yassir Edebbbar, United States of America]	Noted.
13486	1	17	1	17	"Bala Govindasamy (India)" should be changed to "Govindasamy Bala (India/USA)" [Govindasamy Bala, India]	Accepted - text revised
17306	1		90		I am concerned with some of the editorial variation between sections (and also between the various chapters I have looked at) especially where this variation makes the science unclear. I have flagged some of these issues for clarity. One thing I am unsure about is the refence citation style in the text. In some places references are given chronologically, in others alphabetically, and in others it appears to be random. I would normally expect the first form to be used but have not formally reported such instances unless there is an issue of clarity involved. I flagged what I view are the significant inconsistencies and errors, partly to speed up the editorial process overall but also to clarify/improve confidence in the science communication. [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Noted and applied uniform criteria for citations.
33280	1				Suggest Global Carbon Compounds [Michael Schwabe, Uruguay]	Rejected. The authors don't think compounds is the best representation to represent what the chapter focuses, co2, ch4, n2o.
49026	2	1	2	1	I am a bit upset that FOD does not assess fire-related processes/emission fully enough to summarize latest advance of fire research, which to my knowledge has developed quite a lot since AR5. The role of fire in the Earth system is continuously updated from new observational records and modelling researches. Such as peat fires emission, and the interactions of fire and permafrost. The recent fire modelling intercomparison project FireMIP also presents some advances of fire modelling development for Earth system model. The question is will a more detailed assessment be provided in the second draft of the report? [Minchao Wu, Sweden]	Accepted. We have added text in 5.2.3 on the role of fire.

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45638	3	12			Should this be taken to end 2018? The data are accessible in the NOAA database. [Euan Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Updated to 2018
47034	4	9	4	9	Would be helpful to name the acronym ESM at this point already. [Sophie von Fromm, Germany]	Accepted - text revised
9284	5	2	5	23	I suggest to remove from this ToC the headlines of the summary's paragraph. This is overloading the content table unnecessarily. Other chapters escape this feature. Anyway, people interested in the summary will rather read the summary than the ToC! [philippe waldteufel, France]	Noted - This was an automated mistake generated by TSU when processing our chapter. It has further been removed.
40338	5	3	5	5	The main driver of changes in atmospheric GHGs over the past 200 years is the direct emissions from human activities, which have dramatically increased and broken records year on year in the past 30 years since IPCC and UNFCCC began. The actual ... [Michael Wadleigh, United States of America]	Noted
29434	5	12	5	12	in the sentence "The chapter also assesses the remaining carbon budget for halting global warming" the word "halting" (meaning stopping) is not appropriate because the already emitted GHGs have committed us to a certain degree of warming. Suggest changing "halting" to "limiting" or "mitigating" [Rona Thompson, Norway]	Accepted - text revised
37764	5	19			The juxtaposition of the word "appear to" and "high confidence" is a little unsettling. It suggests that confidence may not be quite so high that Chinese emissions have actually declined strongly. A tighter wording would be welcome. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Comment with wrong page/lines.
9374	5	26	5	32	It seems important to include in the executive summary also the following finding: However, the synthesis also reveals substantial regional impacts, in particular in high latitude environments, which were more affected by warming owing to polar amplification. Adding this finding would also be more coherent with the findings described on page 8, lines 13 to 20. [Klaus Radunsky Radunsky, Austria]	No longer applies as paragraph has been rewritten. But note add on regional impacts.
32700	6	1	6	1	How is it that the sections on what has happened to present do not seem to include mention of aerosols (black carbon, sulfate, etc.) or of tropospheric ozone, all of which have significant influences on radiative forcing? Indeed, tropospheric ozone forcing from 21st century emissions are about equal to 21st century forcing from 21st century methane emissions--and together these are about equal to the 21st century forcing from 21st century emissions of CO2 (study I did using MAGICC, wanting to understand what forcing changes were due to 21st century emissions (so those we can potentially control). [Michael MacCracken, United States of America]	Reject: Policy prescriptive - but can explain that such a feedback may increase the airborne fraction and resulting radiative forcing
51630	6	1	6	32	This is beautifully, clearly, written. [Lindsey Cook, Germany]	Thank you
13492	6	1	7	12	Are there no key messages on the global greening that is noted in satellite-derived NPP and LAI products in this report? There are a lot of published papers on this recently. [Govindasamy Bala, India]	Accepted. Included now.

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22172	6	1	9	1	Overall the language in the executive summary needs to be simplified and streamlined. There are inconsistencies in the level of detail and scientific jargon included in the subsections. Even the introductory paragraph assumes the reader has a firm understanding of source-sink dynamics, feedbacks, and the remaining carbon budget. [Gwenaëlle GREMION, Canada]	Accepted. Text modified.
56268	6	1	9	33	The executive summary does not mention uncertainties related to water-Co2 feedbacks, e.g. possible effects of droughts on the land carbon sink. A recent study, which is cited in the chapter (Humphrey et al. 2018, Nature), has shown evidence that these feedbacks are substantial on interannual time scale and that current ESMs appear to underestimate them. This may lead to an underestimated positive feedback between increasing drought in some regions and decreasing land sink. It would be useful to include more material on this in the executive summary. Exchange with Chapters 8 and 11 on this topic would also be useful. [Sonia Seneviratne, Switzerland]	Rejected. We don't have a solid knowledge of the drought effects in the global carbon cycle, very important though at the regional level.
53464	6	1	9	33	Well written and structured ES. The split into sections is useful. [Jan Fuglestedt, Norway]	Thank you
24578	6	1			This ES is very clear and easy to read. [William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Thank you.
28818	6	3	9	4	ES is pretty good. Relatively concise with punchy paragraphs and good level of quantification, covering all areas [Piers Piers Forster, United Kingdom (of Great Britain and Northern Ireland)]	Thank you
13350	6	4	6	8	The sentence is about GHG in general. Hence "carbon" in L. 7 should be replaced with "GHG". [Lydia Keppler, Germany]	Accepted - text revised
45338	6	6	6	6	"carbon reservoirs" ignores other GHGs like N2O [Peter Rayner, Australia]	Accepted - text revised
22174	6	6	6	6	The word "actual" in this line will lead to confusion: a lay person is likely to interpret "actual" as the opposite as false or fictional. The message would be better captured by "net", "exact", or "precise" [Gwenaëlle GREMION, Canada]	Accepted - text revised
55782	6	7	6	7	Multiple carbon reservoirs. The context of this sentence is about all GHGs, so it should technically extend to at least nitrogen. [Christopher Smith, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
28140	6	7	6	8	Not all GHGs are influenced by source-sink dynamics of the carbon cycle. Please rephrase and be more specific. [Alexander Winkler, Germany]	Accepted. Text revised accordingly.

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22176	6	8	6	11	This paragraph states that this chapter will address how biogeochemical processes affect atmospheric GHG amounts and also feedbacks that have led or could lead to a change in GHG accumulation in the atmosphere. The paragraph should also state that changes in the biogeochemical process themselves will be examined for their influence on atmospheric GHG abundance. [Gwenaelle GREMION, Canada]	Accept: edit to be implemented and statement was re-written - The ocean and land CO2 sinks have and are expected to continue growing in response to rising emissions, albeit with strong interannual and decadal variability. Reducing carbonate buffering and ocean arming are expected to emerge as major feedbacks that will slow the ocean uptake in the second half of the century under high emission scenarios.
22178	6	11	6	11	Remaining carbon budget must be defined here, potentially using text from FAQ 5.4 [Gwenaelle GREMION, Canada]	Reject: the statements are grouped according to the type of GHG: CO2, CH4, N2O
22180	6	11	6	12	This sentence is confusing. Is the first part trying to say that the chapter assesses the remaining carbon budget left to use in order to still halt global warming? Because this is how it reads. Consequences of carbon dioxide removal from where/how? Atmosphere? [Gwenaelle GREMION, Canada]	Notice. Yes, that was the intend of the meaning of the sentence.
22182	6	11	6	12	It would be helpful to provide additional context or a definition of the 'remaining carbon budget,' including a reference to its description in earlier chapters. The use of the term 'carbon budget' here is particularly ambiguous because in FAQ 5.4, there are TWO definitions given for the term. [Gwenaelle GREMION, Canada]	Accepted: we have better defined the various types of budgets including the remaining carbon budget.
19200	6	16	6	17	why use data from 2017 and not from 2019? [Baerbel Hoenisch, United States of America]	Accepted: have removed the word onset to clarify the meaning
16028	6	16	6	18	The 2017 concentraion and percentage increase above pre-industrial levels of CH4 are slightly different from the values reported by the WMO Greenhouse Gas Bulletin ( <a href="https://library.wmo.int/doc_num.php?explnum_id=5455">https://library.wmo.int/doc_num.php?explnum_id=5455</a> ). Please check. [SAI MING LEE, China]	Accept: It is referring to sub-section 5.3.4
24580	6	16	6	24	This ES point on the GHGs is very similar to the first couple of ES points in Chapter 2. I suggest conferreing with Ch 2 to decide what goes where. [William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Accept: It is referring to sub-section 5.3.4
13488	6	16	6	24	Excellent message. I think this message relating to the "speed of change" has not been emphasized sufficiently in the past. [Govindasamy Bala, India]	Thanks.
41768	6	17	6	17	very must be in italic [Marc Aubinet, Belgium]	Accepted - text revised
7482	6	17	6	17	Very in "very likely" needs to be italicized. [Rose Abramoff, France]	Accepted - text revised
44100	6	17	6	17	italicize "very" in the confidence statement "very likely" [Sara Kahanamoku, United States of America]	Accepted - text revised



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32666	6	17	6	17	In saying 800,000 years, that is presumably the length of the Vostok record, and not really the time period for which the statement is the case. Because of this, I would suggest making it clear that the statement likely applies over a much longer period--so, if keeping "very likely" (and both words should be italicized), I would suggest saying "in at least the last 800,000 years, which is the length of the existing ice-core record." I'd also comment I don't understand why the ice core record is only "very likely" when the third sentence is also indicated as "very likely" even though evidence on this is weaker--I think ice core record should be "virtually certain". In revising the sentence, another way would be to have this first sentence indicate that the ice core record provides high quality data out to 800,000 years, and then the third sentence could be revised to indicate that geological evidence (or whatever type is appropriate to say), indicates that the present CO2 is high in 2M years. [Michael MacCracken, United States of America]	Accepted: clarified to mean human populations
38840	6	17	6	17	very in 'very likely' should be in italics [Emilie Breviere, Sweden]	Accepted - text revised
13640	6	17			very likely: "very" should also be italic?! [Lena Boysen, Germany]	Accepted - text revised
47320	6	17			is there any reason this might not be true? Have you considered "virtually certain"? [chris jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Changed.
17308	6	18	6	18	Change to Pre-Industrial for consistency elsewhere in the text and other Chapters [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - The same spelling as in the IPCC SR1.5 / SROCC has been applied (pre-industrial has been adopted throughout the chapter for consistency)
22184	6	18	6	18	Including the exact ppm of these increases since the industrial revolution would aid in comparing them to the paleoclimate pulses (lines 26-32). [Gwenaelle GREMION, Canada]	Reject: the sink (magnitude of the flux) increases but the fraction may not
32668	6	19	6	19	With respect to the global average concentration, which is what is presumably being referred to here, there is only one CO2 concentration--it should be singular. [Michael MacCracken, United States of America]	Accepted - text revised
56318	6	19			The text says "Current CO2 concentrations are also very likely to be unprecedented in more than 2 million years" but figure 5.2 shows that the last time CO2 was above 400 ppm was over 20M years before present. How is the reader to reconcile this discrepancy? I assume the answer must lie in the uncertainty in the record shown in figure 5.2 or in the shorter-term variability of atmospheric CO2 concentrations, which "are estimated to have ranged between 350-450 ppm (Martínez-Botí et al., 2015b; Seki et al., 2010)." according to the text on page 15, lines 5-6. But no error bars are shown in figure 5.2 that would clarify this. [Steven Neshyba, United States of America]	Noted. Fixed in text and figure.

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31758	6	26	5	32	This appears to be a technical point about the role of "tipping points" in climate change, and specifically about the likelihood of a single tipping point leading to dramatic changes. We are crossing minor tipping points (changes in ice albedo, glaciers floating clear of obstacles) which have unpredictable consequences. The current phrasing of the paragraph is misleading: there is an implication that some general conclusion about "large, unpredictable" changes has been reached, which would be a highly important and reassuring conclusion. Closer reading suggest that there is perhaps a much more modest result being described here, in the sense that a specific methodology has not produced any positive results. [Martin Jukes, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Text modified.
7484	6	26	6	28	"Abrupt" in this sentence seems misleading because paleoclimatic changes are slow (not abrupt) with respect to contemporary climate change, which is emphasized in the last sentence. [Rose Abramoff, France]	Accept: revised to clarify
56604	6	26	6	30	Not sure whether "pulses" is the better word here compared to "variations" or simply "temporary increases" as many readers might associate pulses with emissions, but the >100ppm indicates the effect on those emission pulses on concentrations. Two sentences below, it says "emission rates FROM THOSE pulses"... Maybe better say, "emission rates implied by those temporary atmospheric increases"... although I agree that this is not necessarily more elegant, but tries to avoid the impression of a concentration -> emissions cause effect chain... [Malte Meinshausen, Australia]	Accept: re-drafted the entire statement
22186	6	26	6	32	This paragraph relies on the reader knowing what paleoclimate is already, this needs to be defined clearly so that the reader can understand the timeframe over which these non-anthropogenic pulses have been observed. A clear distinction between the naturally occurring variation in the paleoclimate record and the recent anthropogenically driving change needs to be made here. Most lay-people are unlikely to know what climate-forcing refers to and would not associate it with "naturally" occurring change. The size of paleoclimate pulses is describe in ppm here which is useful but requires some mental math to understand that CO2 is now ~ 200ppm over pre-industrial revolution levels. Most lay people will not do this math to facilitate to comparing paleoclimate pulses to post-IR change. [Gwenaelle GREMION, Canada]	Accept: centennial time scales
22188	6	26	6	32	This paragraph is unclear. Does the phrase "for future warming of no more than 2 deg C, paleorecords suggest, with medium, confidence, that it is unlikely that a tipping point will be crossed..." mean that exceeding 2 deg C is unlikely to occur? This would seem to contradict previous IPCC findings, and therefore is unlikely to be the correct interpretation of what the author intended. Clarification would be helpful. [Gwenaelle GREMION, Canada]	Noted. Yes we mean below 2 C unlikely tipping point. Therefore, above 2C might or might not be more likely; not assessed here.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
51632	6	26	6	32	Just checking, as the SR1.5C notes above 1.5C as tipping to the irreversible melting of the Greenland Ice Cap, and this might be mentioned again as an example where current 2C warming would have large, unpredictable changes in the state of the climate system - which I sense this melting would have, but I may be mistaken. [Lindsey Cook, Germany]	Noted. We only focus on biogeochemical driven tipping points.
47794	6	26	6	32	Assesmet must be traceable to underlying chapter. I cannot find the uncertainty language terms used in this paragraph in Section 5.1.3.4 [WGI TSU, France]	Accepted. Wording modified and traceability made clear.
46838	6	26	6	32	As your chapter considers CDR approach, this statement needs to be completed with other paleoclimatic evidence, especially those related to declining CO2 such as the Eocene-Oligocene transition. [Roland Séférian, France]	Accepted. New paragraph added.
36208	6	27			The intended meaning of 'climate forcing' is not clear here. I suggest either replacing 'climate forcing' with 'changes in climate', or 'naturally-driven changes in climate'. As written 'climate forcing' could be interpreted as 'radiative forcing', but I don't think that's the intended meaning. [Nathan Gillett, Canada]	Accepted. Changed.
26482	6	28	5	30	too much information in one sentence. Consider using "For future warming of no more than 2°C, paleorecords suggest, with medium confidence, that large, unpredictable changes in the state of the climate system are unlikely." [Nadine Goris, Norway]	Accepted - text revised
17310	6	28	6	28	I suggest changing paleo to palaeo: the latter form is also used in the Chapter (and elsewhere in the Report) and paleo is the American spelling (I have the sense that the style for the document is British English as this is used more commonly) [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
32670	6	28	6	30	This chapter is about biogeochemical cycles--it should not be drawing conclusions about the state of the "climate system"--fine to have conclusion about atmospheric composition changes, but not changes in the climate system (which would include amount of polar ice and changes in sea level--paleoclimatic data suggest an equilibrium sea level sensitivity of perhaps 15-20 meters per degree C change in global average temperature--and that is huge). In addition, I don't see how the statement even makes sense with respect to the biogeochemistry given how rapidly permafrost is thawing--much faster than expected--and what is happening to tropical forests. And I don't understand including the word "unpredictable"--does this mean that if I predict (and really should say project), that this would not be counted--very strange word choice (especially given that cryospheric changes are occurring faster than have been projected). [Michael MacCracken, United States of America]	Accepted. Text modified.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
44102	6	28	6	32	Framing of this paragraph--beginning with a statement indicating that paleo records suggest that warming of 2 degrees is unlikely to lead to tipping points--may cause confusion among those unused to working within a paleo or long-term framework. The paragraph has the potential to be interpreted as making a statement about the low probability of reaching a tipping point if warming is limited to 2 degrees C. Yet, given the uncertainty surrounding the rates and magnitudes of previous emissions events, we have a limited ability to surmise whether we will reach a tipping point if warming is kept to 2 degrees C. Perhaps flip the order of the statements--lead by tempering expectations about the applicability of paleo records for future predictions--in order to limit confusion. [Sara Kahanamoku, United States of America]	Accepted. Text modified.
31756	6	29	6	29	"large, unpredictable" -- please disambiguate "either large or unpredictable .." vs. "large and unpredictable". [Martin Juckes, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Text modified.
26480	6	29	6	29	"were" likely, as these have been in the past [Nadine Goris, Norway]	Accepted - text revised
32672	6	30	6	30	Should not "are" be "were" if talking about drawing information from past changes. [Michael MacCracken, United States of America]	Accepted.
22190	6	30	6	30	Unclear which pulses are being referred to here [Gwenaelle GREMION, Canada]	Accepted. Made it clearer.
36210	6	30	6	32	If a climate variable is uncertain, then a probability can be associated with a range in that variable, not one particular value, for which the probability will generally be zero. So for the ratio between emission rates in paleo climate and current emission rates, the authors should give a range rather than one value, if they want to associate a quantified probability (likely, P>66%). I suggest 'emissions rates from those pulses are likely at least ten times slower than the current anthropogenic emissions', if that is what the evidence supports. Also 'order of magnitude' is somewhat vague, and less easily understandable by the intended audience than 'ten times'. [Nathan Gillett, Canada]	Noted
36238	6	36	6	37	Note that Chapter 7 assesses that 'It is unequivocal that human activity has had a warming affect on the planet since 1750'. Are these consistent? [Nathan Gillett, Canada]	Accepted: re-edited to assess confidence/likelihood levels to virtually certain
37766	6	36	6	39	Please see comment 2 on the entire document. It points out that this statement of low confidence is inconsistent with a statement of likelihood made in Chapter 5 (page 5-7, lines 4 and 5). [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Text modified.
32674	6	36	6	39	Oh come now, how can the first point be only "virtually certain"? There is no alternative explanation that even comes close to being plausible. Why allow for any doubt in this point. And same goes with the second point--we have very good records. This may change as we move toward cutting emissions, but, while I do favor using the IPCC lexicon, these statements are not at all in doubt. If there is a need to indicate uncertainty in the numbers, then do that, but there is no question the levels are the highest in history. [Michael MacCracken, United States of America]	Accepted. Changed.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
47796	6	36	6	39	Are you 'virtually certain' of these values to 1 decimal place? [WGI TSU, France]	Accepted. Changed.
51932	6	36	6	39	It is unclear why the use of a likelihood qualifier is associated with this statement. It is surely unambiguous that the increases are down to human activities based upon multiple robust lines of evidence? The use of virtually certain opens the possibility that it is not. I would urge this being recast as a fact based statement given the weight of evidence. [Peter Thorne, Ireland]	Accepted. Changed.
46840	6	36	6	39	as written it suggests that there are no [detectable] evidence of climate impacts on the carbon sinks over this period. [Roland Séférian, France]	Noted. Correct, no detectable in the trends, adjusted wording
56606	6	37	6	37	Insert in brackets the years after "During the last decade" [Malte Meinshausen, Australia]	Noted
29380	6	41	6	41	"annual average" should be "decadal average", given that it is the mean 2008-2017 [Judith Hauck, Germany]	Accepted - text revised
37720	6	41	6	42	Is this sentence correctly worded? I would think the carbon atoms that are emitted into the atmosphere in molecular form due to human activities in a given 10-year period are not necessarily the same carbon atoms that are taken up by the ocean and terrestrial ecosystems during that period. Rather, doesn't the emission of some carbon atoms into the atmosphere by humans condemn some of the other carbon atoms that would otherwise have stayed longer in the atmosphere to the fate of being taken up by the ocean and terrestrial ecosystems? [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Text modified.
47496	6	41	6	43	Inconsistent with chapter 5 - page 71, line 11-12 box 5.1: Over the past decade (2007–2016) 47% of the emitted CO2 remained in the atmosphere, 23% was stored in the ocean and 30% in the terrestrial biosphere (Le Quéré et al., 2018b) [Birgit van Munster, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Now consistent.
13490	6	41	6	43	In past reports, I recall land uptake was estimated as the residual. It appears that such an approach is not used this time. This may be stated here. [Govindasamy Bala, India]	Accept- updated in the SOD
13352	6	41	6	46	Adding the individual components, I get an imbalance of 0.6PgC (not 0.5 PgC). There seems to be a rounding error. [Lydia Keppler, Germany]	Noted. Due to rounding issues.
36214	6	41	6	46	Because no uncertainties are given on the carbon fluxes quoted, it is impossible to properly interpret the meaning of this imbalance of 0.5 PgC/yr. Do the estimated uncertainties in each of the terms in the budget add (in quadrature, assuming uncertainties are independent) to less than 0.5 PgC/yr, 5-95% range? If so, then the apparent imbalance is within the expected range based on individual uncertainties, and lines 43-45 could be replaced with a statement that this budget is closed to within observational uncertainty. Or does the 0.5 PgC/yr exceed the sum of the uncertainties? If so, then keep the text on lines 43-45 as it is, and add that an alternative explanation is that the uncertainties in one or more terms are underestimated. There is currently no confidence assessment for the budget on lines 41-43, but as well as uncertainties, one should be added. Note that if the budget is closed to within obs uncertainty this will increase confidence, compared if the budget is not closed to within obs uncertainty. [Nathan Gillett, Canada]	Accepted. Explained better in main text, referred here.

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32676	6	41	6	46	Given there is an imbalance in the sum of the central estimates of the various terms, why are there not uncertainties indicated for each of the terms--two and three figure precision on the numbers needs to be tempered with estimates of uncertainty. [Michael MacCracken, United States of America]	Accepted. Added.
26864	6	41	6	46	It should be added in this paragraph that the main reason for the global net land CO2 sink is enhanced vegetation photosynthesis, and that it is likely that its increasing strength is mainly caused by the fertilisation effect from rising atmospheric CO2 concentrations, with a reference to section 5.2.1.4.1. [Jonas Nycander, Sweden]	Accepted. Text modified.
36212	6	41		43	Give the uncertainties in these values. Specifying percentages and absolute fluxes and their uncertainties may be too much, so it might be better to give either absolute carbon fluxes or percentages. But including the uncertainties is important. [Nathan Gillett, Canada]	Accepted. Values added.
45340	6	42	6	43	I'm surprised not to see error bars on uptakes [Peter Rayner, Australia]	Accepted. Added.
27216	6	42	6	43	This expert reviewer recommends to check the numbers with respect to the findings of <a href="https://doi.org/10.1038/s41467-019-08633-z">https://doi.org/10.1038/s41467-019-08633-z</a> [François GERVAIS, France]	Rejected. We are not using ESMs.
22192	6	42	6	43	These sum to 10.3 PgC, which is 0.6 rather than 0.5 PgC (value cited on line 44). Though this is understandable due to rounding, it is irksome and invites criticism. Potentially include another sig-fig so that the math is more transparent? Or add "approximately" to the 0.5 PgC listed in line 44. [Gwenaëlle GREMION, Canada]	Accepted. Numbers changed.
41770	6	44	6	44	computed difference is 0.6 PgC [Marc Aubinet, Belgium]	Accepted - text revised
22194	6	45	6	45	"or both" language here implies that both are equally plausible sources of error. Confidence intervals in Table 5.1 and LeQuere 2018a suggest there is much higher confidence in anthropogenic emissions and that this error is likely attributed to the land-to-atmosphere fluxes. [Gwenaëlle GREMION, Canada]	Accepted. Text added in the corresponding section.
37722	6	45	6	46	The carbon emissions from cement production and other industrial processes seem to be missing here. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Noted. They are not, are part of fossil fuel. Added.
16030	6	45	6	46	The figure "87%" is inconsistent with the figure "86%" in the main text under Section 5.2.1.2. Please check. [SAI MING LEE, China]	Accepted. Fixed.
17312	6	46	6	46	Delete , after 'change' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
22196	6	48	6	48	"The CO2 ocean and land sinks have continued..." should be written "The strength/capacity/uptake of CO2 ocean and land sinks..." [Gwenaëlle GREMION, Canada]	Accepted - text revised

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
25660	6	48	6	49	the following language is difficult to parse: "The CO2 ocean and land sinks have continued to increase at rates that remain close to the rate of increase in atmospheric CO2 (high confidence), albeit with large decadal and interannual variability." It would be much more explicit if it read "The rates of uptake of CO2 by the world ocean and the terrestrial biosphere have continued to increase in proportion to the stock of excess atmospheric CO2 (above preindustrial) (high confidence), albeit with large decadal and interannual variability," if that is in fact what is meant. However I am not convinced that that is what is meant. Key issues with the original sentence. Is the proportionality to the anthro stock in the atmosphere and not the rate of increase of atmospheric CO2? And what is meant by "rates that remain close"? So the sentence needs considerable clarification. This conclusion is enormously important. The language in the FAQ page 5-83, lines 42-47 is much clearer. [Stephen E Schwartz, United States of America]	Accepted. Text modified.
25662	6	48	6	49	Figure 5.10 hardly makes the point. Suggest plot sinks (inferred from difference in emissions and atmos growth) versus atmospheric CO2 to make the point. [Stephen E Schwartz, United States of America]	Noted, and a similar figure now in section 5.2. CO2 Atmosphere.
32678	6	48	6	53	First, it seems to me that for both the land and ocean terms, this needs to say "net sinks". Second, it seems to me that the net land sink is really becoming suspect as the permafrost starts to thaw and as deforestation and the general response to climate change alter low-latitude carbon cycling in tropical forests, etc. In terms of uncertainties, it seems to me that they are both on the side of the sinks tending to shrink rather than continue as strong as they are, given plausible changes in ocean overturning and land-carbon exchanges. It seems to me that there is a serious risk that the land and ocean sinks will not be able to stay at the same percentages of emissions if emissions continue to rise (so permafrost thaw and ocean overturning slow), and that the absolute numbers will drop significantly if the rate of emissions drops. There actually seems to be some disagreement on this in the community and it would be very useful to make clear statements on what is projected to happen to the sink amounts as emissions drop to zero. [Michael MacCracken, United States of America]	Rejected. Although we agree on future changes, so far we haven't been able to observe any changes as yet.
46842	6	48	6	53	Interannual and decadal instead of decadal and interannual [Roland Séférian, France]	Accepted - text revised
46844	6	48	6	53	It should be state somewhere that these variations in carbon sinks drive variations in the accumulation of atm CO2 (See for example Devries et al., 2019, PNAS) [Roland Séférian, France]	Accepted. We agree.
46846	6	48	6	53	as statement on the coastal carbon storage might be relevant here given its relevance for national determined contributions [Roland Séférian, France]	Rejected. Don't have very clear data on the C sequestration in coastal zones to highlight in the ES.
51634	6	50	6	51	Can you stress/highlight that as a consequence, mitigation of anthropogenic GHG is of greater urgency (a policy maker could easily miss this critical point). [Lindsey Cook, Germany]	Reject: Policy prescriptive - but can explain that such a feedback may increase the airborne fraction and resulting radiative forcing

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
26484	6	52	6	52	"these sinks", otherwise one has to guess which fluxes are meant [Nadine Goris, Norway]	Accepted - text revised
22198	6	52	6	52	"Climate forcing" here to refer to changes in modern climate, which differs somewhat from the paleoclimate/"natural" connotation implied on line 27. [Gwenaelle GREMION, Canada]	Accepted. Changed.
45640	7	1	7	2	Should this be taken to end 2018? The data are accessible in the NOAA database. Could mention the synopsis in Nisbet et al. 2019. [Euan Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Accept: all data updated to 2019
22200	7	1	7	2	These lines need to explicitly state that there was short period where CH4 emissions slowed and then seemed to pause from 5.2.2.1 from 1985-1990 (as in 5.2.2.1). Right now the break is implied but not stated. This unnecessarily adds to the perception of uncertainty which casts doubt on the causes mentioned in lines 3 and 4. [Gwenaelle GREMION, Canada]	Accepted. Text modified
32680	7	1	7	6	Should there not be mention of permafrost thawing as a potential emerging source? [Michael MacCracken, United States of America]	Rejected. We don't observed a change in the trends due to permafrost thawing but the issue is brought up later in the ES in relation to future emissions.
22202	7	2	7	6	This paragraph presents the multi-decadal growth trend of atmospheric CH4 as a well-understood phenomenon. But as far as I know, it is still not well understood why atmospheric CH4 stopped rising in the 2000's, and started rising again in the last decade. This paragraph only mentions variability in different sources, but possible variations of the tropospheric and land-surface sinks are not even mentioned. [Gwenaelle GREMION, Canada]	Accepted, and we refer to a cross chapter box addressing this very issue in great detail.
36216	7	2			I suggest not using the term 'accelerated', because the meaning is ambiguous - it suggests a progressive increase in the trend over the 2014-2017 period, but really the authors have just compared the trends over the 2008-2017 period and the 2014-2017 periods. I suggest 'with a higher average growth rate of xx ppb/yr over the period 2014-2017'. [Nathan Gillett, Canada]	Accepted. Changed.
36220	7	3		4	Are the authors saying here that the resumption since 2007 is not primarily driven by natural emissions (i.e. methane feedbacks on climate change)? This is probably of interest to policymakers, since I have heard people question whether the enhanced rate of growth is mainly due to permafrost and other feedbacks. I suggest being clearer on the relative roles of anthropogenic sources versus natural sources e.g. 'is likely to be driven mainly by emissions from fossil fuels and agriculture, rather than by increased emissions from natural sources', if the evidence supports this. [Nathan Gillett, Canada]	Accepted. Text modified.
37724	7	4	7	5	Please see comment 2 on the entire document. It points out that this statement of likelihood is inconsistent with a statement of low confidence made in Chapter 6 (page 6-6, lines 36 to 39). [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Now consistent.
38842	7	10	7	10	in Climate-N2O, the '-' is too long, it should be different hyphen used [Emilie Breviere, Sweden]	Accepted - text revised



Comment ID	From Page	From Line	To Page	To Line	Comment	Response
22204	7	10	7	10	Climate-N2O feedback needs to be defined. Warming induced increases in N2O emissions that will exacerbate further warming. Unclear if this includes anthropogenic emissions. [Gwenaelle GREMION, Canada]	Accepted. Clarified.
17314	7	10	7	12	This is confusing. Is the 30% increase embedded in the 80%, i.e. it was 50% up to the 1980s, or is it 80% to the present day (and if so, where does the 70% fit in: 70% of 30% or 70% of 80%). Please review the narrative for clarity. [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Clarified in text.
39762	7	12			you stated that 70% comes from nitrogen fertiliser and manure, can you be more concrete about the sources and also the remaining 30% [Dagmar Henner, Austria]	Accept: changed the chapter text
27218	7	14	7	14	Although widely used, the wording "acidification" is misleading since oceans are alkaline. "Dealkalinisation" would be more consistent with the ocean pH and should be preferred [François GERVAIS, France]	Rejected. It is now IPCC wording but we explain in relevant section.
56438	7	14	7	32	Ocean Acidification and Ocean de-Oxygenation: No data or trend for the dissolved oxygen levels is given - this is misleading under this header [Daniel Häussinger, Switzerland]	Accept: The rates of uptake of CO2 by the world ocean and the terrestrial biosphere have continued to increase in proportion to the stock of anthropogenic atmospheric CO2 (above preindustrial) (high confidence), albeit with large decadal and interannual variability,....
26488	7	14	7	32	decide on either "de-oxygenation" or "deoxygenation" [Nadine Goris, Norway]	Accepted - text revised (de-oxygenation is agreed and used throughout the chapter)
45342	7	15	7	15	"clear with virtual certainty" perhaps "virtually certain" [Peter Rayner, Australia]	Accepted - text revised
9158	7	16	7	16	It is somewhat inconsistent to use the term "acidification" when seawater has a pH above 8 and is therefore still alkaline. [Jim O'Brien, Ireland]	Incorrect location
51934	7	16	7	17	Again, it is unclear to me where the potential uncertainty arises that leads to the necessity to refer to this using a likelihood qualifier instead of as a fact based statement. What is the source of ambiguity that may lead to this not being anthropogenic in origin? [Peter Thorne, Ireland]	Reject: projection is dealt with in 7-36
32682	7	16	7	18	It would be clearer to say "It is virtually certain that ..."--but once that grammatical change is made, again, why is this only "virtually certain"--this has to be happening and is a fact. On line 18, change "chemistry, which" to "chemistry that"--this is an important phrase that cannot be dropped. [Michael MacCracken, United States of America]	Accepted - text revised

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
22206	7	16	7	32	This section needs to include basic explanations of ocean acidification and de-oxygenation. Suggestion: "Ocean acidification is a change in the water chemistry associated with the amount of carbon dioxide dissolved in the ocean, which is the main mechanism behind the ocean sink of anthropogenic carbon emissions. Ocean de-oxygenation is the result of overloading ocean waters with nutrients that encourage overgrowth of algal that deplete oxygen in the water and produce carbon dioxide that exacerbated ocean acidification." The latter is somewhat defined at line 30-32 but this is a little late for the average reader. [Gwenaëlle GREMION, Canada]	Accepted: include it in the final SOD
36218	7	16			Replace 'clear with virtual certainty' with 'virtually certain' - the latter is correct calibrated likelihood language. [Nathan Gillett, Canada]	Accept: clarification made
17316	7	17	7	17	Subscript '2' in CO2 [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
38844	7	17	7	17	The 2 in CO2 should be superscript [Emilie Breviere, Sweden]	Accepted - text revised
33482	7	17			Ocean CO2 uptake shows significant decadal variation, so when an estimate is given, the years that estimate represents must also be reported. In this case it looks like the Le Quere et al. 2018 estimate for 2007-2017. Those years should be added here. [Adrienne Sutton, United States of America]	Accept: Table 5.1
7486	7	19	7	19	Define carbonate undersaturation [Rose Abramoff, France]	Noted. Text modified.
8316	7	19	7	22	This phrasing isn't very clear. Rephrase to make clearer what threshold is changing depending on emissions scenarios? Month-long undersaturation? What does that really mean? [Sarah Cooley, United States of America]	Accepted: have removed the word onset to clarify the meaning
32684	7	19	7	22	It needs to be stated why this undersaturation is important to ecosystems, society, whatever--otherwise is just jargon. [Michael MacCracken, United States of America]	Accept: it refers to contemporary period. This is clarified in the SOD
9288	7	22	7	22	The § 5.3.3.3 does not exist; probably 5.3.3.2? [philippe waldteufel, France]	Accept: 5.3.3.2
46536	7	22	7	22	Section 5.3.3.3 does not exist [WGI TSU, France]	Accept: 5.3.3.2
32686	7	24	7	29	There needs to be a better, more reader friendly indication of why the changes matter to people, etc. [Michael MacCracken, United States of America]	Accepted. Text modified.
38846	7	28	7	28	there should be no '-' between ocean and de-oxygenation [Emilie Breviere, Sweden]	Accepted - text revised
55844	7	29	7	32	It is not clear what "under highly populated zones" means with respect to coastal systems. Near human populations or below dense populations of marine organisms? [Forrest Hoffman, United States of America]	Accepted: clarified to mean human populations
36222	7	29			Replace 'as likely as not' with 'about as likely as not'. [Nathan Gillett, Canada]	Accepted.
17318	7	30	7	30	Insert , after 'zones' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
32688	7	30	7	30	What does "under highly populated zones" mean? And is this referring to populations of people on nearby land or of species in the ocean? [Michael MacCracken, United States of America]	Noted - the ES is re-edited for the SOD
38848	7	30	7	30	There should be a '-' in deoxygenation [Emilie Breviere, Sweden]	Accepted - text revised

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26486	7	32	7	32	period at the end of the sentence is missing. [Nadine Goris, Norway]	Accepted - text revised
38850	7	32	7	32	There should be a '.' at the end of the sentence. [Emilie Breviere, Sweden]	Accepted - text revised
38852	7	36	7	36	one should read 'oceanic' instead of 'ocean' [Emilie Breviere, Sweden]	Accepted - text revised
47322	7	36	7	37	this sentence not clear - it says sinks will continue to grow, stop growing, and decline. All in one sentence. Please clarify which? [chris jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Clarified.
26490	7	36	7	38	this sentence contradicts itself. Consider writing: "The ocean and terrestrial carbon sinks are expected to first continue to grow due to increased atmospheric CO2 but to subsequently weaken with warming (high confidence) and stop growing or decline under high GHG emissions scenarios". Also, it is not clear which part of the sentence points to the land and which to the ocean carbon sink. Consider revising. [Nadine Goris, Norway]	Noted
32690	7	36	7	38	Is this statement referring to the percentages of emissions of of absolute amounts of material? Is it not also the case that these sinks will weaken (at least in absolute terms) if emissions go toward zero, the present fluxes into the ocean and land being created by the differences in concentration gradient that is created by the ongoing emissions (the gradients existing because there is a lag in the adjustment of the land and ocean systems to the rising CO2 concentration)? I think greater clarity and/or further explanation is needed on the various trends under various conditions. Given that IPCC 1.5 report indicated that emissions must drop to zero in a few decades to stay below 1.5 to 2 C, what is projected to happen to the absolute fluxes--some say the sinks will continue at a high level for decades even with zero emissions while others suggest this is not the case. This whole point needs to be very clear on all of this. [Michael MacCracken, United States of America]	Noted
22208	7	36	7	38	"The ocean and terrestrial carbon sinks are expected to continue to grow..." in magnitude? In spatial distribution and depth? Clarification needed. [Gwenaelle GREMION, Canada]	Accepted. Clarified in text.
28142	7	36	7	38	My understanding is that the ocean and land carbon sinks are rather stable (i.e. CO2 flux; together they take up about half of the CO2 emissions). The carbon pools, however, are growing. [Alexander Winkler, Germany]	Reject: the sink (magnitude of the flux) increases but the g=fraction may not
46848	7	36	7	46	While I agree with this statements, I think the last sentence is not correct : the scenario uncertainty clearly dominates the total uncertainty for the total ocean C uptake but the model uncertainty overruled the other source of uncertainty for the biological pumps and the exports of carbon toward deep ocean. For the land the model uncertainty compete with the scenario uncertainty. (see Lovenduski et al. 2017 ERL, Figure 1 for example) [Roland Séférian, France]	Noted. We now explain this in the corresponding section 5.4.
26492	7	36	7	46	This whole paragraph is too difficult to read. Consider to entangle the effects on land and ocean sinks and check each sentence. [Nadine Goris, Norway]	Accepted. Text modified.
38858	7	36	7	46	this paragraph is unclear. [Emilie Breviere, Sweden]	Accept: made clearer

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
32212	7	36	7	46	When discussing the weakening terrestrial C sink, it might be good to specify whether that trend is considered primarily due to reduced primary productivity or increased respiration. [David Olefeldt, Canada]	Accept: edited for clarification
43106	7	36		38	This Exec Summ point just needs a bit of editing for the sake of clarity. How about: "Ocean and terrestrial carbon sinks are expected to grow as atmospheric CO2 rises but weaken as warming increases (high confidence), and are expected to stop growing or decline under high GHG emissions scenarios." Also, the subsequent text should make it clear that this point pertains only to the high emissions RCP8.5 scenario - this is not necessarily indicative of behaviour under lower concentration scenarios. [David Frame, New Zealand]	Accept: implemented.
22210	7	39	7	39	Maybe it's better to write PgC yr-1, with the C [Gwenaelle GREMION, Canada]	Accepted - text revised
38854	7	39	7	39	There should be a '.' between '6 Pg yr-1' and 'Most'. [Emilie Breviere, Sweden]	Accepted - text revised
6305	7	39	7	41	This sentence is unclear - does it infer that anthropogenic CO2 emissions are declining post 2060? Please revise to make clear what is meant here. [Dave Reay, United Kingdom (of Great Britain and Northern Ireland)]	Accept: edited for clarification
22212	7	40	7	40	begin, without s [Gwenaelle GREMION, Canada]	Accepted - text revised
45344	7	42	7	43	repeated words [Peter Rayner, Australia]	Accepted - text revised
56608	7	43	7	43	Correct doubled sentence fragment. [Malte Meinshausen, Australia]	Accepted - text revised
26494	7	43	7	43	Parts of the sentence are repeated unnecessarily. [Nadine Goris, Norway]	Accepted - text revised
6307	7	43	7	43	Partial sentence repeated [Dave Reay, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
17320	7	43	7	43	Insert full stop after 'limited' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
22214	7	43	7	43	This sentence starts twice [Gwenaelle GREMION, Canada]	Accepted - text revised
38856	7	43	7	43	It is likely that the ocean sink will be limited' is repeated twice. [Emilie Breviere, Sweden]	Accepted - text revised
55784	7	43	7	43	Sentence partially repeated [Christopher Smith, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
29438	7	43	7	43	There is evidence that plants will acclimatize to high CO2 levels on timescales of years to decades meaning that the CO2 fertilization effect could be rather short-lived (Reich et al., Science 2018). This is an important source of uncertainty and should be mentioned. [Rona Thompson, Norway]	Accept: edited for clarification
32692	7	43	7	44	Some duplicative text here. [Michael MacCracken, United States of America]	Accepted - text revised
43764	7	43	7	44	It is an established fact based on aquatic chemistry that the buffer capacity decreases when ocean water contains more inorganic carbon. The phrase "very likely" should be "virtually certain". [Michio Kawamiya, Japan]	Accepted. Changed.
29436	7	43	7	44	repetition of start of sentence [Rona Thompson, Norway]	Accepted - text revised
13642	7	43			Repetition of "It is very likely that the ocean sink will be limited" [Lena Boysen, Germany]	Accepted - text revised

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
24582	7	44	7	46	The differences in scenarios are not an uncertainty (at least not in the same way c-cycle feedbacks are), they are choices we will make. Maybe better to compare the c-cycle uncertainties with "differences" between scenarios. [William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Changed.
7492	7	44	7	46	I don't actually see evidence in Section 5.4 to support this statement, that uncertainty in projections are dominated by emissions scenarios rather than carbon cycle feedback uncertainty. Is this is discussed in a different chapter, or is established in the literature? [Rose Abramoff, France]	Accepted. Further developed in section, clearer traceability
32694	7	44	7	46	While the statement seems clear if emissions continue upward or stay substantial, what is expected if nations bring emissions to zero in the mid-century time frame? How long will sinks stay active and at what levels? Presumably, if the CO2 concentration is said to be long-lived, the sinks will drop significantly in that they are no longer being driven by the gradient caused by ongoing emissions, but this all needs to be explained. [Michael MacCracken, United States of America]	Noted. Better explained in corresponding section.
31754	7	48	7	48	The juxtaposition of "large uncertainty" in magnitude of the response with "medium to high confidence" that the response will be "small" is confusing. It might help to spell out what is meant by "small" here (e.g. "below 50% of ..." or "below 5% of ..."?). [Martin Jukes, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Changed.
22216	7	48	7	48	Does this include anthropogenic sources of NH4 and methane? Unclear and specifying might clarify what is meant in line 51-52 by climate-CH4 and climate-N2O feedbacks [Gwenaelle GREMION, Canada]	Noted. They include both. Text made clearer.
22218	7	48	7	49	Not consistent use between CH4 and methane. [Gwenaelle GREMION, Canada]	Accepted - text revised
51638	7	48	7	53	The way this is written, the non-specialist (non scientist) policy reader cannot understand what this means - is it a problem or not, and are we clear what affect increased CH4 and N2O have on marine life? [Lindsey Cook, Germany]	Rejected. Except for ocean acidification, the mandate of this chapter is not on impacts.
43108	7	48		53	Would probably re-order this (and some other) Exec Summ points - perhaps keep the CO2 ones together and then have the non-CO2 stuff at the end. [David Frame, New Zealand]	Noted.
22220	7	51	7	51	Climate-CH4 feedback needs to be defined. Warming induced increases in CH4 emissions that will exacerbate further warming. Unclear if this includes anthropogenic emissions. [Gwenaelle GREMION, Canada]	Noted. Text modified.
38860	7	52	7	52	in 'climate-N2O', the 'l' used is too long. [Emilie Breviere, Sweden]	Accepted - text revised
47324	7	52			can you define "small" in this context? Wetland methane emissions may change a lot and constitute a significant feedback. How have you assessed "small"? And how is confidence in this derived? [chris jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Clarified and made it more precise.
17322	7	53	7	53	Capital C for century (for consistency elsewhere in Chapter) [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - The AR6_WGI_StyleGuide says to use "century"
22222	8	1	8	2	Biological carbon pump needs to be defined [Gwenaelle GREMION, Canada]	Accepted. Explained.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
44104	8	1	8	4	Explain briefly what "sensitive to changes in the efficiency of the ocean's biological pump" means: outline the relationship of the biological pump to movement of atmospheric carbon into long-term storage in ocean sediments and rocks and directional changes in CO2 sequestration in the geological record during past changes in diversity or abundance of calcifying organisms. [Sara Kahanamoku, United States of America]	Accepted. Explained.
32696	8	1	8	4	Will not the biological pump continue to essentially compensate the supersaturated CO2 released by upwelling waters (that also bring up nutrients)? I would think a bit more explanation is needed. [Michael MacCracken, United States of America]	Accepted, explained..
17324	8	1			I suggest changing paleo to palaeo: the latter form is also used in the Chapter (e.g. line 42 of page 16) [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
29382	8	2	8	2	What is the time-scale considere for long-term? If it refers to the Laufkötter et al and Bopp et al papers, then long term should be specified to "centennial" [Judith Hauck, Germany]	Accepted.
38862	8	2	8	2	very uncertain' should be in italics [Emilie Breviere, Sweden]	Accepted - text revised
32702	8	2	8	30	While cumulative CO2 emissions determine long-term warming, it is also important to make clear that limiting emissions of short-lived species can reduce warming by a half degree of warming. There is more to dealing with the situation that we face than just dealing with CO2 emissions (or CO2e using GWP-100). The robust relationship that is spoken of is the case for ongoing emissions, but focusing solely on this relationship hides an important opportunity for slowing the warming in the near-term and thus does a disservice to decision-makers. [Michael MacCracken, United States of America]	Noted. There is a mention of the role of non-CO2.
22224	8	6	8	6	"...and the relative roles of CO2 versus CH4 as feedback processes." CO2 and CH4 are not processes. Does this statement refer to the emissions of these molecules due to permafrost thawing, and the contribution of each to increased warming? The details that follow (lines 8-11) do not completely explain the first statement. [Gwenaelle GREMION, Canada]	Accepted. Text made clearer.
25464	8	6	8	11	Will this occur everywhere in the permafrost regions - presumably more important in some areas depending on geology? [Sharon Smith, Canada]	Noted. There is no place for finer spatial detail in the ES.
32698	8	6	8	11	Would it not be appropriate to be mentioning that permafrost thawing is occurring faster than has been projected in model simulations and that these types of fluxes could well occur far earlier than 2100. Model projections for the Arctic have been behind observations in several respects and so some mention needs to be made that uncertainties would seem to lean in te direction of greater change than model results project. [Michael MacCracken, United States of America]	Rejected. We don't have evidence that permafrost thawing is occurring faster than what models predict, e.g., in AR5.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
56262	8	6	8	11	Permafrost: There was a lot of recent attention on observed records of thawing permafrost, which regionally seem to occur more quickly than projected in climate models (Farquharson et al. 2019: <a href="https://agupubs.onlinelibrary.wiley.com/doi/abs/10.1029/2019GL082187">https://agupubs.onlinelibrary.wiley.com/doi/abs/10.1029/2019GL082187</a> ). It would be useful to also refer to observed changes in permafrost thawing in this paragraph. [Sonia Seneviratne, Switzerland]	Reject: these are global mean magnitudes
56610	8	9	8	11	Well, the combined effect of both CH4 and CO2 seems VIRTUALLY CERTAIN to be larger than the one of CO2 alone (or if all carbon was emitted as CO2). Thus, I am not sure I understand the formulation "may be". You could either add a (ideally quantitative) description of the size of the effect, like may be X% larger .... [Malte Meinshausen, Australia]	Accepted: re-drafted to take this point.
45346	8	10	8	10	given that CH4 has larger RF than CO2 the combined RF must be at least as large as CO2 [Peter Rayner, Australia]	Accepted: is re-edited for the SOD
22226	8	10	8	10	maybe it's more precise to say "CH4 emission" instead of "production of CH4", because much of the CH4 produced in these areas are also consumed again before it reaches the atmosphere [Gwenaelle GREMION, Canada]	Accepted - text revised
24584	8	11	8	11	Surely the combined radiative forcing "will be" larger than from CO2-only (unless CH4 could be negative?). [William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Noted.
36224	8	11			Replace 'may' with 'will'. How could the combined radiative forcing from CH4 and CO2 not be higher than that due to CO2 alone? [Nathan Gillett, Canada]	Accepted.
56264	8	13	8	14	Abrupt changes are an important topic within the AR6. Would suggest to consider a new topic "Extremes and abrupt changes" as a new cross-cutting topic within the AR6 WG1 report. [Sonia Seneviratne, Switzerland]	Noted. Passed on to TSU
31762	8	13	8	20	What can possibly be meant by not having a runaway feedback over the next 100 years? Is "100 years" an allusion to the remainder of the 21st century, to which the bulk of the modeling evidence applies? Are you talking about onset of runaway feedbacks, emergence of runaway feedbacks (i.e. the processes reaching a significant amplitude) or commitment to runaway feedbacks? I find it hard to believe the the current evidential basis supports any conclusion about commitment to runaway feedbacks, which is a central question of societal interest here. [Martin Jukes, United Kingdom (of Great Britain and Northern Ireland)]	Noted. We agree there is no evidence for runaway climate in the near future.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
43110	8	13		20	These two Exec Summ points need to be consistent with each other. They are, in a way, but many readers may not see that. Maybe quantify the "substantial" feedbacks in the former, and address the vagueness of "Large uncertainties remain on the possibility of additional feedbacks not represented in current models, which could lead to significant departures from the current modelled trajectories." Which uncertainties? How significant? Under which RCPs? Also, in the latter bullet point "robust" sits awkwardly with "Additional Earth system feedbacks like permafrost thawing have the potential to break the constancy of TCRE" Can we use evidence from models here? How many models change by how much in response to permafrost? I think the "robust" bit is worth keeping if you can, since carbon budgets depend on it. But as it stands, the rather nebulous stuff on biogeochemical tipping points kind of undermines the carbon budget calculations. [David Frame, New Zealand]	Accepted. Text made clearer
9160	8	18	8	20	It is good to see the statement that there are large uncertainties in modelling, which uncertainties should be clearly reflected in the final conclusions. [Jim O'Brien, Ireland]	Noted: edited in the SOD
22228	8	18	8	20	Is it possible for this statement quantifying the uncertainty here? Right now it is vague but there are some certainties. It is likely (very likely?) models do not include all potential feedbacks that influence modeled trajectories. Right now it is hard to place this statement into context with the other statements in this section. [Gwenaelle GREMION, Canada]	Accepted. Added.
36226	8	18		20	This is a strong statement on the possible role of additional feedbacks not represented in models. As written this says that there may be missing feedbacks in models causing large uncertainties, without putting any bound on the magnitude of these effects, which may undermine confidence in projections/budgets etc. But, for example, the fact that the authors plan to derive probabilistic carbon budgets (In 37-39) (no confidence level specified, so implied high confidence), suggests that they can put some bounds on these effects. Could the authors add something about the likely maximum magnitude of these effects or similar? The non-ESM feedbacks shown in Fig 5.29 are relatively small. [Nathan Gillett, Canada]	Accepted. Text make clearer the magnitude but there is a component which is not known.
26496	8	22	8	22	this caption is not clear. Consider using "Remaining carbon budget for climate stabilization" [Nadine Goris, Norway]	Accept: the support to this statement is made clear in the SOD
9162	8	24	8	42	The final determination of TCRE is crucial to the conclusions of the Report. [Jim O'Brien, Ireland]	Noted: this is included in the SOD
36228	8	24			I do not think the literature supports the statement that 'Robust physical understanding underpins the near linear relationship between cumulative CO2 emissions and global mean temperature increase'. This linear relationship is primarily a model result, and some studies have shown various simple models which reproduce this behaviour under certain conditions. But, for example, the effect was not predicted before it was found in ESMs. I would replace with a statement saying that the near-linear relationship is a consistently simulated across ESMs and EMICs, or similar. [Nathan Gillett, Canada]	Accepted - the wording has been adjusted



Comment ID	From Page	From Line	To Page	To Line	Comment	Response
22230	8	26	8	26	Implies is a passive verb to use when the relationship between the TCRE and warming is so pronounced. [Gwenaelle GREMION, Canada]	Noted.
6702	8	26	8	27	TCRE implies that CO2 emissions will need to be close to zero to stabilize planetary climate. Where emissions need to be zero, slightly negative or slightly positive is unknown and the subject of ZEC-MIP. [Andrew MacDougall, Canada]	Accepted - the wording adjusted
13494	8	27	8	27	I believe the net emissions need not be zero to stabilize global surface temperature. It can be slightly positive as oceans continue to take up carbon. [Govindasamy Bala, India]	Taken into account - also the opposite case could be true (where emissions need to be slightly negative). Text has been reworded.
22232	8	27	8	28	This statement is lost without a timeframe. [Gwenaelle GREMION, Canada]	Fixed
27220	8	28	8	28	The uncertainty $2.5/0.8 = 3.1$ is too large to be useful [François GERVAIS, France]	Noted
47500	8	28	8	28	For ease of understanding and comparison, include GtCO <sub>2</sub> : 0.8-2.5°C per 1000 PgC(3650 GtCO <sub>2</sub> ) ±PgC [Birgit van Munster, United Kingdom (of Great Britain and Northern Ireland)]	Noted
17326	8	28	8	29	insert , after 'timescales' and 'thawing' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
13554	8	28	8	30	Can permafrost melting be considered as a feedback? If the carbon in permafrost is external to the system (true in the context of cumulative carbon emissions), this cannot be strictly considered as a feedback. [Govindasamy Bala, India]	Permafrost is internal to the Earth system
17328	8	29	8	29	'like' is poor English. I suggest replacing it with 'such as' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
47502	8	32	8	34	Idem: include GtCO <sub>2</sub> : Since pre-industrial times, a total of 690 ±90 PgC (2,530 ±330 GtCO <sub>2</sub> ) [Birgit van Munster, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - this information is included in a different ES statement above.
49108	8	32	8	42	It needs to be clear whether these budgets will be based on GSAT or GMST. This is a policy interface issue which has great potential to cause confusion. Many stakeholders think IPCC moved the goalposts in SR1.5. [Jim Skea, United Kingdom (of Great Britain and Northern Ireland)]	Accepted . The budget will be based on GSAT. We hope stakeholders are receiving the right information in this regard.
49112	8	32	8	42	It has been commented on in relation to the SRCL report that a small table can convey information of the type in this paragraph more transparently. [Jim Skea, United Kingdom (of Great Britain and Northern Ireland)]	Noted
57340	8	32	8	42	I thought we'd lost the battle of units to the WG3 enthusiasts for GtCO <sub>2</sub> , so surprised to see PgC back from the dead. Much as I hate to give in to WG3, it is more important to use consistent units than to maintain WG identity. Everyone knows WG1 is more rigorous anyway, so we can concede here. [Myles Allen, United Kingdom (of Great Britain and Northern Ireland)]	Noted
17330	8	34	8	34	Change to Pre-Industrial for consistency elsewhere in the text and other Chapters [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - The same spelling as in the IPCC SR1.5 / SROCC has been applied (pre-industrial has been adopted throughout the chapter for consistency)
22234	8	36	8	36	What is meant by "non-CO2 forcers"? This sentence is unclear. [Gwenaelle GREMION, Canada]	Noted

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
22236	8	37	8	37	The average reader will not know what "coupled" models are [Gwenaelle GREMION, Canada]	Noted
32706	8	37	8	38	It is not at all clear that limiting ongoing warming to any of these values (assuming that all is done is to sustain warming at these levels) meets the objective of the UNFCCC (namely to prevent dangerous anthropogenic interference with the climate system). The Paris goals were set partly based on what might be possible and partly with notion that such limits would prevent runaway warming. My understanding is also that the Paris Accord did not indicate whether the stated warmings were to be peak levels with retreat afterwards towards no warming or were to be levels that there was agreement could be sustained over time and still meet the UNFCCC objective. With paleoclimatic analyses suggesting that the equilibrium sea level sensitivity is something like 20 meters per degree C change in global average temperature suggests that meeting the UNFCCC objective requires heading back to less than 0.5 C warming--not continuing at a sustained level. It seems to me that there also need to be estimates of how much carbon would need to be pulled out of the atmosphere to get back to 0, 0.5 and 1 C warming. [Michael MacCracken, United States of America]	Noted - the ES can only summarize what is assessed in the underlying chapter. The requested information is not available. The problem of peaking warming is also to be solved before returning warming back to 0, 0.5, or 1°C is on the agenda.
57342	8	37	8	39	I appreciate this seems to have evolved into a tradition, but giving one-sided confidence intervals in this way (50th and 66th percentiles, but not 33rd percentile) is dangerous and led to the perception that the AR5 budget was more wrong than it actually was. I suggest you give central estimates and ranges. Anyone argues that the 33rd percentile is policy-irrelevant doesn't own any potentially stranded fossil fuel assets. [Myles Allen, United Kingdom (of Great Britain and Northern Ireland)]	Noted - 33% is also provided in the Chapter. One could provide a 66% range (17%-83%) in addition to the 66th percentile
32704	8	37	8	42	Again, sharply cutting emissions of short-lived species can mslow the pace of warming, and just controlling CO2 without dealing with the full set of influences can lead to ongoing warming. This seemingly sole focus on CO2 is really problematic as it is virtually impossible to change the rate of warming over the next few decades by just cutting CO2 emissions-- whereas cutting emissions of short-lived species could reduce the projected warming from present to 2050 by about half. This is not to say not to also be dealing with CO2, but ignoring the option afforded by sharply cutting emissions of short-lived species I think does a disservice to the effort to limit warming. [Michael MacCracken, United States of America]	Noted - This can be integrated in the Technical Summary and then again in the Summary for Policymakers. The assessment available in Chapter 5 does not allow to make strong statements about non-CO2 forcing or warming in its ES.
22238	8	38	8	40	Don't forget to change the Ys and Xs to actual numbers. [Gwenaelle GREMION, Canada]	Noted
13644	8	38	8	42	Update X and Y [Lena Boysen, Germany]	Noted.
53466	8	39	8	39	You may insert "reduction" after linear to make it more clear. [Jan Fuglestedt, Norway]	Noted
47504	8	39	8	42	Rather than expressing the remaining carbon budget on a linear trajectory, express the remaining carbon budget on the NDC trajectory. This is policy relevant. [Birgit van Munster, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - The chapter does not have an NDC trajectory assessment to base such a statement on, but this could be considered at a much higher level in the IPCC AR6 Synthesis Report.
53468	8	40	8	40	year --> years [Jan Fuglestedt, Norway]	Accepted - text revised

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
36230	8	40			Insert 'of decreasing emissions' after 'trajectory'. [Nathan Gillett, Canada]	Taken into account
38864	8	41	8	41	There should be no '-' in 'carbon-dioxyde removal'. [Emilie Breviere, Sweden]	Accepted - text revised
32214	8	44	8	44	change "permafrost thawing" to "thawing permafrost ground" [David Olefeldt, Canada]	Accepted - text revised
24586	8	44	8	46	These factors could change the need from "decline to net zero" to "decline to net negative". Maybe "decline to at most net zero" would be better. [William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Noted.
55034	8	44	8	46	I suggest including "at any temperature level" at the end of the sentence "to halt global warming" [Rojas Maisa, Chile]	Taken into account
57344	8	44	8	52	Need to be crystal clear whether carbon budgets refer to budgets to peak warming or to 2100. 30PgC number implies budgets to 2100. Previous bullet implies budgets are to peak warming. Budgets to peak warming are much more policy-relevant (and better constrained) given the huge uncertainties in both anthropogenic and natural emissions after temperatures peak. [Myles Allen, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - the wording has been adjusted
57346	8	44	8	52	Why not also give budgets for CO2-forcing-equivalent emissions: these are much better constrained by TCRE and less scenario-dependent than CO2-only budgets. The argument against them in SR1.5 was that the concept was too novel (it wasn't really), but that no longer applies. [Myles Allen, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - CO2 forcing equivalence is not being assessed in Chapter 5
22240	8	45	8	45	What is meant by "non-CO2 warming"? If this is just warming from non-CO2 GHGs this should be stated more clearly [Gwenaelle GREMION, Canada]	Noted.
56612	8	49	8	49	Change "historic" to "historical" [Malte Meinshausen, Australia]	Accepted - text revised
45348	9	1	9	1	offset by reduced uptake rather than outgassing most likely [Peter Rayner, Australia]	Noted
16210	9	1	9	17	These are very important findings, however, the consequences arising from them are not readily apparent. The first paragraph essentially states that CDR is largely ineffective, the second states that the carbon cycle response to pulse removals beyond 100 PgC is non-linear and asymmetric – these two aspects should be highlighted to caution against reliance on large-scale CDR. [Linda Schneider, Germany]	Taken into account. The statements have been revised to highlight the implications of these findings.
38866	9	3	9	3	'be opposed' should be replaced by another term, counteract perhaps. [Emilie Breviere, Sweden]	Accepted - text revised
51640	9	3	9	9	I do not understand this bold statement, 'opposed by'? Nor how what is happening now with sinks (as CDR) will change (or not) in future scenarios, as if we suddenly create more natural CDR sinks? What are you trying to say here, for us to understand? [Lindsey Cook, Germany]	Noted. Rephrased.
22242	9	3	9	9	It would be helpful to include the relative timeframe by which outgassing and reservoir repartitioning will occur in response to CDR. If 100 PgC is removed, 50% will remain in the atmosphere... immediately? Within 10 yrs? 100 yrs? A reference to another section or brief mention of the relative rates of land-atmosphere and ocean-atmosphere carbon exchange would be useful. [Gwenaelle GREMION, Canada]	Accepted. The time frame has been specified.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
32708	9	3	9	17	In that the processes by which CO2 is taken up by the terrestrial biosphere and the ocean are different than the processes that would return CO2 to the atmosphere from the terrestrial biosphere and oceans, how is it that the return fractions (or at least the return times) would be the same? For the oceans, while the mixed layer content would adjust in a similar way, for the deep ocean carbon content, uptake is by downwelling to the bottom and the biological pump to supersaturation in some mid-layer, while the return of mid to deep ocean carbon to the surface is due to a slow and broad upwelling in lower latitudes, etc. (and it is not clear how removal of CO2 from the atmosphere would affect those processes by much). For the land, long-term carbon uptake goes into the roots, etc., and it is not clear how lowering the atmospheric CO2 concentration by pulling carbon out would accelerate loss of carbon from the long-term store in the soils. So, it seems to me that the time constants of uptake and then of release would be quite different. [Michael MacCracken, United States of America]	Noted. This asymmetry is addressed to some extent in section 5.6.2.1
56614	9	7	9	7	Provide a range or a one sided percentage, as - strictly speaking - the current sentence is false. The fraction is EXACTLY 50% with only ZERO chance,, but might be VERY LIKELY BELOW 50% (or likely within 40% to 60%) etc... [Malte Meinshausen, Australia]	Accepted. The likelihood statement has been revised.
36232	9	7			The probability that the fraction of CO2 removed that remains out of the atmosphere is exactly 50% for 100 PgC removed is zero. In order to associate a probability (very likely, P>95%), you need to specify a range for the fraction, not one number. [Nathan Gillett, Canada]	Accepted. The likelihood statement has been revised.
51642	9	11	9	17	Again, do not understand the consequences of what you are trying to say. Layman's (policy makers) language? [Lindsey Cook, Germany]	Accepted. Rephrased.
13556	9	11	9	17	Is the asymmetry for large carbon removals because of the logarithmic dependence of radiative forcing and T on CO2 concentration? [Govindasamy Bala, India]	It is due to nonlinearities and dependence of the carbon cycle response on the climate state - see section 5.6.2.1.
28816	9	11	9	26	Which way does the asymmetry go? This currently appears to contradict the Ch4 ES which says that CDR doesn't work.. [Piers Piers Forster, United Kingdom (of Great Britain and Northern Ireland)]	Positive emissions are more effective at changing atmospheric CO2 than CDR. This has been clarified.
13646	9	13			Update "initial model runs" [Lena Boysen, Germany]	Noted
22244	9	15	9	15	The average reader will not know what "state dependencies and non-linearities" are. Suggestion: "... because the processes that resulted in the current atmospheric carbon budget do not all react to a lower atmospheric carbon pool by themselves reversing" [Gwenaelle GREMION, Canada]	Accepted. The sentence has been reworded.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
32710	9	19	9	22	The text needs to give at least an example of a potential negative consequence of CO2 removal. Given that it is agreed that the world would benefit greatly from not putting out any more emissions, how is it that taking CO2 out of the atmosphere (which is equivalent to reducing emissions) would have negative consequences that, as stated here, are roughly comparable to the beneficial consequences of removing the CO2. The statement here makes no sense at least as long as emissions are larger than the amount removed per year. And it is also hard to understand how the negative consequences would be at all comparable to the benefits even when fossil-fuel based emissions are zero; if one considers the sea level rise impact alone, the goal needs to be to get the CO2 concentration back to 350 ppm or below in an effort to stop ongoing loss of ice from the ice sheets as sea level rise has devastating global consequences. I just do not think the very balanced statement here is at all justified. [Michael MacCracken, United States of America]	Taken into account. Paragraph has been rewritten.
16212	9	19	9	26	This paragraph does not do justice to the literature on risks and potentials of large-scale CDR. Stating that CDR methods can have both beneficial and adverse environmental side effects makes it sound as if one was as likely as the other. However, the CDR literature and studies of their risks and adverse impacts clearly shows that risks and environmental damage outweigh the „environmental benefits“ by far, at least when referring to industrial, technological CDR schemes. In fact, it is very unlikely that those would have any environmental benefits at all. Also, these are not necessarily project- and regionally specific. Large-scale deployment of BECCS or afforestation with monoculture trees destroys natural ecosystems and biodiversity, jeopardizes food security in large regions or even globally, leads to land grabs and land tenure rights violations – particularly the earlier aspects are true for all localities and regions. Also, this section should differentiate more clearly between technological CDR and ecosystem-based approaches („Natural Climate Solutions“) to sequester CO2. While the former comes with an array of detrimental social and environmental impacts, the latter has significant potential for environmental co-benefits and creates synergies with other internationally agreed goals, such as the SDGs. See SR1.5 SPM for an initial appraisal of these findings. [Linda Schneider, Germany]	Taken into account. Paragraph has been rewritten.
29384	9	23	9	26	"decreasing ocean acidification", "reverse acidification". Adding alkalinity to the ocean only reverses the acidification if you add huge amounts of alkalinity or if we stop emitting CO2. The first thing that happens under high or rising CO2, is that when adding alkalinity, more CO2 is taken up from the atmosphere, and that makes pH go down again to the same level as before adding alkalinity. More CO2-uptake, but doesn't help against acidification. See Hauck et al (2016) doi:10.1088/1748-9326/11/2/024007 [Judith Hauck, Germany]	Taken into account. The statement has been reworded to clarify that we are referring to CDR methods that do not affect ocean pH directly (land-based methods or direct air capture).

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
32712	9	24	9	26	So, in the opening points of this section it is suggested that the CO2 among the various reservoirs will adjust, implying pretty quickly, but here it seems that the carbon inserted into the deep ocean will be sequestered there of a long period and not easily taken out. These statements seem to me in conflict--my view is that the statement here is closer to the situation than the former ones implying that CDR would face a rather rapid readjustment of how much carbon stays in each reservoir. [Michael MacCracken, United States of America]	The mixed layer adjusts quickly to a decline in atmospheric CO2 whereas the deep ocean takes longer (centuries to millennia) to adjust.
22246	9	24	9	26	Again, a brief mention of the timeframes would be helpful. Ocean acidification at the sea surface will be ameliorated on the order of years? How many orders of magnitude longer will deep ocean de-acidification take? 100s of ys? 1000s? [Gwenaelle GREMION, Canada]	Taken into account. The time frame has been included.
56616	9	25	9	25	Is "CDR" here implicitly and strictly defined as land-based carbon dioxide removal? Because if some folks could understand also ocean-based carbon dioxide removal (iron fertilization), the acidification response might be not that clear or opposite?! [Malte Meinshausen, Australia]	Taken into account. The statement has been reworded to clarify that we are referring to CDR methods that do not affect ocean pH directly (land-based methods or direct air capture).
46850	9	28	9	33	There is an high agreement on the sign of the response of carbon sink to SRM but the processes dominating this response are poorly understood given the lack of understanding of ecophysiological processes (and the model uncertainty for land carbon model, See for example Plazzotta et al. 2019 Earth Future) [Roland Séférian, France]	Noted.
12826	9	28	9	33	Mention that although net primary production may go up, crop yields will not necessarily rise. See Jonathan Proctor et al., Estimating global agricultural effects of geoengineering using volcanic eruptions, Nature (August 2018). [Durwood Zaelke, United States of America]	Noted. Included reference.
16214	9	28	9	33	This paragraph could be easily interpreted as promoting SRM in that it highlights a benefit while not highlighting the risks associated with it. Why not start off with the statement on the relationship between SRM and ocean acidification, which is one of the most significant downsides of SRM? The underlying chapter (5-80) even states that ocean acidification in the deep ocean may be accelerated as a result of SRM-induced ocean circulation change. [Linda Schneider, Germany]	Noted. Text largely remains the same and highlight further the ocean acidification issue.
47506	9	28	9	33	Add this information in the summary: "and is very likely to have a negative impact on ocean ecosystems. There is a risk that yields of rice, maize, wheat and soy will be reduced. A sudden termination of the deployment of SRM because of engineering failure or the lack of agreement for the maintenance of SRM would cause a rapid increase in global temperature and precipitation, and a reduction in sea ice area. Sudden termination of SRM would increase both land and ocean temperature rise to an extent that far exceeds that predicted for future climate change without SRM." [Birgit van Munster, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. The chapter focus is not on agricultural systems or impacts in general. Outside of scope, however we mentioned crops.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
51644	9	28	9	33	You are trying to highlight the concerns, but I think could be more clear in what it means' to increase global mean net primary production and carbon storage on land'. An additional sentence detailing the dangerous consequences so the reader understands. We have a responsibility to be clear on warning, lest policy makers think this is an easy option (have your cake and eat it too). [Lindsey Cook, Germany]	Noted. Rephrased.
32714	9	28	9	33	How is it that the most important influence of SRM on the biosphere and biogeochemical cycles, namely reducing the amount of climate change and so reducing the changes in ecosystems and loss of biodiversity, are not the first point mentioned? SRM would be expected to: reduce warming of the ocean, so affecting CO2 uptake; reduce warming and so reduce CO2 emissions from demand for air-conditioning; reduce the die off of species from heat stress, so generally lessen effects on the biosphere; and on and on. The presentation here is very incomplete and not at all a comprehensive discussion of the implications of SRM. And on the point made in the first sentence, this CO2 fertilization takes place any way with ongoing emissions, so the only difference (aside from the unmentioned climate effect) is the "highly uncertain diffuse-radiation fertilisation effect" but the overall effect is deemed "very likely"--well, yes, but this would be happening anyway, so why associated it with SRM? I think this single paragraph is a totally inadequate statement of the significance of SRM, and it is made totally out of context of the significance of the impacts of ongoing climate change due to human influences. As a summary of SRM implications with respect to the topics covered in this chapter, this summary is very seriously lacking, not at all seeming to weigh the ongoing pace of climate change and its implications for society and the environment against the implications of pursuing SRM. [Michael MacCracken, United States of America]	Accepted. Mentioned.
22248	9	28	9	33	The mechanism behind SRM needs to be laid out here. Suggestion: induce cooling by injecting aerosols into the atmosphere A brief definition of solar radiation modification (or examples?) is needed. From the text provided, I assume SRM here is limited to aerosol injection in the upper atmosphere? "diffuse-radiation fertilization effect" is too technical. Suggestion: the impact of increased aerosols on photosynthesis. [Gwenaelle GREMION, Canada]	Yes to the question. Accepted to include brief description.
56618	9	28			For a balanced reflection of SRM, it seems pertinent to mention potential effects on precipitation patterns and the consequential effects on terrestrial carbon storage. As a minimum a qualifier like "next to potentially wide ranging effects on climate variables apart from global-mean temperatures...." seems appropriate. [Malte Meinshausen, Australia]	Accepted. Text modified.
47326	9	28			can you quantify at all the increase in NPP? (and more importantly storage?) [chris jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Explained in the corresponding section 5.6

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
56266	9	29	9	29	Change "will" to "would". "Will" gives the impression that this is anyway going to happen, however there are substantial concerns associated with such deployment as highlighted in the IPCC SR15. [Sonia Seneviratne, Switzerland]	Accepted. Changed.
29440	9	30	9	30	as for p7 line 43 [Rona Thompson, Norway]	Noted.
22250	9	32	9	33	Is confidence language needed here? [Gwenaelle GREMION, Canada]	Accepted. Added.
24588	10	1	10	1	This introduction is clear and reads very well [William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Noted, Thanks
17332	10	1	10	1	Change paleo to palaeo [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
47798	10	1	17	22	Assessment Structure: If possible, assessment conclusions should be provided in a structured traceable account of how these statements were derived. For example, sections / subsections can start with previous IPCC report conclusions (AR5 or AR6 Special Reports) and then provide an update of the more recent literature, clearly laying out the lines of evidence. Each section / subsection can then conclude with assessment statements. This section currently reads more as a review than an assessment. [WGI TSU, France]	Accepted. Implement on key subsection levels the structure suggested.
22252	10	1	17	42	The majority of the introduction and paleo context focuses on ocean-atmosphere dynamics, with far less detail provided for land-atmosphere feedbacks. Is there a lack of research in this area (relative to ocean source-sink dynamics) or can some more detail be included? I think this would be especially helpful in linking with subsequent sections. [Gwenaelle GREMION, Canada]	Accepted. Draft has been revised
27222	10	3	10	3	This sentence is more than questionable because 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 acceleration of emissions starting in 1945, the increase of temperature has been only about 0.4°C up to the plateau before (and after) the El Niño peak of 2016, among which one half might be anthropogenic. [François GERVAIS, France]	Accepted. We made it clear that only from mid last century GHG changes dominate climate change.
17334	10	4	10	4	Insert s after 'Earth' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
17336	10	6	10	6	Change GHG to GHGs [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
55786	10	7	10	7	Multiple carbon reservoirs. The context of this sentence is about all GHGs, so it should technically extend to at least nitrogen. [Christopher Smith, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised



Comment ID	From Page	From Line	To Page	To Line	Comment	Response
22254	10	8	10	8	I believe the value should actually be 660 +/- 95 (from Table 8 in Le Quere et al. 2018). Citation: Le Quéré, C., Andrew, R. M., Friedlingstein, P., Sitch, S., Hauck, J., Pongratz, J., ... Zheng, B. (2018). Global Carbon Budget 2018. Earth System Science Data, 10(4), 2141–2194. <a href="https://doi.org/10.5194/essd-10-2141-2018">https://doi.org/10.5194/essd-10-2141-2018</a> [Gwenaëlle GREMION, Canada]	Accepted, changed.
22256	10	8	10	8	In Chapter 5.1 there is an inconsistency on how to report historical dates. In general, in figure captions the authors use BCE/CE, whereas in the text, BCE is used and CE is omitted. I would recommend to generally use BCE/CE for historical records. [Gwenaëlle GREMION, Canada]	Accepted
22258	10	9	10	9	I think there should be a citation for the phrase, "of which less than half remains in the atmosphere today". If this is in reference to the "partitioning" part of Table 8 in Le Quere et al. 2018, where it shows the growth rate in atmospheric CO2 concentration is only 275 +/- 5 PgC, whereas there's been a growth rate in the ocean and terrestrial sink where more than half of the emitted CO2 has been sequestered, it would help to have this explicitly referenced. Furthermore "of which less than half" could be quantified: 165 PgC exported to the ocean, 215 PgC uptake in terrestrial reservoirs, 275 PgC stays in the atmosphere; Citation: Le Quéré, C., Andrew, R. M., Friedlingstein, P., Sitch, S., Hauck, J., Pongratz, J., ... Zheng, B. (2018). Global Carbon Budget 2018. Earth System Science Data, 10(4), 2141–2194. <a href="https://doi.org/10.5194/essd-10-2141-2018">https://doi.org/10.5194/essd-10-2141-2018</a> [Gwenaëlle GREMION, Canada]	Accepted. Reference included.
32716	10	13	10	16	There is also growing interest in pulling the CO2 concentration back down (via CDR, etc.), and that there is interest merits consideration and mention as well. The Paris Accord temperature levels, if sustained, would not, for quite a number of reasons, meet the UNFCCC (1992) objective, and this needs to be considered and how to bring the GHG concentrations down needs to be discussed. [Michael MacCracken, United States of America]	Accepted. A whole new sentence added with to cover CO2 removal.
17338	10	15	10	15	Delete comma after 'impacts' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
37726	10	18	10	19	Ozone is a greenhouse gas, and Chapter 7 indicates that it has a bigger RF than N2O. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Accepted, we no longer say they are the most dominant.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
32718	10	18	10	19	While this is the case if one looks at total forcings, this is not nearly so strongly the case if one considers the impacts of the species whose 21st century emissions can be controlled--it turns out to be essential to consider the lifetimes of the species. If one takes the MAGICC model and goes to zero emission in 2000, the main ongoing forcing is for CO2 as the methane, tropospheric ozone, and aerosol forcings quickly go to zero. So, this ongoing CO2 forcing is the overwhelming legacy from 20th century emissions. If one then adds in the 21st century contributions to forcing of the 21st century emissions of each of the species, one can see what limitations are possible by controlling CO2 emissions, which is all we can really control. Doing this, only about half of the additional, time-integrated GHG forcing during the 21st century is due to 21st century CO2 emissions; the other half is due mainly to methane and tropospheric ozone--yes, they will have no legacy forcing into the 22nd century, but they will have contributed a significant warming influence, so caused earlier warming than considering just CO2 or all species treated as CO2e using the 100 year GWP. For this reason, and as explained in the UNEP assessment on this and the Shindell et al. Science article on this, cutting of emissions of short-lived species can have an important effect, roughly cutting projected warming from the present to 2050 in half (so a total of about 0.5 C) that then carries forward over time. This is not to say not to cut CO2 emissions, for their influence extends into following centuries, but there is a real advantage in cutting emissions of short-lived species, and this approach merits coverage in this IPCC assessment. Based on the statement here, it appears this whole opportunity is to be totally ignored, which would be a real disservice to the negotiators. [Michael MacCracken, United States of America]	Rejected. The mandate of the chapter is to cover co2, ch4 and N2O. Other important GHGs are covered in other chapters including ch7
22260	10	18	10	19	Maybe add "three GHGs of which ecords exist". Water vapor is the most important GHG and through feedback mechanisms also influenced by anthropogenic climate change. However, no reliable paleo records exist and human perturbation is substantial through secondary processes. [Gwenaelle GREMION, Canada]	Accepted. Added suggestion.
47242	10	21	10	22	Sentence lacks clarity - not sure what is meant by "overview of the place in Earth's history of the current and future scenarios..." [Katrina Nilsson-Kerr, United Kingdom (of Great Britain and Northern Ireland)]	Accepted, changed.
22262	10	21	11	4	It would be much easier to follow these paragraphs if they all consistently started with the phrase, "Section 5.X", similar to the paragraphs outlining what is in Section 5.6 and 5.7 on page 11. [Gwenaelle GREMION, Canada]	Accepted.
13256	10	27	10	55	Nohara et al 2013 discuss about the carbon cycle on CO2 emission pathways.  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:10.1088/1748-9326/8/1/0 [Masamichi Ohba, Japan]	Rejected. Here we are describing what the chapter will cover, no need for references.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
17340	10	29	10	29	Change 'advancements' to 'advances (better English) [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
38868	10	37	10	37	There should be a '-' in deoxygenation [Emilie Breviere, Sweden]	Accepted - text revised
43316	10	37		40	The sentence should be shortened. [Onema Adojoh, United States of America]	Accepted, shorten
17342	10	40	10	40	Define DIC [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. Removed because other review comments.
38870	10	40	10	40	DIC' should be spelled out, it is encountered for the first time in this chapter. [Emilie Breviere, Sweden]	Accepted - text revised
51492	10	45	10	51	are terrestrial permafrost processes included in any of the models? [Christian Beer, Germany]	Yes, but very few models particularly for fully coupled earth system models; a few more have permafrost in an offline model
17344	10	48	10	48	Change 'in' to 'on' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
9644	10	50	10	51	I suggest changing "Uncertainties and the limits of our models to predict future dynamics for a given GHG emissions trajectories are given." to "Uncertainties and the limits of our models to predict future dynamics for GHG emissions trajectories are given." [Brian Magi, United States of America]	Accepted - text revised
22264	10	50	10	51	trajectory has to be singular (not "trajectories"). [Gwenaelle GREMION, Canada]	Taken into account - see comment 9644, which now requires "trajectory" to be plural
44846	10	51	10	51	There are two "given" in one sentence. "are given" should be "are provided"? [Kaoru Kubota, Japan]	Taken into account
9290	10	53	10	53	Maybe you might introduce an acronym for this achievement which certainly deserves it; perhaps CBC for Carbon Budget Concept, or whatever. That would save some room! [philippe waldteufel, France]	Rejected, for clarity for the reader.
17346	10	55	10	55	Change to Pre-Industrial for consistency elsewhere in the text and other Chapters [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - The same spelling as in the IPCC SR1.5 / SROCC has been applied (pre-industrial has been adopted throughout the chapter for consistency)
29386	11	7	11	7	wouldn't it be more accurate to talk about albedo modification rather than solar radiation management? The sun will still have the same radiation. See: National Research Council 2015 Climate Intervention: Reflecting Sunlight to Cool Earth (Washington, DC: The National Academies) ( <a href="http://www.nap.edu/catalog/18988/climate-interventionreflecting-sunlight-to-cool-earth">http://www.nap.edu/catalog/18988/climate-interventionreflecting-sunlight-to-cool-earth</a> ) [Judith Hauck, Germany]	Rejected. AR6 WGI made the decisions to use solar radiation management
51936	11	17	12	51	This is also covered, more extensively, in chapter 2. Some thought is required as to how to handle this. It is clearly in the scope of both chapters so some redundancy is inevitable. As far as I can tell the content is consistent but some cross-referencing and reconciliation may be necessary to avoid overt repetition. [Peter Thorne, Ireland]	Accepted and add cross references to ch2
47806	11	19	11	21	Chapters 2, 5 and 7 class methane as long-lived but chapter 6 classes it as short-lived. [WGI TSU, France]	Noted. Agreed across all chapters to call methane a short-lived.
22266	11	19	11	21	It may help the reader to vertically highlight 1750 in Figure 5.1 to more quickly follow along with this text. [Gwenaelle GREMION, Canada]	Accepted. Figure changed, last panel.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
24590	11	19	11	47	This text (and figure 5.1) repeats information that was already covered in chapter 2. This should at least reference chapter 2 and be consistent with it. If figs 2.3 and 5.1 are different, which one is the "Authoritative IPCC GHG timeseries"? [William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Agreed on the exact figure and consistency between the two chapters.
44930	11	19	12	42	This section on the "time context..." overlaps with section 5.1.3, "paleo trends..." I suggest combining and streamlining the two into a single section. If there's need to retain both, please explain the purpose of each in the first paragraph of each section so their unique purposes are clear. Also, the discussion is difficult to follow; it skips around in time for 800 ka to PETM to the industrial era, to the Pliocene, back to 800 ka then to the 20th century. [Darrell Kaufman, United States of America]	Accepted. We have reduced the overlap by cutting text.
37728	11	20	11	21	CH4 is characterised here as long-lived. It is characterised as short-lived in Chapter 6. See comment 2 on the entire document on the need for consistency in the discussion of methane. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Noted. We removed long lived and only use well mix GHG as agreed across chapters. Ch6 I uses short lived but there isn't inconsistency across chapters..
17348	11	21	11	21	Change era to Era [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
41772	11	23	11	23	specify the year when these values were reached [Marc Aubinet, Belgium]	Rejected. The value is already included, 2017.
16032	11	23	11	25	The 2017 concentraion and percentage increase above pre-industrial levels of CH4 are slightly different from the values reported by the WMO Greenhouse Gas Bulletin ( <a href="https://library.wmo.int/doc_num.php?explnum_id=5455">https://library.wmo.int/doc_num.php?explnum_id=5455</a> ). Please check. [SAI MING LEE, China]	Accepted. Changed to WMO data..
47718	11	23	11	27	Please check the use of this IPCC uncertainty language term. Are you able to provide a traceable account to assigning this uncertainty statement? Note that likelihood statements are quantified terms - phrases like likely and very likely have quantifiable probabilities associated with them. Please check it has been used correctly here. Please refer to the IPCC guidance note on uncertainty: <a href="https://wg1.ipcc.ch/SR/documents/ar5_uncertainty-guidance-note.pdf">https://wg1.ipcc.ch/SR/documents/ar5_uncertainty-guidance-note.pdf</a> [WGI TSU, France]	Accepted. We use now the confidence language as appropriate.
19202	11	24	11	24	"very likely" seems weaker than necessary. Why are ice core records not considered more reliable than that? Boron isotope records, which are much less certain than ice core records, have confirmed those values (e.g. Chalk e al. 2017, Hönisch et al. 2009). I would suggest to ascribe greater confidence to this statement. [Baerbel Hoenisch, United States of America]	Accepted. We say "with high confidence"
17350	11	25	11	25	Change to Pre-Industrial for consistency elsewhere in the text and other Chapters [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
22268	11	26	11	26	The observation that CO2 concentrations are unprecedented in the past 2 Ma is reconstructed from proxy signals. As the authors mention ice core records further up (line 19), they raise the impression that all cited data are from ice cores. The authors should mention the used CO2 proxies either here or latest in the following section (lines 48-53). Also it is helpful to mention back to which time (800 ka BP?) ice core records exist and from where on indirect proxy signals have to be used. [Gwenaelle GREMION, Canada]	Accepted. We say now we are using proxy data.
44932	11	26	11	27	Here is one example of where it would make sense to refer to CH2 for the details. More than one citation is needed, along with a confidence level, for this key IPCC assessment point (last time CO2 was as high as now). Instead, I suggest referring to CH2 for the details, such as "information from multiple studies discussed in section 2.4.1.1.1 indicate that CO2 concentrations are now... (high confidence)". [Darrell Kaufman, United States of America]	Accepted. We now cite ch2
22270	11	27	11	27	Reference is wrong, probably Martinez-Boti et al., 2015a is right here. [Gwenaelle GREMION, Canada]	Rejected. We don't understand the comment. The references that is suggested to be wrong is the same as suggested in right.
17352	11	32	11	35	Subscripts required for the 2s in the chemical formulae [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
38872	11	32	11	43	The numbers in the chemical terms 'CO2', 'CH4' and 'N2O' should be superscripts in the figure caption. [Emilie Breviere, Sweden]	Editorial
17354	11	35	11	35	Space required between number and unit (800 k) [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
31958	11	40	11	40	after might be better than 'until'. Although this should be decided for all the chapters. [Marie-France Loutre, Switzerland]	Accepted, changed.
22272	11	48	11	52	It should be mentioned which kind of records/proxies have been used here. Further up, air bubbles in ice cores are reported as method for direct observations. For clarity, it should be mentioned, how atmospheric CO2 concentrations are reconstructed beyond the periods covered by ice cores. [Gwenaelle GREMION, Canada]	Accepted. We have included the type of proxy.
6718	11	50	11	50	kyr (Myr) and ka (Ma) are used interchangeable throughout the chapter. I recommend changing to ka (Ma) throughout the chapter. [Andrew MacDougall, Canada]	Accepted to use only one units only. Rejected, to use Ka. For the intended audience, particularly for the Abstract, years is something that a broader audience will be more familiar with, not annum.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
22274	11	50	11	53	The data in Figure 5.2 doesn't seem to match the statement, "increased from ~800 to 2200 ppm" in as little as 4-20Kyr which appears to be trying to reference the Palaeocene-Eocene Thermal Maximum (PETM). The axis on Figure 5.2 has a maximum value of 1,000 ppm. In Foster et al. 2017 (cited in Figure 2) the proxy based CO2 ppm concentrations are only upwards of 2,000 ppm greater than ~150 million years ago, far before the PETM (see Figure 4 in Foster et al. 2017). Citation: Foster, G. L., Royer, D. L., & Lunt, D. J. (2017). Future climate forcing potentially without precedent in the last 420 million years. Nature Communications, 8, 14845. <a href="https://doi.org/10.1038/ncomms14845">https://doi.org/10.1038/ncomms14845</a> [Gwenaelle GREMION, Canada]	Accepted. We have corrected the text and included citation.
51490	11	51	11	51	CO2 increase not visible in Fig 2 [Christian Beer, Germany]	Rejected. Fig. 2 does show a strong increase at 55.8Myr.
49006	11	51	11	53	Maybe an inconsistency here: It states that atmospheric CO2 concentration increased up to about 2200 ppm related to PETM, but neither the three suggested references nor Figure 5.2 explicitly indicate this number. [Minchao Wu, Sweden]	Rejected. Figure 3 of Gutjahr shows the data. Accepted: We have now removed the reference to Figure 5.1 which is not relevant.
36234	11	51			If you specify a range of years, you don't need 'as little as'. I suggest just deleting 'as little as', unless the increase could have occurred over a timescale longer than 20 Kyr, in which case delete 'as little as' and replace '20 Kyr' with the assessed upper end of the range of timescales. [Nathan Gillett, Canada]	Accepted.
47328	11	51			this line appears to contradict figure 5.2? Which only shows CO2 going up to about 800, but not 2200? [chris jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Figure reference removed.
22276	11	55	11	56	Add a baseline value, i.e. what was the original ocean pH before it was decreased by 0.15-0.30 units (in order to improve comparability) [Gwenaelle GREMION, Canada]	Accepted. Added suggestion.
22280	12	1	12	1	I think it might be over-reaching to state, "deleterious consequences for shelf and pelagic marine ecosystems". In cited reference McInerney and Wing (2011), there is a marked extinction of benthic foraminifera, but Ostracodes ranges from decreased diversity and abundance to little or no change, with other organisms seeing shifts in geographic ranges and evolutionary change. Furthermore, a recent paper by Ivany et al. 2018, suggests little lasting impact of the Paleocene-Eocene Thermal Maximum on shallow marine molluscan faunas. Perhaps the stating shifts in ecosystems occurred in conjunction could be more reflective? Citations: (1) Ivany, L. C., Pietsch, C., Handley, J. C., Lockwood, R., Allmon, W. D., & Sessa, J. A. (2018). Little lasting impact of the Paleocene-Eocene Thermal Maximum on shallow marine molluscan faunas. Science Advances, 4(9), eaat5528. <a href="https://doi.org/10.1126/sciadv.aat5528">https://doi.org/10.1126/sciadv.aat5528</a> ; (2) McInerney, F. A., & Wing, S. L. (2011). The Paleocene-Eocene Thermal Maximum: A Perturbation of Carbon Cycle, Climate, and Biosphere with Implications for the Future. Annual Review of Earth and Planetary Sciences, 39(1), 489–516. <a href="https://doi.org/10.1146/annurev-earth-040610-133431">https://doi.org/10.1146/annurev-earth-040610-133431</a> [Gwenaelle GREMION, Canada]	Accepted. And removed deleterious.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
22278	12	1	12	4	The PETM has been highlighted as a well-studied interval that can be used to understand the rapid rate of change in current CO2 emissions. However, the following commentary highlights only changes in the ocean-atmosphere system. Perhaps including a line of discussion regarding effects on terrestrial ecosystems would be helpful (changes in regional ice-sheet extent, etc) [Gwenaëlle GREMION, Canada]	Accepted. Terrestrial included now.
22282	12	3	12	4	The way this is currently written, it seems as though ocean deoxygenation is being tied to the absorbance of CO2 by the ocean (ocean acidification) rather than the rate of CO2 emissions. A suggested re-write: "These observations highlight that the rate of CO2 emissions is crucial in determining the severity of adverse consequences to ocean systems such as ocean acidification (caused by absorption of CO2) and de-oxygenation (associated with warming ocean temperatures)." The mechanisms for ocean acidification and de-oxygenation may not even need to be outlined here since they are outlined in lines 35-43 of pg 10, and detailed in Section 5.3. [Gwenaëlle GREMION, Canada]	Accepted - text revised
22284	12	6			Add details on how "medium confidence" was concluded (e.g., "high agreement and limited evidence", or "low agreement and robust evidence") [Gwenaëlle GREMION, Canada]	Accepted. Include suggestion.
22286	12	7	12	8	Change "by at least a factor of 10 higher" to "by approximately a factor of 10 higher". Reason: According to (Zeebe et al., 2016): "...the maximum sustained PETM carbon release rate was therefore 0.6–1.1 Pg C yr <sup>-1</sup> . Given currently available palaeorecords, we conclude that the present anthropogenic carbon release rate (~10 Pg C yr <sup>-1</sup> ) is unprecedented during the Cenozoic (past 66 Myr)". Thus, the maximum rate was by a factor of 9 to 17 higher. [Gwenaëlle GREMION, Canada]	Rejected. The authors show that the maximum sustained release is 1.1PgC/yr, and current emissions are 11.6GtC, year 2018, that makes it about 10 time more as already in the text.
31960	12	11	12	11	A gradual decline'. I assume that it is the trend (over which variability is superimposed) [Marie-France Loutre, Switzerland]	Accepted. No action required.
19204	12	12	12	12	I would suggest to remove all references but Foster et al. 2017 here. Why select only boron isotope studies, and the Gutjahr study refers only to the PETM? There are many other proxies that reflect this image as well, and they are compiled in Foster et al. 2017. There is too much focus on boron isotope studies in this report [Baerbel Hoenisch, United States of America]	Accepted. Removed Anagnostou.
22288	12	12	12	12	I could not find these numbers in the cited literature. [Gwenaëlle GREMION, Canada]	Accepted. Correct. The value was determined using a linear regression through the data published in the 3 referenced sources, and is now explicitly mentioned in the text.
17356	12	13	12	13	Capital C for century (for consistency elsewhere in Chapter) [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
47244	12	15	15	17	It is not clear in the sentence that a lowering of CO2 was conducive in enabling the development of ice-sheets in Antarctica - make this more explicit (i.e. to highlight the importance of lower levels of CO2 in the atmosphere necessary for development of ice-sheets) [Katrina Nilsson-Kerr, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Added suggestion.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
22290	12	16	12	16	In DeConto et al., 2008 as well as other literature (e.g. Zhang et al., 2013b), 750 ppm is given as the critical threshold for Antarctic glaciation. Why making it 750 to 1000 ppm here? [Gwenaëlle GREMION, Canada]	Accepted. Amended in text.
51542	12	19	12	20	I cannot see this range of levels in Fig 2 [Christian Beer, Germany]	Accepted. Figure updated.
13690	12	19	12	22	The mid- to late Pliocene estimates of CO <sub>2</sub> (Martinez-Boti et al. 2015a) are based on 11B from the foraminifera species <i>G. ruber</i> . It has now been shown, that CO <sub>2</sub> estimates based on a different species (here: <i>T. sacculifer</i> ) from the same sediment core lead to on average 90 ppm smaller values in atmospheric CO <sub>2</sub> (Dyez et al. 2018). I therefore suggest to revise the sentence accordingly, e.g. "The most recent interval characterised by atmospheric CO <sub>2</sub> level higher than during the last 800,000 years was the mid- to late Pliocene (3-3.3 Myr). CO <sub>2</sub> might have been as high as 400-450 ppm (Martinez-Boti et al. 2015a), but new evidence suggest that results might be species-specific and if based on proxies from a different foraminifera species might have been ~90 ppm lower (Dyez et al., 2018)". Full reference: Dyez, K. A., Hönisch, B., & Schmidt, G. A. (2018). Early Pleistocene obliquity-scale pCO <sub>2</sub> variability at ~1.5 million years ago. <i>Paleoceanography and Paleoclimatology</i> , 33, 1270–1291. <a href="https://doi.org/10.1029/2018PA003349">https://doi.org/10.1029/2018PA003349</a> [Peter Köhler, Germany]	Accepted. Caveat has been highlighted in the text.
19206	12	20	12	20	please note that these high CO <sub>2</sub> levels may be an artifact of the foraminifer species used, see Dyez et al. 2018 [Baerbel Hoenisch, United States of America]	Noted. Caveat has been highlighted in the text
22292	12	20	12	20	Two open brackets, only one closed, in general there is an inconsistency in the text on how to deal with bracketed numbers followed by bracketed references, see also page 12, lines 29-30. [Gwenaëlle GREMION, Canada]	Accepted - text revised
15000	12	20	12	21	The inclusion of 40 ppm Myr <sup>-1</sup> figure here is unclear: did CO <sub>2</sub> decline at this rate for the rest of the Pliocene and Pleistocene or just for the interval 3.0-2.7 Ma? The CO <sub>2</sub> proxy record is not continuous so it is important to be clear about the timing where data are available. [Erin McClymont, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The pCO <sub>2</sub> record is indeed discontinuous for the period not (yet) covered by ice core pCO <sub>2</sub> reconstructions (i.e. pre 800 kyr). The sentence was amended to clarify this specific aspect
6720	12	24	12	24	There is no year zero in the Gregorian Calendar. 1 BCE is directly followed by 1 CE. [Andrew MacDougall, Canada]	Accepted, changed.
17358	12	24	12	24	Delete 'of' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised



Comment ID	From Page	From Line	To Page	To Line	Comment	Response
13658	12	26	12	27	Ice core CO2 data in the deep part of EPICA Dome C have been revised in Bereiter, B., S. Eggleston, J. Schmitt, C. Nehrass-Ahles, T. F. Stocker, H. Fischer, S. Kipfstuhl, and J. Chappellaz (2015), Revision of the EPICA Dome C CO2 record from 800 to 600 kyr before present, Geophys. Res. Lett., 42, 542–549, doi:10.1002/2014GL061957. Due to the content of this paper glacial minima in CO2 vary between 170 and 200 ppm, and interglacial maxima between 280-300 ppm for the last 400 kyr and between 240 and 260 between 400-800 kyr. The sentence starting with "Concentrations" should be changed, my suggestion: "Concentrations of CO2 during that time period oscillated cyclically between 170-200 ppm during glacial minima and 280-300 ppm (the last 400,000 years) or 240-260 ppm (800,000-400,000 BCE) during interglacial maxima (Bereiter et al., 2015)." [Peter Köhler, Germany]	Noted.
17360	12	29	12	29	Change 'twentieth century' to '20th Century' for consistency [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
13354	12	31	12	33	This sentence is in contrast with P.11,L.23-27, which states that the current GHG concentrations are not unprecedented, but have been this high 800,000 years ago (2mio years ago for CO2). Hence, only the rates seem to be unprecedented. [Lydia Keppler, Germany]	Accepted. Clarified in new sentence.
36236	12	31	12	33	Specify the period over which the present concentration and growth rates of the GHGs are unprecedented. The previous paragraph discusses a period when CO2 concentration was comparable to present. [Nathan Gillett, Canada]	Accepted. Clarified in new sentence.
9646	12	33	12	33	For clarity, I suggest changing "are unprecedented." to "are unprecedented relative to the past 800 kyr." [Brian Magi, United States of America]	Accepted - text revised
22294	12	35	12	35	"For the period with the highest resolution..." which one is this? 0-1900? I recommend to re-write the sentence: "For the last two millenia, the highest resolution of paleo and atmospheric records exists. Data show that growth rates..." [Gwenaelle GREMION, Canada]	Accepted. Clarified in new sentence.
17362	12	35	12	35	Change paleo to palaeo [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
46660	12	47	12	47	Is it possible to add ocean pH to this plot? The ongoing deviation from the geologic past is striking for this variable. [WGI TSU, France]	Noted and accepted. The pH reconstructions is now part of this plot
17364	12	48	12	48	Space required between number and unit (800 k) [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - No longer applicable
19208	12	53	14	7	please also consider Pavia et al. 2019: global warming feedback on subtropical gyres and oxygen minimum zones [Baerbel Hoenisch, United States of America]	Accepted. The paper the referee is referring to (i.e. Shallow particulate organic carbon regeneration in the South Pacific Ocean, PNAS) has been taken into consideration in the revised draft.
56624	12	53			Section 5.1.2 - As a section on introduction and paleo context, I'd say a single sentence on nitrogen and phosphorus limitation and a pointer to the later discussion on that would be good. [Malte Meinshausen, Australia]	Accepted. Sentence added as suggested.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
17366	13	2	13	2	No capital c for Climate [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
38874	13	2	13	2	there should be no spaces around the '-' in Carbon-climate and there should be no capital C at 'climate'. [Emilie Breviere, Sweden]	Taken into account
22296	13	2	13	5	The thought process in this sentence is quite difficult to follow. For clarity, I would suggest it being broken up into 3 sentences: 1 defining carbon feedback, 1 defining climate feedback, 1 linking how the interaction between the two impacts the fraction of anthropogenic emissions remaining in the atmosphere. [Gwenaelle GREMION, Canada]	Accepted. Rewritten.
46852	13	3	13	3	warming and wind speed are not the only climate drivers controlling potential climate-carbon feedbacks. Hydrological variable such as soil moisture, precipitation may also plays a role over land; ocean stratification (combination of warming and E-P) might impact ocean C uptake. [Roland Séférian, France]	Noted: included the hydrological cycle ... and warming, changes in the hydrological cycle and wind stress ....
22298	13	8	13	11	I think erosion and/or organic carbon burial should be included in this list under slower processes. It's been highlighted as an important mechanism to help sequester released carbon (e.g. see Gutjahr et al. 2017), and there are various regions in the Arctic that are now undergoing enhanced erosion with the net effects of this still in question (e.g. Kokelj et al. 2017). Citation: (a) Gutjahr, M., Ridgwell, A., Sexton, P. F., Anagnostou, E., Pearson, P. N., Pälike, H., ... Foster, G. L. (2017). Very large release of mostly volcanic carbon during the Palaeocene–Eocene Thermal Maximum. <i>Nature</i> , 548(7669), 573–577. <a href="https://doi.org/10.1038/nature23646">https://doi.org/10.1038/nature23646</a> ; (b) Kokelj, S. V., Lantz, T. C., Tunnicliffe, J., Segal, R., & Lacelle, D. (2017). Climate-driven thaw of permafrost preserved glacial landscapes, northwestern Canada. <i>Geology</i> , 45(4), 371–374. <a href="https://doi.org/10.1130/G38626.1">https://doi.org/10.1130/G38626.1</a> [Gwenaelle GREMION, Canada]	Noted: sentence edited: , ocean circulation as well as vegetation, soil, peat permafrost formation and decomposition.
47036	13	8	13	11	5.1.2: What about thawing/freezing of permafrost as a positive/negative feedback? [Sophie von Fromm, Germany]	Noted: see above
51544	13	11	13	11	peat dynamics but also soil C dynamics in general [Christian Beer, Germany]	Noted. We are just providing examples of slow and fast processes, not an extensive list of them.
22300	13	13	13	23	Only one reference given for the entire section. [Gwenaelle GREMION, Canada]	Accepted: (MacDougall, 2016; MacDougall, A.H., 2017: The oceanic origin of path-independent carbon budgets, <i>Scientific Reports</i> , 7, 10373, doi:10.1038/s41598-017-10557-x; Williams et al., 2015).
47330	13	13	13	24	can you be consistent with your fractions? This paragraph says atmosphere fraction both 50% and 45%, then figure 5.3 says 44%. [chris jones, United Kingdom (of Great Britain and Northern Ireland)]	Accept: 44% derived from Le Queré et al., 2018; Table 5.1; is used throughout
51546	13	14	13	14	why not biophysical process on land? [Christian Beer, Germany]	Reject: biogeochemical processes on land are part of the same sentence

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
36242	13	15		16	The text says that the land and ocean have maintained a 'constant mean decadal fraction of the total emissions', but the date range given is for one decade only. Do the authors mean that this fraction has remained constant over the whole historical period, or just over this particular ten year period? [Nathan Gillett, Canada]	Accept: The text is clarified as follows: Notwithstanding the interannual and decadal variability during the historical period (1960 - 2017), biogeochemical and physical processes in both the ocean and land have demonstrated a remarkable capacity to keep up with the growth of CO2 emissions (Le Queré et al., 2018; Section 5.2.1.5; Table 5.1). The partitioning of the total CO2 emissions (fossil fuels & land use change) into the Airborne (44%), Ocean (20%) and Land (30%) fractions in the most recent decadal average (2008 - 2017) was used to depict the carbon - climate feedback processes (Figure 5.3).
26498	13	16	13	16	consider removing the "and" before "with" [Nadine Goris, Norway]	Accepted - text revised
29400	13	16	13	16	"50% of emissions remaining in the atmosphere". This does not include the emissions by land-use change, and therefore the number is different from what is published in the Global Carbon Budget (44% or emissions from fossil-fuels and land-use stay in the atmosphere). This is particularly unclear, as in the same sentence you speak of "total emissions". Could this be handled in the same way as in the global carbon budget (or else at least made clear?) [Judith Hauck, Germany]	Accept: Have standardized on an airborne fraction of 44% based on Le Queré et al., 2018 and Table 5.1
24592	13	16	13	22	The airborne fraction is quoted as 50% at the top of this paragraph and 45% towards the end. [William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Accept: Have standardized on an airborne fraction of 44% based on Le Queré et al., 2018 and Table 5.1
13664	13	16	16	22	line 16: "50% of emissions remaining in the atmosphere". This is the airborne fraction, which is mentioned in line 22 to be close to 45%, citing Table 5.1. My comments: Both numbers at line 16 and 22 need to be the same, since they address the same, so probably change 50% in line 16 to "45%". However, this number is NOT directly given in Table 5.1, there, CO2 budget for different time intervals are given. I therefore suggest also to include here, for which time interval the airborne fraction of 45% is obtained. Also note, that in Figure 5.3 (given as citation for this 50% in line 16) an airborne fraction of 44% is stated (without given on which time window this has been calculated). I suggest to give throughout the same number either 44% or ~45%, and state on which time window this is based on. [Peter Köhler, Germany]	Accept: 44% derived from Le Queré et al., 2018; Table 5.1; is used throughout
17368	13	17	13	17	No capital b for Black [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Changed.
29444	13	18	13	18	in the sentence "impact on the air-sea and air-land CO2 gradient" I think "exchange" is a better term than "gradient" [Rona Thompson, Norway]	Accepted - text revised
17370	13	21	13	21	Change 'Airborne Fraction' to 'airborne fraction' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
38876	13	22	13	22	Parentheses are missing around 'Table 5.1'. [Emilie Breviere, Sweden]	Accepted - text revised

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
24594	13	22	13	24	These "excess heat" fractions should be taken from Chapter 7, which should be referenced. [William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Accept: Reference to Chapter 7 and
51938	13	22	13	24	Given that this is discussed extensively in chapters 7 and 9 and likely out of scope of the present chapter I would suggest removal of these lines and associated figure content and, instead, refer to the substantive assessment of the matter in chapters 7 and 9 if necessary. At a minimum cross-referencing to and ensuring consistency with these is required. [Peter Thorne, Ireland]	Accept: We have cross referenced the magnitudes to Chapter 7: This subsection highlights that in order to understand and assess positive and negative feedbacks in the ocean and the land concentration and climate feedbacks need to be understood together. This integrated assessment is not done in any other chapter.
13558	13	22	13	24	93% of heat going into the ocean was discussed in AR5 too. [Govindasamy Bala, India]	Noted. IPCC reference added.
36244	13	22		24	This discussion on the Earth's heat budget should rely on the assessment in Chapter 7, and just summarise it here if necessary. [Nathan Gillett, Canada]	Accept: we now make a reference to Chapt 7
47816	13	23	13	23	This stat of 93% ocean absorbing heat is repeated in Chapter 7 and 9 but is currently consistent. [WGI TSU, France]	Accept: we refer more clearly to Chapter 7
37730	13	24			In AR5, and in another chapter of the AR6 FOD, the atmospheric and terrestrial warming and ice melting are shown not to be approximately equal. Terrestrial heating and ice melting are each at about the 3% level, and atmospheric energy (mostly thermal and latent - it's not solely warming) is at the 1% level. That 1% level is confirmed by published calculations based on reanalysis data (Simmons et al., 2017; doi: 10.1002/qj.2949). [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Accept: This is corrected as with reference to Chapter 7 and Simmons et al., 2017
6722	13	27	13	27	TCRE was defined in AR5 as the 'Transient Climate Response to cumulative CO2 Emissions'. Not 'Transient Climate Response to cumulative carbon Emissions'. Using the second definition is inconsistent with previous reports and annoys those who study atmospheric methane. [Andrew MacDougall, Canada]	Accepted. Changed
13356	13	28	13	28	It is not a true "linear" relationship, but "near-linear" as outlined in Section 5.5. [Lydia Keppler, Germany]	Accept: the correction was made to be consistent with section 5.5
56626	13	28	13	28	I'd suggest to use a consistent description of the linearity as "quasi-linear" as in line 32 below. Replace "linear relationship" with "quasi-linear relationship". [Malte Meinshausen, Australia]	Accept: the correction was made to be consistent with section 5.5
13668	13	29	13	31	It is stated here that TCRE is linear. However, this is only the case for simulations using full-GCMs. When EMICs are used for 5 trillion tons of carbon emitted in simulations the relation is no longer linear, see Tokarska, K. B.; Gillett, N. P.; Weaver, A. J.; Arora, V. K. & Eby, M. The climate response to five trillion tonnes of carbon Nature Climate Change, 2016, 6, 851-855 [Peter Köhler, Germany]	Accept: This point was brought into the assessment

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
36246	13	29		31	Just because the ocean dominates heat and CO2 uptake, it does not follow that warming will be proportional to cumulative emissions. First, cumulative emissions do not just depend on ocean carbon uptake - land carbon change and change in the carbon content of the atmosphere are also important and both vary over time. Second, while the rate of ocean heat uptake is related to the temperature in an energy balance equation, cumulative emissions include the total ocean carbon anomaly - i.e. emissions are related to cumulative ocean carbon uptake, but temperature is related to the first derivative of cumulative ocean heat uptake - and the first derivative of a function is not in general proportional to the function itself. So just because the ocean is involved in carbon and heat uptake, it doesn't automatically follow that warming is proportional to cumulative emissions. Finally the statement is made without any citation of underlying literature. [Nathan Gillett, Canada]	Accept: This was not very well explained. The intention was not to suggest that there is a simple relationship between the ocean uptake of heat and CO2 and the quasi-linear TCRE. The mechanisms that link the two aspects are not yet well understood. However the assessed literature does suggest that the TCRE is significantly controlled by the physical ocean processes that also govern CO2 concentration and climate negative feedbacks (MacDougall, 2016; MacDougall, A.H., 2017: The oceanic origin of path-independent carbon budgets, Scientific Reports, 7, 10373, doi:10.1038/s41598-017-10557-x). This does not exclude a role for terrestrial carbon uptake (Williams et al., 2015).
47332	13	30			I disagree with this statement that ocean dominates TCRE. It certainly contradicts the figure 5.3, where land has more uptake than ocean (29% vs 22%). Surey the TCRE aspect is that ocean heat uptake roughyl parallels TOTAL (land+ocean) uptake. It doesn't require it to be all ocean carbon uptake. If land uptake was double or zero then TCRE would be strongly affected. So better to say here that ocean and land sinks roughyl respons together to allow TCRE constancy [chris jones, United Kingdom (of Great Britain and Northern Ireland)]	Noted: The contribution by terrestrial carbon uptake to TCRE remains inadequately unconstrained with most theoretical and analytical approaches to examine this suggesting that ocean physics and physical chemistry of CO2 my be the dominant processes. (MacDougall, 2016; MacDougall, A.H., 2017: The oceanic origin of path-independent carbon budgets, Scientific Reports, 7, 10373, doi:10.1038/s41598-017-10557-x; Williams et al., 2015).
6704	13	31	13	31	Also cite 'MacDougall, A.H., 2017: The oceanic origin of path-independent carbon budgets, Scientific Reports, 7, 10373, doi:10.1038/s41598-017-10557-x' [Andrew MacDougall, Canada]	Noted: reference added
17372	13	41	13	41	No capital r for Red [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
17374	13	49	13	49	No capital p for Purple [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
17376	13	50	13	50	No capital b for Brown [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
32720	14	1	14	1	With respect to the word "predicted"--the tradition has been to say "projected" as the calculations are conditional on the inputs. [Michael MacCracken, United States of America]	Accepted - text revised
38164	14	3	14	3	Figure 5.1.3 --> Figure 5.3 [Hiroaki Kondo, Japan]	Accepted - text revised
7488	14	3	14	3	Suggest "among other things" instead of "inter alia". This is a style choice, but the latin doesn't save much space and is less understandable. [Rose Abramoff, France]	Accepted - text revised

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
9648	14	3	14	5	For clarity, I suggest changing "Examples include inter alia permafrost thawing on land and the combined impacts of warming on solubility and decreased buffering capacity in the ocean weakening the terrestrial and ocean carbon sinks" to "Examples include, inter alia, permafrost thawing on land (weakening the terrestrial carbon sink), and the combined impacts of decreased solubility of carbon dioxide in warmer waters and decreased buffering capacity of the ocean overall (weakening the ocean carbon sinks)" [Brian Magi, United States of America]	Accepted - text revised [also including the comment 7488]
56320	14	3			I think "(purple arrows in Figure 5.1.3)" is intended to read "(purple arrows in Figure 5.3)" [Steven Neshyba, United States of America]	Accepted - text revised
22302	14	5	14	7	This sentence is rather introductory and belongs before section 5.1.1. [Gwenaelle GREMION, Canada]	Accepted. Removed.
27968	14	10	14	10	AR5 mentions about GHG drivers in the paleo climate (what causes the change in these GHGs), however this is not mentioned in AR6. CO2 drivers, like iron fertilisation are not mentioned, which was particularly uncertain in AR5, it would be beneficial to know if there is any research which has updated knowledge in this respect (see Figure 6.5 AR5). [roderik van de wal, Netherlands]	Accepted. Good point. CO2 drivers are generally discussed in CH02, but adding a few sentences about Fe-fertilization in this section appears useful.
17378	14	10	14	10	Change paleo to palaeo [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
44934	14	10	17	22	Section 5.1.3 is a very nice overview of a variety of paleoclimate topics, but I don't think it achieves the stated goal to "help appraise sensitivities and point toward potentially dominant mechanisms of change." I see the need for an overview of long-term changes in carbon cycling, but I think that the current version attempts to cover too much, and in doing so, some of the major points are buried. I suggest trimming this section to a just a high-level introduction, and integrating the most important new (since AR5) information into other sections of CH5 where they can be covered in more depth and alongside modern-process studies. This would also help achieve the goal of AR6 to weave paleo information throughout the report. As one example, the text on abrupt releases of GHGs would be more impactful if it was combined with the section on abrupt changes and tipping points (5.4.8). [Darrell Kaufman, United States of America]	Fair points. The section has been streamlined to focus more on societally-relevant topics. Tipping points are a delicate aspect in the geological record, as timescales are often too unprecise to robustly characterize abrupt changes. A recent, community-wide review effort assessing the feedbacks/consequences associated with 1.5-2 degree warming in the past relative to today does not report any major, irreversible tipping point to be faced (Fischer et al., 18, NGeo).
44936	14	10	17	22	There are no assessment statements using confidence levels in Section 5.1.3, yet the section contains key points about long-term changes in the carbon cycle. Instead of a textbook-style review, I suggest that just a few of the most important topics be selected and featured more prominently by expanding on the current state of knowledge and developing a summary statement with a formal assessment. I think this could help promote the paleo perspective within and beyond the chapter. [Darrell Kaufman, United States of America]	Accepted

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
44944	14	10	17	22	I suggest that section 5.1.3 begin by reviewing the primary conclusions from AR5 and SROCC that relate to the paleo carbon cycle. Any relevant assessments from the previous reports should be explicitly highlighted. The section should then present a compressive account of new literature related to these key points and consider whether they support or refute the previous assessment. I think this could help promote the paleo perspective within and beyond the chapter. [Darrell Kaufman, United States of America]	Accepted. Good point.
36248	14	10	17	22	Note that 2.2.4.1 discusses paleo-observations of the concentrations of CO2, CH4 and N2O, which is similar to the topic of this section (5.1.3). This section mainly focusses on paleo constraints on feedbacks and what the observations tell us about processes, which is as it should be - my only suggestion would be to include more cross-references to 2.2.4.1 for discussion of the obs themselves, and that the intro to the section makes clear the relationship between this section and 2.2.4.1. [Nathan Gillett, Canada]	Noted
17380	14	12	14	12	Change paleo to palaeo [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
32722	14	12	14	15	What is really important to say here in order to pre-empt the doubters (who dismiss human-induced climate change by saying that climate change has occurred in the past and so is just normal) is to very strongly make the point that large changes in the climate in the past were not random occurrences, but were associated with causal factors (continental drift and resulting collisions and uplifts, changes in shapes of oceans, orbital variations, changes in atmospheric composition, periods of strong volcanic activity, asteroid impacts, etc.) and that the climate changes that resulted were related to the intensity of the effect on the Earth's energy balance, including the distribution and redistribution of enery sources and sinks over the Earth. And then to make the point that what paleoclimatic considerations can to is to put the human GHG-related influences in this context, thus creating a rough quantitative framework for estimating and characterizing the effects of the rising GHG concentrations on the present and future climate. Basically, make the point that what causes concern is that the climates of the past have been so different, providing a clear indication that the climate can change--and that what scientific research has shown is that the climate changes are not just random fluctuations, but are forced responses of the system (and with forcings comparable in the past to the human-induced forcing of today). [Michael MacCracken, United States of America]	Noted
22304	14	17	14	20	As in this section also results from non-ice core records are reported, other possibilities of past CO2 reconstruction should be mentioned as well. In general, the reader gets the impression that all reported records stem from ice cores. [Gwenaelle GREMION, Canada]	Noted

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
36250	14	18	14	20	By itself, higher GHG concentrations during warm periods could be interpreted as evidence that increased GHGs cause warming. More evidence needs to be considered to conclude that the increase in the GHGs is caused by the warming. Also, if this is indeed the conclusion, then it might be worth spelling out that this corresponds to a positive feedback on climate change. [Nathan Gillett, Canada]	Noted
17382	14	22	14	22	Change to Pre-Industrial for consistency elsewhere in the text and other Chapters [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - The same spelling as in the IPCC SR1.5 / SROCC has been applied (pre-industrial has been adopted throughout the chapter for consistency)
22306	14	22	14	29	I think modern evidence of export of methane from ice-sheet beds (Lamarche-Gagnon et al. 2019) suggests that exports of large reserves of methane (from biologically active methanogenic wetlands beneath ice-sheets) could also have been a major pre-industrial source of CH4. Citation: Lamarche-Gagnon, G., Wadham, J. L., Lollar, B. S., Arndt, S., Fietzek, P., Beaton, A. D., ... Stibal, M. (2019). Greenland melt drives continuous export of methane from the ice-sheet bed. <i>Nature</i> , 565(7737), 73. <a href="https://doi.org/10.1038/s41586-018-0800-0">https://doi.org/10.1038/s41586-018-0800-0</a> [Gwenaelle GREMION, Canada]	Noted
17384	14	31	14	31	Change to Pre-Industrial for consistency elsewhere in the text and other Chapters [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - The same spelling as in the IPCC SR1.5 / SROCC has been applied (pre-industrial has been adopted throughout the chapter for consistency)
22308	14	34	14	35	This is right, but it may be worth mentioning that terrestrial sources dominate on shorter time scales (the same reference). [Gwenaelle GREMION, Canada]	Accepted
41058	14	35	14	37	Fossil fuels are listed last in this statement on anthropogenic methane sources, even though some studies put them as the largest source, and almost all studies put fossil fuels ahead of rice and waste. [Robert Howarth, United States of America]	Noted
13670	14	40	14	44	This discussion of stable carbon isotope data for the transition from the last ice age to Holocene misses one fundamental aspect, which is a change in ocean circulation, or more detailed (supported by data) "a reduction on oceanic residence time of CO2". I therefore suggest to change the sentence to (bold is new): "Carbon isotope measurement on CO2 covering the transition from the last ice age to the Holocene highlight that the reconstructed CO2 rise was primarily related to CO2 outgassing from the ocean subsurface, likely due to a reduction in oceanic residence time of CO2 (Skinner et al 2017), a weakening of the biological carbon pump and rising ocean temperature and to a lesser degree by carbon sources on land (Bauska et al., 2016; Galbraith and Jaccard, 2015; Schmitt et al., 2012)." New reference: Skinner, L. C.; Primeau, F.; Freeman, E.; de la Fuente, M.; Goodwin, P. A.; Gottschalk, J.; Huang, E.; McCave, I. N.; Noble, T. L. & Scrivner, A. E. 2017. Radiocarbon constraints on the glacial ocean circulation and its impact on atmospheric CO2. <i>Nature Communications</i> , 8, 16010, DOI: 10.1038/ncomms16010. [Peter Köhler, Germany]	Accepted



Comment ID	From Page	From Line	To Page	To Line	Comment	Response
47720	14	42	14	42	Please check the use of this IPCC uncertainty language term. Are you able to provide a traceable account to assigning this uncertainty statement? Note that likelihood statements are quantified terms - phrases like likely and very likely have quantifiable probabilities associated with them. Please check it has been used correctly here. Please refer to the IPCC guidance note on uncertainty: <a href="https://wg1.ipcc.ch/SR/documents/ar5_uncertainty-guidance-note.pdf">https://wg1.ipcc.ch/SR/documents/ar5_uncertainty-guidance-note.pdf</a> [WGI TSU, France]	Accepted
16152	14	42			"outgassing from the ocean subsurface". This is an unfamiliar expression, because CO2 always outgasses from the sea surface. "outgassing from the ocean" ? [AKIHIKO MURATA, Japan]	by "outgassing from the ocean subsurface", we intend to say "net CO2 transfer from the ocean interior to the atmosphere"
22310	14	42			Regards the use of confidence language: "likely" is (probably) unintentionally used in a context which could be interpreted as the deliberate use of uncertainty language. Thus, I propose the use of a synonym to avoid any confusion. [Gwenaelle GREMION, Canada]	Accepted. Text has been revised.
22312	14	46	14	46	On wich time scale? Is this over the entire last glacial cycle? Please clarify. [Gwenaelle GREMION, Canada]	Accepted. Yes, the relative contribution of these two gases to the total greenhouse gas forcing remains approximately constant over the time interval of interest. Sentence has been amended to clarify this specific aspect
41774	14	46	14	46	It would be more clear if you specify already here that the radiative forcing is computed across the last glacial cycle [Marc Aubinet, Belgium]	Accepted
36252	14	46			The meaning of 'natural global radiative forcing' isn't clear at first, because radiative forcing is usually defined relative to a preindustrial baseline. Replace with 'change in radiative forcing across the last glacial cycle'. [Nathan Gillett, Canada]	Accepted
22314	14	48	14	49	Schilt et al., 2014 is only about the last deglaciation, probably Schilt et al., 2010b is the better reference here. [Gwenaelle GREMION, Canada]	Accepted.
46854	14	53	14	53	I suggest to also include a section on declining CO2 period (such as EOT) because of it relevance for climate reversibility and other CDR topics (see for example, Lear et al., 2008; Pagani et al., 2005; Pearson et al., 2009) [Roland Séférian, France]	Interesting point. This aspect should however, if anything, be placed in the section dedicated to CDR.
51940	14	53	15	24	Chapter 2 also assesses both CO2 and temperature over this period and work is required to ensure consistency. The change is explicitly given to chapter 2 so that chapter must cover this. Does the same apply to chapter 5 or is there here an opportunity to simply cross-reference for the observed component and process instead here on the process aspects? [Peter Thorne, Ireland]	This issue has now been fixed. CH02 specifically discusses temporal changes in CO2 concentrations, while CH05 focuses on growth rates and feedbacks. Both aspects have been properly cross-referenced.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
22316	15	1	15	1	Burke et al., 2018 is the wrong reference. It is no paleo-study and does not mention the mPWP at all. [Gwenaelle GREMION, Canada]	Rejected. We feel that the reference (Burke et al., Pliocene and Eocene provide best analogs for near- future climates, PNAS, 2018) is adequate to support the point made in the sentence. (Pliocene section has now been removed altogether. This particular aspect is not treated in a dedicated cross-chapter box in CH02)
17386	15	1	15	1	Change 'was' top 'were', 'analogue' to 'analogues' and 'offers' to 'offer' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
22318	15	3	15	4	The statement "Many parallels can be drawn between the mPWP and modern observations as well as future climate projections, due to similar continental configuration, land elevation, and ocean bathymetry" should reference Dowsett et al. 2010. Also, quotations should be considered because the wording is almost identical to the first line of the third paragraph in Haywood et al. 2016. Citations: (1) Dowsett, H. et al. The PRISM3D paleoenvironmental reconstruction. Stratigraphy 7, 123–139 (2010). (2) Haywood, A. M., Dowsett, H. J., & Dolan, A. M. (2016). Integrating geological archives and climate models for the mid-Pliocene warm period. Nature Communications, 7, 10646. <a href="https://doi.org/10.1038/ncomms10646">https://doi.org/10.1038/ncomms10646</a> [Gwenaelle GREMION, Canada]	Accepted. Dowsett et al., 2010 has been added to the list of references
47038	15	3	15	7	5.1.3.1.: Is there a reason/explanation why the temperature was higher compared to today, eventhough the CO2 concentration was similiar? [Sophie von Fromm, Germany]	Discussion of this topic has been expanded. Difference relate to feedbacks associated with albedo, ocean circulation and continental configuration. (Pliocene section has now been removed altogether. This particular aspect is not treated in a dedicated cross-chapter box in CH02)
13692	15	5	15	6	Due to new evidence (Dyez et al 2018) change the sentence as follows: "During warm intervals of the Pliocene, atmospheric CO2 concentrations are estimated to have ranged between 300-450 ppm (Dyez et al., 2018; Martínez-Boffí et al., 2015b; Seki et al., 2010)." Full reference: Dyez, K. A., Hönisch, B., & Schmidt, G. A. (2018). Early Pleistocene obliquity-scale pCO2 variability at ~1.5 million years ago. Paleoclimatology and Paleoclimatology, 33, 1270–1291. <a href="https://doi.org/10.1029/2018PA003349">https://doi.org/10.1029/2018PA003349</a> [Peter Köhler, Germany]	Accepted. (Pliocene section has now been removed altogether. This particular aspect is not treated in a dedicated cross-chapter box in CH02)
22320	15	5	15	7	The Pliocene is literally "pre-industrial", which may cause confusion to laymen. Thus, I propose to phrase the latter part differently. [Gwenaelle GREMION, Canada]	Accepted (Pliocene section has now been removed altogether. This particular aspect is not treated in a dedicated cross-chapter box in CH02)
19210	15	6	15	6	again, please also consider Dyez e al. 2018 and species differences [Baerbel Hoenisch, United States of America]	Noted

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
36254	15	6		7	The text here indicates that 'as a result' of high CO2, the global average temperature was 3-4C warming during warm intervals of the Pliocene. Wasn't the warmth also partly caused by other factors, such as orbital forcing, or the configuration of the continents? If not, then doesn't this undermine the argument on pg 14, In 19-20 that variations in GHGs are driven by variations in climate (if the GHGs explain all of the warming, this implies that there was no initial warming to induce the increase in GHGs). [Nathan Gillett, Canada]	Accepted. The generally warmer conditions characterising the Pliocene were dominantly related to higher CO2 levels (e.g. Lunt et al., 2008), but further factors, such as reduced albedo (there were no major ice-sheets in the Northern Hemisphere) and different orbital configuration also played a role in maintaining higher-than-present average global temperatures. Sentence has been amended for clarification. (Pliocene section has now been removed altogether. This particular aspect is not treated in a dedicated cross-chapter box in CH02)
17388	15	7	15	7	Change to Pre-Industrial for consistency elsewhere in the text and other Chapters [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - The same spelling as in the IPCC SR1.5 / SROCC has been applied (pre-industrial has been adopted throughout the chapter for consistency)
17390	15	7	15	8	This is a poor expression, and also poor science (although becoming increasingly common in the media!). Temperatures cannot warm, they increase/decrease or it gets warmer/cooler. I suggest replacing 'warmed' with 'increased' (and consider quantifying) [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Sentence has been amended
47040	15	9	15	9	5.1.3.1.: Acronym (SST) not explained. [Sophie von Fromm, Germany]	Accepted - text revised
22322	15	9			The abbreviation SST has not been used before in the chapter. Thus, I propose to write it out the first time to avoid confusing non-experts. [Gwenaelle GREMION, Canada]	Accepted - text revised
22324	15	10	15	11	A reference is needed for the statement: "supported subarctic North Pacific deep convection and a Pacific meridional overturning circulation". [Gwenaelle GREMION, Canada]	Accepted. Appropriate reference was added to the text (Burls et al., 2017 (Sci. Adv.)) (Pliocene section has now been removed altogether. This particular aspect is not treated in a dedicated cross-chapter box in CH02)
22326	15	17	15	18	Reference missing. [Gwenaelle GREMION, Canada]	Noted
51548	15	17	15	18	I dont understand CO2 radiative forcing relation [Christian Beer, Germany]	Noted
9654	15	17	15	24	This paragraph is generally unclear and should be re-written to very specifically outline why the warming is not accounted for during the mPWP by CO2 alone and yet the ECS is half as strong. Are there leading explanations about this problem? I also am unclear about what the last sentence refers to ("Predictions of ECS...") - is this referring to strictly modern day ECS? if so, perhaps clarifying that a Pliocene-like future is the modern-day climate prediction may help. [Brian Magi, United States of America]	Noted. ESS and ECS have been defined and the context related to the mPWP has been extended. (Pliocene section has now been removed altogether. This particular aspect is not treated in a dedicated cross-chapter box in CH02)

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
55788	15	17	15	24	Chapter 7 updates the ranges of ECS in the FOD: likely 2.5-3.5, very likely 2-5. I am missing an explanation of how ESS in the Pliocene is related to ECS in the "Pliocene-like" future but perhaps I misunderstood. [Christopher Smith, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Paragraph has been revised. (Pliocene section has now been removed altogether. This particular aspect is not treated in a dedicated cross-chapter box in CH02)
7490	15	18	15	18	Typo: "...warmth the CO2 alone." should be "...warmth to CO2 alone." [Rose Abramoff, France]	Accepted - text revised
9650	15	18	15	18	Change "to attribute mPWP warmth the CO2 alone" to "to attribute mPWP warmth to CO2 alone" [Brian Magi, United States of America]	Accepted - text revised
17392	15	18	15	18	Insert 'to' after 'warmth' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
56322	15	18			I think "attribute mPWP warmth the CO2 alone" is intended to read "attribute mPWP warmth to CO2 alone" [Steven Neshyba, United States of America]	Accepted - text revised
36258	15	19			This sentence does not clearly define the ECS or ESS, or make clear the difference between the two. [Nathan Gillett, Canada]	Accepted. Both terms are defined in the revised version of the text. (Pliocene section has now been removed altogether. This particular aspect is not treated in a dedicated cross-chapter box in CH02)
36260	15	19			IPCC AR5 glossary: 'In IPCC reports, equilibrium climate sensitivity refers to the equilibrium change in the annual global mean surface temperature following a doubling of the atmospheric equivalent carbon dioxide concentration'. How does 'Earth Climate Sensitivity' differ from this? And if it does differ, I strongly suggest explaining the difference, and using an acronym other than 'ECS' for this. [Nathan Gillett, Canada]	Accepted. Sentence has been modified to avoid unnecessary confusion
36256	15	21		22	The meaning of 'because ESS was analogue for the two intervals' is not clear. [Nathan Gillett, Canada]	Accepted. Sentence has been modified.
22328	15	22	15	22	Lunt et al., 2010 does not appear in the literature list. [Gwenaelle GREMION, Canada]	Noted. (Pliocene section has now been removed altogether. This particular aspect is not treated in a dedicated cross-chapter box in CH02)
9652	15	22	15	22	This is unclear "ESS was analogue for the two intervals". Perhaps "analogue" should be "analogous"? [Brian Magi, United States of America]	Accepted - text revised
24596	15	22	15	24	This statement on ECS should use values from chapter 7, which should be referenced. [William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Statement has been revised
56324	15	22			I think "ESS was analogue for the two intervals" is intended to read "ESS was analogous for the two intervals" [Steven Neshyba, United States of America]	Accepted - text revised
36262	15	23			This 'currently accepted range' for ECS is different to the assessed likely range in Chapter 7. Cross-reference Chapter 7 and update. [Nathan Gillett, Canada]	Noted

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
27970	15	26	15	26	Repetition between 2 sections: page 16, line 14-16: "The generally gradual increase in atmospheric CO2 from the last ice age into the Holocene was punctuated by three sub-millennial 10–15 ppm increments. These transient CO2 outgassing events have been associated with the release of CO2 previously sequestered in the ocean interior." Page 39, line 28-30: "The increase in atmospheric CO2 was punctuated by three abrupt 10–15 ppm increments spanning a few hundred years, associated with the rapid transfer of CO2 previously sequestered in the ocean interior and permafrost (Bauska et al., 2016; Köhler et al., 2014)." In general, there is a bit of repetition between these two sections (5.1.3.2 Glacial-interglacial changes and 5.3.1.2 Last deglaciation (18-11 kyr ago)). [roderik van de wal, Netherlands]	Noted
47334	15	26			throughout this section please can you be specific which direction through time you mean - sometimes palaeo text and figures go back in time and quote changes from present to past. Sometimes they go forward. This affects the sign of "warming" and "cooling". e.g. p.16, line 14 is a really good example of being clear on this. Can we assume that all the sentences in this section also refer to going forward in time? [chris jones, United Kingdom (of Great Britain and Northern Ireland)]	Noted
17394	15	29	15	29	Change paleo to palaeo [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
43318	15	29		33	Sentence is too long. [Onema Adojoh, United States of America]	Noted
17396	15	33	15	33	Insert 'to' after 'prior' and change 'era' to 'Era' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
22330	15	35	15	54	The text moves from general glacial-interglacial transitions (before line 40) to the last deglaciation without clearly indicating to which time periods the reported numbers or events correspond. In line 39 the authors state "during past ice ages" in general, but all cited literature only refers to the last glacial maximum or deglaciation. [Gwenaelle GREMION, Canada]	Accepted. Sentence has been clarified to avoid confusion
17398	15	36	15	36	Change to Pre-Industrial for consistency elsewhere in the text and other Chapters [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - The same spelling as in the IPCC SR1.5 / SROCC has been applied (pre-industrial has been adopted throughout the chapter for consistency)
9656	15	36	15	37	This introductory paragraph is unclear and unnecessary. I suggest stating the CO2 concentration of 90 ppm and how it compares with preindustrial CO2 concentration in the paragraph starting on line 39. Something like "a generally colder climate state with CO2 concentrations of 90 ppm (about 190 ppm less than preindustrial; Luthi et al 2008) and a weaker hydrological cycle contributed to a substantial decline of..." [Brian Magi, United States of America]	Accepted. Sentence has been clarified
22332	15	39	15	39	The term "ice age" is wrong here. It should be "glacial". An ice age is a period during which one or both poles are glaciated. Hence, we are currently in the Cenozoic ice age. The last ice age was the Karoo ice age 360-260 MaBP whereas the last glaciation ended 11.7 ka BP. This mistake occurs several times in the chapter. [Gwenaelle GREMION, Canada]	Noted. "Ice age" and "glacial" have been used indistinctly throughout the text. Not sure how to which extent this semantic subtlety is accepted in the community.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
51494	15	39	15	45	Soil C dynamics more important on that time scale. [Christian Beer, Germany]	Noted
41776	15	42	15	42	These numbers are not clear. Do they represent the stock values or the difference with present values? What are the present values ? [Marc Aubinet, Belgium]	Accepted. Good point. Sentence has been modified for clarification
13672	15	43	15	43	Reference Jeltsch-Thömmes et al. 2018 needs update (this is a discussion paper), final version in published in 2019: Jeltsch-Thömmes, A., Battaglia, G., Cartapanis, O., Jaccard, S. L., and Joos, F.: Low terrestrial carbon storage at the Last Glacial Maximum: constraints from multi-proxy data, <i>Clim. Past</i> , 15, 849-879, <a href="https://doi.org/10.5194/cp-15-849-2019">https://doi.org/10.5194/cp-15-849-2019</a> , 2019. [Peter Köhler, Germany]	Accepted. Reference has been updated
17400	15	48	15	48	Please give the dates of the Bolling/Allerod warm interval [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted
13674	15	50	16	1	Permafrost destabilization during rapid northern hemisphere mentioned here is based on models only, but there are now sediment data which support these ideas, also pointing towards a contribution from rapid sea level rise flooding the Arctic shelf and therefore releasing carbon from permafrost. I suggest to revise this sentence as follows: "Radiocarbon data from organic biomarkers found in marine sediments have now confirmed the release of old carbon from Arctic permafrost, potentially activated by deglacial meltwater runoff, northern hemisphere warming and through the flooding of the Arctic shelf during melt water pulses, to be partly responsible for the CO2 release during the three rapid CO2 rises across Termination I (Winterfeld et al 2018, Meyer et al. 2019), supporting earlier model-based suggestions (Crichton et al., 2016; Köhler et al., 2014)." New references: Winterfeld M, Mollenhauer G, Dummann W, Köhler P, Lembke-Jene L, Meyer V D, Hefter J, McIntyer C, Wacker L, Kokfelt U, Tiedemann R 2018 Deglacial mobilization of pre- aged terrestrial carbon from degrading permafrost. <i>Nature. Communications.</i> , 9 3666 DOI: 10.1038/s41467-018-06080-w. Meyer, V. D.; Hefter, J.; Köhler, P.; Tiedemann, R.; Gersonde, R.; Wacker, L., Mollenhauer, G. 2019 Permafrost-carbon mobilization in Beringia caused by deglacial meltwater runoff, sea-level rise and warming. <i>Environmental Research Letters</i> , in press, DOI: 10.1088/1748-9326/ab2653 (DOI might need update once the final version is online).  The following sentence on isotopic measurements on CO2 should also be revised in accordance to those new papers. Furthermore, the relation to "geological" sources for carbon is in the connection with CO2 rather uncommon, but geological sources are more discussed when	Accepted. Thank you for highlighting those new papers, which have been overlooked in the FOD. These references are now included and the relevant sentences have been modified.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
47722	15	53	15	53	Please check the use of this IPCC uncertainty language term. Are you able to provide a traceable account to assigning this uncertainty statement? Note that likelihood statements are quantified terms - phrases like likely and very likely have quantifiable probabilities associated with them. Please check it has been used correctly here. Please refer to the IPCC guidance note on uncertainty: <a href="https://wg1.ipcc.ch/SR/documents/ar5_uncertainty-guidance-note.pdf">https://wg1.ipcc.ch/SR/documents/ar5_uncertainty-guidance-note.pdf</a> [WGI TSU, France]	Accepted
15002	15	55	15	55	This is technically the "Mid-Piacenzian warm period" given the revisions to the end of the Pliocene epoch, and it is also more commonly referred to as "3.3-3.0 Ma". [Erin McClymont, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. This comment refers to p. 14 presumably. Fair comment. This being said, we tend to avoid references to specialized jargon. (Pliocene section has now been removed altogether. This particular aspect is not treated in a dedicated cross-chapter box in CH02)
48392	16	3	16	3	Grammar: plural ... "suggests" should be changed to "suggest" [Stephen Parks, United States of America]	Accepted - text revised
56326	16	3	16	12	A recent paper seems to reinforce these conclusions about deep-sea CO2 storage during the last ice age, although with a larger role for iron than this passage suggests. Quoting from the paper: "We conclude that despite important contributions to individual carbon components, circulation and sea ice changes had only a modest and unrobust net effect on glacial ocean carbon storage and atmospheric CO2, whereas temperature and iron were more important than previously thought due to their effects on disequilibrium carbon storage." See Khatiwala et al., Sci. Adv. 2019; 5: eaaw4981 (12 June 2019). [Steven Neshyba, United States of America]	Accepted. The recently published paper has now been considered and referenced. On a side note, this study is quite controversial within the community and time will tell whether the conclusions outlined on the paper will withstand scrutiny
13694	16	5	16	9	Mentioning of data-based estimate of an increased residence time of CO2 in the ocean in glacial times (14C data, Skinner et al 2017) is missing. Therefore, I suggest to revise as: "A combination of increased CO2 solubility associated with generally colder oceanic temperatures, increased oceanic residence time of CO2 caused by weaker overturning (Skinner et al., 2017), altered oceanic alkalinity (Cartapanis et al., 2018; Hoogakker et al., 2018; Yu et al., 2010a); and a generally more efficient biological carbon pump (Galbraith and Jaccard, 2015; Hain et al., 2010; Martinez-Garcia et al., 2014; Ziegler et al., 2013); likely conspired to partition CO2 into the ocean interior (Anderson et al., 2019)". New reference: Skinner, L. C.; Primeau, F.; Freeman, E.; de la Fuente, M.; Goodwin, P. A.; Gottschalk, J.; Huang, E.; McCave, I. N.; Noble, T. L. & Scrivner, A. E. 2017. Radiocarbon constraints on the glacial ocean circulation and its impact on atmospheric CO2. Nature Communications, 8, 16010. [Peter Köhler, Germany]	Accepted. Good point. Skinner et al., 2017 has now been included.
22334	16	9			Regards the use of confidence language: "likely" is (probably) unintentionally used in a context which could be interpreted as the deliberate use of uncertainty language. Thus, I propose the use of a synonym to avoid any confusion. [Gwenaëlle GREMION, Canada]	Noted
17402	16	11	16	11	Italicise 'via' as per line 3 of page 14 [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
22336	16	14	16	14	Here, again the term "ice age" is wrong and should be replaced by "glaciation". [Gwenaelle GREMION, Canada]	Noted
13696	16	14	16	21	<p>There are now also data suggesting substantial contribution from northern hemisphere permafrost thaw to these three rapid CO2 spikes (Winterfeld et al., 2017, Meyer et al., 2019). Please revise sentence, e.g. "These transient CO2 outgassing events (Marcott et al., 2014), have been associated with the release of CO2 from both the ocean interior (Jaccard et al., 2016; Rae et al., 2018; Schmitt et al., 2012; Skinner et al., 2010) and permafrost carbon mobilization (Winterfeld et al., 2017, Meyer et al., 2019)."</p> <p>The sentence "The two later pulses are associated with the rejuvenation of the Atlantic Meridional Overturning Circulation (Marcott et al., 2014)." is in the light of these new data on permafrost carbon release no longer correct, and should be deleted.</p> <p>Full references: Winterfeld M, Mollenhauer G, Dummann W, Köhler P, Lembke-Jene L, Meyer V D, Hefter J, McIntyer C, Wacker L, Kokfelt U, Tiedemann R 2018 Deglacial mobilization of pre-aged terrestrial carbon from degrading permafrost. Nature. Communications,. 9 3666 DOI: 10.1038/s41467-018-06080-w.</p> <p>Meyer, V. D.; Hefter, J.; Köhler, P.; Tiedemann, R.; Gersonde, R.; Wacker, L., Mollenhauer, G. 2019 Permafrost-carbon mobilization in Beringia caused by deglacial meltwater runoff, sea-level rise and warming. Environmental Research Letters, in press DOI: 10.1088/1748-9326/ab2653 (DOI might need update once the final version is online). [Peter Köhler, Germany]</p>	Accepted
17404	16	17	16	17	Italicise 'via' as per line 3 of page 14 [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
13698	16	21	16	24	<p>A new review paper (Gottschalk et al., 2019) on the role of mechanisms of millennial-scale changes and CO2 (with focus in ocean circulation) is in press, which highlights, that results are to a large degree scenario and model-dependent. Please add the following sentence: "However, details in simulated response of the marine carbon cycle and of atmospheric CO2 to changes in ocean circulation changes, mainly in the Atlantic realm, depends to a large degree on the chosen model and the applied scenario (Gottschalk et al., 2019)". Full references: Gottschalk, J.; Battaglia, G.; Fischer, H.; Frölicher, T. L.; Jaccard, S. L.; Jeltsch-Thömmes, A.; Joos, F.; Köhler, P.; Meissner, K. J.; Menviel, L.; Nehrbass-Ahles, C.; Schmitt, J.; Schmittner, A.; Skinner, L. C. &amp; Stocker, T. F. 2019 Mechanisms of millennial-scale atmospheric CO2 change in numerical model simulations. Quaternary Science Reviews, in press, DOI: 10.1016/j.quascirev.2019.05.013 (to be published 15 Aug 2019) [Peter Köhler, Germany]</p>	Accepted
22338	16	22	16	22	Schmittner et al., 2008 is not the right reference here. The paper describes a future climate simulation. Do the authors mean Schmitt et al., 2012? [Gwenaelle GREMION, Canada]	Accepted. Correct. Reference has been revised
29446	16	26	16	26	EMIC appears not to have been previously defined [Rona Thompson, Norway]	Accepted - text revised



Comment ID	From Page	From Line	To Page	To Line	Comment	Response
36264	16	30			Are there observations of variable volcanic outgassing? If the models close the budget and include this, then there must be some constraints on this. [Nathan Gillett, Canada]	Accepted. These values have been determined based on 13C and 14C isotope mass balance calculations. The statement is now properly referenced.
44848	16	31	16	31	The word ""non-glacial part" is unclear. What is this? [Kaoru Kubota, Japan]	Accepted. Poor wording indeed. Sentence has been modified.
48394	16	35	16	47	Overall, the paleoclimate discussion has been fascinating. Many of my questions have been answered. But one thing remains: Why was the Holocene thermal overshoot seen in previous deglaciations so limited? The text mentions a disruptive change in ocean circulation; yet there has been little change in ocean-basin topography since the previous deglaciation. Understanding why the Holocene temperature has been so level and stable could have important implications for the "anthropocene" thermal changes. It deserves a discussion of its own in this context. [Stephen Parks, United States of America]	Rejected (very interesting point, this being said). This aspect is discussed in CH02. CH05 focuses on carbon cycle feedbacks
47042	16	37	16	37	5.1.3.3.: Time span might be not right. Pre-industrial instead of today might be more accurate because since the industrial period the global climate is not that stable anymore. [Sophie von Fromm, Germany]	Accepted. Correct.
32724	16	37	16	38	What "large changes in insolation"? Changes in orbital parameters do not significantly change the annual, global total of incoming solar radiation, which is what the IPCC uses as its paradigm for forcings that would cause climate change. Yes, the orbital elements have caused significant changes in the seasonal and latitudinal distributions of the solar radiation, but this, per the IPCC paradigm relating to global forcing, would not have much of a climatic influence (showing a shortcoming in the IPCC paradigm on forcing). But the statement here seems seriously flawed or at least not properly qualified as total incoming insolation at the top of the atmosphere has not changed much at all. [Michael MacCracken, United States of America]	Accepted. Good point. Statement was modified as suggested
46118	16	37	16	39	You can combine these sentences together to ease reading. [Amy Featherstone, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account
22340	16	37	16	42	References missing for the Holocene climate description. [Gwenaelle GREMION, Canada]	Noted
29388	16	37	16	46	The paragraph starts to define the Holocene as the period 11.7 ka - today, and ends by saying that there was a 20 ppm increase in atm CO2. Clearly, this does not refer to the period until today, but likely to the period 11.7ka until 1750? [Judith Hauck, Germany]	Accepted. Correct. The statement has been amended
15186	16	37	16	55	This section (5.1.3.3 Holocene changes) omits a discussion of Holocene changes in biomass burning, despite its recognized importance in this section's parent section (5.1.3 Paleo trends a feedbacks). The global charcoal database can offer some insight into global biomass burning changes during the Holocene. See: Power, Mitchell James, et al. "Changes in fire regimes since the Last Glacial Maximum: an assessment based on a global synthesis and analysis of charcoal data." Climate dynamics 30.7-8 (2008): 887-907. [Richard Vachula, United States of America]	Accepted. Good point. This aspect has been overlooked and has been included.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
51550	16	37	16	55	I miss a fig for that section. Younger Dryas not important? why is climate stable despite insolation changes? [Christian Beer, Germany]	Rejected. A figure illustrating the YD, which doesn't have a dedicated section, would be superfluous.
43320	16	37		39	Three Holocene warming started during the early 11.7 ka and stop at 6.5 ka. So, the 5 ka should be corrected to 6.5 ka as applicable to the global standard. [Onema Adojoh, United States of America]	Noted
17406	16	38	16	38	Delete 'of time' to remove the tautology [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
17408	16	38	16	39	Change to Pre-Industrial for consistency elsewhere in the text and other Chapters [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - The same spelling as in the IPCC SR1.5 / SROCC has been applied (pre-industrial has been adopted throughout the chapter for consistency)
31282	16	39	16	41	The Early Holocene was not globally warm. These statements should either be revised to make clear that they only refer to northern mid- to high latitudes, or possibly they should be eliminated as they do not necessarily belong in this chapter. [Iain Colin Prentice, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Argument has been removed.
44938	16	39	16	41	We'll need to be sure that this overview matches the more detailed assessment of Holocene temperature in CH2. Also, the Holocene has now been formally subdivided. I think we need to adhere to the current internationally ratified conventions. [Darrell Kaufman, United States of America]	Accepted. Sentence has been amended for the sake of consistency.
17410	16	40	16	40	See previous comment on this. Replace 'coolest' with 'lowest' (and give the lowest temperature identified) [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted
32726	16	41	16	46	I thought the preferred explanation for the indicated cooling was the change caused by precession of the orbit in the time of closest approach to the Sun going from NH summer to NH winter. The change in the AMOC, if that occurred, would have been a response and not a cause. [Michael MacCracken, United States of America]	Noted. However, changes in orbital forcing are relatively small and need to be reinforced by feedbacks internal to the climate system, such as changes in ocean circulation among other factors.
41778	16	42	16	42	Meaning of AMOC should be given [Marc Aubinet, Belgium]	Taken into account
47044	16	42	16	42	5.1.3.3.: Acronym (AMOC) not explained. [Sophie von Fromm, Germany]	Taken into account
22342	16	42			The abbreviation AMOC has not been used before in the chapter. Thus, I propose to write it out the first time to avoid confusing non-experts. [Gwenaelle GREMION, Canada]	Accepted - text revised
31284	16	43	16	46	This is a very partial treatment of the causes of the 20 ppm rise in CO2 during the Holocene. Other candidate mechanisms, not mentioned, include CaCO3 compensation (in reaction to the Early Holocene drawdown of CO2 due to biosphere growth) and changing SSTs. There is a significant literature on this topic that is not mentioned here. [Iain Colin Prentice, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Sentence has been amended for clarification
32728	16	48	10	55	This is not very clearly explained. If there was a higher sink due to peat, why was the CO2 concentration not pulled down--probably because the ocean was adjusting to the warming and emitting CO2 as it came into equilibrium with the new temperatures caused by the orbital changes. [Michael MacCracken, United States of America]	Accepted

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
26672	16	48	16	52	For peat accumulation since LGM see Treat et al. (2019) (Claire C. Treat, Thomas Kleinen, Nils Broothaerts, April S. Dalton, René Dommain, Thomas A. Douglas, Judith Z. Drexler, Sarah A. Finkelstein, Guido Grosse, Geoffrey Hope, Jack Hutchings, Miriam C. Jones, Peter Kuhry, Terri Lacourse, Outi Lähteenoja, Julie Loisel, Bastiaan Notebaert, Richard J. Payne, Dorothy M. Peteet, A. Britta K. Sannel, Jonathan M. Stelling, Jens Strauss, Graeme T. Swindles, Julie Talbot, Charles Tarnocai, Gert Verstraeten, Christopher J. Williams, Zhengyu Xia, Zicheng Yu, Minna Väliranta, Martina Hättstrand, Helena Alexanderson, Victor Brovkin: Widespread global peatland establishment and persistence over the last 130,000 y, Proceedings of the National Academy of Sciences, 2019, 116 (11) 4822-4827; DOI: 10.1073/pnas.1813305116 [Thomas Kleinen, Germany]	Noted
44940	16	49	16	50	I suggest that we move away from using the "HTM" to imply a single time period. For older interglacial periods, we don't have a choice but to be looser about which millennium is which, but for the Holocene, we do know that maximum warmth was time transgressive over millennia. Can this statement be rewritten as, "rates were higher under relatively high temperatures during the early and middle Holocene than for the cooler late Holocene." [Darrell Kaufman, United States of America]	Accepted. Wording has been modified to clarify this specific aspect as recommended.
31286	16	49	16	52	The statement about peat growth keeping CO2 within bounds is speculative and undocumented. [Iain Colin Prentice, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Fair point. Sentence has been removed
43322	16	49		52	Requires sentence reduction [Onema Adojoh, United States of America]	Noted
32224	16	50	16	50	I would recommend citing Loisel et al 2014 The Holocene, instead of Yu et al 2010. The 2014 publication is a newer and better dataset. [David Olefeldt, Canada]	Accepted
56328	16	52			I think "contributing to maintain Holocene atmospheric CO2" is intended to read "contributing to maintaining Holocene atmospheric CO2" [Steven Neshyba, United States of America]	Accepted - text revised
26670	16	54	16	55	Have you considered coral reef formation as C source? Kleinen et al. (2016) point to this as an explanatory factor. (Kleinen, T., Brovkin, V., and Munhoven, G. (2016). Modelled interglacial carbon cycle dynamics during the Holocene, the Eemian and Marine Isotope Stage (MIS) 11. Clim. Past 12, 2145–2160. doi:10.5194/cp-12-2145-2016) [Thomas Kleinen, Germany]	No, we haven't.
17412	17	5	17	5	Change 'a' to 'an' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
29662	17	10	17	10	I think it would be appropriate to insert here a paragraph about the lead-lag analysis of the records of temperature and CO2, somewhat similar to the following: "Thus, the recent studies of the lags between global mean surface air temperature and atmospheric carbon dioxide concentration resulted in understanding that the lagged correlation does not necessarily represent causal relationships included in a climate model (Muryshev et al., 2017): the lags and leads may merely reflect the internal time scales of the climate model that determine the model response to the variety of periodical external forcings." (Muryshev, K. E., Eliseev, A. V., Mokhov, I. I. and Timazhev, A. V.: Lead-lag relationships between global mean temperature and the atmospheric CO2 content in dependence of the type and time scale of the forcing, Glob. Planet. Change, 148, 29–41, doi:10.1016/j.gloplacha.2016.11.005, 2017.) [Georgii Alexandrov, Russian Federation]	Accepted. Good point. A discussion regarding this specific aspect has been added in the revised version of the draft.
17414	17	11	17	11	Change paleo to palaeo [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account
31288	17	11	17	11	It is not clear what is meant by "profound ecological disruptions" (an emotive expression) and how they are supposed to be causally linked to changes in CO2. [Iain Colin Prentice, United Kingdom (of Great Britain and Northern Ireland)]	Fair point. Severe ecological perturbations are directly related to widespread ocean acidification reported for the PETM, a useful comparison in the context of contemporary climate change.
22420	17	11	17	14	The 'multiple recent instances' of the increase in CO2 are between 23000 and 9000 years (Marcott et al., 2014) i.e., upto early Holocene. However, the statement kind of misleads with the sense that the changes in CO2 is more close to the industrial era. [Gwenaelle GREMION, Canada]	Accepted. Sentence has been modified to avoid confusion
17416	17	14	17	14	Change paleo to palaeo [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account
47724	17	14	17	14	Please check the use of this IPCC uncertainty language term. Are you able to provide a traceable account to assigning this uncertainty statement? Note that likelihood statements are quantified terms - phrases like likely and very likely have quantifiable probabilities associated with them. Please check it has been used correctly here. Please refer to the IPCC guidance note on uncertainty: <a href="https://wg1.ipcc.ch/SR/documents/ar5_uncertainty-guidance-note.pdf">https://wg1.ipcc.ch/SR/documents/ar5_uncertainty-guidance-note.pdf</a> [WGI TSU, France]	Accepted
22344	17	14			Regards the use of confidence language: "likely" is (probably) unintentionally used in a context which could be interpreted as the deliberate use of uncertainty language. Thus, I propose the use of a synonym to avoid any confusion. [Gwenaelle GREMION, Canada]	Accepted. Sentence has been modified to avoid confusion

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
27972	17	18	17	21	Page 17, lines 18-21, "A recent observation-based synthesis of the understanding of past intervals with temperatures within the range of projected future warming suggest that there is low risk of crossing a tipping-point in the climate system leading to large, unpredictable changes in the state of the system for warming of no more than 2°C (Fischer et al., 2018)." This sentence is ambiguous. Does it mean that if warming kept to below 2 °C then there will be a low risk of crossing a tipping-point? If so, I think the sentence structure should be reversed so that "for warming of no more than 2°C" comes at the beginning of the statement. [roderik van de wal, Netherlands]	Accepted. Yes, this is what is meant. Sentence has been modified as recommended.
56330	17	18	17	22	It seems to me that the reference cited (Fischer et al, 2018) describes a more impacted climate resulting from 1-2 °C warming, than is represented here, especially in terms of ice sheet and sea level impacts: "A global average warming of 1–2 °C with strong polar amplification has, in the past, been accompanied by significant shifts in climate zones and the spatial distribution of land and ocean ecosystems. Sustained warming at this level has also led to substantial reductions of the Greenland and Antarctic ice sheets, with sea-level increases of at least several metres on millennial timescales." [Steven Neshyba, United States of America]	Certainly correct. However, CH05 focuses on carbon feedbacks. Aspects related to ice sheets and sea level impacts are discussed in CH09.
12828	17	18	17	22	Note that some feedbacks exist between 1.5 and 2C, which could amplify warming that would then bring us closer to these tipping points. Drijfhout S., et al. (2015) Catalogue of abrupt shifts in Intergovernmental Panel on Climate Change climate models, PROC. NAT'L. ACAD. SCI. 112(43):E5777–E5786, E5777 ("Abrupt transitions of regional climate in response to the gradual rise in atmospheric greenhouse gas concentrations are notoriously difficult to foresee. However, such events could be particularly challenging in view of the capacity required for society and ecosystems to adapt to them. We present, to our knowledge, the first systematic screening of the massive climate model ensemble informing the recent Intergovernmental Panel on Climate Change report, and reveal evidence of 37 forced regional abrupt changes in the ocean, sea ice, snow cover, permafrost, and terrestrial biosphere that arise after a certain global temperature increase. Eighteen out of 37 events occur for global warming levels of less than 2°, a threshold sometimes presented as a safe limit."); Lenton T. M., et al. (2008) Tipping elements in the Earth's climate system, PROC. NAT'L. ACAD. SCI. 105(6):1786–1793, 1786 ("In discussions of global change, the term tipping point has been used to describe a variety of phenomena, including the appearance of a positive feedback, reversible phase transitions, phase transitions with hysteresis effects, and bifurcations where the transition is smooth but the future path of the system depends on the noise at a critical point. We offer a formal definition, introducing the term "tipping element" to describe subsystems of the Earth system that are at least subcontinental in scale and can be switched—under certain circumstances— into a qualitatively different state by small perturbations. The tipping point is the corresponding critical point—in forcing and a feature of the system—at which the future state of the system is qualitatively altered."). [Durwood	Noted

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
12830	17	18	17	22	<p>Because the Arctic is warming at twice the rate as the global average and because Arctic sea ice is susceptible to this increased temperature, Arctic sea ice will be reduced—potentially becoming ice-free for the first time in a matter of decades—that would contribute to a positive feedback within the climate system that can further amplify warming, especially in the region where it can impact permafrost thaw and melting over the Greenland ice sheet. All of these feedbacks can further amplify warming that further risks overshooting the goal of staying well below 2C. Steffen W., et al. (2018) Trajectories of the Earth System in the Anthropocene, PROC. NAT'L. ACAD. SCI. 115(33):8252–8259; 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; Arctic Monitoring and Assessment Programme (AMAP) (2017) SNOW, WATER, ICE, AND PERMAFROST IN THE ARCTIC: SUMMARY FOR POLICYMAKERS; 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; Xu Y., et al. (2018) Global warming will happen faster than we think, NATURE, Comment 564:30–32; Wilkerson J., et al. (2019) Permafrost nitrous oxide emissions observed on a landscape scale using the airborne eddy-covariance method, ATMOS. CHEM. PHYS. 19:4257–4268. [Durwood Zaelke, United States of America]</p>	Noted

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42336	17	18	17	22	Note that some feedbacks exist between 1.5 and 2C, which could amplify warming that would then bring us closer to these tipping points. Drijfhout S., et al. (2015) Catalogue of abrupt shifts in Intergovernmental Panel on Climate Change climate models, PROC. NAT'L. ACAD. SCI. 112(43):E5777–E5786, E5777 (“Abrupt transitions of regional climate in response to the gradual rise in atmospheric greenhouse gas concentrations are notoriously difficult to foresee. However, such events could be particularly challenging in view of the capacity required for society and ecosystems to adapt to them. We present, to our knowledge, the first systematic screening of the massive climate model ensemble informing the recent Intergovernmental Panel on Climate Change report, and reveal evidence of 37 forced regional abrupt changes in the ocean, sea ice, snow cover, permafrost, and terrestrial biosphere that arise after a certain global temperature increase. Eighteen out of 37 events occur for global warming levels of less than 2°, a threshold sometimes presented as a safe limit.”); Lenton T. M., et al. (2008) Tipping elements in the Earth’s climate system, PROC. NAT'L. ACAD. SCI. 105(6):1786–1793, 1786 (“In discussions of global change, the term tipping point has been used to describe a variety of phenomena, including the appearance of a positive feedback, reversible phase transitions, phase transitions with hysteresis effects, and bifurcations where the transition is smooth but the future path of the system depends on the noise at a critical point. We offer a formal definition, introducing the term “tipping element” to describe subsystems of the Earth system that are at least subcontinental in scale and can be switched—under certain circumstances— into a qualitatively different state by small perturbations. The tipping point is the corresponding critical point—in forcing and a feature of the system—at which the future state of the system is qualitatively altered.”). [Gabrielle	Noted (identical comment ID 12828)

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
12662	17	18	17	22	Note that some feedbacks exist between 1.5 and 2C, which could amplify warming that would then bring us closer to these tipping points. Drijfhout S., et al. (2015) Catalogue of abrupt shifts in Intergovernmental Panel on Climate Change climate models, PROC. NAT'L. ACAD. SCI. 112(43):E5777–E5786, E5777 (“Abrupt transitions of regional climate in response to the gradual rise in atmospheric greenhouse gas concentrations are notoriously difficult to foresee. However, such events could be particularly challenging in view of the capacity required for society and ecosystems to adapt to them. We present, to our knowledge, the first systematic screening of the massive climate model ensemble informing the recent Intergovernmental Panel on Climate Change report, and reveal evidence of 37 forced regional abrupt changes in the ocean, sea ice, snow cover, permafrost, and terrestrial biosphere that arise after a certain global temperature increase. Eighteen out of 37 events occur for global warming levels of less than 2°, a threshold sometimes presented as a safe limit.”); Lenton T. M., et al. (2008) Tipping elements in the Earth’s climate system, PROC. NAT'L. ACAD. SCI. 105(6):1786–1793, 1786 (“In discussions of global change, the term tipping point has been used to describe a variety of phenomena, including the appearance of a positive feedback, reversible phase transitions, phase transitions with hysteresis effects, and bifurcations where the transition is smooth but the future path of the system depends on the noise at a critical point. We offer a formal definition, introducing the term “tipping element” to describe subsystems of the Earth system that are at least subcontinental in scale and can be switched—under certain circumstances— into a qualitatively different state by small perturbations. The tipping point is the corresponding critical point—in forcing and a feature of the system—at which the future state of the system is qualitatively altered.”). [Kristin	Noted (identical comment as above, ID 12828)



Comment ID	From Page	From Line	To Page	To Line	Comment	Response
12664	17	18	17	22	Because the Arctic is warming at twice the rate as the global average and because Arctic sea ice is susceptible to this increased temperature, Arctic sea ice will be reduced—potentially becoming ice-free for the first time in a matter of decades—that would contribute to a positive feedback within the climate system that can further amplify warming, especially in the region where it can impact permafrost thaw and melting over the Greenland ice sheet. All of these feedbacks can further amplify warming that further risks overshooting the goal of staying well below 2C. Steffen W., et al. (2018) Trajectories of the Earth System in the Anthropocene, PROC. NAT'L. ACAD. SCI. 115(33):8252–8259; 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; Arctic Monitoring and Assessment Programme (AMAP) (2017) SNOW, WATER, ICE, AND PERMAFROST IN THE ARCTIC: SUMMARY FOR POLICYMAKERS; 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; Xu Y., et al. (2018) Global warming will happen faster than we think, NATURE, Comment 564:30–32; Wilkerson J., et al. (2019) Permafrost nitrous oxide emissions observed on a landscape scale using the airborne eddy-covariance method, ATMOS. CHEM. PHYS. 19:4257–4268. [Kristin Campbell, United States of America]	Noted (identical comment as above, ID 12830)
45790	17	19	17	19	not consistent with IPCC definition of risk [Katja Mintenbeck, Germany]	Accepted.
47800	17	22	37	56	Assessment Structure: If possible, assessment conclusions should be provided in a structured traceable account of how these statements were derived. For example, sections / subsections can start with previous IPCC report conclusions (AR5 or AR6 Special Reports) and then provide an update of the more recent literature, clearly laying out the lines of evidence. Each section / subsection can then conclude with assessment statements. [WGI TSU, France]	Noted
22422	17	25	37	56	I suggest to restructure the chapters such that the budgets (for carbon, methane, and nitrous oxide) come first (before historical trends, and variability of the respective sub chapters). This gives a better overview of all the different processes before they are discussed in more detail. [Gwenaelle GREMION, Canada]	Noted.
47064	17	25			5.2.: Would be helpful to put the CO2, CH4, and N2O budget at the beginning of the corresponding sections and not at the end. This would help to first get an idea about fluxes and stocks and then read/learn more about the individual components. The way it is written right now makes it a little bit difficult to have the big picture in mind when reading first about the individual components and then seeing everything put together in the budgets. [Sophie von Fromm, Germany]	Reject: we have taken a different approach emphasizing the variability and trends upfront, which is what is new since AR5, and also forms the basis for a better understanding of the way the budget numbers are derived.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
23380	17	27	17	28	This two lines should be rephrased to capture the header section. Although, the 5.2 section is clearly stated, but I think the statement seems more like a double statement. Atmospheric accumulation of what? I suggest: This section describes the trends and variability in atmospheric accumulation of the three main GHGs-CO2, CH4 and N2O, its sources and sinks as well as their budget during the historical period (1750-2018). [Gwenaelle GREMION, Canada]	Accepted. Changed.
23456	17	30	17	30	This seems to be a run-on statement. Although "also" is like a conjugate link, but I think it should be restructured. Suggestion: The season is not limited to the radiative forcing processes and driving trends of the GHGs and its variability response on seasonal to decadal scales; also providing good insight on the mechanism governing ....supporting evidence from... [Gwenaelle GREMION, Canada]	Rejected. We don't understand the meaning of the proposed construction.
23382	17	30			This seems to be a run-on statement. Although "also" is like a conjugate link, but I think it should be restructured. Suggestion: The season is not limited to the radiative forcing processes and driving trends of the GHGs and its variability response on seasonal to decadal scales; also providing good insight on the mechanism governing ....supporting evidence from... [Gwenaelle GREMION, Canada]	Identical comment as review comment 23456
23384	17	40	17	41	relevant literature will make so clear this statement [Gwenaelle GREMION, Canada]	Accepted. We have added more references (Bacastow et al., 1980; Conway et al., 1988; Nakazawa et al., 1993; Conway et al., 1994)
45350	17	41	17	41	I doubt the uncertainty in the average growth rate is 10% that would project into about 10ppm uncertainty in concentration change [Peter Rayner, Australia]	Accepted. The range in values are based on 1-sigma uncertainties estimated on annual growth rates by NOAA. We have clarified that by adding "(range from 1-sigma standard deviation of annual values)". As the measurement network increased, this uncertainty reduced from 0.28 in the 1960s to 0.07 in the 2009-2018.
55010	17	41	17	41	Noticed examples that require proof-reading are not commented while it may be useful to share the need to correct "multiple locations around the world" in this line with plural "locations." [Kilkis Siir, Turkey]	Accepted
22346	17	41	17	44	When recalculating the data provided by (Dlugokencky and Tans, 2019), I obtain: 2.30±0.08 ppm yr-1 for the most recent decade of 2009-2018 (i.e., rate slightly corrected; uncertainty added similar to the values mentioned before) [Gwenaelle GREMION, Canada]	Accepted. Modified as "2.30±0.07 ppm yr-1 during the most recent decade"
47046	17	41	17	47	5.2.1.1: Shorter sentence. Probably not necessary to write that it is the CO2 accumulation rate again. [Sophie von Fromm, Germany]	Accepted. Changed to "..., with the growth rate almost tripling ..."
19212	17	44	17	44	I believe the correct citation should be Tans & Keeling 2019, please check Mauna Loa website [Baerbel Hoenisch, United States of America]	Rejected. We have used global CO2 trends from NOAA, which says "How to reference content from this page: Ed Dlugokencky and Pieter Tans, NOAA/ESRL (www.esrl.noaa.gov/gmd/ccgg/trends/)"

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
23386	17	46	17	47	Is the previous paragraph in agreement with the carbon estimation of the global carbon project? The influence of anthropogenic emission of carbon are estimated yearly by this project group. If yes, this paragraph should be left as it is, and if No, line 41-44 should be restructured to center the project. [Gwenaelle GREMION, Canada]	Noted. In general, yes, the results should be consistent with GCP
48390	17	46	18	8	The carbon-14 pulse injected into the ecosystem also can be used to evaluate the size and elasticity of the active carbon reservoir in the worldwide ecosystem. Although the atmospheric remainder of the pulse is small, perhaps it can be used to evaluate the long-term sequestration and removal rates that would occur if the rate of fossil CO <sub>2</sub> were to change. Conversely, the declining rate of C14 can be used as a marker for how fast fossil carbon is being removed from the atmosphere. (Elsewhere in AR6 it is implied that the dwell time for anthropogenic CO <sub>2</sub> is centuries; that is probably misleading.) [Stephen Parks, United States of America]	Accepted. As discussed in the text, Levin et al (2010) showed that by combining global atmospheric 14C observations and modelling, it is possible to constrain global fossil fuel emission rates to within about 25%. Further refinement at the global scale is not feasible with current measurement programs and uncertainties in the rate of ocean uptake of 14C and natural cosmogenic 14C production. There is a significant body of research on the transport of "bomb" 14C into the oceans and biosphere, with a particular focus on the penetration of anthropogenic carbon into the oceans, but unfortunately we cannot dig deeper in this assessment.
41780	17	49	17	49	This is not a gradient but a difference [Marc Aubinet, Belgium]	Accepted - text revised
23388	17	51	17	55	A more recent literature should be cited also. This is simply to maintain the trend and make stronger the hypothesis [Gwenaelle GREMION, Canada]	Accepted. We have cited a couple of publications here
43324	17	51		55	This sentence should be reduced [Onema Adojoh, United States of America]	Noted. The sentence is slightly modified for clarity
45352	17	54	17	54	should not be molecule of carbon compound since some contain several carbon atoms [Peter Rayner, Australia]	Accepted. Changed to "...for every atom in a carbon compounds burned one molecule of oxygen..."
56440	17	55			wrong Titel in Reference! Goddard 2007:doi.org/10.1111/j.1600-0889.2006.00228.x: Correct title: "On the long-term stability of reference gases for atmospheric O <sub>2</sub> /N <sub>2</sub> and CO <sub>2</sub> measurements" [Daniel Häussinger, Switzerland]	Accepted. We have added a more recent reference to "Keeling et al., PNAS, 2017; Ishidoya et al., Tellus, 2012"
27974	18	1	18	3	Page 18, line 1-3: "evidence for fossil fuel emissions causing CO <sub>2</sub> increase comes from the measurements of radiocarbon (14C) at Wellington, because the fossil fuels of millions of years old carbon are completely devoid of radiocarbon". This is not explained very well, why does this mean the carbon dioxide increase is due to fossil fuel burning? An ignorant reader will not understand this, please clarify. Page 18, line 5-6: "the high levels gradually decreased back to close to background levels in 2015 due to dilution by the large amounts of 14C avoided-fossil fuel emissions". The word "avoid" is not correct in this context, "devoid" should instead be used. [roderik van de wal, Netherlands]	Accepted. We have revised this sentence and associated text for clarity

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
13560	18	1	18	3	"which show a decrease in C14/C12 ratio" may be inserted after "Wellington" [Govindasamy Bala, India]	Accepted - text revised
6724	18	1	18	7	I believe this paragraph could be rewritten for clarity. There are several confounding effects occurring to explain the 14C curve and they are poorly explained here. [Andrew MacDougall, Canada]	Accepted. We have revised the text significantly for clarity
37732	18	5	18	6	Changing "back to close" to "to reach close" would make the sentence clearer, though I believe (but am not entirely sure) that "lost" should be "loss" near the end of the sentence. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
22424	18	6	18	7	This sentence (also by lost to the ocean interior and to the upper atmosphere) sounds awkward [Gwenaelle GREMION, Canada]	Accepted. We have revised this sentence
6309	18	6	18	7	Unclear sentence - poor English use? States 'dilution by by the large amounts of 14C avoided-fossil fuel emissions' should it say 14C-depleted fossil fuel emissions? [Dave Reay, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We have revised this sentence for clarity using "devoid of 14C"
36266	18	6			Should '14C avoided' be '14C-depleted'? [Nathan Gillett, Canada]	Accepted. We have revised this sentence
13562	18	12	18	18	What is "meg units"? [Govindasamy Bala, India]	Noted - $\delta(O_2/N_2)$ decrease in per meg units = $(FF/M) \times 10^6$ ; where FF is the number of moles of O2 consumed by fossil-fuel burning, and M = $3.706 \times 10^{19}$ moles is a reference value for the total number of O2 molecules in the atmosphere (taken from : <a href="http://scripps2.ucsd.edu/faq">http://scripps2.ucsd.edu/faq</a> )
41782	18	15	18	15	The term $d(O_2/N_2)$ appears ambiguous as it is computed as a ration between two O2 concentrations. Please clarify ! [Marc Aubinet, Belgium]	Discussed in our reply comment#13562. This is well established in the community. The values of O2 changes are expressed with respect to N2. see also : <a href="http://scripps2.ucsd.edu/units-and-terms">http://scripps2.ucsd.edu/units-and-terms</a>
41784	18	23	18	24	I don't understand this sentence. Appears in contradiction with text before. [Marc Aubinet, Belgium]	Accepted. We have modified this sentence and added references
23390	18	23	18	24	The line should be broken. That will make more logical relevance. There should be a full stop after fraction. Then substantiate with a literature. "the fraction of anthropogenic..." statement then can continue [Gwenaelle GREMION, Canada]	Accepted - text revised
32730	18	23	18	24	This statement needs clarification--is it the fraction of fossil fuel and industrial (cement) emissions that have accumulated in the atmosphere, or the fraction of total human-influenced emissions (so fossil fuel/cement plus biospheric loss) that is used to determine the airborne fraction? Does "anthropogenic emissions" refer just to fossil fuel emissions or the sum of fossil fuel and biospheric emissions? [Michael MacCracken, United States of America]	Accepted. Modified as "...anthropogenic emissions due to fossil fuel consumption and cement production that..."
47820	18	23	18	27	The first sentence could be taken out of context. Please consider leading the paragraph with the point that the land and ocean sinks are increasing before stating the relative unchanging CO2 airborne fraction. [WGI TSU, France]	Noted. We have modified this paragraph for better clarity.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
17418	18	24	18	24	Change 'suggest' to 'suggests' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
29450	18	24	18	24	Suggest stating what the airborne fraction is in this sentence, i.e. circa 45% (Canadell et al., PNAS, 104, doi:10.1073/pnas.0702737104, 2007) [Rona Thompson, Norway]	Accepted. However, we have cited AR5 rather than Canadell et al. (2007) for an updated assessment
29452	18	26	18	26	word missing in this sentence, should be "albeit with large inter-annual.." [Rona Thompson, Norway]	Accepted - text revised
23392	18	32	18	33	I think from AR5, the two predominant CO2 sources are the Agriculture, forestry and other land use (AFOLU) and fossil fuel. This has always contributed to the global carbon cycle fluxes and storage. This section should be expanded to capture all sources. [Gwenaelle GREMION, Canada]	Noted. Agriculture as a process (not as a sector as it is treated in WGIII) is a tiny contributor to CO2 emissions. The non-CO2 gases from agriculture, the major component, are treated in the subsequent sections. Forestry is already part of the data provided.
47050	18	32	18	33	5.2.1.2.: Sentence structure. [Sophie von Fromm, Germany]	Accepted. Text revised to "The two anthropogenic CO2 sources are fossil fuel and industry CO2 emissions, and the net flux from land use, land use change and forestry."
47048	18	32			5.2.1.2: The main message of this section is not well/clear written. [Sophie von Fromm, Germany]	Noted. We have improved it.
49116	18	35	18	37	Mention emissions from biomass burning are treated under AFOLU? [Jim Skea, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We are covering emissions from fires in the SOD.
22426	18	35	18	41	This section especially the method for calculating CO2 emission, was misinterpreted or wrongly explained. Le Quéré et al. (2018b) provide details of the method. For example, the CO2 emissions from industry is based on cement production data (see below). Hence, production of cement is considered as a measure of CO2 emission rather than a major source of CO2 emission. The statement of Le Quéré et al. (2018b) is given below. 'CO2 emissions from fossil fuels and industry (EFF) are based on energy statistics and cement production data, respectively, while emissions from land-use change (ELUC), mainly deforestation, are based on land-cover change data and bookkeeping models.' (Le Quéré et al., 2018b). [Gwenaelle GREMION, Canada]	Rejected. We don't understand the point and the text is already consistent with Le Quere et al. 2018.
23394	18	37	18	39	A more recent literature should be cited in order to maintain the trend. Usually 2016-2019 literature [Gwenaelle GREMION, Canada]	Taken into account. Text revised to "(Peters et al., 2012; LeQuéré et al., 2018b)".
17420	18	38	18	38	Change 'era' to 'Era' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
49114	18	40	18	40	I thought we had agreed across WGs to use GtCO2 not PgC? [Jim Skea, United Kingdom (of Great Britain and Northern Ireland)]	Ch5 uses PgC and provides both PgC and GtCO2 for the Remaining C budget section to link it with the existing literature in GtCO2
22428	18	43	18	44	Please consider rewriting the sentence as 'Since AR5, fossil fuel and industry CO2 emissions show little or no growth (growth rate) between 2014 and 2016, after...' [Gwenaelle GREMION, Canada]	Accepted.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
47052	18	43	18	46	5.2.1.2: Sntentence structure: fossil fuel and industrial CO2 emissions. [Sophie von Fromm, Germany]	Taken into account (assumed to be included in comment 22428).
47054	18	43	18	46	5.2.1.2: Time periods are overlapping which makes it difficult to compare numbers with the mentioned time periods. [Sophie von Fromm, Germany]	Taken into account. Text revised to provide all numbers consistently pre/post-1750.
22430	18	45	18	46	Please provide the rate of the growth. [Gwenaelle GREMION, Canada]	Growth rates are now provided.
29390	18	46	18	46	as a reader, I'd also be interested in the growth rate numbers since 2016 [Judith Hauck, Germany]	Taken into account under comment 22430.
47056	18	51	18	56	5.2.1.2: What are bookkeeping models? Might be helpful to explain this model type briefly. [Sophie von Fromm, Germany]	Taken into account. Added "combine changes in land use areas with observation-based, biome-level carbon densities and response curves for biomass decay and regrowth to quantify the net land use change flux. "
13564	18	54	18	54	BLUE: Can this be expanded? [Govindasamy Bala, India]	Taken into account. Text revised to "Bookkeeping models are from Hansis et al., 2015 and Houghton and Nassikas, 2017 "
23396	19	6	19	8	should be made readable and interchanged with line 8-11 of page 9. line 8-11 will well introduce line 6-8 [Gwenaelle GREMION, Canada]	Rejected/taken into account -- not clear what the reviewer means: line 8-11 of page 9 seems wrong reference. Text simplified in response to comment 22432.
22432	19	6	19	20	I have difficulties in reading and understanding this section primarily due to complex sentence structure. This section should be rewritten by using simple and easy to read sentences. [Gwenaelle GREMION, Canada]	Accepted. Text has been rewritten using simpler sentences.
49494	19	7	19	7	Biomass losses from deforestation are far from the only gross land use sources of carbon to the atmosphere. Land use driven changes to soil organic carbon stocks are well known and particularly important carbon sources, primarily in areas outside of the tropics where grasslands are the primarily land cover affected by land use change. See for example Sanderman et al. 2017 ( <a href="https://doi.org/10.1073/pnas.1706103114">https://doi.org/10.1073/pnas.1706103114</a> ); Spawn et al. 2019 ( <a href="https://doi.org/10.1088/1748-9326/ab0399">https://doi.org/10.1088/1748-9326/ab0399</a> ), Poelplau et al. 2011 ( <a href="https://doi.org/10.1111/j.1365-2486.2011.02408.x">https://doi.org/10.1111/j.1365-2486.2011.02408.x</a> ), Don et al. 2011 ( <a href="https://doi.org/10.1111/j.1365-2486.2010.02336.x">https://doi.org/10.1111/j.1365-2486.2010.02336.x</a> ), Li et al. 2018 ( <a href="https://doi.org/10.1111/gcb.14328">https://doi.org/10.1111/gcb.14328</a> ), among many others. Since this report is widely cited, it seems prudent to highlight the important roll and significance of these soil organc carbon changes. Soil organic carbon changes are mentioned later beginning later in line 33 but should be mentioned earlier along side deforestation. [Seth Spawn, United States of America]	Taken into account. Text revised to "e.g. loss of biomass and soil carbon in clearing, biomass burning" in first sentence on net land use change fluxes.
22434	19	8	19	8	Please replce 'The net land use change flux' with 'The Net flux from land use change' [Gwenaelle GREMION, Canada]	Accepted - text revised

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
49470	19	9	19	9	Defining "land use change flux" as being related to "human interference with the terrestrial vegetation cover" may be true in the case of deforestation, but seems unnecessarily narrow and contradicts what is said later in line 17 about land use change including changes in land management. What is described here in line 9 is more accurately described as "land cover" change. [Seth Spawn, United States of America]	Taken into account. Text revised: "cover" removed.
22436	19	9	19	11	separating it conceptually from carbon fluxes occurring due to..' may be written as 'and is separated from the natural carbon fluxes occurring due to .....' [Gwenaëlle GREMION, Canada]	Accepted - text revised
47058	19	14	19	14	5.2.1.2: Acronym (UNFCCC) not explained. [Sophie von Fromm, Germany]	Accepted - text revised
29458	19	15	19	16	I think one line of explanation is needed why an underestimate of the net land use change flux (i.e. a source to the atmosphere) would mean that the land sink (i.e. excluding land use changes) is also underestimated [Rona Thompson, Norway]	Taken into account. Text revised to "which would imply an underestimation of the land sink to meet the observation-based net land-atmosphere exchange".
29454	19	16	19	16	I don't think DGVM has previously been defined [Rona Thompson, Norway]	Accepted - text revised
41786	19	16	19	16	DGVM: meaning is not given. [Marc Aubinet, Belgium]	Accepted - text revised
47060	19	16	19	16	5.2.1.2: Acronym (DGVM) not explained. [Sophie von Fromm, Germany]	Accepted - text revised
23398	19	16	19	20	forest management have larger impacts on global fluxes. I think there is a mis-reference of this statement. DVGMS has previously been discussed in chapter 2 (Land climate interaction) of one of the IPCC synthesis papers. Chapter 2 page 49 line 31-page 50 line 1-3. forest management have not been ignored totally [Gwenaëlle GREMION, Canada]	Taken into account. Text revised to "have included anthropogenic land cover change, but do not all include all land management practices". (Note: reviewer meant p. 31, l. 49, not p. 49, l. 31 in SRCLL.
56334	19	16			"DGVM" is used here for the first time in this chapter, but it isn't defined in the text until page 22 [Steven Neshyba, United States of America]	Accepted - text revised
36268	19	18			The meaning of 'for each practice' isn't clear? How many different practices were considered? [Nathan Gillett, Canada]	Taken into account. Text revised to "Sensitivity studies find that practices such as wood and crop harvesting increase land use emissions".
48148	19	20	19	20	Pugh et al. (2019) show that those DGVMs that account for demographic effects on the magnitude of carbon uptake by re-growing forests consistently simulate a large sink associated with demographic changes alone, that is however significantly smaller than estimates by the book-keeping model of Houghton et al. (2012). [Vanessa Haverd, Australia]	Taken into account. We have added several more sentences and references as an assessment of underrepresented processes.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
36718	19	21	19	22	<p>Around here, it is highly recommended to add a paragraph on recent findings about the effects of biomass burning on global CO2 budget. The amount of carbon released by biomass burning is about 2 Pg C (e.g., van der Werf et al., 2017), and Andela et al. (2017) showed a recent declining trend of burnt area in satellite data.</p> <p>Andela, N., Morton, D.C., Giglio, L., Chen, Y., van der Werf, G.R., Kasibhatla, P.S., DeFries, R.S., Collatz, G.J., Hantson, S., Kloster, S., Bachelet, D., Forrest, M., Lasslop, G., Li, F., Mangeon, S., Melton, J.R., Yue, C., Randerson, J.T., 2017. A human-driven decline in global burned area. <i>Science</i> 356, 1356–1362.</p> <p>van der Werf, G.R., Randerson, J.T., Giglio, L., van Leeuwen, T.T., Chen, Y., Rogers, B.M., Mu, M., van Marle, M.J.E., Morton, D.C., Collatz, G.J., Yokelson, R.J., Kasibhatla, P.S., 2017. Global fire emissions estimates during 1997–2016. <i>Earth System Science Data</i> 9, 697–720. [Akihiko Ito, Japan]</p>	Taken into account. Text revised to include that individual practices such as fire suppression may also create carbon sinks (Andela et al., 2017), see response to comment 48148. Since biomass burning itself is part of the DGVM and bookkeeping modelling, no more changes seem required.
17422	19	22	19	22	Change 'era' to 'Era' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
47822	19	22	19	31	Please ensure the carbon budgets are an update from the Special Report on Land (SRCL). The relevant SRCL section is 2.4.1 [WGI TSU, France]	Noted. Text is consistent with LeQuere et al, Global carbon budget 2018, which is underlying the SRCL (section 2.3.1).
13566	19	22	19	31	What is the reason for the sharp rise and fall in LULCC fluxes in the 1950s and again in 1980s (Fig. 5.5). A brief discussion would be useful to the readers. There are many sharp turns. [Govindasamy Bala, India]	Rejected. Beyond the scope of the report. The 1950s peak is due to change from populations-based to FAO-based agricultural area estimation, Pongratz et al in prep.
45354	19	25	19	25	inversed -> reversed [Peter Rayner, Australia]	Accepted - text revised
17424	19	26	19	26	Capital C for century (for consistency elsewhere in Chapter) [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
17426	19	27	19	28	Text needs editing: ..the 1980s, related, amongst others, to different land-use forcings used in Hansis et al. (2015)/the DGVMs and in Houghton and Nassikas (2017). [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted.
29456	19	28	19	28	Appears to be a word missing in "...used in/ the DGVMs and in [missing word]" [Rona Thompson, Norway]	Accepted. Text revised to "related, amongst other, to Hansis et al., 2015 and the DGVMs using a different land use forcing than Houghton and Nassikas, 2017".
45356	19	29	19	29	this point of lost sink capacity deserves expansion [Peter Rayner, Australia]	Taken into account, see comment 56336.
29460	19	30	19	30	I think the "loss of additional sink capacity" with growing CO2 needs an explanation. [Rona Thompson, Norway]	Taken into account, see comment 56336.



Comment ID	From Page	From Line	To Page	To Line	Comment	Response
56336	19	30			I'm just confused about how to interpret the statement "the loss of additional sink capacity (Gitz and Ciais, 2003), which is growing with atmospheric CO2". Does this mean that sink capacity diminishes with increasing CO2? Or that additional sink capacity diminishes with increasing CO2? [Steven Neshyba, United States of America]	Taken into account. Text revised to "... they include the loss of additional sink capacity, meaning that the sink capacity of the biosphere is reduced by decreasing the residence time of carbon by transforming forests to agricultural areas (Gitz and Ciais, 2003); this loss of additional sink capacity is growing with atmospheric CO2".
17428	19	33	19	33	Change to Pre-Industrial and Industrial-Era [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - The same spelling as in the IPCC SR1.5 / SROCC has been applied
13568	19	33	19	38	Does this mean the LULCC fluxes during the industrial era are ~140 PgC after subtracting the values for the preindustrial period? This may be discussed. [Govindasamy Bala, India]	Taken into account, see comment 36272.
41788	19	33	19	38	sentence not clear: to what refer these numbers (116 and 80) ? To what are they compared (1800, 447) ? Can they be related to Fig 5.5 ? How? [Marc Aubinet, Belgium]	Taken into account. Text revised to use Tab. 5.5 numbers (which underlie Fig. 5.5).
36270	19	33		38	The text here divides LUC emissions into those before and after 1800. The total LUC emissions since 1800 are assessed at (116-80) + 447-353 PgC = 130 PgC. This is different to the 1750-2017 LUC emissions quoted on pg 25, In 16 of 235 +/- 95 PgC. Were LUC emissions between 1750 and 1800 really 105 PgC - this seems high? In any case I strongly suggest using a consistent period here and on pg 25 elsewhere to separate industrial and pre-industrial LUC emissions. [Nathan Gillett, Canada]	Taken into account. Text revised to provide all numbers consistently pre/post-1750.
36272	19	33		38	The very large pre-1800 LUC emissions assessed here of 80 + 353 = 433 PgC (more than total fossil fuel emissions to 2017 of 430 PgC, pg 25 In 15) deserve further discussion and assessment. First are the authors confident that these numbers are correct? Second, given that atmospheric CO2 concentration did not change very much before 1800, these LUC emissions (comparable to fossil fuel emissions) must have been in balance with enhanced uptake by the sinks during this period. Was this just because the changes happened very slowly? Or perhaps this estimate includes e.g. periodic human-caused forest burns, compensated for by subsequent re-growth? - if these kind of emissions have been going on for thousands of years, then I could imagine deriving a high cumulative total. And any such estimate would be sensitive to the start time - are the estimates quoted here from the first human-induced land use changes? Without further discussion and assessment, this text will prompt the question that if humans emitted 430 PgC before 1800 with no effect on atmospheric CO2, how can we be so confident that 430 PgC of fossil fuel emissions since 1800 are the primary driver of the recent observed increase in atmospheric CO2? [Nathan Gillett, Canada]	Taken into account. Text revised to include additional references, an assessment of how well we know preindustrial emissions and more elaboration on how processes in the preindustrial era differed from today.
49472	19	34	19	34	"Sanderman et al. 2018" is an incorrect reference. This reference is to a correction to the original paper. The correct reference should be Sanderman et al. 2017 ( <a href="https://www.pnas.org/content/114/36/9575">https://www.pnas.org/content/114/36/9575</a> ) [Seth Spawn, United States of America]	Accepted. New reference cited.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
22438	19	34	19	34	Is the phrase 'global complications' correct? [Gwenaelle GREMION, Canada]	Not Applicable (the text reads "compilation", not "complication").
56628	19	38	19	41	The sentence "Earth System Models with coral reef formation .... Carbon budget thereafter" sounds intriguingly interesting, but I think needs to be unpacked a bit for clarity. Also, "scenarios" are generally used for the future, so hence maybe explain upper-end "assumption / approaches / modelling results "... and also explain what kind of carbon budget is referred to and over which timeframe. [Malte Meinshausen, Australia]	1) Accepted, text extended to clarify the role of natural slow carbon cycle processes – release of CO2 during formation of coral reefs and CO2 uptake during peat accumulation. 2) Accepted, text revised: "scenarios for" replaced by "assumptions about".
22440	19	39	19	39	Please correct the statement '....anthropogenic emission in last 3 ka'. [Gwenaelle GREMION, Canada]	Not Applicable (the text reads "...anthropogenic emissions are needed in the last 3 ka", not "... anthropogenic emissions in last 3 ka").
9658	19	41	19	43	I suggest changing "Overall, uncertainties in attributing to processes the CO2 increase measured from ice-cores between the early Holocene and the beginning of the industrial era are still large (Brovkin et al., 2016; Ruddiman et al., 2016)." to "Overall, the CO2 increase measured from ice-cores between the early Holocene and the beginning of the industrial era is still plagued by large uncertainties (Brovkin et al., 2016; Ruddiman et al., 2016)." [Brian Magi, United States of America]	Accepted - text revised
17430	19	43	19	43	Change to 'Industrial Era' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
36716	19	45	19	52	Minor carbon flows such as BVOC emission and POC/DOC exports may account for a part of the uncertainties in land carbon budget. Ito (2018) assessed the impacts of the minor carbon flows on global land carbon budget. Ito, A., 2018. Disequilibrium of terrestrial ecosystem CO2 budget caused by disturbance-induced emissions and non-CO2 carbon export flows: a global model assessment. Earth System Dynamics. doi:10.5194/esd-2018-62 [Akihiko Ito, Japan]	Taken into account, see comment 48148.
13262	19	48	19	49	There is no reason to restrict inversion interpretation to regions where environmental changes are known. The sentence is not correct. [Frederic Chevallier, France]	Rejected/taken into account. The text did not say that inversions can be used only in regions where environmental changes are known, but that inversions can be used to infer the land use change flux only in regions where env. changes are known (because the inversion-derived net land-atmosphere exchange needs to be partitioned into the net land use change flux and the natural sinks/sources). Revised text to "...can provide estimates of the net land use change flux only for regions..." to clarify.
29462	19	49	19	49	here it is not clear what "They" is referring to, the satellite based emission estimates or those based on atmospheric inversions [Rona Thompson, Norway]	Taken into account, see comment 49474.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
49474	19	50	19	50	A global satellite-based biomass map now exists: Santoro et al. 2018 ( <a href="https://doi.pangaea.de/10.1594/PANGAEA.894711">https://doi.pangaea.de/10.1594/PANGAEA.894711</a> ). Most of these satellite-based products referenced though are limited to reporting biomass of large trees (diameter at breast height > 10cm) following FAO guidelines and thus overlook the biomass of smaller trees and other landcovers (grass, shrub, etc.) that are also vulnerable to land use change. [Seth Spawn, United States of America]	Taken into account. Text revised to reference only the meanwhile finalized SRCCL, which contains a comparison of satellite-based land use fluxes and a discussion of how approaches differ.
29464	19	51	19	51	"model flux estimates" what kind of models are being referred to? Generally this paragraph (L49-52) is difficult to follow [Rona Thompson, Norway]	Taken into account, see comment 49474.
36274	20	1			Replace 'almost certain' with calibrated uncertainty language. Perhaps 'virtually certain', if supported by the evidence. [Nathan Gillett, Canada]	replaced with very likely
17432	20	2	20	2	Italicise 'almost certain' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
22442	20	2	20	2	Please provide the full form of PgC when it was used first time [Gwenaelle GREMION, Canada]	Accepted - text revised [Change made in the first instance in Section 5.1]
32732	20	2	20	5	Is it the growing total burden of anthropogenic CO2 that is leading to the increased uptake, or is it that with growing emissions, the gradient of the atmospheric CO2 to the upper ocean CO2 is growing that is driving the greater uptake. If the former, as the text suggests, does this not somehow imply that the preindustrial CO2 level is somehow a magical level for the climate system and that the sinks will return us to this level no matter what, so the sinks will stay strong even if emissions go to zero (if so, then the excess CO2 would seem to be on a path to be removed in 50 years or so)? I'd suggest that it is the latter--that the flux into the ocean is caused by the instantaneous gradient caused by emissions pushing the atmospheric level up with the concentration of the ocean mixed layer lagging; if this is the case, then zero emissions would mean that the flux into the ocean pretty quickly goes to zero, and the elevated CO2 concentration will stay for a very long time as the CO2 gets repartitioned to the deep ocean and biosphere (note that a similar argument applies for the terrestrial biosphere, etc.). This is really important to state clearly as I've heard a talk by a prominent ocean scientist and seen a paper submitted by a scientist that in essence argue the former situation is the case, and if this is so, then the recovery to 300 ppm or so will only take several decades after zero emissions and the climate system can then adjust back to a preindustrial climate--and I just don't think that is what the carbon cycle model analyses have been suggesting as it would somehow need to explain the basis for 278 ppm or so being a level to which things would return quickly. [Michael MacCracken, United States of America]	Accepted: sentence is hopefully now clear in respect of the intended meaning. It is almost certain that the contemporary (past 50 years) ocean sink of CO2 is strengthening (1.7±0.5 PgC yr-1 to 2.5±0.5 PgC yr-1 between early 1980s and the 2000s respectively; (see SROCC Chapter 5: Section 5.2.2.3) in direct response to the growing anthropogenic CO2 emissions corresponding to a multidecadal mean uptake of 25±5% of CO2 emissions (Le Quéré et al., 2018b; Table 5.1)
29392	20	3	20	3	"between early 1980s and 2000s" - is it not possible to go closer to present day than 2000s? E.g. Gruber et al., Science 363, 1193–1199 (2019) [Judith Hauck, Germany]	Accepted - text altered

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
22444	20	5	20	8	The sentence (This growing sink both mitigates ..... and, though the effect is still small during the contemporary, drive future weakening of the ocean CO2 sink) is complex. The word 'drive' is used too many times. Please consider rewriting the sentence. [Gwenaelle GREMION, Canada]	Accepted - revised sentence: This growing sink both mitigates global warming and drives the already observable changes to ocean carbonate chemistry also associated with ocean acidification. Though this effect is still small during the contemporary period, it is very likely to drive future weakening of the ocean CO2 sink under low mitigation scenarios (Bates et al., 2014; Landschützer et al., 2018; Sutton et al., 2016).
29466	20	7	20	7	appears to be a word missing in "...during the contemporary [missing word].." - contemporary what? [Rona Thompson, Norway]	Accepted - ... contemporary period ... see revised sentence in comment 22444
32734	20	8	20	8	This says there were three advances, but only two are listed [Michael MacCracken, United States of America]	Not applicable - sentence changed
46856	20	8	20	10	only reads "two major advances" [Roland Séférian, France]	Accepted - text revised
13764	20	8	20	11	It could be worth adding a statement that for the first time, we now have also spatially-resolved continuous pCO2 climatologies (including seasonality) for the global coastal ocean (Laruelle et al., Biogeosciences, 2017 <a href="https://doi.org/10.5194/bg-14-4545-2017">https://doi.org/10.5194/bg-14-4545-2017</a> ). This developemnt is also relevant for coastal acidification. [Pierre Regnier, Belgium]	Accepted - text altered
29394	20	8	20	11	it would be great to have the matching references directly after the statement and not all at the end of the sentence, i.e. "obs constraints (Landsch. 16, Rö 15), storage (Gruber 19), seasonal cycle (Landsch., 18), This is true foir the full paragraph, but most disturbing in this sentence. [Judith Hauck, Germany]	Accepted - revised the sentence: Three major advances since AR5 have been the new observational constraints for decadal variability in ocean uptake (Landschützer et al., 2016, Rödenbeck et al., 2015; Gregor et al., 2019), storage (Gruber et al., 2019; DeVries et al., 2019), and the observation product based changes in pCO2 seasonal cycle amplitude in response to changing ocean carbonate chemistry (Landschützer et al., 2018).
17434	20	12	20	12	Change 'coordination' to 'co-ordination' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
12964	20	12	20	13	LDEO (expand it), sentence need clarification [RADEN DWI SUSANTO, United States of America]	Accepted - LDEO is now spelled out in the text
17436	20	13	20	13	define LDEO and remove ) after O [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
22446	20	13	20	13	Please provide the full form of LDEO [Gwenaelle GREMION, Canada]	Accepted - text revised
47062	20	13	20	13	5.2.1.3: Acronyms (SOCAT and LDEO) not explained [Sophie von Fromm, Germany]	Accepted - text revised [Note that SOCAT was already defined, but we have now defined LDEO]
22448	20	16	20	16	Please explain the meaning of 'ocean interior'. I think the authors mean deep ocean. [Gwenaelle GREMION, Canada]	Taken into account - we mean the ocean below the upper mixed layer. We have added this to the text.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
36276	20	16		19	This text just names two ocean carbon projects as being central to constraining decadal variability. Focus on a description of the evidence underlying the improved assessment of decadal variability, including assessing literature on project results where relevant, rather than naming the projects. [Nathan Gillett, Canada]	Accepted - additional text has been added
46858	20	18	20	18	I would avoid to use "constraining" here we only have a few decades of observations: with these data we can only document and characterize this variability not constraining it [Roland Séférian, France]	Accepted - we have changed "constraining" to "characterising"
12966	20	22	22	29	should mention that global ocean CO2 measurements are not complete, some oceans (i.e. Southeast Asia waters) have limited or no observation at all. [RADEN DWI SUSANTO, United States of America]	Taken into account - text altered
29396	20	23	20	25	I don't see why the Lenton 13 paper is cited for this sentence, this is a regional Southern Ocean paper and doesn't give an overview about all recent developments, such as SOCOM. For the pCO2 obs it is usually Rödenbeck et al., 2015, BG. McKinley as cited, gives an overview of various methods; equally well would the latest Global Carbon Budget Paper LeQuere et al 2018, ESSD, fit. [Judith Hauck, Germany]	Taken into account - we have added the Rodenbeck and LeQuere references and removed the Lenton reference
46860	20	28	20	28	please include Berthet et al. 2018 in the reference list ( <a href="https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2019MS001644">https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2019MS001644</a> ) [Roland Séférian, France]	Accepted - reference added
29398	20	29	20	29	it seems that here all refs as in Fig 5.6 is given - then ref to Paulsen et al., 2017 is missing. [Judith Hauck, Germany]	Accepted - revised text: (Aumont and Bopp, 2006; Doney et al., 2009; Hauck et al., 2018; Le Quéré et al., 2010, 2018c; Schwinger et al., 2016; Paulsen et al., 2017).
56338	20	49	20	51	The statement, "GOBM reconstructions demonstrate that the global ocean carbon sink has grown over past six decades, but also reveal a slowdown in the sink in the 1990s, consistent with that found from the observationally-based products." seems to be poorly supported ... at least, I don't find evidence for it in the figures provided. [Steven Neshyba, United States of America]	Taken into account - Figure 5.6 has been redrawn in response to other comments
22450	20	50	20	51	I do not understand the meaning of 'consistent with that found from the observationally-based products'. [Gwenaëlle GREMION, Canada]	No longer applicable
26500	20	54	20	55	references about seasonal characteristics are very biased towards the Southern Ocean. Please add this work about the seasonal cycle of pCO2 and its implication for the carbon uptake in the North Atlantic: Goris, N., J.F. Tjiputra, A. Olsen, J. Schwinger, S.K. Lauvset, and E. Jeansson, 2018: Constraining Projection-Based Estimates of the Future North Atlantic Carbon Uptake. J. Climate, 31, 3959–3978, <a href="https://doi.org/10.1175/JCLI-D-17-0564.1">https://doi.org/10.1175/JCLI-D-17-0564.1</a> . This is also of importance as some Models are performing well in the North Atlantic and poorly in the Southern Ocean and vice versa. [Nadine Goris, Norway]	Taken into account - we have added the Goris reference
41792	20	55	20	55	"is" missing before "likely" [Marc Aubinet, Belgium]	Accepted - text revised
22454	20	55	20	55	is' missing between it and likely [Gwenaëlle GREMION, Canada]	Accepted - text revised

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
47726	20	55	21	1	Please check the use of this IPCC uncertainty language term. Is this likely term the result of the assessment? Are you able to provide a traceable account to assigning this uncertainty statement? Note that likelihood statements are quantified terms - phrases like likely and very likely have quantifiable probabilities associated with them. Please check it has been used correctly here. Please refer to the IPCC guidance note on uncertainty: <a href="https://wg1.ipcc.ch/SR/documents/ar5_uncertainty-guidance-note.pdf">https://wg1.ipcc.ch/SR/documents/ar5_uncertainty-guidance-note.pdf</a> [WGI TSU, France]	Taken into account
22452	20	55	21	1	"Likely" occurs three times in this sentence, but only one is italicized indicating confidence language, so what do the other "likely" occurrences mean? [Gwenaëlle GREMION, Canada]	Taken into account - text altered
29402	20	55	21	3	this sentence says 3x likely and 3x linked, just a note :). [Judith Hauck, Germany]	Accepted: Sentence re-edited: Moreover, while it is likely that decadal and interannual modes of variability found in ESMs are linked to natural forcing (Li and Ilyina, 2018a), the 1990–2000 decadal mode in the air-sea fluxes of CO <sub>2</sub> in the Southern Ocean points to a climate sensitivity about as likely as not linked to the influence of anthropogenic climate forcing on winds absent in coupled and forced models (Bronse laer et al., 2018; Gregor et al., 2018; Gruber et al., 2019c; Roobaert et al., 2018; Swart et al., 2014)
36278	20	55		56	It is unclear what this part of the sentence is trying to say. Li and Ilyina (2018a) do report an effect of volcanic eruptions on carbon uptake - is this what is being described here? But they don't examine links between decadal modes of variability and natural forcing as is stated here. Note that 'natural forcing' is usually taken to mean solar and volcanic forcing. [Nathan Gillett, Canada]	Accepted: the meaning of the sentence was clarified - Moreover, it is as likely as not that decadal and interannual modes of ocean - atmosphere CO <sub>2</sub> fluxes found in ESM large ensemble runs are linked to unforced variability (Li and Ilyina, 2018a). However, the 1990–2000 decadal mode in the air-sea fluxes of CO <sub>2</sub> in the Southern Ocean also points to a climate sensitivity, about as likely as not, linked to the influence of anthropogenic climate forcing on winds, which is weak or absent in coupled and forced models (Bronse laer et al., 2018; Gregor et al., 2018; Gruber et al., 2019c; Roobaert et al., 2018; Swart et al., 2014)(Figure 5.6)

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
46862	20	56	20	56	this sentence is not clear. [Roland Séférian, France]	Accepted: Sentence re-edited: Moreover, while it is likely that decadal and interannual modes of variability found in ESMs are linked to natural forcing (Li and Ilyina, 2018a), the 1990–2000 decadal mode in the air-sea fluxes of CO2 in the Southern Ocean points to a climate sensitivity about as likely as not linked to the influence of anthropogenic climate forcing on winds absent in coupled and forced models (Bronse laer et al., 2018; Gregor et al., 2018; Gruber et al., 2019c; Roobaert et al., 2018; Swart et al., 2014)
46864	20	56	20	56	The interannual or decadal mode of variability are related to ocean or coupled climate modes of variability. These latter are internal mode of variability and hence not forced by "natural forcing". While i understand the meaning of 'natural forcing' it is not correct to use this terms here. [Roland Séférian, France]	Taken into account - we have changed the wording from "natural forcing" to "natural modes of climate variability"
41794	21	1	21	1	shouldn't "likely" appear in italic ? [Marc Aubinet, Belgium]	Accepted - text revised
41796	21	1	21	2	shoudn't this write "SO points to a climate sensitivity likely linked to the influence..."? [Marc Aubinet, Belgium]	Accepted - text revised
36280	21	1		2	The meaning of this text is unclear. What is the '1990-2000 decadal mode'? Why focus on trends over one decade if the trend discussed is really due to anthropogenic forcing? What are the 'prognostic models' referred to here - CMIP6-type models? I think the authors are trying to say that there may be a wind-stress induced effect on the Southern Ocean carbon sink which is missing in climate models, but this is not very clear. This is potentially important, so should be fully explained in the text, in such a way that it makes sense to a reader who has not already read all the cited references. [Nathan Gillett, Canada]	Taken into account - text altered. The reviewer's interpretation of our intended message is correct and we've amended the text to hopefully make this clearer.
41798	21	2	21	2	It is not clear to me what "prognostic models" are: are they GOBM ? Where do they appear in Fig 5.6 ? [Marc Aubinet, Belgium]	Accept: In this context, prognostic models are forced process based predictive models (GOBMs), which are the basis for the model reconstruction trend line. This has been clarified in the SOD.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
22456	21	2	21	2	I do not understand what the authors mean by the 'influence of climate on winds' [Gwenaelle GREMION, Canada]	Accepted: referring to the influence of ozone hole and global warming on the characteristics of the mid latitude cyclones. Revised sentence: Moreover, while it is as likely as not that decadal and interannual modes found in ESM through large ensemble runs are linked to internal variability (Li and Ilyina, 2018a), the 1990–2000 decadal mode in the air-sea fluxes of CO2 in the Southern Ocean points to a climate sensitivity about as likely as not linked to anthropogenic forcing of the westerly wind stress, which is weak or absent in coupled and forced models (Bronseleer et al., 2018; Gregor et al., 2018; Gruber et al., 2019c; Roobaert et al., 2018; Swart et al., 2014)(Figure 5.6)
13766	21	5	21	7	I recommend adding the reference to Roobaert et al., (2018) - already in the reference list - here too as this contribution investigates the effect of the wind speed (and wind formulation) on the air-sea flux in a very systematic way. [Pierre Regnier, Belgium]	Accepted: reference added
13264	21	9	21	9	The characterization of the slowdown was claimed by the GOBM in line 5-20-50, now it is claimed by the observation-based products. [Frederic Chevallier, France]	Accepted: Sentence revised to make it clear that the slow down was first characterized by the observational products. GOBM reconstructions demonstrate that the global ocean carbon sink has grown over past six decades, but also reveal a slowdown in the sink in the 1990s, that were first found from the observationally-based products (Landschützer et al., 2014; 2015).
41800	21	10	21	10	It is not clear to me what "prognostic models" are: are they GOBM ? Where do they appear in Fig 5.6 ? [Marc Aubinet, Belgium]	Duplicate comment
29404	21	10	21	10	"absent from prognostic models". In Figure 5.6, the prognostic models and data-products actually compare quite well. I'm somewhat surprised to see the choice of 6 data products plotted in fig. 5.6. In the GCB, we usually use just the two products which compare best to measurements (Landschützer, Rödenbeck). How were these products chosen (there are many others around)? Did they have to meet any evaluation criteria? [Judith Hauck, Germany]	The observationally-based products plotted in fig 5.6 are a subset of the SOCOM participating mapping methods that have been updated in 2019 for inclusion in the IPCC report. In the spirit of IPCC, all of the available, updated products were included and no weighting schemes were applied. Indeed, this is different than the GCB approach or the Rodenbeck SOCOM paper.
22458	21	18	21	18	Please explain the trends in each panel (c,d,e,f, g, h) of the Figure 5.7 [Gwenaelle GREMION, Canada]	Accepted: This is done in the revised caption.



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55510	21	27	21	28	Line suggests "strong modulation" - is it possible to provide some sort of quantification of how strong this is? [Wesley Fraser, United Kingdom (of Great Britain and Northern Ireland)]	Noted
46866	21	27	21	37	It might be useful to clarify what comes from model results or observation-derived estimates. Why do you refer to Li and Ilyina, 2018b alongside observational papers ? It is a ESM-only study. [Roland Séférian, France]	Accepted: correction made: Li and Ilyina, 2018 incorrectly assigned
23400	21	28	21	30	substantiate with literature [Gwenaëlle GREMION, Canada]	Accepted: (Gruber et al., 2019; Gregor et al., 2019; Li and Ilyina, 2018b)
36282	21	28		29	The meaning of 'natural and climate forced modes of regional interannual and decadal variability' is unclear. AR5 glossary definition of 'mode of variability': 'Underlying space-time structure with preferred spatial pattern and temporal variation' i.e. a feature of the internal variability. As usually understood all modes of variability are natural. So it is not clear what a 'climate forced' mode of variability is. [Nathan Gillett, Canada]	Taken into account - text altered
23402	21	32	21	35	this line should be made more friendly to readers. [Gwenaëlle GREMION, Canada]	Accepted: edited the sentence: It is likely that the invigoration of ocean CO2 uptake (2000–2016) was driven from the mid and high latitudes, particularly the Southern Ocean. It is likely that the model uncertainty also reflects the sparseness of observations in the Southern Ocean (Figure 5.7);
22460	21	33	21	34	In what way does Fig5.7 demonstrate this? Through the spread of lines of the models represented in these panels? [Gwenaëlle GREMION, Canada]	Accepted: Clarified in the text
17438	21	35	21	35	) missing at end of line [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
17440	21	37	21	37	Delete ( at start of line [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
47824	21	40	22	32	Please check consistency with SROCC. Chapter 5 Section 5.2.2 covers this topic. Please add a call out to the report [WGI TSU, France]	Taken into account
17442	21	41	21	41	Italicise 'virtually certain' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
22462	21	41	21	41	"virtually certain" should be confidence language? [Gwenaëlle GREMION, Canada]	Accepted
23404	21	41	21	44	statement should be more readable and concise. There is actually a run-on statement. [Gwenaëlle GREMION, Canada]	Accepted - text revised

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
32736	21	41	21	44	This is a really critical statement to get right. First, strictly speaking, I assume a proportional amount of the new uptake remains in the mixed layer so its concentration tends toward equilibration with the atmospheric CO2 amount (so this might be stated). Second, the net transfer to the deep ocean is the difference between what is going down, which depends on the present atmospheric concentration and uptake into cold descending waters (the rate of which can change) and the concentration (typically super-saturated) in upwelling waters. The question that is critical is how these fluxes will change as emissions go to zero? Will the amount downwelling stay the same and so keep pulling CO2 from the atmosphere at the near current rate to supply the CO2 going to the deep ocean, which would seem to imply that the removal from the atmosphere will continue to be strong and pull the atmospheric excess CO2 concentration down quickly, or will the amount going down drop sharply with zero CO2 emissions so that the atmospheric concentration stays elevated for a long time? Or is most downward mixing really along isopycnals and so not to very deep in the ocean so that the upward flux will rapidly be adjusting to potential changes in the downward flux. Again, I'd note that there are those prominent in the community saying the downward flux to the deep ocean will continue to be strong and this will quickly pull the excess atmospheric CO2 concentration down. Is this the case? I would urge going through all of this to make sure that what is said is consistent with explaining what will happen as CO2 emissions go to zero. [Michael MacCracken, United States of America]	Taken into account - covered in Section 5.3.3.4.
51944	21	41	21	44	It feels premature to open a section with the substantive assessment finding, and this is at odds with the style used in the 4 preceding chapters. Suggest to redraft so that the finding closes out and follows from the substantive assessment performed in the section instead [Peter Thorne, Ireland]	Rejected - This change would remove the clarifying nature of the capitalized characters for the EMIC acronym
36284	21	41		43	The authors should not associate a quantified probability (such as virtually certain, P>99%) with a vague non-quantitative statement that CO2 transport from the atmosphere to the surface and from the surface to the deep ocean occur 'at about the same rate'. The probability for the statement to be true depends on the understanding of 'about the same', which is undefined. For example, does 'about the same rate' mean the same to within 1%, 10% or 100%? The probabilities will be very different for each. Replace 'about the same rate' with 'the same rate to within +/-xx%'. [Nathan Gillett, Canada]	Taken into account - text revised following the comment #23404 and #51944. Here, "about the same rate" was removed.
16154	21	42			"at about the same rate". Same rate as what? Should be clear. [AKIHIKO MURATA, Japan]	Accepted - text revised. "about the same rate" was removed.
23406	21	44	21	48	the text should be shorten. There should be a full stop after CO2 increases with a literature citation. Then starts with "this shows" [Gwenaelle GREMION, Canada]	Not applicable - text revised. Issues of changes in CO2 buffering capacity and productivity were removed.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
22464	21	45	21	46	The meaning of the sentence is not very clear because the increase in CO2 was first described in percentage and then in PgC. It should either be in percentage or in PgC. [Gwenaelle GREMION, Canada]	Accepted - text revised so that total ocean CO2 inventory change since the Industrial Revolution until 2007 and its increment between 1994 and 2007 are described both in the unit of PgC and percent of total anthropogenic CO2 emission.
22466	21	47	21	47	I suggest to incorporate finding reported by Jonkers et al (2019) Nature doi: 10.1038/s41586-019-1230-3 [Gwenaelle GREMION, Canada]	Taken into account - Jonkers et al (2019) is not cited because the impact of the change in primary productivity on carbonate chemistry is mentioned here. However, text revised.
33462	21	47			This reads as if buffer capacity and productivity changes are not yet significant, contrary to results reported elsewhere in this report. Change to "are not yet significantly impacting CO2 storage" [Adrienne Sutton, United States of America]	Accepted - text revised. Issues of changes in CO2 buffering capacity and productivity" are not addressed here.
32738	21	48	21	51	That the net transfer to the deep ocean is as sensitive as mentioned here suggests that perhaps the flux to the deep ocean will drop quickly one CO2 emissions go to zero, and that perhaps what is happening is driven by gradients and not absolute amounts, but I speculate here, and the text should be revised to speak authoritatively about what would happen when emissions go to zero. [Michael MacCracken, United States of America]	Taken into account - covered in Section 5.3.3.4 in terms of ocean acidification.
22468	21	54	21	54	I did not find this reference in the reference list! [Gwenaelle GREMION, Canada]	Not applicable - Kouketsu and Murata (2014) was removed from the text.
29406	21	54	21	54	31% - this number appears to be high to me. Is that again without accounting for land-use change emissions? [Judith Hauck, Germany]	Noted - text revised - this is the ratio of the atmospheric CO2 increase for the same period.
36286	21	55		56	Why should changes in ocean carbon content be 'corrected for the losses from the inventory of natural CO2 due to upwelling in the Southern Ocean and ocean warming'? These changes are real changes in ocean carbon, so this doesn't sound like a correction. Also, if these changes are driven by ocean warming, as is implied, isn't this a climate feedback which should be captured? If this is retained, I suggest replacing 'When corrected for' with 'Removing the effect of'. And add further justification for why these effects are removed from reported changes in ocean carbon content. [Nathan Gillett, Canada]	Accepted – text revised: not "anthropogenic CO2 uptake" but "total inventory increase of CO2" is described.
16156	21	56			5+-3 PgC too big? [AKIHIKO MURATA, Japan]	Not Applicable: text revised and no longer mentioned.
29408	22	4	22	4	The IPCC in 2001 made a great job in using multiple lines of evidence to constrain the ocean carbon sink for the 1990s. (Denman et al., 2001, AR4, WG1, chapter 7, page 519). This is still used as a constraint for the ocean carbon cycle models in the GCB. It would be super cool to get an update of that number for the 2000s, using all available lines of evidence. [Judith Hauck, Germany]	Accepted - a table showing the decadal mean ocean CO2 uptake rates evaluated from various approaches is prepared and shown in the text.

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16158	22	6			Figure 5.8. In this figure, unit of mol/m <sup>2</sup> /yr is used. But in the main text, unit of PgC/yr is used. I hope that the same unit as in the text is used also in the figure. [AKIHIKO MURATA, Japan]	Taken into account - Shown in Figure 5.8 is the inventory change in the anthropogenic CO <sub>2</sub> per unit area. We changed the unit to "gC m <sup>-2</sup> yr <sup>-1</sup> ".
17444	22	10	22	10	Change 'letter' to 'lesser' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
47826	22	16	22	29	Are you able to add assessment language to these statements? Is this covered in SROCC? The relevant section would be 5.2.2. Please add a call out to the Special Report ,if necessary. [WGI TSU, France]	Accepted - text revised.
33484	22	24			Another recent paper that supports this result and should be included is "Carter, B. R., Feely, R. A., Wanninkhof, R., Kouketsu, S., Sonnerup, R. E., Pardo, P. C., et al. (2019). Pacific anthropogenic carbon between 1991 and 2017. Global Biogeochemical Cycles, 33, 597–617. <a href="https://doi.org/10.1029/2018GB006154">https://doi.org/10.1029/2018GB006154</a> . [Adrienne Sutton, United States of America]	Accepted - text revised.
23408	22	25	22	27	there should be other factors responsible to this [Gwenaelle GREMION, Canada]	Accepted - text revised: an increased release of anthropogenic CO <sub>2</sub> into the atmosphere is considered as another factor responsible.
55512	22	25	22	27	It would be useful to include a very brief summary of how the oceanic ventilation pattern has changed. [Wesley Fraser, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - changes in the circulation is covered in chapter 9.
8928	22	32	22	32	Terrestrial Cabon should be Terrestrial Carbon dioxide (as this section does not include CH <sub>4</sub> , etc) [Benjamin Lamptey, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
28882	22	32	23	5	AR5 contains important information on regional atmosphere-to-land CO <sub>2</sub> fluxes (AR5 Sect. 6.3.2.6.2) showing large differences between DGVMs and atmospheric inversions and large uncertainties (AR5 Fig. 6.15). The AR6 draft report is hardly mentioning this important topic (Sect. 5.2.1.4 is limited to global trends and anomalies, not discussing regional fluxes). I recommend to add a new subsection 5.2.1.4.X dedicated to this topic or to add at least a short paragraph to mention the topic of absolute values of regional terrestrial carbon fluxes and their uncertainties. To be specific, here my proposal: Please add on page 23, line 6: Sizeable uncertainties also exist for regional terrestrial CO <sub>2</sub> fluxes. For example the AR5 estimate of the Europe carbon sink based on atmospheric inversions of in situ observations is 0.4 +/- 0.4 PgC/yr (AR5, Fig. 6.15), whereas Reuter et al., 2017 ( <a href="https://journals.ametsoc.org/doi/full/10.1175/BAMS-D-15-00310.1">https://journals.ametsoc.org/doi/full/10.1175/BAMS-D-15-00310.1</a> ) provides evidence based on satellite CO <sub>2</sub> and other observations that the European carbon sink is likely significantly larger, namely around 0.95 +/- 0.33 PgC/yr, which is consistent with atmospheric inversions based on in situ observations using the method of Chevallier et al., 2010 ( <a href="https://doi.org/10.1029/2010JD013887">https://doi.org/10.1029/2010JD013887</a> ) carried out in the framework of the Copernicus Atmosphere Monitoring Service (CAMS) (see <a href="https://climate.copernicus.eu/greenhouse-gases">https://climate.copernicus.eu/greenhouse-gases</a> ). [Michael Buchwitz, Germany]	Taken into account. Due to the page limit, we do not include a specific section for magnitude of regional land-atmosphere CO <sub>2</sub> exchange. However, we do account this comment by highlighting sizeable uncertainties remain in estimates of regional land-atmosphere CO <sub>2</sub> exchange in section 5.2.1.4.1.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
15176	22	32	23	30	Elsewhere in this chapter, permafrost is noted as a source of terrestrial carbon, however, it is absent from this section. Though this section (5.2.1.4.1) is focusing on global trends, it may be judicious to provide a brief description of the latitudinal gradient in terrestrial carbon sourcing/sinking. This gradient is explained later in this chapter (Page 53, lines 4-9), so this is somewhat contradictory. This could also help to integrate this section with other components of the report describing Arctic amplification (e.g Page 17, lines 21-22) and the role of permafrost (Page 48, lines 13-21). [Richard Vachula, United States of America]	Taken into account. A brief description on trends of land CO2 sink over northern hemisphere is added.
47234	22	32			Despite increased water use efficiency, increased aridity (low to medium confidence, cf. Chapter 8) and increased drought frequency or severity (low to medium confidence, cf. Chapter 11) may be a serious limitation for the terrestrial carbon sink at least at the regional scale. [Hervé Douville, France]	Rejected. This section is on historical trend of terrestrial carbon sink at global scale. The drought and aridity change reported (cf Chapter 8 (section 8.3.2.11)) is at smaller regional scale. Future change of land carbon sink under changing drought frequency/severity is beyond the scope of this section.
36312	22	34			Assess wildfire effects on land carbon here and cite Arora et al. (2018) Reduction in global area burned and wildfire emissions since 1930s enhances carbon uptake by land - <a href="https://www.nature.com/articles/s41467-018-03838-0">https://www.nature.com/articles/s41467-018-03838-0</a> . [Nathan Gillett, Canada]	Accepted. We add a paragraph assessing wildfire effects on land carbon sink. Arora et al. (2018) is included as suggested.
43880	22	36	22	36	Le Quere et al., 2018c does not exist in Referece list [Tomoko Nakano, Japan]	Taken into account
17446	22	38	22	38	Insert 'the' after 'during' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
17448	22	39	22	39	Change 'evidences' to 'evidence' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
38166	22	41	22	41	The explanation of abbreviation should be placed at the first appearance of the word (here DGVM). [Hiroaki Kondo, Japan]	Accepted - text revised
13266	22	41	22	41	Rayner et al. (2015) did not present an atmospheric inversion (their forward model is not an atmospheric model). [Frederic Chevallier, France]	Accepted. Reference is removed from the citation.
13648	22	41			DGVM: Abbreviation has already been used since page 19 (but not written out there) [Lena Boysen, Germany]	Accepted - text revised
26866	22	44	23	5	There is an additional line of evidence for the main statements in these two paragraphs. Li et al ("Quantification of the response of global terrestrial net primary production to multifactor global change", Ecological Indicators, vol. 76, 2017, 245–255) analysed a comprehensive global database of field measurements of NPP between 1961 and 2010 using artificial neural networks (essentially a statistical approach). They found that the global NPP increased by 21% during this period, and also that the increasing atmospheric CO2-concentration is the most important factor behind this. [Jonas Nycander, Sweden]	Rejected. All the evidence listed here is for gross primary production, while the work of Li et al. is for net primary production. We appreciate the value of the work, but it is not the most appropriate one to be cited here.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
49476	22	47	22	47	Two studies have also found that enlarged seasonality may be due in part to agriculture (i.e. intensively managed systems rather than the primarily "natural" system examples cited here) Gray et al. 2014 ( <a href="https://www.nature.com/articles/nature13957">https://www.nature.com/articles/nature13957</a> ) and Zeng et al. 2014 ( <a href="https://www.nature.com/articles/nature13893">https://www.nature.com/articles/nature13893</a> ) [Seth Spawn, United States of America]	Rejected. Here we emphasize the link between enlarging seasonal CO2 amplitude and enhanced vegetation photosynthesis. The two studies proposed have explored one of the reasons (agriculture) driving the increment of vegetation photosynthesis, and consequently the seasonal CO2 amplitude. Thus, they are not the most appropriate ones to be cited here.
36722	22	47	22	47	Amplification of seasonal CO2 cycle related to terrestrial activity is described here but very shortly. Considering the recent accumulation of works on this matter, this topic should be described more (e.g., Gray et al., 2014; Zeng et al., 2014; Zhao et al., 2016). Gray, J.M., Frohking, S., Kort, E.A., Ray, D.K., Kucharik, C.J., Ramankutty, N., Friedl, M.A., 2014. Direct human influence on atmospheric CO2 seasonality from increased cropland productivity. <i>Nature</i> 515, 398–401. Zeng, N., Zhao, F., Collatz, G.J., Kalnay, E., Salawitch, R.J., West, T.O., Guanter, L., 2014. Agricultural Green Revolution as a driver of increasing atmospheric CO2 seasonal amplitude. <i>Nature</i> 515, 394–397. Zhao, F., Zeng, N., Asrar, G., Friedlingstein, P., Ito, A., Jain, A., Kalnay, E., Kato, E., Koven, C.D., Poulter, B., Rafique, R., Sitch, S., Shu, S., Stocker, B., Viovy, N., Wiltshire, A., Zaehle, S., 2016. Role of CO2, climate and land use in regulating the seasonal amplitude increase of carbon fluxes in terrestrial ecosystems: a multimodel analysis. <i>Biogeosciences</i> 13, 5121–5137. [Akihiko Ito, Japan]	Taken into account. Text revised to "The net land use change CO2 flux (Pg yr-1) as estimated by two bookkeeping models and 16 dynamic global vegetation models (DGVM) for the global annual carbon budget 2018 (Le Quéré et al., 2018a). The two bookkeeping models are from Hansis et al., 2015 and Houghton and Nassikas, 2017 both updated to 2018; their average is used as to determine the net land use change flux in the annual global carbon budget. The DGVM estimates are the result of differencing a simulation with and one without land use changes run under observed historical climate and CO2, following the TRENDY v7 protocol; they are used to provide an uncertainty range to the bookkeeping estimates. "
36750	22	47	22	47	Only one record appears for Forkel et al. (2016) in References. [Akihiko Ito, Japan]	Accepted. Reference citation is corrected.
36826	22	48	22	48	Several studies other than Cheng et al. 2017 have addressed the global change in water-use efficiency (e.g., Ito and Inatomi, 2012; Zhou et al., 2017). Ito, A., Inatomi, M., 2012. Water-use efficiency of the terrestrial biosphere: a model analysis on interactions between the global carbon and water cycles. <i>Journal of Hydrometeorology</i> 13, 681–694. Zhou, S., Yu, B., Schwalm, C.R., Ciais, P., Zhang, Y., Fisher, J.B., Michalak, A.M., Wang, W., Poulter, B., Huntzinger, D.N., Niu, S., Mao, J., Jain, A., Ricciuto, D.M., Shi, X., Ito, A., Wei, Y., Huang, Y., Wang, G., 2017. Response of water use efficiency to global environmental change based on output from terrestrial biosphere models. <i>Global Biogeochem. Cycles</i> 31, doi:10.1002/2017GB005733. [Akihiko Ito, Japan]	Taken into account. To avoid the confusion, we revise this sentence as "observation-driven inference of increasing photosynthesis CO2 uptake by enhanced water use efficiency and evapotranspiration (Cheng et al., 2017)." Because the intention is to list new estimates of global GPP change that are independent from process modelling, modelling studies on water use efficiency are therefore not included.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
48146	22	49	22	49	Suggest commenting on the magnitude of the GPP increase inferred by Campbell et al. (2017). (Note 2017a and 2017b are duplicates). The COS-inferred increase in GPP proportionally matches the increase in CO2 concentration over the last century, implying a maximal response of global photosynthesis to changing CO2 (i.e. insignificant down-regulation of photosynthetic capacity and an increase in leaf-area that off-sets CO2-induced stomatal closure globally). Cernusak LA, et al. (2019) Robust Response of Terrestrial Plants to Rising CO2 Trends Plant Sci. 24(7):578-586. [Vanessa Haverd, Australia]	Taken into account. The duplicate reference of Campbell et al. (2017) is corrected. However, Cernusak et al. (2019) is not included, because it is on photosynthesis, rather than net land CO2 sink.
47728	22	51	22	51	Please check the use of this IPCC uncertainty language term. Are you able to provide a traceable account to assigning this uncertainty statement? Note that likelihood statements are quantified terms - phrases like likely and very likely have quantifiable probabilities associated with them. Please check it has been used correctly here. Please refer to the IPCC guidance note on uncertainty: <a href="https://wg1.ipcc.ch/SR/documents/ar5_uncertainty-guidance-note.pdf">https://wg1.ipcc.ch/SR/documents/ar5_uncertainty-guidance-note.pdf</a> [WGI TSU, France]	Accepted. The sentence is changed to "Increasing strength of global net land CO2 sink since 1980s is mainly driven by the fertilisation effect from rising atmospheric CO2 concentrations (medium to high confidence)"
32740	22	51	22	52	Again, a need to be very careful in how this is stated. Is the uptake determined by the gradient of the present CO2 concentration to the biosphere as in equilibrium with the preindustrial CO2 concentration, or is it driven by the gradient of the present atmospheric CO2 concentration to the biosphere as it has changed and adjusted to the rising CO2 concentration. This difference matters if one then considers what happens when one goes to zero emissions--will the net uptake continue at the present absolute rate or will it drop to near zero within a few years as the biospheric adjustment catches up to the prevailing CO2 concentration. I'd suggest, based on results from FACE experiments, that the latter is the case, namely that while the biosphere will show a fertilization effect with CO2 increase, it only takes several years for the biosphere to adjust and come into equilibrium with the higher CO2 level, at which point the uptake returns to the level before the uptick in the CO2 concentration. In any case, it is really important to state the dependencies very, very carefully and in ways that make sense for situations with a rising, steady and declining atmospheric CO2 level (and so rate of emissions). [Michael MacCracken, United States of America]	Taken into account. We accordingly specify the time-period discussed here is since 1980s, during which atmospheric CO2 concentration increases rapidly.
13550	22	51	23	5	The relative importance of CO2 fertilization, N-deposition and climate warming has been studied recently for the historical period and future scenarios in these two papers:1) N. Devaraju, G. Bala, K. Caldeira and R. Nemani, 2015: A model based investigation of the relative importance of CO2-fertilization, climate warming, nitrogen deposition and land use change on the global terrestrial carbon uptake in the historical period, Climate Dynamics, DOI 10.1007/s00382-015-2830-8; 2) Thejna, T., G. Bala, N. Devaraju and R. Nemani, 2018: Potential roles of CO2 fertilization, climate warming, nitrogen deposition and land use and land cover change on the global terrestrial carbon uptake in the 21st century, Climate Dynamics, <a href="https://doi.org/10.1007/s00382-018-4388-8">https://doi.org/10.1007/s00382-018-4388-8</a> [Govindasamy Bala, India]	Taken into account. Devaraju et al. (2016) is added to the citation list. The future projection is, however, beyond the scope of this section.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
36288	22	55	23	1	This sentence just says that the influence of climate change on the net land CO2 sink is divergent across DGVMs, without saying anything about its sign or likely magnitude. Is the sign consistent across DGVMs? If so, report the sign. Or if not, then say that even the sign of the effect is divergent across models. [Nathan Gillett, Canada]	Taken into account. It is revised as "The contribution of climate change to changes of the global net land CO2 sink is divergent across DGVMs that even the sign of the effect is not the same".
13268	23	22	23	22	Computing a standard deviation on 4 numbers (for the inversions), and even less for the first years, is not appropriate. The range should be used. [Frederic Chevallier, France]	Accepted. Range of inversions are used in the revised figure.
23412	23	27	23	30	to explain the net land CO2 increase or to mitigate it? I think the mitigation of land CO2 increase is through greening [Gwenaelle GREMION, Canada]	Taken into account. We clarify that the paragraph is to explain the acceleration of net land CO2 sink in the recent decades.
13552	23	27	23	30	The relative importance of CO2 fertilization, N-deposition and climate warming has been studied recently for the historical period in NCAR CESN: N. Devaraju, G. Bala, K. Caldeira and R. Nemani, 2015: A model based investigation of the relative importance of CO2-fertilization, climate warming, nitrogen deposition and land use change on the global terrestrial carbon uptake in the historical period, Climate Dynamics, DOI 10.1007/s00382-015-2830-8; The importance of Nitrogen deposition was also assessed in idealized model simulations in this paper: G. Bala, N. Devaraju, R. K. Chaturvedi, K. Caldeira, R. Nemani, 2013: Nitrogen Deposition: How important is it for global terrestrial carbon uptake? Biogeosciences, 10, 7147–7160, doi:10.5194/bg-10-7147-2013 [Govindasamy Bala, India]	Taken into account. Devaraju et al. (2016) is added to the citation list. The future projection is, however, beyond the scope of this section.
23410	23	27			substantiate with literature [Gwenaelle GREMION, Canada]	Accepted. Piao et al. (2018a) is added.
36290	23	27			This acceleration of the trend in the net land CO2 sink since the 1990s is far from clear to me from Figure 5.9. 'Acceleration' implies to me a positive second derivative of the land sink with respect to time. The graph just appears to show an upward trend. What quantitative analysis underlies this assessment? [Nathan Gillett, Canada]	Taken into account. The analyses were performed by Piao et al. (2018a), which is added to the sentence.
45358	23	33	24	1	(5.2.1.4.2) "interannual" is a risky term when so many of these effects are frequency dependent, perhaps define the term? [Peter Rayner, Australia]	Accepted. "interannual variability" is explained following the recent review by Piao et al.
17450	23	34	23	34	Change 'Interannual' to 'interannual' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
47830	23	34	24	38	Are you able to add assessment language to this statement? [WGI TSU, France]	Accepted. Assessment language is added.
43882	23	36	23	36	"semi-arid ecosystems over the tropics" is inappropriate, because "semi-arid" is a part of the arid (or dry) climate zone and the cited studies (Poulter et al., 2014; Zhang et al., 2018a) did not intend "semi-arid ecosystems over the tropics". Thus "over the tropics" in this description should be deleted. [Tomoko Nakano, Japan]	Taken into account. A recent review paper clearly indicated that the semi-arid over the tropics, rather than the extra-tropics, are mostly responsible for global IAV.
43884	23	37	23	37	Korth et al., 2015 is seemed to be inappropriate for being cited here, because the title of this article shows that this study is related to Mercury's magnetosphere, not land-atmosphere CO2 exchange. [Tomoko Nakano, Japan]	Accepted. It is replaced by Ahlström et al. (2015).
43326	23	38		47	Requires sentence reduction [Onema Adojoh, United States of America]	Accepted. The sentence is reduced for conciseness.



Comment ID	From Page	From Line	To Page	To Line	Comment	Response
13768	23	39	23	41	It could eventually be worth referencing the work by Hastie et al. (Global Change Biology, 2019 <a href="https://doi.org/10.1111/gcb.14620">https://doi.org/10.1111/gcb.14620</a> ) who suggested that integrating the aquatic C cycle with the terrestrial carbon cycle dampens the interannual variability of the NEP in the Amazon basin. [Pierre Regnier, Belgium]	Rejected. The recommended reference is out of the spatial scale of this section, which is at the scale of the globe and large latitudinal bands.
13286	23	44	23	44	Satellite measurements (with the measurement definition from BIPM) are radiances. The text refers here to satellite retrievals or, vaguely said, to satellite observations, not to satellite measurements. [Frederic Chevallier, France]	Accepted - text revised
22470	23	49	23	49	varies by or varies up to? [Gwenaelle GREMION, Canada]	Rejected. It is "varies by". The difference among reported values are a bit larger than two-folds
9660	23	53	23	55	Suggestion is to change "Distinguishing the relative contribution of moisture and temperature anomalies in carbon cycle variability, remain challenging, not only because of the covariations between anomalies of temperature and that of moisture" to "Distinguishing the relative contribution of moisture and temperature anomalies to carbon cycle variability remains challenging, not only because of the covariations between anomalies of temperature and that of moisture" [Brian Magi, United States of America]	Accepted. Texts are revised accordingly.
13270	24	10	24	10	Computing a standard deviation on 4 numbers (for the inversions), and even less for the first years, is not appropriate. The range should be used. [Frederic Chevallier, France]	Accepted. Range of inversions are used in the revised figure.
17452	24	21	24	21	delete , after al. [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - No longer applicable
42810	24	24			After Figure 5.10, we should mention current mismatches between top-down and bottom-up estimated CO2 fluxes such as Kondo et al., 2018 by adding some sentences and a figure. [Takashi Maki, Japan]	Taken into account. This belongs to section 5.2.1.5 (CO2 budget), and the current mismatches between top-down and bottom-up estimates have been detailed as the budget imbalance.
42812	24	24			After Figure 5.10, we should summarize current understandings and future expectations of satellite-based top-down CO2 fluxes such as Sander et al., 2015. [Takashi Maki, Japan]	Taken into account. Currently, the studies of satellite inversion focus on the spatial pattern of CO2 budget, not on interannual variability and trends due to limited years of available data. However, we include the progress made through satellite based inversions (Houwelling et al., 2015)
46868	24	26	24	26	An equivalent section for ocean carbon model might be useful. Tjiputra et al. 2016 and Kwiatkowski et al. 2017 also show emergent properties that can be used to constrain model projections/results. [Roland Séférian, France]	Taken into account - this is in section 5.4.6 which covers emergent constraints
47236	24	26			Beyond the evaluation of new processes and degrees of freedom, the evaluation of basic processes such as the water control on the terrestrial water cycle suggests serious model deficiencies (e.g. Humphrey et al. 2018 already quoted in Chapter 5). [Hervé Douville, France]	Not Applicable. The model evaluation section is no longer included in the revised text.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
47336	24	26			for symmetry, should the ocean section have a parallel evaluation paragraph? [chris jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The model evaluation section of land model was cancelled for symmetry as agreed in the chapter author meeting.
9662	24	28	24	28	suggestion is to change "[CO2]" to "CO2 concentration" [Brian Magi, United States of America]	Accepted - text revised [we changed to concentrations, plural]
48142	24	29	24	30	"The DGVM ensemble mean can generally reproduce the sensitivity of global net land 30 CO2 sink to interannual temperature variations": this statement is not really substantiated by the references which both show considerable deviations of individual models from obs-based apparent T-sensitivies and don't present the model ensemble mean. [Vanessa Haverd, Australia]	Taken into account. The sentence is revised to "Most DGVMs can reproduce the sensitivity of global net land CO2 sink to interannual temperature variations (yIAV) within its observational uncertainties ". Since the section of model evaluation is being incorporated into the sections of trends and variability, it is moved to the section of 5.2.1.4.2 (interannual variability in land-atmosphere CO2 exchange).
48144	24	31	24	31	This is an over-optimistic presentation of modelling capability to simulate the increase in NH seasonal amplitude in CO2. Graven et al. (2013) and Thomas et al. (2016) show that models consistently underestimate the amplitude increase, and can't capture the present-day amplitude either. Bastos et al. (2019) show that the TRENDYv7 ensemble underestimates the NH increase in sesonal amplitude compared with atmospheric inversion based-estimates. Refs: Bastos A, et al. (2019) Contrasting effects of CO2 fertilisation, land-use change and warming on seasonal amplitude of northern hemisphere CO2 exchange. Atmos. Chem. Phys. Discuss.; Graven HD, et al. (2013) Enhanced Seasonal Exchange of CO2 by Northern Ecosystems Since 1960. Science 341(6150):1085.; Thomas RT, et al. (2016) Increased light-use efficiency in northern terrestrial ecosystems indicated by CO2 and greening observations. Geophys. Res. Lett. 43(21):11,339-311,349. [Vanessa Haverd, Australia]	Taken into account. The sentence is revised to reflect that ensemble model mean can reproduce seasonal CO2 amplitude change since 1980s (Forkel et al., 2016; Piao et al., 2018), but not the seasonal CO2 amplitude change since 1960s (Graven et al., 2013; Thomas et al., 2016). Since the section of model evaluation is being incorporated into the sections of trends and variability, it is moved to the section of 5.2.1.4.1 (trend in land-atmosphere CO2 exchange).
9664	24	31	24	31	suggestion is to change "[CO2]" to "CO2 concentration" [Brian Magi, United States of America]	Accepted - text revised [we changed to concentrations, plural]
55790	24	31	24	31	Does [CO2] here mean concentrations of CO2 as in the chemistry usage? "CO2 concentrations" should be spelt out if so to make this more accessible. [Christopher Smith, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
31290	24	31	24	32	The observed increase in amplitude of the seasonal cycle of CO2 in high latitudes is not correctly modelled by DGVMs in general. See Thomas et al. (2016) GRL. [Iain Colin Prentice, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. See also the response to Comment ID 48144.
48138	24	32	24	32	replace "The ensemble" by "the mean of the ensemble" [Vanessa Haverd, Australia]	Accepted. "ensemble mean" is now used.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
39764	24	32	24	37	can you number the uncertainties when you state that you can robustly model the .... It would be interesting to give some information about the differences in processes and parameters of the different DGVM models in the cluster, maybe in the form of a table. [Dagmar Henner, Austria]	Taken into account. This table is available from Le Quere et al. (2018). However, given the limited page room, we refer the author to Le Quere et al. (2018) for detailed model information.
24598	24	35	24	37	Using DGVMs for the land sink seems a large change from AR5. Should this be raised to an ES point? [William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. The lead author meeting decides not to include the point into the executive summary.
17454	24	37	24	37	Delet one ( [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)])	Accepted - text revised
47832	24	39	24	40	Can a citation be added here? [WGI TSU, France]	Accepted. Le Quere et al. (2018a) is added.
17456	24	40	24	40	Change 'earth' to 'Earth'. There is inconsistency with captialisation of 'systems' in the context of 'Earth Systems'. My view is that it should be capitalised [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised for earth to Earth. "systems" is not capitalised but we made it consistent throughout the chapter.
47338	24	40			could show a table of which models include processes such as nutrients? [chris jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. This table is available from Le Quere et al. (2018). However, given the limited page room, we refer the readers to Le Quere et al. (2018) for detailed model information.
36828	24	43	24	43	Around here, it is worth mentioning about the model intercomparison of fires in DGVMs, i.e. FireMIP, considering the importance of biomass burning in the global carbon cycle (e.g., Forkel et al. 2019). Forkel, M., Andela, N., Harrison, S.P., Lasslop, G., van Marle, M., Chuvieco, E., Dorigo, W., Forrest, M., Hantson, S., Heil, A., Li, F., Melton, J., Sitoh, S., Yue, C., Arneeth, A., 2019. Emergent relationships with respect to burned area in global satellite observations and fire-enabled vegetation models. <i>Biogeosciences</i> 16, 57–76. [Akihiko Ito, Japan]	Accepted. One sentence is added describing progress on fire simulations in DGVMs, with recommended reference included.
22472	24	45	24	45	Worth adding this reference here: doi: 10.1038/d41586-019-01313-4 [Gwenaelle GREMION, Canada]	Taken into account. It is a commentary not peer-reviewed, we cite Schurer et al., 2015 and McGuire et al., 2018 instead.
48140	24	47	24	47	consider replacing "include management practices" by "include management practices (Pongratz et al 2018b) and the dynamics of secondary forest regrowth as influenced by forest age structure (Pugh et al. 2019)" [Vanessa Haverd, Australia]	Accepted. Revised as suggested.
36830	24	47	24	47	Thurner et al. (2017) does not appear in References (but only 2014 one). I guess the below one should be there. Thurner, M., Beer, C., Ciais, P., Friend, A.D., Ito, A., Kleidon, A., Lomas, M.R., Quegan, S., Rademacher, T.T., Schaphoff, S., Tum, M., Wiltshire, A., Carvalhais, N., 2017. Evaluation of climate-related carbon turnover processes in global vegetation models for boreal and temperate forests. <i>Global Change Biol.</i> 23, 3076–3091. doi:10.1111/gcb.13660 [Akihiko Ito, Japan]	Accepted.
17458	24	48	24	48	Capital E for 'earth' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
56630	24	53			For the readers of the IPCC report, the fact that "carbon budget" has been used by two of our sub-communities for different things (i.e. explaining/closing the carbon cycle pools and fluxes or calculating the "remaining carbon budget") continuously leads to confusion. Is there a possibility to gently introduce a terminology that distinguishes better among the two? For example, the headline for section 5.2.1.5 already goes into that direction by calling the carbon cycle one "CO2 budget"... Suggestion: Option (1): Call the carbon cycle budget consistently "CO2 budget" and keep "carbon budget" term for the cumulative emissions vs temperature issues. Option (2), introduce even more distinction by calling the former "Carbon Cycle budget" or "The carbon cycle's budget" or similar. I fully acknowledge that both communities used the term carbon budget now for a long time, and also "methane budget" is used, but maybe it is worth to start adding clarity by adding "cycle" to the name of those budgets (e.g. "carbon cycle budget", "methane cycle budget" vs. the "remaining carbon budget" that policy makers are mostly concerned about) from the time of AR6 onwards. [Malte Meinshausen, Australia]	Accepted. We use CO2 budget, and we'll use "remaining carbon budget" for the other.
17460	24	56	24	56	Change 'era' to 'Era' and 'Industrial era' to 'Industrial Era' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
9666	25	1	25	1	change "concentrations" to "concentration" [Brian Magi, United States of America]	Accepted - text revised
36292	25	1		6	The description here of a budget 'constructed for the first time in the IPCC' makes it sound as though the chapter is doing novel analysis rather than assessing the published literature. [Nathan Gillett, Canada]	Accepted. Changed.
13570	25	2	25	6	Congratulations to the global carbon cycle team!! I believe this (independent estimate of land carbon sink) is a major achievement in the history of carbon cycle science. This may be highlighted in the executive summary. [Govindasamy Bala, India]	Thanks
23414	25	6			there should be an equation number to represent this for proper referencing. [Gwenaelle GREMION, Canada]	Rejected. It is the only equation in the section; not needed.
37734	25	8	25	10	See comment 231 - some of the carbon taken up by the ocean and terrestrial ecosystems during 2008-2017 was carbon that was already in the atmosphere at the beginning of 2008. That carbon was replaced by some of the carbon emitted between 2008 and 2017, and emissions also accounted for an additional 47TgC accumulated in the atmosphere and for the remainder of the carbon taken up by ocean and terrestrial ecosystems. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We have change the sentence to reflect this information
49118	25	8	25	10	Naïve WG III query here. When you say that 29% of the C emitted 2008-17 was taken up by terrestrial systems, don't you mean the terrestrial-atmosphere flux was 29% of emissions (the language in Table 1.5)? This is not quite the same thing as the C taken up may be stuff that has been up there for decades/centuries. Phrasing it this way can result in confusion among non-scientists about the role of terrestrial ecosystems. [Jim Skea, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We have change the sentence to reflect this information

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
36296	25	8	25	11	Because no uncertainties are given on the carbon fluxes quoted, it is impossible to properly interpret the meaning of this imbalance of 0.5 PgC/yr. Do the estimated uncertainties in each of the terms in the budget add (in quadrature, assuming uncertainties are independent) to less than 0.5 PgC/yr, 5-95% range? If so, then the apparent imbalance is within the expected range based on individual uncertainties, and lines 10-11 could be replaced with a statement that this budget is closed to within observational uncertainty. Or does the 0.5 PgC/yr exceed the sum of the uncertainties? If so, then keep the text on lines 10-11 as it is, and add that an alternative explanation is that the uncertainties in one or more terms are underestimated. There is currently no confidence assessment for the budget on lines 8-10, but as well as uncertainties, one should be added. Note that if the budget is closed to within obs uncertainty this will increase confidence, compared if the budget is not closed to within obs uncertainty. [Nathan Gillett, Canada]	Accepted. Uncertainties provided.
23416	25	8			syntax is very odd. Start wit proper wording [Gwenaelle GREMION, Canada]	Accepted. Sentence restructured.
29410	25	9	25	9	"22% were taken up by the ocean". See my previous comment 11. Here the numbers from the GCB are used and the percentages calculated as referd to total emissions from fossil-fuels AND land-use change. Please be consistent in using the same numbers throughout the report, else it'll be confusing for the reader. [Judith Hauck, Germany]	Accepted. Numbers updated for consistency.
29474	25	10	25	10	should state whether or not the imbalance term (0.5 PgC/y) is within the uncertainty [Rona Thompson, Norway]	Noted. Text addresses the issue
23418	25	11	25	13	is there still need for this text. I think it have been well highlighted in the previous sections of the draft. If really it should be kept, I suggest that the text be substantiated with literature and merged with section 5.2.1.4.2 [Gwenaelle GREMION, Canada]	Accepted. Removed from here and consolidated on new text un Atmospheric subsection on the airborne fraction.
36298	25	12			The land and ocean carbon sinks respond directly to the CO2 concentration of the atmosphere, not to the annual emissions, to which this is only indirectly linked. Would it also be correct to say that the land and ocean sinks increased largely in proportion to the increase in atmospheric CO2? If so, I would recommend saying this, because the physical link is to atmospheric CO2, not directly to the emissions. [Nathan Gillett, Canada]	Accepted. Changed as suggested.
17462	25	15	25	15	Change 'era' to 'Era' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
23420	25	15	25	16	substantiate with literature [Gwenaelle GREMION, Canada]	Accepted. Added.
17464	25	15	25	17	Don't split numbers and units across a line [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account
22474	25	16	25	16	Please rewrite '235+95 PgC from land use change' as 'emission from land use change was 235+95 PgC'. [Gwenaelle GREMION, Canada]	Accepted - text revised
51946	25	16	25	18	It is implausible that the sink partitioning has no uncertainty. Uncertainties should be added to the sink partitioning estimates for completeness. [Peter Thorne, Ireland]	Accepted. Added.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
33486	25	17			This conflicts with the estimate reported on page 7 line 17. This chapter should use consistent CO2 uptake estimates throughout and include the time period that estimate refers to. [Adrienne Sutton, United States of America]	Noted. There are two different periods on which the fraction has been derived. We have now made clear the period, the one in the abstract, the past decade, and here since the beginning of the industrial revolution.
22476	25	22	25	22	Drake et al 2018 is a strange reference here, the only refernce in the article of soil-to-river freshwater carbon flux is in the introduction and is based on older references than the more appropriate Resplandy et al reference. Suggest deleting Drake reference here. [Gwenaelle GREMION, Canada]	Accepted. Replaced.
36300	25	22			Make clear that this carbon flux includes the natural flux - it is not just the anthropogenic perturbation. [Nathan Gillett, Canada]	Accepted. Made clear.
51552	25	24	25	24	CO2 outgassing reference Raymond et al. 2013 doi:10.1038/nature12760; NH wetlands/lakes CH4 outgassing reference Wik et al 2016 doi: 10.1038/NGEO2578 [Christian Beer, Germany]	Accepted. Added.
9668	25	24	25	24	seems like "via outgassing in lakes, rivers and estuaries" requires a citations to support the statement [Brian Magi, United States of America]	Accepted. Citation added.
22478	25	24	25	24	Suggest adding the following refence at the sentence ending on this line: doi: 10.1007/s10021-018-0284-7 [Gwenaelle GREMION, Canada]	Accepted. Added.
49008	25	24	25	26	This sentence is not clear: "Thus, the net export of carbon from the terrestrial domain to the open oceans is likely to be small (0.1 PgC yr <sup>-1</sup> , (Regnier et al., 2013)) ...", But according to Fig. 1 in (Regnier et al., 2013), the number 0.1 is about the estimate for anthropogenic perturbation, but the total carbon export from fresh water to open ocean is up to 0.75, including 0.6 from the natural system, why here only use 0.1 instead of 0.75? [Minchao Wu, Sweden]	Noted. Numbers changed accordingly
47730	25	25	25	25	Please check the use of this IPCC uncertainty language term. Are you able to provide a traceable account to assigning this uncertainty statement? Note that likelihood statements are quantified terms - phrases like likely and very likely have quantifiable probabilities associated with them. Please check it has been used correctly here. Please refer to the IPCC guidance note on uncertainty: <a href="https://wg1.ipcc.ch/SR/documents/ar5_uncertainty-guidance-note.pdf">https://wg1.ipcc.ch/SR/documents/ar5_uncertainty-guidance-note.pdf</a> [WGI TSU, France]	Accepted. Changed to "medium confidence"
17466	25	38	25	38	delete , after al. [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - No longer applicable
17468	25	39	25	39	Change to Pre-Industrial for consistency elsewhere in the text and other Chapters [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - The same spelling as in the IPCC SR1.5 / SROCC has been applied

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
13770	26	1	26	4	This is an important comment: I strongly recommend modifying the current sentence by : " export of carbon from soils to rivers, burial of carbon in freshwater lakes, reservoirs, ESTUARIES AND COASTAL ECOSYSTEMS and transport of carbon by rivers to the OPEN ocean are all assumed to be pre-industrial fluxes and are sourced from (Regnier et al., 2013)." Indeed the numbers as currently in the figure include the contribution from estuaries (incl. marshes & mangroves) and coastal ocean, which are significant. [Pierre Regnier, Belgium]	Accepted. Sentence modified.
47066	26	9	26	19	Table 5.1: Make it more visible that the time span in the first column represent the entire time period and that the following comlumns only represent sub-periods. [Sophie von Fromm, Germany]	Accepted. We have added a larger separation line between the two blocks.
13572	26	11	26	16	It may be mentioned in the table caption that the net land sink is now estimated from TRENDY Models and not as a residual of the global carbon budget. [Govindasamy Bala, India]	Noted/Rejected. The figure legend doesn't provide the methods to estimate the various fluxes but the main text makes it clear now.
30462	26	11	26	16	in table 5.1 caption, the meaning of superscripts (A-F) is missing [Edoardo Cremonese, Italy]	Accepted. Added.
27224	26	11	26	17	Please add units in Table 5.1 [François GERVAIS, France]	Accepted. Added.
13662	26	11	26	17	Table 5.1: Units are missing, content of footnotes a-f are missing [Peter Köhler, Germany]	Accepted. Added.
47340	26	11			table 5.1 lacks units - I assume the first column of numbers are a total (in PgC) ad the remainder are rates per year (PgC/yr)? [chris jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Added in table.
25664	26	16	26	16	label units: Pg, Pg yr-1 [Stephen E Schwartz, United States of America]	Accepted. Added.
17470	26	16	26	17	Please give details of the superscripts a-f in the legend or as a footnote [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Added.
23422	26	16	26	17	I suggest improving the presentation of the table. It doesn't communicate any meaning at its present form. Addition of notes should be maintained. [Gwenaëlle GREMION, Canada]	Accepted. Improved.
47828	26	22	32	37	Please ensure the methane trends and budgets etc are an update from the Special Report on Land (SRCL). The relevant SRCL section is 2.4.2. Please add a call out to the Special Report once it is published. [WGI TSU, France]	Accepted. A couple of sentences are added at the end of the first paragraph referring to AR5 and SRCL
24600	26	22			Section 5.2.2: Some of this methane description is repeated in section 6.2.2.4, the authors of chapters 5 and 6 should confer. This section could be more assessment and less review. Some of the papers cited are reviews in themselves which has the danger of turning this into a review of reviews. As an example: the citation for 90% of methane destroyed by OH is given as Saunois et al. 2016, however Saunois in turn give a citation of Ehhalt 1974 for this 90% figure. This section should focus more on assessing (for each term) what we know now that we didn't in AR5 and how we know it (better data, better conceptual understanding ...). Is there agreement/disagreement between different studies or between models and measurement. [William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Text improved and original references provided.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
24602	26	24	26	25	The variability in CH4 is "entirely" a result of net balance between emissions and losses, and not just seasonal to interdecadal, but on all timescales - by definition. [William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Sentence revised and time scales moves to a new sentence
22480	26	25	26	25	'surface emissions': specify which surface (e.g. Earth surface) [Gwenaelle GREMION, Canada]	Accepted - changed to "Earth's surface emissions and chemical losses in atmosphere"
24604	26	25	26	26	It wasn't clear what the sentence starting "Atmospheric transport only ..." is trying to say. [William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Sentence revised
37736	26	25	26	26	The sentence that spans these lines does not apply to the stratosphere. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Sentence revised for generality
24606	26	26	26	28	It seems that the +/- values here are the interannual variability rather than an uncertainty on the measurements. In which case it may be better to give ranges rather than mean+/- . [William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Value ranges are given.
32742	26	27	26	27	Four figure precision seems a bit too much when one says "about" in front of the number. [Michael MacCracken, United States of America]	Noted. We now have given ranges for the values
13574	26	27	26	28	The CH4 lifetime of 10 years can be inferred from the burden and flux (source or sink). This lifetime may be stated here. [Govindasamy Bala, India]	Accepted. The estimated lifetime is given
22482	26	27	26	28	Sentence should read: of CH4 is about 5001±54 Tg, with an emission and loss of about 543±18 and 522±8 Tg yr-1 during 2007–2016, respectively (1Tg = 1012g; 1-± standard deviation for the interannual variations is shown as the range). [Gwenaelle GREMION, Canada]	Noted
45646	26	27			Update burden to 2018. [Euan Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Updated.
29478	26	28	26	28	need a reference for the total emissions and sink - are these numbers based on an ensemble of models - and what kind of model? The uncertainties are based on 1 SD of inter-annual variations but the model-to-model differences are larger than this (see Saunio et al. ESSD, 2016, and references therein) [Rona Thompson, Norway]	Accepted. Values are given based on multi-model mean from Saunio et al., 2019
24608	26	28	26	32	Are these percentages the same as in AR5? Have Patra et al. 2016 or Saunio et al. 2016 used any new sources of information (new models / new data) or simply cited previous estimates (it seems the 90% comes from Ehhalt 1974).? Do Patra et al. and Saunio et al. (or the sources within them) agree on these values? What's the uncertainty? Section 5.2.2.3 states 5% rather than 6% for soils. [William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Yes, these values are available from Saunio et al. and Patra et al. We have given range of the estimations based on multiple models
22484	26	29	26	29	please add a brief explanation on 'soil oxidation' [Gwenaelle GREMION, Canada]	Accepted - changed to "by bacterial soil oxidation". We lack space to explain each of the emissions separately.



Comment ID	From Page	From Line	To Page	To Line	Comment	Response
29480	26	31	26	31	Loss of CH4 due to oxidation by Cl radicals is actually predominantly in the troposphere (not in the stratosphere). This is simply because CH4 is much more abundant in the troposphere, so the loss there accounts for ~90% of the total loss, of which 3 to 7% is due to Cl (Brenninkmeijer et al., JGR, 1995; Gettelman et al. JGR, 1997) [Rona Thompson, Norway]	Noted. but that is not likely true anymore! please see the paper by Gromov et al. in ACP 2016, which include Carl Brenninkmeijer as a coauthor
22486	26	32	26	34	It is virtually certain that CH4 emissions are dominantly anthropogenic, as also shown in Table 5.2 and later in section 5.2.2; This phrasing seems however to suggest that this is not known / uncertain [Gwenaelle GREMION, Canada]	Noted. We meant to say that some sources cannot be clearly marked as anthropogenic or natural. One example is the rice fields, which could otherwise behave like natural wetland.
24610	26	34	26	34	This statement on the categories of emissions sources seems a bit out of place. Is it even necessary (these terms only seem to be referred to in box 5.1)? [William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Noted. We believe it is good to have these mentioned early on
24612	26	37	26	39	The sentence on emissions exceeding losses doesn't seem to convey a useful message. For methane the sources and sinks are in approximate balance (within 10 Tg/yr). What is important is that emissions have increased by a factor of more than 2 over the last two centuries, so to maintain balance the abundance has increased correspondingly. [William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We revised the sentence as "CH4 emissions have nearly doubled and persistently exceeded the losses "
22488	26	44	26	44	1970s: why not give the actual starting year of the measurements? [Gwenaelle GREMION, Canada]	Rejected. Giving specific year is difficult as the measurements were made sporadically in the 1970s. We have tried our best to gather all longer-term measurements in making the time series Figure but some exclusions still possible.
13278	26	44	26	44	Fig 5.2 includes GOSAT retrievals, but these are not direct measurements of CH4 in the atmosphere in contrast to what the text suggests. [Frederic Chevallier, France]	Accepted. Sentence in the figure caption modified as "...XCH4 (total-column), retrieved from radiation spectra measured by the Greenhouse..."
22490	26	44	27	11	Isotopic data are not at all discussed in section 5.2.2.1 even though they are presented in Figure 5-12. A brief discussion of the trends and what they can be used for (like done in 5.2.3.1 for N2O) including a reference to cross-chapter box 5.1 could be helpful [Gwenaelle GREMION, Canada]	Accepted. We have added a sentence at the end of this para
24614	26	45	26	45	Is this growth rate consistent with that quoted in chapter 2? Chapter 2 should be referenced here. [William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We have cross referenced the data with Chap 2
22492	26	46	26	46	please add a brief explanation on 'green revolution' [Gwenaelle GREMION, Canada]	Accepted. This sentence is modified as "rapid CH4 growth was observed following the green revolution for increased crop-production and fast pace of industrialisation, because that period experienced rapid increases in CH4 emissions from"

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
33326	26		26		Table 5.1 is very interesting but would be even better if additional time periods were included (esp. 1750-1849; 1850-1899; 1900-1979) [Erika Wise, United States of America]	Noted. We don't have the detail information for previous decades but information is provide now on previous years.
36302	26				Table 5.1: For the 1750-2017 period, the uncertainties in the land-to-atmosphere flux alone are apparently +/- 210 PgC. Given that this is one of the terms comprising the carbon budget, we would therefore expect the apparent budget imbalance to be comparable to this. While certainly possible, therefore a 5 PgC/yr imbalance, is small compared to the summed uncertainties in the balance term. This term does not require further explanation - it is small compared to what would be expected based on the uncertainties in the individual terms in the budget. Also, based on this, having a separate row labelled 'budget imbalance' may give this number undue prominence. [Nathan Gillett, Canada]	Noted. Improved table.
29482	27	1	27	1	"oil and gas exploration" I think what is meant here is "exploitation". The term "exploration" (according to IPCC sector definitions) only applies to the drilling of wells etc. for prospecting for oil and gas, and does not include the by far larger emissions associated with extraction, storage and transport. [Rona Thompson, Norway]	Accepted. We have changed to "oil and gas industry" to be more general
22494	27	2	27	3	This point could use more references regarding the decline in gas flaring and correlation to oil prices. [Gwenaelle GREMION, Canada]	Noted. We have given one key reference, and this is sort of an accepted matter of fact and included in the emission inventories such as the EDGARv432
47732	27	3	27	3	Please check the use of this IPCC uncertainty language term. Are you able to provide a traceable account to assigning this uncertainty statement? Note that likelihood statements are quantified terms - phrases like likely and very likely have quantifiable probabilities associated with them. Please check it has been used correctly here. Please refer to the IPCC guidance note on uncertainty: <a href="https://wg1.ipcc.ch/SR/documents/ar5_uncertainty-guidance-note.pdf">https://wg1.ipcc.ch/SR/documents/ar5_uncertainty-guidance-note.pdf</a> [WGI TSU, France]	Noted. Yes, our model simulations and inventory estimations agree well. References given. We have rewritten sentence to more clearly show the traceability with the multi model agreement.
45642	27	5			cite Dlugokencky, Edward J., et al. "Global atmospheric methane: budget, changes and dangers." Philosophical Transactions of the Royal Society A: Mathematical, Physical and Engineering Sciences 369.1943 (2011): 2058-2072. [Euan Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Accepted
37738	27	5			It is strange to have references with publication dates as old as 2003 and 1992 relating to issues that "are debated", especially as the issues include "persistence through 2006". Could this be simplified by referring the reader back to AR5 instead? [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Text modified using AR5 and SRCL documents
17472	27	7	27	7	Change to '..due to a lower surface temperature..' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted
29484	27	9	27	9	I think further references should be given concerning both the pause in CH4 growth rate in the late 1990s and early 2000s and the subsequent increase in growth rate after 2006. [Rona Thompson, Norway]	Noted. Text modified using AR5 and SRCL documents instead of older references

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
43610	27	9	27	11	Several papers argue that asking about the cause of the renewed increase of CH <sub>4</sub> is the wrong question, given that one could say that CH <sub>4</sub> trends have only returned to normal, and the question we need to ask is why there was abnormally low growth for the decade prior. It doesn't invalidate any of the discussion of sources and sinks but gives a different flavour of what the actual scientific puzzle is. [Andy Reisinger, New Zealand]	Noted. Yes, one could ask this question differently. We discuss here the causes of the "normal" CH <sub>4</sub> growth rate since the 1970s, then the decrease in the 1990s and regrowth since the late 2000s. You can see that the all these changes are related to different natural events and activities of human kind
45644	27	9			Maybe give actual growth rate numbers for the 2014-post years? [Euan Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Post-2007 growth rate is given
13280	27	16	27	16	Mole fractions are with respect to dry air. This should be made clear. [Frederic Chevallier, France]	Accepted. Modified as "CH <sub>4</sub> dry-air mole fraction "
41806	27	16	27	25	Fig 5.12: differences about 150 ppb are observed from site to site. An explanation of this difference would be welcome. [Marc Aubinet, Belgium]	Accepted. We mention about this inter-hemispheric gradient which imply that the emissions are human driven and more concentrated in the northern mid-high latitudes. Please see the first sentence of Section 5.2.2.2.
24616	27	16			Is the data in figure 5.12 different to figure 2.4c? This should be mentioned. [William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Noted. You mean Fig. 2.4b,e? Yes, we try to include more data sources, e.g., that going back to the 1970s. Also we show more data, e.g., NH and SH separately, GOSAT etc. The number of CH <sub>4</sub> related parameters are also more in our case.
45648	27	25			Maybe comment on the evidence that the isotopic trend to heavier values, that had lasted for 200 years or so, reversed in 2007 on. [Euan Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Added a sentence "However, an increasing contribution from biogenic emissions is evidenced from reversal of $\delta^{13}C$ trend to lighter values post 2007; the opposite of that lasted for prior 200 years."
22496	27	30	30	1	The structure and ordering of these sections is quite confusing. Section 5.2.2.2 is called 'anthropogenic emissions' but does not discuss all anthropogenic emissions, biomass burning & biofuel consumption are e.g. not discussed here. I would suggest to try to stick to the categories of Table 5.2 as much as possible for consistency. [Gwenaelle GREMION, Canada]	Accepted. A large fraction of all biomass burning is probably anthropogenic. We have moved that paragraph to the Anthropogenic emission section, and also the para on Rice cultivation
51948	27	30			In this section all figures are given as deterministic estimates, often with qualitative qualifiers implying uncertainty. Can instead ranges be given that are defensible by reference to / use of figures from table 5.2? Perhaps better integrating this table into the assessment text may help? [Peter Thorne, Ireland]	Taken into account. Yes, the ranges in Table 5.2 are more directly used in the text
22498	27	32	27	33	Transport is mentioned as a separate subcategory within Table 5.2, but what about excavation and pumping? Do they fall in the 'industry' subcategory in Table 5.2? [Gwenaelle GREMION, Canada]	Noted. Here "transport" means transport of the fossil fuels from the source to use locations .

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
41060	27	32	27	44	Worden et al. (2017) showed that Schwietzke et al. greatly underestimated fossil fuel emissions, by erroneously assuming that biomass burning has not been changing over time. Also, Petrenko et al. (2017) demonstrated that natural methane emissions from geological seeps are 35 to 50 Tg/yr less than assumed in many earlier studies (such as Saunio et al. and Etiope et al), and that fossil fuel emissions must therefore be correspondingly larger by this same amount (based on the C14 analysis by Lassey et al. 2007). This paragraph and Table 5.2 need to rely less on Saunio et al. and Etiope et al, and include this more recent literature. [Robert Howarth, United States of America]	Taken into account. We had added this sentence here "The apportionment of multiple CH4 source sectors using spatially aggregated atmospheric del13C data remained underdetermined to infer the global total emissions from fossil fuel industry, biomass burning, agriculture (Rice et al., 2016; Schaefer et al., 2016; Worden et al., 2017)."
47068	27	32	27	44	5.2.2.2: Might be helpful to explain the difference between top-down and bottom-up approaches. [Sophie von Fromm, Germany]	Accepted. We have introduced the concept in Section 5.2.2
41070	27	32	29	1	The report should cite Turner et al. (2016), who used satellite data to show that 30% to 60% of the large global increase in methane emissions since 2007 occurred in the United States. This is consistent with a larger fossil fuel source, and is not at all consistent with increased emissions from biogenic sources: wetlands are a small source for the US, and populations of cow and cattle were decreasing over this time. See <a href="https://agupubs.onlinelibrary.wiley.com/doi/full/10.1002/2016GL067987">https://agupubs.onlinelibrary.wiley.com/doi/full/10.1002/2016GL067987</a> [Robert Howarth, United States of America]	Rejected. Thank you for the suggestion. Unfortunately this results are not supported by regional inverse modelling studies. See for example Saunio et al., 2019 database. CH4 is a long-lived gas it is not easy to derive regional emission information from the trends differences between locations. For instance the growth rate over USA could be affected by emission increase from China, India etc.
41072	27	32	29	1	The report should cite Schneising et al. (). They used satellite data to infer a massive increase in methane emission from shale gas and oil development in the US since 2017. Schneising, O., Burrows, J.P., Dickerson, R.R., Buchwitz, M., Reuter, M., and Bovensmann, H., Remote sensing of fugitive emissions from oil and gas production in North American tight geological formations, Earth's Future, 2, 548–558, doi:10.1002/2014EF000265, 2014. [Robert Howarth, United States of America]	Taken into account. Similar to reply for comment #41070. It is very hard to cover regional studies on minor sectors in this report.
36304	27	32			I recommend reserving the word 'attributed' for use in the context of detection and attribution, as per IPCC WGI AR5 glossary. I suggest replacing 'attributed to' with 'originate from'. [Nathan Gillett, Canada]	Accepted
15178	27	34	27	35	Please consider making explicit what differentiates 'top-down' and 'bottom-up' estimations of fossil fuel emissions. Though the differences are alluded to elsewhere in this chapter (e.g. Page 28, Lines 9-14), it would be helpful to readers to explain it clearly here, as this is the introduction of these terms in this chapter and these terms are important in following sections. [Richard Vachula, United States of America]	Taken into account. Some of the inverse models explicitly optimize the sectorial emissions and some inverse models infer the rate of anthropogenic vs natural emission changes for given regions. The details can be found in the cited references, and thus a long explanation is omitted in this report (keeping brevity in mind)
45650	27	35			Mention Schwietzke et al paper? Schwietzke, S., Sherwood, O. A., Bruhwiler, L. M., Miller, J. B., Etiope, G., Dlugokencky, E. J., ... & Tans, P. P. (2016). Upward revision of global fossil fuel methane emissions based on isotope database. Nature, 538(7623), 88. [Euan Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. Sorry, these results are based on Saunio et al., the GCP synthesis, as given in the cited Table. In fact Schwietzke et al. have suggested a decrease in fossil fuel emissions

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
26838	27	38	27	40	A paper that might be relevant: <a href="https://www.atmos-chem-phys.net/19/7859/2019/acp-19-7859-2019.pdf">https://www.atmos-chem-phys.net/19/7859/2019/acp-19-7859-2019.pdf</a> [Ragnhild Bieltvedt Skeie, Norway]	Noted. This paper does not present the trends in emissions explicitly, which is needed for the discussions of CH4 growth rates. However, a fine modelling framework
41062	27	38			Satellite data show that the increase in methane emissions from coal in China has been relatively small, at 1.1 Tg/year (Miller et al.). Note that most of the coal mining in China is surface mined, and emissions from surface mines are far less than from deep mines, since the methane has been degassed over geological time. This is discussed in Howarth et al. (2011), and the 1.1 Tg/yr value from Miller et al. is quite consistent with the emission factor for surface-mined coal reported in Howarth et al. 2011. Miller, S.M., Michalak, A.M., Detmers, R.R., Hasekamp, O.P., Bruhwiler, L.M.P., and Schwietzke, S., China's coal mine methane regulations have not curbed growing emissions, Nat. Commun., 10, 303, doi:10.1038/s41558-019-0432-x, 2019. Howarth, R.W., Santoro, R., and Ingraffea, A., Methane and the greenhouse gas footprint of natural gas from shale formations, Climatic Change Letters, 106, 679–690, doi:10.1007/s10584-011-0061-5, 2011. [Robert Howarth, United States of America]	Taken into account. Miller et al. focus on the GOSAT period, which do not cover the CH4 regrowth and stabilisation period. We can check good consistency between the Miller et al. and other inversions going back to the 1990s/2000s. A new sentence is added.
29486	27	40	27	40	"faster emission increase" it's not clear what this is in relation to - the top-down or other? [Rona Thompson, Norway]	Accepted. Added "but a faster emission increase still persists between 2003–2007 in EDGARv4.3.2"
29488	27	41	27	44	Thompson et al, GRL, 2018 found a much smaller difference in the fossil fuel emissions with respect to inventories using the same d13C values as Schwietzke et al., so the Schwietzke et al. result appears to be very specific to their model set-up. [Rona Thompson, Norway]	Accepted. We have added a sentence here "The apportionment of multiple CH4 source sectors using spatially aggregated atmospheric 13C data remained underdetermined to infer the global total emissions from fossil fuel industry, biomass burning, agriculture (Rice et al., 2016; Schaefer et al., 2016; Schwietzke et al., 2016; Thompson et al., 2018; Worden et al., 2017). "
22500	27	41	27	44	This sentence appears to be incomplete and is not clearly written. I fail to understand what this sentences is trying to say and therefore can't suggest a better sentence. I specifically don't understand why the box model shows 20 to 60% greater fossil emissions than inventories (of which period?) and how this goes together with "a gradual decrease in fossil fuel emission". [Gwenaelle GREMION, Canada]	Noted. We have modified this sentence.
36306	27	41		44	The latter part of this sentence doesn't make sense. Also is the 25 Tg /yr figure the total change in fossil fuel emissions over the 2001-2014 period? As written it sounds like the figure is the trend in annual emissions, but if that's what it is, it should have units Tg / yr^2. [Nathan Gillett, Canada]	Noted. We are trying to say about the rate of emissions between the two periods. We understand the acceleration in the unit of Tg/yr^2 is more accurate but not easily appreciated by the readers

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
6908	27	42	27	44	<p>We can read that “methane emissions from natural gas, oil and coal production and their 43 usage are 20 to 60% greater than shown by the inventories”. Actually some studies delivered additional information pointing to gas extraction related hidden CH4 emissions. We used to think that using gas to produce electricity released less GHG than oil or coal. But according to this study published in Geophysical Research Letters (A.J. Turner et al., 2016, [1]), the US methane leaks from oil and gas extraction account for a large part of global increase of CH4 emissions, because of the shale oil and gas “boom”. Another study published in PNAS (R.A. Alvarez et al., 2012, [2]) showed, with official data, that leakages occurring during gas extraction have an huge impact on climate change. In 2015, the Environmental Defense Fund (3) found that CH4 emissions in Pennsylvania were largely underestimated, and that both conventional and unconventional oil and gas extractions surge were to blame. During their electric transition, several countries use gas as a way to solve intermittency of renewables but gas extraction emissions are underestimated and pretending that gas can be “green” is at best misinformed, as shows this study published in Environmental Research Letters (C. Shearer et al., 2014, [4]). There is a wide difference between official CH4 emissions from the gas extraction industry, and scientific measures. This article published in Nature (D.Zavala-Araiza et al., 2017, [5]) explained that the industry suffers from huge CH4 leaks in some “super-emitters” wells. Last year, in Science, a study (R.A. Alvarez et al., 2018, [6]) showed that gas supply chain emissions were 60% higher than previously thought. Despite those alerts, gas electricity is still considered as the greenest fossil energy, and I really think that this belief must be tackled in the AR6.</p> <p>1 :</p>	<p>Noted - these studies are small scale or at most cover the regional scale. We have added this following sentence to account for your and other comments "The apportionment of multiple CH4 source sectors using spatially aggregated atmospheric del13C data remained underdetermined to infer the global total emissions from fossil fuel industry, biomass burning, agriculture (Rice et al., 2016; Schaefer et al., 2016; Schwietzke et al., 2016; Thompson et al., 2018; Worden et al., 2017). "</p>
22502	27	43	27	43	<p>It is not clear what period "greater than shown by the inventories" refers to. Is it 2001-2014, 2007-2012, or 2003-2007 ? Please clarify [Gwenaelle GREMION, Canada]</p>	<p>Accepted. We have vastly modified this sentence</p>
41064	27	46	28	4	<p>As with the fossil fuel paragraph just before this, this chapter on ruminants needs to be revised in light of the Petrenko et al. (2017) paper, and as is relies excessively on the earlier synthesis by Saunio et al. If Petrenko et al. are correct, and it seems likely to me they are, then natural geological emissions are smaller than assumed in Table 5.2 (by 325 to 50 Tg/yr or so), meaning fossil fuel emissions must be greater by the same amount (because of the 14C constraint; Lassey et al. 2007). Therefore, the biogenic emissions from human activity must be lower by this same amount as well, and it seems likely the ruminants the major source of this. Recent work by Klaus Butterbach-Bahl (presented at the 8th Int. Symposium on Non-CO2 GHG's in Amsterdam, June 2019) shows methane from cows in Africa (which make up 25% of all cows globally) produce less than half the methane assumed by Saunio et al. (I think this has been published, although I cannot find a reference). [Robert Howarth, United States of America]</p>	<p>Taken into account. From the online search we find the paper by Ndung'u et al. (2018) who have carried our emission factor measurements over a region in Kenya. Their EFs for calves and adult cows are higher or lower than the IPCC Tier I estimates, but the net effect on the emissions from the whole family is unclear. We require a more deterministic value to be included in this assessment. Hope we come across that before the end of the year</p>

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36308	27	47		50	The text refers to livestock rumen, and then names several animals and 'dairy, beef'. Replace with 'cattle'. Also the first sentence says that CH4 emissions come from livestock rumen. The second sentence implies that pigs and poultry are the second and third largest source of ruminant emissions. But these animals aren't ruminants and don't have rumen. Probably the emissions are from manure etc, but if so this should be explained - it is confusing as written. [Nathan Gillett, Canada]	Accepted. Text and the whole paragraph has been revised
17474	27	48	27	48	Change 'rumen' to 'rumens' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
17476	27	48	27	48	Don't use 'etc', give all the information required. [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - this is a good general point
22504	27	48	27	48	Anaerobic may be removed here; already implies by the fact that CH4 is produced [Gwenaelle GREMION, Canada]	Accepted - text revised
39766	27	48	27	49	you should also state the percent attributed to cattle, or did you mean all cattle but pigs and poultry? [Dagmar Henner, Austria]	Not applicable - the whole paragraph has been revised and rewritten
26856	27	48	27	50	I would recommend to address where the CH4 emissions from pig and poultry mainly comes from, since it is well described for the ruminants. And the strategies for reducing CH4 are then different between animal species [Rebecca Danielsson, Sweden]	Not applicable - the whole paragraph has been revised and rewritten
22506	27	48	27	51	Does this apply to enteric fermentation only, or both to enteric fermentation and manure? Please clarify [Gwenaelle GREMION, Canada]	Accepted. The paragraph has been rewritten to clarify the division between enteric fermentation and manure emissions.
29490	27	49	27	49	Should state what percentage is due to cattle [Rona Thompson, Norway]	Not applicable - the whole paragraph has been revised and rewritten
33274	27	49	27	50	Pigs and poultry are evidently not ruminants. That said, there are CH4 emissions associated with monogastric production systems including rice feed, manure management and supply chains. It is unclear to me what the emissions mentioned in the report refer to. [Henry Neufeldt, Denmark]	Not applicable - the whole paragraph has been revised and rewritten
26858	27	50	27	51	I suggest that health should be added as a parameter as well that effect CH4 emissions [Rebecca Danielsson, Sweden]	Accepted. Added health with a reference to Williams et al. 2015
43612	27	52			Can the authors provide an uncertainty range for this number of 102 Tg please - it makes little sense to give a single number for something that is actually quite uncertain. [Andy Reisinger, New Zealand]	Accepted. We have given range from various estimations.
22508	27	53	27	53	'significantly' as shown statistically? Else, please use a different phrasing [Gwenaelle GREMION, Canada]	Accepted. The paragraph has been revised and rephrased and the word "significantly" is no longer used.
22510	27	56	27	56	increase from 89 Tg/yr in 2000-2009' compared to what in what period? To 102 Tg/y in 2010-2017? Please clarify [Gwenaelle GREMION, Canada]	Accepted. The paragraph has been revised and rephrased and it now stating that "The livestock emissions have been continuously increasing since 2000 from about 100 Tg yr-1 to 119 Tg yr-1 in 2017"

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29492	28	3	28	3	Need a reference for the increase in manure emissions [Rona Thompson, Norway]	Rejected. The trend in manure emissions is no more shown. The paragraph has been modified and now it is stated that "The livestock emissions have been continuously increasing since 2000 from about 100 Tg yr-1 to 119 Tg yr-1 in 2017 (EDGAR, v4.3.2; Janssens-Maenhout et al., 2019; Statistics division. Food and Agriculture Organisation of the United Nations, 2018; Wolf et al. 2017)."
22512	28	3	28	3	Skip part between brackets, repeated from line 2 [Gwenaëlle GREMION, Canada]	Accepted - text revised
47070	28	7	28	18	Table 5.2.: A graph might be better to read/understand than this table with so many numbers and different methods. [Sophie von Fromm, Germany]	Noted. We have considered your suggestion, but then realized that the table allows us to clearly show the number and range in nice details. The smaller emission sectors get hidden in a plot
41080	28	9	28	9	A big problem with Table 5.2: For the fossil fuel numbers, this overly relies on Saunio et al., and they in turn are relying on emission factors developed by the US EPA. A large body of literature has been published in recent years showing these emission factors are far too low for natural gas, probably by at least 2-fold. This is reviewed in Howarth (2014), which is cited in Chapter 1 of this AR6 draft, but not referred to at all here in Chapter 5. In particular, see the Miller et al. paper referenced in Howarth (2014). Howarth, R.W., A bridge to nowhere: Methane emissions and the greenhouse gas footprint of natural gas, Energy Science & Engineering, 2, 47-60, doi: 10.1002/ese3.35, 2014. [Robert Howarth, United States of America]	Noted. We agree the regional emissions by sectors are not well constrained. Please note that we also make assessments using the regional emission estimations using atmospheric data. The number we show here take in to account both bottom-up and top-down estimations. Some uniformity are maintained across the regions so that the global totals for all and sectorial emissions are self-consistent.
41082	28	9	28	9	Further issue with Table 5.2: the estimates for freshwater lakes and for wetlands is again overly relying on Saunio et al.. An entire special issue of Limnology & Oceanography on emissions from freshwater lakes and wetlands was published in November 2016 (volume 61, issue S1); this includes review papers on these emissions, presenting data that is far more current than that used by Saunio et al. [Robert Howarth, United States of America]	Taken into account- text has been revised and new references added (Stanley et al. 2016, Crawford et al. 2017, DelSontro et al. 2018, Beaulieu et al. 2019). While the papers in the special issues of LO are meritorious and illustrate several interesting processes which are highly important for understanding the spatial and temporal variation in GHG emissions from inland waters, they do not really provide global upscaling. Unfortunately in AR6 there is not enough space for addressing and synthesizing all these phenomena.
28416	28	9	28	16	Kirschke et al. 2013 should be cited for the values reported for 1980s and 1990s in Table 5.2 [Claude-Michel Nzotungicimpaye, Canada]	Noted. The reference is added to the Table caption



Comment ID	From Page	From Line	To Page	To Line	Comment	Response
28418	28	9	28	16	It is surprising to see that Table 5.2 shows the same information (sectorial and total sources/sinks of CH4) for 2000-2009 and 2010-2017. It is not clear whether this is based on one of the cited work (none of the references has explicit information for the global CH4 budget for 2010-2017) or if this is something that will be updated before the final version. [Claude-Michel Nzotungicimpaye, Canada]	Taken in to account. The values for the 2010-2017 period are just placeholders. Now that the GCP-CH4 paper is submitted (Saunois et al., 2019), we have updated all values for this latest assessment period
24618	28	9	28	16	Need to comment on whether the ranges in table 5.2 are due to differences between the different studies or ranges assessed by individual studies. [William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Ranges are based on different studies in most cases.
41066	28	9	28	16	Table 5.2 overly relies on Saunois et al., and does not reflect much recent literature. I will break this down into several comments, to make sure my concerns are clear. First, the Table uncritically accepts the estimates of Etiope et al. for natural geological emissions; the extrapolation by Etiope et al. (in the 2019 paper, but also earlier work) is based on relatively slim data and a lot of assumptions. That might be OK, except the paper by Petrenko et al. (2017) directly challenges this, by using 14C measurements in methane from ice cores laid down 11,500 years ago in Antarctica: natural geological seeps were very small then, and there is not reason to believe these have increased, as Petrenko et al. (2017) explain. Therefore, it seems likely the Etiope et al. estimates are far, far too high. [Robert Howarth, United States of America]	Noted. Your final statement in agreement with our conclusions. That also helps us to bring the bottom-up and top-down estimations closer.
41068	28	9	28	16	Another problem with Table 5.2: the termite estimates of 9 to 11 Tg/yr are far too high. The origin for the termite estimates are two papers published by the same team in the 1980s, where they measured methane produced by termites in the lab. Subsequent work has clearly shown that the methane produced by termites is mostly oxidized by bacteria before ever reaching the atmosphere. See Brümmer C, Papen H, Wassmann R, and Brüggemann N. 2009. Fluxes of CH4 and CO2 from soil and termite mounds in south Sudanian savanna of Burkina Faso (West Africa). Global Biogeochem Cycles. doi: 10.1029/2008GB003237 AND  Jamali H, et al. 2011. The importance of termites to the CH4 balance of a tropical savanna woodland of northern Australia. Ecosystems 14: 698–709, doi: 10.1007/s10021-011-9439-5 [Robert Howarth, United States of America]	Noted. Yes, we understand the emissions may be higher than those suggested by the recent studies, which are based on small scale measurements. We require peer-reviewed publications for global emission estimations, as upscaling point scale measurements to global emissions is beyond the scope of AR6.
27226	28	9	28	17	Please add units in Table 5.2 [François GERVAIS, France]	Noted. Units are given at the top-left corner of the Table. Also added in the Table caption now

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43614	28	9			Table 5.2: please clarify whether the numbers in brackets represent confidence intervals, or max-min, or how they are constructed. Currently this is not transparent and not traceable. Given that e.g. Wolf et al (2017) puts livestock CH4 at 120 Tg, I'm not sure how the average or the range in brackets in the table [102(87-117)] have been arrived at. Also I think the authors need to be more careful to recognise that many of the studies they cite for this table, at least for livestock, use very simplistic estimation methods (Tier 1) that have significant uncertainties not only in absolute emissions but also in trends because they fail to capture changes in emissions per animal over time. Overall the numbers for the 2010-2017 period from bottom up studies seem to be on the low side compared to the range from available studies? [Andy Reisinger, New Zealand]	Accepted. The numbers are based on the database developed in Saunio et al. (2019). We have further accounted for the results from Wolfe et al. (2017) in the revised document
43616	28	9			Table 5.2: for the period 2010-2017, bottom up, the range in brackets for rice emissions is incorrect, it seems to be simply copied from the range for landfill & waste emissions. [Andy Reisinger, New Zealand]	Taken in to account. The values for the 2010-2017 period are just placeholders. Now that the GCP-CH4 paper is submitted (Saunio et al., 2019), we have updated all values for this latest assessment period
22514	28	14	28	14	add 'in the atmosphere' [Gwenaelle GREMION, Canada]	Accepted - text revised
41808	28	15	28	15	Is it normal that many numbers (all top down estimates but also BU estimates of sinks) the two last columns are identical ? Maybe this could be commented. [Marc Aubinet, Belgium]	Taken in to account. The values for the 2010-2017 period are just placeholders. Now that the GCP-CH4 paper is submitted (Saunio et al., 2019), we have updated all values for this latest assessment period
17478	28	15	28	16	Table text too small to read clearly [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Clarity improved
27752	28	15	28	16	Improve the design of the table [Poot Delgado Carlos Antonio, Mexico]	Noted. Clarity improved
26840	28	15	28	16	Rice emissions not consistent with <a href="https://www.geosci-model-dev.net/11/369/2018/">https://www.geosci-model-dev.net/11/369/2018/</a> based on FAO [Ragnhild Bieltvedt Skeie, Norway]	Taken into account. Hoesley at al. reference and data included in the assessment
26842	28	15	28	16	Specify what the range in the paranteses mean. [Ragnhild Bieltvedt Skeie, Norway]	Noted. The range in the parentheses are the range of the estimation. This is clarified now.
22516	28	15	28	17	Consider making indents in the first coloumn, so it is easier to understand that the "sub categories" (e.g, "Biomass burning" and "Biofuels") are part of and included in the their sum in their "main" categories (e.g., "Biomass burning & biofuels") [Gwenaelle GREMION, Canada]	Accepted. Indentation done in the first column
22518	28	15	28	17	Could you comment somewhere in the text why th numbers for "Sinks" and "Natural Soruces" are identical for the periods of 2000-2009 and 2010-2017 ? [Gwenaelle GREMION, Canada]	Taken in to account. The values for the 2010-2017 period are just placeholders. Now that the GCP-CH4 paper is submitted (Saunio et al., 2019), we have updated all values for this latest assessment period

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39768	28	15	29	18	In Table 5.2 it is unclear to me what biomass burning and biofuels really includes and how these have slowed down recently [Dagmar Henner, Austria]	Taken in to account. As shown in van der Werf et al. (2017) the fire emissions have reduced from the "Savanna, grassland and shrubland" areas, globally. The biomass and biofuel emissions includes those are seen from the space, and indoor burning such as for cooking fuel
22520	28	16	28	16	Sum of sources for 1980-1989 bottom-up: 536+789 should be 536-789. [Gwenaelle GREMION, Canada]	Accepted. Correction made
22522	28	16	28	16	Why are no imbalances calculated for the bottom-up estimates? Do the presented values represent the means or medians from the range of values? [Gwenaelle GREMION, Canada]	Noted. The bottom-up imbalances do not comply with the atmospheric growth rates, and thus not of any particular interests to this assessment. Mean values are given
47836	28	21	29	32	This topic is also covered in the SRCL Sec 2.4.2. Please ensure consistency and provide a call out to the relevant sections. [WGI TSU, France]	Taken in to account. The SRCL has been called out and checked for consistency
22524	28	23	28	23	Freshwater wetlands are the single largest natural global source. Important to add 'natural' for clarity. [Gwenaelle GREMION, Canada]	Accepted - text revised
24620	28	24	28	26	How have the Sauniois et al. values been updated? What new data has come available? [William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Sauniois et al. (2016) data have been updated in Sauniois et al. (2019), now in ESSD(Discuss).
24622	28	29	28	30	Reasons for the differences with AR5 should be explained. Is this new data or a new methodology? [William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. This is because "the new wetland maps and ecosystem model simulations are available (Poulter et al., work in prog.)"
47734	28	30	28	30	Please check the use of this IPCC uncertainty language term. Are you able to provide a traceable account to assigning this uncertainty statement? Note that likelihood statements are quantified terms - phrases like likely and very likely have quantifiable probabilities associated with them. Please check it has been used correctly here. Please refer to the IPCC guidance note on uncertainty: <a href="https://wg1.ipcc.ch/SR/documents/ar5_uncertainty-guidance-note.pdf">https://wg1.ipcc.ch/SR/documents/ar5_uncertainty-guidance-note.pdf</a> [WGI TSU, France]	Noted. We have provided quantitative statement
45652	28	30	28	34	Sources other than wetlands' - this is a very bold statement and dismisses both sink changes and a variety of wetland discussions. I would strongly suggest toning it down. Is it really 'likely' that non-wetland sources drove it? There have been hardly any decent studies of wetlands in situ from aircraft and we really don't know enough. Moreover, many tropical wetlands in Africa especially are also teeming with cattle. When you're talking about the Sudd or the central African wetlands how do you separate the two? [Euan Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Taken in to account. We have now revised this part to inform the readers a bit more specifically. This is our assessment based on the present data from various sources. Emission data from wetlands do not show consistent increase for explaining the systematic increase in CH4. Some reviewers also lauded this assessment
43618	28	30			I'm not convinced that "likely" is a correct use of the uncertainty guidance, since I doubt that the authors have undertaken a probabilistic assessment to arrive at this judgement. Use confidence rating instead. [Andy Reisinger, New Zealand]	Noted. We have provided quantitative statement

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22526	28	32	28	32	Please remove "but their role cannot be totally ruled out." The following sentence already implies this, by using the word "mainly". [Gwenaelle GREMION, Canada]	Accepted - text revised
24624	28	32	28	35	This is a nice clear assessment of the evidence to rule out wetlands. [William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Noted. thank you
29494	28	32	28	35	I disagree with the isotopic data being used here to suggest that the increase in emissions is likely not due to wetlands (reference to Schaefer et al. 2016). In fact, the isotope record indicates a change to a globally more depleted CH4 source, which strongly suggests that microbial sources (which include wetlands) are contributing to the increase in emissions. From the isotope record it is not possible, however, to distinguish how much of this is due to agricultural versus wetland sources. There are top-down studies that indicate that microbial source (which could include wetlands) have contributed to the recent CH4 increase (e.g. Worden et al., Nature Comm., 2017; Thompson et al., GRL, 2018). [Rona Thompson, Norway]	Taken into account. Here we are presenting multiple lines of evidence, take the wetland models, isotopic signatures, regional inverse models. These evidences should not be treated separately. We agree that wetland emission signatures are similar to those of the agriculture, but we have multiple animal emission inventories to suggest an increase in emissions from that sector. Also note that we need a systematic increase in emissions for CH4 growth rates to be sustained, for which the wetlands emission estimations do not provide support, so far.
47072	28	32	29	1	5.2.2.3: Biogenic methane fluxes are depleted in 13C compared to what? More information is needed to understand this statement or to be able to put in the context of methane concentration changes in the atmosphere. [Sophie von Fromm, Germany]	Accepted. Modified as "depleted in 13C, relative to the ambient air"
41084	28	33	28	33	I suggest not referencing the Schaefer et al. (2016) paper here, for two reasons. First, Schietzke et al. (2016) six months later published a similar paper (with many of the co-authors in common), and stated that the 14C data set used by Schaefer et al. was inadequate. And second, Worden et al. (2017) pointed out that both Schaefer et al. and Schwietzke et al. were wrong when they assumed that biomass burning was constant over time. When Worden et al. (2017) corrected for this issue, they reached a substantially different conclusion: the 13C data for methane since 2017 actually show that increased emissions from fossil fuels exceeded those biogenic sources. [Robert Howarth, United States of America]	Taken into account. We have added a sentence in the first paragraph of section 5.2.2.2 as "The apportionment of multiple CH4 source sectors using spatially aggregated atmospheric del13C data remained underdetermined to infer the global total emissions from fossil fuel industry, biomass burning, agriculture (Rice et al., 2016; Schaefer et al., 2016; Schwietzke et al., 2016; Thompson et al., 2018; Worden et al., 2017)." Because with one measurement parameter, one cannot optimise more than one source type. So we need to combine information from many different sources. This sentence is also modified to reflect that.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
22348	28	33	29	1	Robert Howarth has a paper in review that is recalculating CH4 budgets (especially those attributed to ruminant CH4 production) based on a revised analysis of 13CH4 values. The authors suggest iron and sulfate reducing bacteria fractionate the methane signature of natural gas as it migrates through shale stone, significantly depleting 13CH4 signatures. The isotopic signature of shale gas is thus significantly depleted relative to conventional fossil fuels (-52 relative to -44 permil) and closer to biogenic sources (-62 permil). Additionally, they attribute >60% of the increase in global gas production over the past decade to shale gas production in North America (at the same time, beef cow inventories have decreased from 1995-2015: USDA-NASS inventory, and therefore cannot explain increased CH4 emissions). It might make sense to reduce the confidence in statements regarding agricultural vs. fossil fuel sources of CH4 until there is more evidence. See Worden et al. (2017), Schwietzke et al., (2016), Turner et al., (2016). [Gwenaelle GREMION, Canada]	Taken into account. We have added a sentence in the first paragraph of section 5.2.2.2 as "The apportionment of multiple CH4 source sectors using spatially aggregated atmospheric del13C data remained underdetermined to infer the global total emissions from fossil fuel industry, biomass burning, agriculture (Rice et al., 2016; Schaefer et al., 2016; Schwietzke et al., 2016; Thompson et al., 2018; Worden et al., 2017)." Because with one measurement parameter, one cannot optimise more than one source type. So we need to combine information from many different sources. This sentence is also modified to reflect that. We will further revise these assessments when the new data from Howarth et al. become available.
36310	28				Table 5.2. Consider showing this information in graphical form, perhaps as a set of stacked bars for each decade. There is a lot of information in the table, and it is hard to digest. A histogram would make it much easier to see at a glance which are the dominant sources and sinks, and how each has changed over time. [Nathan Gillett, Canada]	Taken into account. We have considered that option, and decided to keep the table to show much more details than just the major source sectors. The data are carefully revised using the new GCP synthesis (Saunio et al., ESSDD, 2019)
22528	29	1	29	1	depleted in 13C compared to what? [Gwenaelle GREMION, Canada]	Accepted. Modified as "depleted in 13C, relative to the ambient air"
24626	29	3	29	6	In what way is the role of trees "potentially relevant"? Does it affect any of the budgets listed in table 5.2? [William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. "potentially relevant" removed, text revised
26966	29	3	29	6	This needs to be better explained. How do trees influence CH4 emissions from soils? Are they to be considered as "facilitators", "doorkeepers" or "producers"? Is the influence positive or negative or both, and what does this depend on? [Joachim Rock, Germany]	Accepted - added a sentence "Trees contribute to CH4 emissions by producing it in photochemical reactions in the living parts, by conducting CH4 from soil into the atmosphere and by the methanogenesis taking place in the stem (Covey and Mecognical, 2019)."

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
24224	29	3	29	6	Growing evidence indicates tree-stem methane emissions may be an important and un-accounted for component of local, regional and global carbon budgets. Although large uncertainties exist when up-scaling data from small-scale temporal measurements, dead mangrove tree-stem emissions may account for ~26% of the net ecosystem methane flux (Jeffrey, L. C., Reithmaier, G., Sippo, J. Z., Johnston, S. G., Tait, D. R., Harada, Y. and Maher, D. T. (2019), Are methane emissions from mangrove stems a cryptic carbon loss pathway? Insights from a catastrophic forest mortality. New Phytol. Accepted Author Manuscript. doi:10.1111/nph.15995). [Natasha Barbolini, Sweden]	Accepted. We have revised this sentence by adding Jeffrey et al. .
55514	29	3	29	6	The work of Frank Keppler on aerobic methane from plants, and subsequent studies by others, should be mentioned here. While there has been some debate on the matter, it is still a biogenic source to the atmosphere and as such should be briefly discussed. [Wesley Fraser, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - aerobic abiotic methane production is now mentioned (see reply to review comment #26966). Keppler is not mentioned since his work was assessed in AR5 and no new evidence of really significant CH4 emissions through aerobic production has emerged.
47736	29	3	29	54	Please check the use of this IPCC uncertainty language term. Are you able to provide a traceable account to assigning this uncertainty statement? Note that likelihood statements are quantified terms - phrases like likely and very likely have quantifiable probabilities associated with them. Please check it has been used correctly here. Please refer to the IPCC guidance note on uncertainty: <a href="https://wg1.ipcc.ch/SR/documents/ar5_uncertainty-guidance-note.pdf">https://wg1.ipcc.ch/SR/documents/ar5_uncertainty-guidance-note.pdf</a> [WGI TSU, France]	Taken into account. We have provided more evidences for the patchy nature of the CH4 emissions.
22530	29	8	29	8	which makes rice fields significant anthropogenic CH4 sources. Important to add 'anthropogenic' for clarity. [Gwenaelle GREMIION, Canada]	Accepted
24628	29	8	29	15	This reads a bit a of text book explanation of rice. It might be better to focus on how the estimates have change since AR5 and what new evidence has led to this change. [William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Text revised by including data from Hoesly et al., FAO and ecosystem model simulations
43620	29	8	29	15	The numbers in this para don't seem to match with the numbers provided in Table 5.2. The para says rice emissions were 33 Tg in 2003 and have increased 20% since then. But Table 5.2 puts rice emissions in 2010-2017 at only 30 Tg. [Andy Reisinger, New Zealand]	Taken into account. The data in the table 5.2 are updated now (only place holder in the FOD) and text modified accordingly
26844	29	8	29	15	The emissions from Hoesly et al ( <a href="https://www.geosci-model-dev.net/11/369/2018/">https://www.geosci-model-dev.net/11/369/2018/</a> ) based on FAOSTAT show a different temporal development of emissions from rice cultivation due to lower emissions from China. Please consider to include these emission estimates as well. [Ragnhild Bieltvedt Skeie, Norway]	Taken into account. Text revised by including data from Hoesly et al., FAO and ecosystem model simulations

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
28420	29	9	29	11	Based on the paragraph context, the reported decadal mean CH4 burden increase of 42, 17, 6, 20 Tg yr-1 for 1980s, 1990s, 2000s, 2010s, respectively are in reference to Table 5.2 (page 5-28) and Figure 5.3 (5-150). But there is an inconsistency in the reported numbers for 1980s (34 Tg yr-1 based on Table 5.2) and for 2010s (no information in Table 5.2 but 29±10 Tg yr-1 in Figure 5.3). Since Figure 5.3 is (also) based on the information from Table 5.2, it would be good to have the best estimate reported in the table then in this paragraph and in the figure. [Claude-Michel Nzotungicimpaye, Canada]	Taken into account. The data in the table 5.2 are updated now (only place holder in the FOD) and text modified accordingly. Please note that the data in Table 5.2 for the 2010s were just pace holders - sorry for the confusion
22532	29	12	29	13	Please explain, or give example(s) of, "organic amendments". [Gwenaelle GREMION, Canada]	Noted. Organic amendments are referred a practice that the farmers to maintain the quality of the soil by adding organic materials, such as compost fertilisers
22536	29	17	29	17	Order is confusing within this paragraph. Does the biomass burning on line 23 apply to burning from wildfires or as an energy source? The order could be changed such that the natural and anthropogenic emissions are split more clearly [Gwenaelle GREMION, Canada]	Taken into account. We have added "open" to biomass burning as these are seen from satellites
22534	29	17	29	18	Who defines that ~8% is large? Maybe a different phrasing could be better [Gwenaelle GREMION, Canada]	Accepted. Removed "a large fraction"
24630	29	17	29	23	It might be worth emphasising that using satellite burned area to detect IAV is new (assuming it is). [William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Noted. It is not new since the AR5 but we have growing confidence on the quality of satellite derived biomass burning emissions.
22538	29	17	29	26	I think there is a bit of uncertainty with the use of language regarding fire emissions. The way it is used in this report it seems that "biomass burning" is treated to specify purely anthropogenic emissions, just like "biofuels". "Wildfires" is relatively clear, even though many of these can be started by humans (especially in N-America and Russia). "Open biomass burning" on the other hand seems to be used to for natural fire emissions in this section because it gets related to ENSO activity. However, open biomass burning can be of natural or anthropogenic origin (agricultural). Please specify more clearly what is meant here or include the clear definitions (as used in this report) somewhere, like a glossary or in the Atlas. [Gwenaelle GREMION, Canada]	Noted. We slightly revised the opening sentence as "Biomass burning and biofuel consumption (include both natural and anthropogenic processes), causing an emission of ...". Unfortunately it is difficult to separate the natural and anthropogenic components of the biomass burning, for example, people take advantage of clearing forest or peatland when the conditions are dry over the tropics during El Nino.
22540	29	18	29	19	Wildfires comprise a small natural CH4 source globally. Important to add 'natural' for clarity. [Gwenaelle GREMION, Canada]	Accepted - text revised

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
45654	29	23	29	45	CH4 emisisons 'show' a decreasing ternd. Is this 'show' valid? Emissions depend on fuel, wetness during burning, lots of variables. Might be worth mentioning the C-isotopes of methane here and the Worden hypothesis? Shakhova studies - see also Berchet, A., Bousquet, P., Pison, I., Locatelli, R., Chevallier, F., Paris, J. D., ... & Worthy, D. E. (2016). Atmospheric constraints on the methane emissions from the East Siberian Shelf. Atmospheric Chemistry and Physics, 16(6), 4147-4157 . Note also the observations of France, James L., et al. "Measurements of $\delta^{13}C$ in CH4 and using particle dispersion modeling to characterize sources of Arctic methane within an air mass." Journal of Geophysical Research: Atmospheres 121.23 (2016), who are not seeing current big Arctic emissions. Nor are Fisher, Rebecca E., et al. "Measurement of the $^{13}C$ isotopic signature of methane emissions from northern European wetlands." Global Biogeochemical Cycles 31.3 (2017): 605-623. Also Nisbet et al. 2019 find the Arctic lagging, not leading (except in 2007). For open oceans, note there is a small marine source: Karl, D. M., Beversdorf, L., Björkman, K. M., Church, M. J., Martinez, A., & Delong, E. F. (2008). Aerobic production of methane in the sea. Nature Geoscience, 1(7), 473. [Euan Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. Not clear! The decreasing trend is referred to emissions from Biomass burning only, which is seen from remote sensing data.
24632	29	24	29	26	What is this "recent evidence"? New satellite data? [William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Not really new satellite but a more detailed analysis by linking various meteorological data and satellite observed fire occurrence
15180	29	24	29	26	Fire-climate relationships are not confined to the tropics, and as such this section would benefit from a more thorough explanation of global fire-climate relationships. [Richard Vachula, United States of America]	Taken into account. That is true but the emission variabilities are smaller for the temperate and boreal regions (please refer to the Cross-Chapter Box 1)
22542	29	28	29	28	comprises about 5% of the total chemical atmospheric CH4 sink. Please add 'atmospheric' for clarity. [Gwenaelle GREMION, Canada]	Accepted - text revised
36314	29	28		32	Add an overall assessment for changes in the soil CH4 sink. [Nathan Gillett, Canada]	Rejected. The existing estimates do not allow assessing the possible decadal changes. Text has been revised.
22544	29	29	29	29	Please write 'methane loss' instead of 'methane uptake' [Gwenaelle GREMION, Canada]	Accepted - text revised
49010	29	29	29	29	" ... upland soils during three decades ..." is not clear, may I know which three decades ? [Minchao Wu, Sweden]	Not applicable - text no longer included in the chapter as the whole paragraph was revised



Comment ID	From Page	From Line	To Page	To Line	Comment	Response
24634	29	30	29	32	What new evidence supports the higher uptake since AR5? Is this higher uptake now the central AR6 assessment? Is the single Ni and Groffman study assessed as being important (are the values large) and robust? [William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - the paragraph has been modified and a sentence was added, stating "Evidence of both increasing (Yu et al., 2017) and decreasing (Ni and Groffman, 2018) soil microbial uptake during recent decades, due to higher temperature and higher precipitation, respectively, have emerged from experimental and modelling studies, but they do not allow making an assessment. "
22546	29	32	29	33	Given the recently emerging data specifically on permafrost contributions under rapid melting conditions to CH4 release in the sub-Arctic and other areas, it seems like there should be a mention of the studies and uncertainty of this source and its contributions to the overall total in the atmosphere (e.g. Walter Anthony et al. 2018 Nature Comm). [Gwenaelle GREMION, Canada]	Noted. The permafrost CH4 emissions are discussed in the feedback section of this Chapter 5. Please check section 5.4.7
47074	29	37	29	37	5.2.2.4: but instead of and [Sophie von Fromm, Germany]	Accepted - text revised
41074	29	37	29	40	The information on coastal ocean fluxes is out of date. Refer to the special issue of Limnology & Oceanography; see comment above. [Robert Howarth, United States of America]	Noted. We have attempted to summarise the data from this special issue, however, we do not have a mechanism to upscale the measurements to global maps/fluxes
17480	29	38	29	38	Reference(s) required after 'sparse' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We are actually discussing results from only a few studies in the following sentences, in support of the first sentence.
17482	29	38	29	38	Change 'are' to 'is an' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
25466	29	39	29	39	Melting of geological storage is poor terminology as phase change only occurs in the water/ice portion not sediments or rock. It is better to say "loss of geological storage due to warming and thawing of permafrost and hydrate dissociation (or decomposition?)" [Sharon Smith, Canada]	Accepted
29496	29	39	29	40	The emission estimates of Shakhova et al. (2010; 2017) have more been shown to be inconsistent with atmospheric measurements (Berchet et al. ACP, 2016). The amount of CH4 reaching the atmosphere from geological sources on the Arctic shelf is strongly debated and I think the statement should reflect the large uncertainty for these emissions and that the number is likely much lower than Shakhova et al. (2010; 2017) estimate. [Rona Thompson, Norway]	Noted. Yes, the estimation given in the next sentence reflect that
17484	29	41	29	41	Italicise 'l' in second 'likely' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
17486	29	41	29	41	Change to Pre-Industrial [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - The same spelling as in the IPCC SR1.5 / SROCC has been applied

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
41076	29	42	29	43	This estimate from Etiope et al. is speculative, and not consistent with the Petrenko et al. (2017) paper. I suggest deleting this. [Robert Howarth, United States of America]	Noted. Sorry, we would like to keep both these views in the report. Hope further studies will validate one or the other
36316	29	42		44	What is the difference between 'all geological sources' and 'the ventilation of geological CH4'? The numbers given for each are different. [Nathan Gillett, Canada]	Noted. All geological sources include mud volcanos and others
17488	29	43	29	43	Change second 'are' to 'is' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
22548	29	43	29	44	I am not sure if the fjords and coastal oceans are included in the estimate of the study of Petrenko et al. (2017). If this is indeed not the case, then the given numbers are not directly comparable. [Gwenaelle GREMION, Canada]	Noted. Petrenko et al. do not estimate each sectors of emission. The numbers from fjords and coastal ocean seepage are a part of total geological sources
27754	29	44	29	44	subscript (CH4) [Poot Delgado Carlos Antonio, Mexico]	Accepted - text revised
26860	29	44	29	44	4 should be in subscript [Rebecca Danielsson, Sweden]	Taken into account
36320	29	44		45	Explain what this 'gap between top-down and bottom-up estimations' is. Also clearly explain how each of these budgets is derived. [Nathan Gillett, Canada]	Taken into account. We have referenced to Table 5.2 here. The definitions of top-down and bottom-up estimations are given early on in Section 5.2.2
36318	29	44			Replace 'likely to be smaller than about 15 Tg/yr' with 'likely to be smaller than xx Tg/yr'. 'About' is imprecise. Choose a higher number if required, such that the probability can be quantified as 'likely'. [Nathan Gillett, Canada]	Accepted
43668	29	47	29	55	In fact, inland water would be more appropriate than freshwater here because many lakes are also under salty conditions, but they belong to inland water. [Xinghui Xia, China]	Accepted
6311	29	47	29	55	Either here or in the 'energy' section it would be worth mentioning CH4 emissions from hydroelectric - significant (albeit poorly constrained) source. See Reay et al (2018) Methane and Global Environmental Change. Annual reviews. For all these freshwater systems there's also the interaction with CO2 fert effect and changing allochtonous organic C inputs - combined with higher temps this can enhance CH4 production and emission [Dave Reay, United Kingdom (of Great Britain and Northern Ireland)]	Noted.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
49478	29	47	30	1	While rivers and streams are mentioned in the first sentence of this paragraph, it makes no reference to this literature. Stanley et al. 2016 ( <a href="https://doi.org/10.1890/15-1027">https://doi.org/10.1890/15-1027</a> ) generate a constrained estimate of annual CH4 emissions from fluvial systems (28.6 TgCH4/yr). Crawford et al. 2017 ( <a href="https://doi.org/10.1002/2016JG003698">https://doi.org/10.1002/2016JG003698</a> ) shows empirically the spatial heterogeneity of CH4 concentrations in these systems that leads to uncertainty in their flux (as mentioned here in line 52). Finally, there's no mention of human induced amplification of these emissions as has been documented by DelSontro et al. 2018 ( <a href="https://doi.org/10.1002/lol2.10073">https://doi.org/10.1002/lol2.10073</a> ), Crawford et al. 2016 ( <a href="https://doi.org/10.1890/15-1330">https://doi.org/10.1890/15-1330</a> ), and Beaulieu et al. 2019 ( <a href="https://doi.org/10.1038/s41467-019-09100-5">https://doi.org/10.1038/s41467-019-09100-5</a> ) as well as the massive literature on CH4 emissions from more frequent freshwater impoundments [Seth Spawn, United States of America]	Taken into account - references added (Stanley et al. 2016, Crawford et al. 2015, 2017, DelSontro et al. 2018, Beaulieu et al. 2019)
36322	29	47		49	Specify the uncertainty ranges for these budgets, not just one value. [Nathan Gillett, Canada]	Accepted. Ranges are given
45360	29	50	29	50	top-down estimates don't need to account for fluxes but just map them, please clarify [Peter Rayner, Australia]	Taken in to account. We have tried to clarify why the top-down models do not account for the inland waters. Given that the global total loss is determined by OH concentration there is little scope for increasing global total CH4 emission. The latter is constrained by atmospheric CH4 observations
36324	29	50			Why is freshwater CH4 emission unaccounted for in top-down models? Can this term just be added? [Nathan Gillett, Canada]	Taken in to account. We have tried to clarify why the top-down models do not account for the inland waters. Given that the global total loss is determined by OH concentration there is little scope for increasing global total CH4 emission. The latter is constrained by atmospheric CH4 observations
29498	29	52	29	52	The statement "The freshwater CH4 emission remains unaccounted in the top-down models" is not correct, although the inversion estimates do not resolve specifically the emissions from freshwater, they are included in the total surface-atmosphere emissions. Thus, this source type does not contribute to a gap between bottom-up versus top-down approaches in the sense that these would be missing in the top-down estimates. [Rona Thompson, Norway]	Noted. Please note that this statement emerges because we do not account explicitly for the inland waters in the top-down modelling.
43670	29	53	29	53	There is a lack of reference about CH4 emissions from lotic ecosystems, so we should add relative reference, which is Stanley et al. 2016. Stanley, E. H., Casson, N. J., Christel, S. T., Crawford, J. T., Loken, L. C., & Oliver, S. K. (2016). The ecology of methane in streams and rivers: Patterns, controls, and global significance. Ecological Monographs, 86(2), 146–171. <a href="https://doi.org/10.1890/15-1027">https://doi.org/10.1890/15-1027</a> [Xinghui Xia, China]	Accepted - a reference added

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
36326	29	54		55	What does 'partially overlapping' mean here? Does this mean there is double counting? Is this in the top-down or bottom up estimate? [Nathan Gillett, Canada]	Taken into account. Added in the text term "double accounting"
43672	30	1	30	1	The author should add contents about reservoir and estuary emissions that contribute ~13 Tg/yr (Deemer et al., 2016) and ~ 7 Tg/yr (Borges & Abril, 2011) Deemer, B. R., Harrison, J. A., Li, S., Beaulieu, J. J., DelSontro, T., Barros, N., et al. (2016). Greenhouse gas emissions from reservoir water surfaces: A new global synthesis. <i>Bioscience</i> , 66(11), 949–964. <a href="https://doi.org/10.1093/biosci/biw117">https://doi.org/10.1093/biosci/biw117</a> Borges, A. V., & Abril, G. (2011). Carbon dioxide and methane dynamics in estuaries. In E. Wolanski & D. McLusky (Eds.), <i>Treatise on Estuarine and Coastal Science-Volume 5</i> (pp. 119–161). London: Elsevier. [Xinghui Xia, China]	Accepted. We have added a sentence following your suggestion
45656	30	4			The great methane sink, [OH], gets very short shrift here. The Cl sink, which is small but has a very big isotopic lever, doesn't seem to be mentioned at all? Hossaini, Ryan, et al. "A global model of tropospheric chlorine chemistry: Organic versus inorganic sources and impact on methane oxidation." <i>Journal of Geophysical Research: Atmospheres</i> 121.23 (2016). The methane budget is not just about sources - there needs to be a sink discussion. See Wolfe, Glenn M., et al. "Mapping hydroxyl variability throughout the global remote troposphere via synthesis of airborne and satellite formaldehyde observations." <i>Proceedings of the National Academy of Sciences</i> 116.23 (2019): 11171-11180. Also Nicely, Julie M., et al. "Changes in global tropospheric OH expected as a result of climate change over the last several decades." <i>Journal of Geophysical Research: Atmospheres</i> 123.18 (2018): 10-774. Also see Nisbet et al. 2019 discussion of OH change. LATER COMMENT : I see OH is well discussed later on page 31, but maybe the sinks could be gathered together, rather than having soil separated from the other sinks? [Euan Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. We have the Cross-chapter box for this. Please check that part for the atmospheric sinks.
36328	30	6			Explain what the 'top-down' and 'bottom-up' budgets are, and how they are derived. [Nathan Gillett, Canada]	Noted. We have done that at the beginning of Section 5.2.2, first para
22550	30	7	30	7	Please explain, or give example(s) of, "sectorial emissions". [Gwenaelle GREMION, Canada]	Noted. Already discussed in the previous subsections
29500	30	8	30	9	See comment for P29, L52 [Rona Thompson, Norway]	Noted
24636	30	9	30	11	It is not clear which part of the sentence is "virtually certain". Is it the values 42, 17, 6 and 20, or that the burden can be estimated from the observed atmospheric increase? [William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Taken in to account. We referred to the numbers that are estimated from the mean growth rates. There is no doubt that burden is proportional to the concentration.
15182	30	11	30	14	Consider adding a brief statement explaining how OH variability affects CH4. As written, the text assumes prior knowledge or leaves the reader to find the information in Box 5.1.. [Richard Vachula, United States of America]	Rejected. We have discussed CH4 loss processes and their relative importance in Section 5.2.2, first para

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
36330	30	12		16	Is the reader to take from this that variability in OH is not important at timescales longer than interannual? Readers will be mainly interested in the long-term budget, so the authors should include an assessment of the role of OH changes in the long-term budget, even if it is very uncertain. [Nathan Gillett, Canada]	Noted. This issue of long-term in OH is discussed in the cross-chapter box 5.1
22552	30	19	30	20	Add that the reservoirs have units of Tg CH4 and that the average increase is in Tg CH4. [Gwenaelle GREMION, Canada]	Accepted
47834	30	25	23	33	Please note this topic is also covered in SRCCL in section 2.4.2 (Methane). The Exec Summary statement on this topics is as follows. "The pause in the rise of atmospheric CH4 concentrations between 2000 and 2006 and the subsequent renewed increase appear to be partially associated with land use and land use change. The recent depletion trend of the 13C isotope in the atmosphere indicates that higher biogenic sources explain part of the current CH4 increase and that biogenic sources make up a larger proportion of the source mix than they did before 2000 (high confidence). In agreement with the findings of AR5, tropical wetlands and peatlands continue to be important drivers of inter-annual variability and current CH4 concentration increases (medium evidence, high agreement). Ruminants and the expansion of rice cultivation are also important contributors to the current trend (medium evidence, high agreement). There is significant and ongoing accumulation of CH4 in the atmosphere (very high confidence)." please check for consistency and provide an update from the SRCCL. A callout to this special report is needed. [WGI TSU, France]	Accepted. We have added a sentence following your suggestion
28422	30	27	30	27	Drivers of atmospheric methane changes during 1980-2017 (emphasis on atmospheric methane) [Claude-Michel Nzotungicimpaye, Canada]	Accepted
22554	30	30	30	31	Explain what is meant by 'quasi-equilibrium' [Gwenaelle GREMION, Canada]	Noted. Please see the low growth rate value, which mean CH4 concentration did not change much during that period, thus q "quasi"-equilibrium
24638	30	32	30	34	It would be better to refer to the underlying data (e.g. figure 5.12) to make an assessment about growth rates rather than using a second-hand assessment via Nisbet. [William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Accepted
22556	30	34	30	34	CH4 growth rates are observed to be high during El Niño.' This could be nicely visualised by adding El Niño periods to the growth rates presented in Figure 5.12b, in a way similar to how this is done in the inset of Figure 5.14a. [Gwenaelle GREMION, Canada]	Accepted - citation changed to Fig. 5.12
29502	30	34	30	34	In fact there is evidence for increased emissions during La Nina (not El Nino) (see Pandey et al., Scientific Reports, 2017). This statement seems incorrect and in any case needs to be backed up with references. [Rona Thompson, Norway]	Rejected. We have shown clear correlation in Fig. 5.12b. Now cited in the text here.
37740	30	34			Change "2015" to "2016". The prolonged El Nino condition extended into 2016. The positive SST anomaly in the tropical Pacific peaked in late 2015 but was still present in the first few months of 2016. And aspects of the atmospheric signal lag behind the ocean. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Accepted

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
51950	30	41			Why the use of the qualifier might here? A better formulation may be to note that methane is a much more potent greenhouse gas than CO2 and thus that unexpected increases will raise substantively the risk of greater warming than we may have expected? Regardless, the use of might seems unwise and. different formulation less ripe for misinterpretation / mis-use would be useful here. [Peter Thorne, Ireland]	Accepted. Changed to "the past decade will lead to higher temperatures ". Yes, IPCC and other works have shown that CH4 is a more potent GHG than CO2 at 20-40 years time scale
22558	30	46	30	46	The figure refers to 2012 rather than 2010-2017. Which one is correct? [Gwenaelle GREMION, Canada]	Noted. We have updated the figure
17490	30	48	30	48	I suggest inserting 'sources' after (on shore) for clarity [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted
17492	30	49	30	49	I would not class enteric fermentation as an anthropogenic source (at least, not in the context as written!) [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Noted. But we do not have better ways of sorting the sources. Many of the other sources as well are not entirely natural or anthropogenic.
22560	31	5	31	5	Year for reference Dlugokencky missing. It might be Dlugokencky et al. 2003 [Gwenaelle GREMION, Canada]	Accepted
45660	31	5			update to 2018 [Euan Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Accepted
22562	31	7	31	7	smoothed over which time interval? Please add [Gwenaelle GREMION, Canada]	Accepted. Details given
22564	31	14	31	14	the role of emissions' which emissions? Perhaps rephrase to 'the role of the various emissions' [Gwenaelle GREMION, Canada]	Accepted - text revised
36714	31	14	31	25	A few studies addressed the historical change in CH4 emission from the Arctic region. For example, Ito (2019) estimated northern wetland CH4 emission in 1901–2016 with a process-based model and showed emission stability in the last decade. Ito, A., in press. Methane emission from pan-Arctic natural wetlands estimated using a process-based model, 1901–2016. Polar Science. <a href="https://doi.org/10.1016/j.polar.2018.12.001">https://doi.org/10.1016/j.polar.2018.12.001</a> [Akihiko Ito, Japan]	Noted. We have tried to provide some information on the emissions from the arctic region, but we wish more observational evidences become available in due course
51952	31	14	31	25	This paragraph is confusing to me. The isotope analysis seems to point firmly to fossil and biogenic burning storylines yet the remainder of the paragraph suggests a broad range of hypotheses, some on face of it inconsistent with the isotope evidence are in play. Which is it? Some further synthesis and iading the reader would, I think, be very useful here [Peter Thorne, Ireland]	Noted - We have tried to make this as clean as possible. There are still uncertainties in attribution of CH4 sources and sinks. Some confusions relating to source sectors are likely to remain unresolved in AR6.
13576	31	15	31	15	Fig.1 does not show the isotope ratios [Govindasamy Bala, India]	Taken into account. cited the right figure 5.12
47076	31	15	31	15	Cross-chaper box 5.1: Figure 1 does not show 13C-CH4 and D-CH4 isotopes [Sophie von Fromm, Germany]	Taken into account. cited the right figure 5.12
45662	31	15			apart from NIWA time series and some other work we have very little information on delta D in CH4 since the shut down of the nOAA record. Didn't see this in Fig 1 but maybe I'm looking at the wrong figure (have a very clunky 10yr old laptop) [Euan Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We have cited correct figure 5.12 now

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
24640	31	16	31	16	May be clearer to use "fossil" rather than "thermogenic". [William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Noted - the thermogenic sector include coal mining not really fossil fuel burning. We have an explanation for the thermogenic, pyrogenic and biogenic emissions in the main text
47078	31	18	31	23	Cross-chapter box 5.1: It seems to be enough saying that thermogenic emissions decreased and biogenic emissions increased. The individual components of those emission sources were already mentioned before and do not need to be repeated again at this point. [Sophie von Fromm, Germany]	Noted. Still for the sake of completeness we have provided some details.
45664	31	20			This paragraph is a bit out of date and maybe should be revised. Could cite Nisbet, E. G., M. R. Manning, E. J. Dlugokencky, R. E. Fisher, D. Lowry, S. E. Michel, C. Lund Myhre et al. "Very strong atmospheric methane growth in the 4 years 2014–2017: Implications for the Paris Agreement." Global Biogeochemical Cycles 33, no. 3 (2019): 318-342 [Euan Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We have changed citation to Nisbet et al., 2019. We have to assess as much as possible since the AR5
29504	31	21	31	21	In fact Worden et al. find a decrease in pyrogenic emissions and an increase in both thermogenic and microbial emissions. Their finding is corroborated by Thompson et al, GRL, 2018. The coincident increase in thermogenic and microbial sources should be mentioned as a possible (or even likely) scenario. [Rona Thompson, Norway]	Noted. Further references added
24642	31	23	31	25	The statement that ethane "helps understanding" doesn't seem necessary - unless it has been used as evidence in the assessments made here. [William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Noted. We believe this brings an extra line of evidence
29506	31	23	31	25	More recently, ethane has increased again and has been used as evidence for increasing thermogenic emissions (Thompson et al. GRL, 2018; Helmig et al., Nat. Geosci. 2016; Hausmann et al., ACP, 2016). This should be mentioned. [Rona Thompson, Norway]	Noted. It is not very clear from the Figure A1 in Hausmann et al. whether C2H6 is increasing systematically in the recent years (2000-2015). The paper by Helmig et al. look a bit more convincing that C2H6 emissions over USA is increasing.
26846	31	23	31	25	How does this "help understand"? This could be more clearly written. [Ragnhild Bieltvedt Skeie, Norway]	Taken into account. Sentence modified as "The decrease (1985-2000) in ethane (C2H6), .."
36332	31	23		25	What does the analysis of ethane tell us? [Nathan Gillett, Canada]	Noted. As stated in this sentence
22566	31	24	31	24	What is meant by the 'remote atmosphere'? [Gwenaelle GREMION, Canada]	Noted - remote means away from the source regions. This is a common terminology.
17494	31	24	31	24	Change 'help understanding' to 'helps understand' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
22568	31	27	31	27	A few studies have emphasised the role of OH, the primary sink of methane, in driving changes'. This needs to be rephrased. The reaction of OH with methane is a sink of CH4, not OH itself. [Gwenaelle GREMION, Canada]	Accepted - modified as "role of OH, the primary sink of methane by chemical reaction, in driving "
24644	31	31	31	31	I'm not sure the [ ] are necessary to understand the sentence. [William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Accepted

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
45666	31	31			Maybe also Wolfe, Glenn M., et al. "Mapping hydroxyl variability throughout the global remote troposphere via synthesis of airborne and satellite formaldehyde observations." Proceedings of the National Academy of Sciences 116.23 (2019): 11171-11180. [Euan Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. Wolfe et al. did not derive interannual variability or trends of OH, thus not relevant for this discussion.
47738	31	33	31	33	Please check the use of this IPCC uncertainty language term. Are you able to provide a traceable account to assigning this uncertainty statement? Note that likelihood statements are quantified terms - phrases like likely and very likely have quantifiable probabilities associated with them. Please check it has been used correctly here. Please refer to the IPCC guidance note on uncertainty: <a href="https://wg1.ipcc.ch/SR/documents/ar5_uncertainty-guidance-note.pdf">https://wg1.ipcc.ch/SR/documents/ar5_uncertainty-guidance-note.pdf</a> [WGI TSU, France]	Taken into account. Changed "likely" from to "arise" from
26848	31	38	31	38	A paper to consider: Zhao, Y., et al.: Inter-model comparison of global hydroxyl radical (OH) distributions and their impact on atmospheric methane over the 2000-2016 period, doi:10.5194/acp-2019-281, Atmos. Chem. Phys. Discuss., 2019. [Ragnhild Bieltvedt Skeie, Norway]	Noted. Zhao et al. do not estimate trends in OH but discuss the different OH model fields on CH4 growth rate
17496	31	39	31	39	Insert 'a' after 'play' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
22570	31	46	31	46	Methane emissions corresponding to their best OH estimates'. I do not understand which emissions you mean here. Is it the sum of sources in Table 5.2? Please elaborate [Gwenaëlle GREMION, Canada]	Accepted - modified as "Methane emissions by inversions corresponding ". No we do not use these kind of inversion results in synthesis of CH4 sources and sinks
24646	31	46	31	56	The points this paragraph is trying to make don't come across clearly. [William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The main aim is to discuss regional emission estimation and emission trends which are well supported by emission inventories. Further attempts have been made to improve the clarify
17498	31	48	31	48	insert 'the' after 'animals,' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
36334	31	48		52	So an increase in methane emissions from budget calculations is inconsistent with global anthropogenic inventory emissions, but it is more consistent with regional calculations? This implies that the sum of the regional emissions does not equal the global anthropogenic inventory emissions - is that right? Are the inverse analyses are telling us that the inventory emissions are wrong? Explain more clearly. [Nathan Gillett, Canada]	Noted. Not exactly. The problem is the box-model approach. We only get a global total number and no ways to check the validity of the results in box-models. But using regional inversions, we are able to check where possibly the inventories are right or wrong.
45668	31	49			see also the discussion of these points in Nisbet et al 2019. [Euan Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Here are we are trying to emphasize the usefulness of regional inversions. Many aspects of CH4 emissions are not clear just from measurements or box modelling approaches, which do not account for the region-specific emissions, losses and transport



Comment ID	From Page	From Line	To Page	To Line	Comment	Response
22572	31	56	32	1	This figure 2 has really low resolution (I understand it is in prep.) but it seems that "Boreal Asia" is not depicted anywhere. [Gwenaelle GREMION, Canada]	Taken into account. This figure is improved for clarity. Boreal Asia is replaced by Russia - Thank you.
45670	32	3			recent growth is driven by emissions' - i.e. not sinks. While this may well be true, and I'd probably think right, this brusque dismissal of the sink hypothesis doesn't sit well with the more nuanced discussion in the previous paragraphs. Chandra et al - Citing an unreviewed ms in prep is probably not acceptable here - AGU would exclude it. [Euan Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Some discussion on the OH trends has been given already in the previous paragraph. Please note that Chandra et al. uses well established tools for inversion and forward model from Patra et al. (2016, 2018). Longer inversion and additional information have been brought together.
22574	32	5	32	5	strong link between the emissions and CH4 growth rate'. Which emissions are meant here? All emissions? Please specify [Gwenaelle GREMION, Canada]	Accepted - modified as "the total emission variability and CH4 growth "
22576	32	9	32	9	Fig 5.2 in the cross-chapter is hard to read at this scale, as it's very busy. [Gwenaelle GREMION, Canada]	Taken into account. Improved Box Fig 2
43622	32	20	32	31	This para makes several statements using likelihood expressions, but none of them are obviously based on a probabilistic assessment. I think the authors need to double check their use of the uncertainty language. Using expressions of confidence seems more appropriate for the material presented. [Andy Reisinger, New Zealand]	Noted. The assessment of uncertainty language is improved
47788	32	21	32	21	IPCC uncertainty language used incorrectly: a confidence statement (e.g., high/medium/low confidence) is made up of 2 clauses (evidence and agreement), which must be used together. Please refer to the IPCC guidance note on uncertainty for correct use of terms: <a href="https://wg1.ipcc.ch/SR/documents/ar5_uncertainty-guidance-note.pdf">https://wg1.ipcc.ch/SR/documents/ar5_uncertainty-guidance-note.pdf</a> [WGI TSU, France]	Noted. The assessment of uncertainty language is improved
47742	32	23	32	23	IPCC uncertainty language used incorrectly: a confidence statement (e.g., high/medium/low confidence) is made up of 2 clauses (evidence and agreement), which must be used together. Do not use evidence or agreement statements on their own. / High evidence is not a correct IPCC uncertainty term. Please refer to the IPCC guidance note on uncertainty for correct use of terms: <a href="https://wg1.ipcc.ch/SR/documents/ar5_uncertainty-guidance-note.pdf">https://wg1.ipcc.ch/SR/documents/ar5_uncertainty-guidance-note.pdf</a> [WGI TSU, France]	Noted. The assessment of uncertainty language is improved
22578	32	23	32	25	No confidence interval is provided for this statement. The discussion above in this cross-chapter box seems to suggest either low or medium agreement. Can the authors add this? [Gwenaelle GREMION, Canada]	Noted. The assessment of uncertainty language is improved
47740	32	24	32	26	Please check the use of this IPCC uncertainty language term. Are you able to provide a traceable account to assigning this uncertainty statement? Note that likelihood statements are quantified terms - phrases like likely and very likely have quantifiable probabilities associated with them. Please check it has been used correctly here. Please refer to the IPCC guidance note on uncertainty: <a href="https://wg1.ipcc.ch/SR/documents/ar5_uncertainty-guidance-note.pdf">https://wg1.ipcc.ch/SR/documents/ar5_uncertainty-guidance-note.pdf</a> [WGI TSU, France]	Noted. The assessment of uncertainty language is improved

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
45672	32	25			no mention of the 'warming feeds the warming' hypothesis - that heating and wetter wetlands are emitting more. Seems to be big exclusion! Also it would be good to mention Miller et al, who showed very strong increases in emissions across tropical Africa as well as in China. Miller, S. M., Michalak, A. M., Detmers, R. G., Hasekamp, O. P., Bruhwiler, L. M., & Schwietzke, S. (2019). China's coal mine methane regulations have not curbed growing emissions. Nature communications, 10(1), 303. [Euan Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The feedback processes are discussed later in section 5.4.7. The feedback processes are have not been clearly identified for the period 1980-2018, which is what the topic of this cross chapter box. Yes, Miller et al. is cited in the main text and we have given a reference to section 5.2.2.2 instead of repeating much of that here.
22580	32	27	32	27	a priori emissions' Could you add (bottom-up) here for clarity? [Gwenaelle GREMION, Canada]	Accepted
17500	32	30	32	30	Change 'increae' to 'increases' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
47080	32	37			5.2.3: Headers of the sub-chapters in this chapter do often not match with the following content of those sub-chapters. Would be good, to distinct between natural and anthropogenic emissions and within those two sub-chapters between terrestrial, aquatic and atmospheric emissions. Try to keep always the same order of those chapters for CO2, N2O and CH4. [Sophie von Fromm, Germany]	accepted - text revised, with the exception of atmospheric emissions, as this does not apply to N2O
17502	32	41	32	41	I suggest defining N2O for consistency (elsewhere methane and carbon dioxide are defined) [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	editorial
17504	32	42	32	42	Change to Pre-Industrial [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
22582	32	42	32	43	Refs are not in chronological order like in previous sections. [Gwenaelle GREMION, Canada]	Taken into account
41816	32	45	32	46	14N and 15N : numbers in exponent [Marc Aubinet, Belgium]	Accepted - text revised
22584	32	46	32	47	as well as the 15N site preference, in N2O has changed since 1940'. Has this been shown statistically? It is not obvious from Figure 5.14. [Gwenaelle GREMION, Canada]	Accepted - Statistical assessment of the trend is provided by Prokopiou et al. 2017, ACP, cited in the FOD. The relevant data have been added to Figure 5.14
45658	32	53			"High accuracy and density" - that's a bit overstating the accuracy and especially the density of long time series [Euan Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Noted. This is relative to the prior time periods. The data before 1990 do not allow us to calculate the growth rate at good confidence and that situation is changed. Yes, the network size is still smaller than those for CH4 or CO2.
13284	33	1	33	5	why are units by volume (ppbv) suddenly used rather than by mass ones (ppb) ? [Frederic Chevallier, France]	Accepted - text revised
47744	33	7	33	7	Please check the use of this IPCC uncertainty language term. Are you able to provide a traceable account to assigning this uncertainty statement? Note that likelihood statements are quantified terms - phrases like likely and very likely have quantifiable probabilities associated with them. Please check it has been used correctly here. Please refer to the IPCC guidance note on uncertainty: <a href="https://wg1.ipcc.ch/SR/documents/ar5_uncertainty-guidance-note.pdf">https://wg1.ipcc.ch/SR/documents/ar5_uncertainty-guidance-note.pdf</a> [WGI TSU, France]	Accepted - text revised

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22586	33	13	33	13	with 'atmospheric', do you mean 'tropospheric' here? [Gwenaelle GREMION, Canada]	Accepted -text revised
22588	33	19	33	19	The y-axis of the inset of Figure 5.14a should better be $d(N_2O)/dt$ like for CH4 in Figure 5.12b [Gwenaelle GREMION, Canada]	accepted - Figure revised
22590	33	19	33	19	Legend placement in Fig 5.14 makes it hard to read. [Gwenaelle GREMION, Canada]	accepted - Figure revised
17506	33	24	33	24	Change Nino to Niño [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
51600	33	33	33	36	It would help if there were some simple comparison to overall GHG emissions - what % this N2O figure means in the overall picture. Is that possible? Hard to appreciate the meaning, without comparisons. [Lindsey Cook, Germany]	Taken into account - dealt with in Section 5.2.4
6313	33	33	33	46	Key interaction here that may be worth flagging up in terms of Nr deposition, N2O emissions and CO2 uptake. It's referred to earlier, and also for N dep to oceans, but not here for terrestrial (where it's arguably even more important) (e.g. Reay et al. 2008 Global nitrogen deposition and carbon sinks [Nature Geos.], Reay et al 2012 Global agriculture and nitrous oxide emissions [Nature Climate Change]). [Dave Reay, United Kingdom (of Great Britain and Northern Ireland)]	taken into account - this is dealt with in Sections 5.2.3.4
17508	33	35	33	35	Delete , after 'mamangemtn' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
17510	33	36	33	36	) required after 2010 [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
17512	33	41	33	41	Move 'adequately' to after 'account' to remove split infinitive [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
26862	33	44	33	46	This sentence is not easy to understand, 70% of the increase of N2O is due to increased use of nitrogen fertiliser and manure, that's fine. But then, "20% is due to an increase of manure application in in pasture/range/paddock" is the 70% before related to arable land only? Manure management 10%, is that due to a changed management? [Rebecca Danielsson, Sweden]	Accepted - text revised to clarify that 70% is indeed only from agriculture
17514	33	46	33	46	Full stop required [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
22592	33	51	33	51	What does NMIP stand for? [Gwenaelle GREMION, Canada]	Accepted - text revised
22594	33	56	33	56	Shouldn't "forcings" be replaced by "fluxes"? [Gwenaelle GREMION, Canada]	accepted - text changed
22596	34	5	34	5	Please change title to 'Non-agricultural anthropogenic sources' for clarity [Gwenaelle GREMION, Canada]	Accepted - text revised
17516	34	12	34	13	Exponential expressions need to be as superscripts [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
22598	34	20	34	22	In the CH4 budget (Table 5.2) the emissions from grassland, savannah and first fires could be quantified separately as natural sources, whereas here (and in Table 5.3) they are included as anthropogenic sources. I understand that the budget may not allow separation between the various sources of N2O release from biomass burning, but I do think it should be acknowledged that not all biomass burning is of anthropogenic origin. [Gwenaelle GREMION, Canada]	Accepted - text revised

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
22600	34	20	34	22	Here, the definition of "biomass burning" or what is contained under this umbrella term is different from the use on page 29 (Lines 17-26) or the Table 5.2. (see also, my comment No. 12) [Gwenaelle GREMION, Canada]	Accepted - text revised according to comment 22598
22602	34	25	34	25	Please change title to 'Ocean emissions' as freshwater emissions are not discussed here [Gwenaelle GREMION, Canada]	Accepted - text revised
13772	34	25	34	25	You need to remove "freshwater emissions" from the title as this section only addresses ocean fluxes [Pierre Regnier, Belgium]	Accepted - text revised
22604	34	25	35	27	I suggest to present 5.2.3.4 (Land emissions and sinks) before 5.2.3.3 (Ocean and freshwater emissions) to allow for a clearer flow of the text. [Gwenaelle GREMION, Canada]	rejected - section structure remains consistent with CO2 and CH4 sections
43674	34	25	35	28	The caption of the 5.2.3.3 section is "Ocean and freshwater emissions". However, freshwater emissions were not introduced here, and by contrast, they were mentioned in the 5.2.3.4.2 section. These two parts could be combined together with the caption being "Ocean and inland water emissions". [Xinghui Xia, China]	Accepted - text revised: sections have been merged
51954	34	39	34	45	It would seem worth referring to chapter 2 (expansion of these zones) and chapter 4 (projected continued increase) in this paragraph and maybe also chapter 9 as well as section 5.3? [Peter Thorne, Ireland]	Accepted - text revised to link to Section 5.3, where this is dealt with in detail
36336	34	47		48	Briefly describe the source of this anthropogenic nitrogen deposition (nitrates in run-off?). And give the sign of the influence on ocean emissions of N2O. [Nathan Gillett, Canada]	Accepted - text revised
17518	34	49	34	49	Change to Pre-Industrial [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - The same spelling as in the IPCC SR1.5 / SROCC has been applied
22606	34	50	34	52	Please specify "larger". This seems to be a very specific source and it would be interesting to see how relevant it is. [Gwenaelle GREMION, Canada]	Accepted - text revised to highlight regional variability
29130	34	50	34	52	5.2.3.3. The role of lakes as N2O sinks and sources should be clarified. There is recent evidence from regional studies that majority (67%) of small agricultural ponds (Webb et al. 2019 (PNAS 116:20) and over 40% unproductive lakes can be net N2O sinks (Soued et al. 2015 Nature Geoscience DOI: 10.1038/NGEO2611.) Citation from Webb et al. 2019 (PNAS 116 :20) 'Inland water bodies are currently considered to be nitrous oxide (N2O) sources to the atmosphere, based on limited studies on large lakes and reservoirs. However, emissions from small artificial waterbodies, such as farm reservoirs, are currently unaccounted for in global models. We present a regional-scale study of N2O in farm reservoirs and demonstrate that the majority of these waterbodies act as N2O sinks. Our findings contradict previously held assumptions that nitrogen enriched and eutrophic surface waters within agricultural landscapes are strong sources of N2O' [Tuula Larmola, Finland]	Accepted - text revised
29132	34		35		The structure with two paragraphs 5.2.3.3. (P34L50-52) and 5.2.3.4.2 (P35L22-27) containing information on freshwater N2O emissions is confusing. [Tuula Larmola, Finland]	Accepted - freshwater and marine sections have been combined

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
43676	35	1	35	20	Considering the great importance and our limited knowledge for N2O sink in land, the following sentence should be added in this section. "More research should be comprehensively conducted to improve the accuracy of current N2O sink estimation." [Xinghui Xia, China]	rejected - there is not need to single out one particular term of the budget as particularly research worthy, in particular since the magnitude of the N2O sink is small compared to the uncertainty in other major fluxes. The budget table is fully traceable with respect to the uncertainty of each budget item, providing sufficient evidence for fluxes that need better constraint
39356	35	3	35	14	This section should include a discussion of the decline in tropical forest area and how that has caused a net decrease in natural soil emissions. This will be in the updated work by Tian et al. [Eric Davidson, United States of America]	Accepted - text revised
22608	35	14	35	14	I assume "Nr" should be "N" [Gwenaelle GREMION, Canada]	Accepted - text revised
44876	35	16	35	16	Add the following subclause to this sentence: "Boreal and arctic regions (> 55 °) contribute little to the overall natural N2O flux (0.3 Tg N yr-1 during 1980-2016), but in future emissions from the Arctic may be rising since some studies indicate increased risk of N2O release with permafrost thawing and/or warming (Voigt et al., 2017a; Voigt et al., 2017b; Abbott et al., 2015; Elberling et al., 2010)". [Christina Biasi, Finland]	noted - this is dealt with in Section 5.4.7
44878	35	16	35	16	References above: Voigt C, Lamprecht RE, Marushchak ME et al. (2017a) Warming of subarctic tundra increases emissions of all three important greenhouse gases - carbon dioxide, methane, and nitrous oxide. Glob Chang Biol, 23, 3121-3138. Voigt C, Marushchak ME, Lamprecht RE et al. (2017b) Increased nitrous oxide emissions from Arctic peatlands after permafrost thaw. Proceedings of the National Academy of Sciences of the United States of America, 114, 6238-6243. Elberling B, Christiansen HH, Hansen BU (2010) High nitrous oxide production from thawing permafrost. Nature Geoscience, 3, 332-335. Abbott BW, Jones JB (2015) Permafrost collapse alters soil carbon stocks, respiration, CH4, and N2O in upland tundra. Global Change Biology, 21, 4570-4587. [Christina Biasi, Finland]	noted - this is dealt with in Section 5.4.7
39354	35	16	35	17	The citation of the Davidson 2015 paper here does not make sense, or perhaps there is a mistake in the references. The Davidson 2015 paper listed there has nothing to do with N2O. I made an oral presentation at the AGU meeting in 2015 on this topic, and perhaps that is what is intended here, although I'm not sure if meeting abstracts can be cited. Another possibility is to site a work in progress by Tian et al. that includes the soil N2O sink. [Eric Davidson, United States of America]	Accepted - reference removed and replaced by Schlesinger et al. 2013

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
47746	35	18	35	18	Not Likely? Unlikely? Please check the use of this IPCC uncertainty language term. Are you able to provide a traceable account to assigning this uncertainty statement? Note that likelihood statements are quantified terms - phrases like likely and very likely have quantifiable probabilities associated with them. Please check it has been used correctly here. Please refer to the IPCC guidance note on uncertainty: <a href="https://wg1.ipcc.ch/SR/documents/ar5_uncertainty-guidance-note.pdf">https://wg1.ipcc.ch/SR/documents/ar5_uncertainty-guidance-note.pdf</a> [WGI TSU, France]	noted - text revised to avoid use of IPCC calibrated language in this sentence
43678	35	22	35	27	As previous research only focused on lowland waters, the data of N2O emission from high-altitude waters is sparse, limiting the accuracy of current estimation for global inland waters. Therefore, the following sentences are suggested to be added at the end of 5.2.3.4.2: "In addition, recent research showed that N2O emission from high-altitude rivers is different from that of the lowland rivers (Wang et al., 2018). As previous research only focused on lowland waters, the data of N2O emission from high-altitude waters is sparse, limiting the accuracy of current estimation for global inland waters. Therefore, more research should be conducted in high-altitude rivers and the fluvial networks in high-elevation regions should be taken into account to make the estimation more accurate. Gongqin Wang, Junfeng Wang, Xinghui Xia, Liwei Zhang, Sibao Zhang, William H. McDowell, Lijun Hou, Nitrogen removal rates in a frigid high-altitude river estimated by measuring dissolved N2 and N2O, Science of the Total Environment, 2018, 645: 318–328 [Xinghui Xia, China]	rejected - FOD text is not inconsistent with suggested additional reference
22610	35	23	35	23	DIN is only used once. Please remove it. [Gwenaelle GREMION, Canada]	Accepted - text revised
22612	35	23	35	27	Please change to "...point to lower range of emissions (0.1–1 TgN yr-1) than AR5 of 0.3 TgN yr-1 resulting from..." and give an actual range for AR5 values or otherwise change the wording and remove "range". [Gwenaelle GREMION, Canada]	Accepted - text revised
22614	35	35	35	35	It may be better to use units of TgN (and TgN/yr) rather than TgN2O-N (and TgN2O-N/yr) to be in line with the rest of the section [Gwenaelle GREMION, Canada]	accepted - text revised
17520	35	35	35	36	Don't split numbers and units across a line [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account
22616	35	36	35	36	There are no nitrous oxide pools shown in Fig 5.16 [Gwenaelle GREMION, Canada]	rejected - atmospheric pools are shown. Land and ocean pools are unknown and therefore not shown
22618	35	41	35	43	I suggest to write "Anthropogenic and natural N2O emissions" instead of "Section 5.2.3.2", also because 5.2.3.2 only covers anthropogenic, whereas the number "17.2" refers to total emissions. [Gwenaelle GREMION, Canada]	Accepted - text revised
22620	35	43	35	43	Please write "uncertainty range" instead of "range". [Gwenaelle GREMION, Canada]	accepted - text revised
17522	35	49	35	49	Change 'estimates' to 'estimated' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
13272	35	49	35	49	Measurements are not inverted, but the chemistry-transport model is. [Frederic Chevallier, France]	Accepted - text revised

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17524	35	50	35	51	Thompson et al. date missing [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	editorial
22622	35	50	36	2	Year for reference Thompson missing. [Gwenaelle GREMION, Canada]	editorial
39368	35	53	36	2	This summary of the work in progress by Thompson et al. is not quite right. The land source from Africa is poorly constrained by data and the estimate is determined mostly by the "priors." For Brazil, Thompson et al. describe this result of a large calculated emission factor based on the inverse modeling as "puzzling" because it does not seem to be consistent with other bottom-up studies showing low N2O emissions from agriculture in that region, and several confounding processes are possible. [Eric Davidson, United States of America]	Accepted - text revised
22624	36	1	36	1	Did ocean emissions increase, or has their accounted number been increased? Could you please specify this or give an example of why ocean emissions did increase? [Gwenaelle GREMION, Canada]	accepted - text revised (it is the ocean source that has increased according to the inversion based estimates)
17526	36	2	36	2	Thompson et al. date missing [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	editorial
27756	36	6	32	6	replace with published article [Poot Delgado Carlos Antonio, Mexico]	editorial
13274	36	8	36	8	I am not aware of an satellite-based estimate of N2O abundance that would bring additional information to the accurate surface measurements, given the large retrieval uncertainty. [Frederic Chevallier, France]	accepted - the word satellite-based has been removed
36338	36	9			Compare this with the lifetime assessed in AR5. [Nathan Gillett, Canada]	accepted - text revised
22626	36	15	36	15	I suggest to insert Table 5.3 earlier in the text, i.e. before section 5.2.3.2, also to be in line with section 5.2.2 [Gwenaelle GREMION, Canada]	rejected - chapter structure unchanged (compatibility with CO2 section)
47082	36	15	36	58	Table 5.3: Would be helpful to make the design/structure of this table the same as in table 5.2 [Sophie von Fromm, Germany]	accepted - revised taking into account to lower data availability for N2O
39366	36	17	36	56	Table 5.3 needs to be updated. [Eric Davidson, United States of America]	noted
17528	36	25	36	25	Thompson et al. date missing [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	editorial
17530	36	26	36	57	It would be good to have units flagged in the table as well, also the superscripts a-n need defining in the legend or as footnotes [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	editorial
13774	36	40	36	40	It is not fully clear how the range 0.5 (0.2-1.4) was reached as it appears higher than what is reported in section 5.2.3.4.2. Is it because it also includes the emissions from coastal upwelling systems (from Nevison, 0.2 PgC / yr-1) ? [Pierre Regnier, Belgium]	accepted - inconsistency between table and text has been removed
36340	36				Table 5.3. At a minimum I would recommend adopting the same format and periods for this table and Table 5.2. Better I would suggest replacing both with histograms showing changes in the sources and sinks - Consider showing this information in graphical form, perhaps as a set of stacked bars for each decade. There is a lot of information in the table, and it is hard to digest. A histogram would make it much easier to see at a glance which are the dominant sources and sinks, and how each has changed over time. [Nathan Gillett, Canada]	rejected - budget tables have been an essential component of IPCC AR and they provide important and accessible quantitative information. The design of Table 5.3 has been made more consistent with that of Table 5.2 in the SOD. However, different data availability and source categories prevent identical layout.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
36342	36				Table 5.3: What uncertainty model was used to combine errors on the individual sources to get the uncertainty in the total anthropogenic source? It looks as though the uncertainties may have been simply added together - this is very conservative, and assumes perfectly correlated errors between components. Assuming independent errors and adding in quadrature may be more appropriate. In any case the approach used to combine errors should be described in the caption to the table. [Nathan Gillett, Canada]	accepted - FOD table was designed with the AR5 approach. The SOD table adopts a clearer definition of the error bounds. Given the diverse nature of the individual budget estimates the error propagation approach suggested by the reviewer is unfortunately not possible yet.
47840	37	2	38	1	Section 5.2.4 on the relevance of CO2, CH4, N2O as a good framing subsection. Consider moving this earlier in this section of the chapter. Understanding why something is important helps to set an initial context for the reader. [WGI TSU, France]	noted
47842	37	2	38	1	The Special Report on 1.5°C discussed the merits and limitations of GWP and a counter metric GWP*. Would it be relevant to call out to this section of the SR15 here? The relevant material is found in Cross-Chapter Box 2, in Chapter 1, Section 1.2.4. Can a GWP* assessment be discussed here? Please note Ch7 has increased the ERF estimate for methane (please see Section 7.3.2.2). [WGI TSU, France]	Accepted. We have included GWP* and ERF discussion briefly here by citing the SR1.5 & Chap7, respectively
53478	37	3	37	56	I don't think section "5.2.4 The relative importance of CO2, CH4, and N2O" fits in here; at least not as it is written now. The forcing calculations and metrics will be covered in chapter 7. It can be useful to briefly mention the relative contribution of the 3 gases but this should be consistent with and with a reference to chapter 7. [Jan Fuglestedt, Norway]	Noted. We believe this is a policy relevant discussion dealing with GHG sources. No other chapter handle emissions in such details.
36348	37	3	37	56	What is the scientific or policy justification for showing combined anthropogenic and natural fluxes of the CO2-eq emissions of the three greenhouse gases by region? Combining effects of different GHGs for the effects of examining mitigation pathways makes sense. Looking at regional net sources/sinks based on inverse methods of each GHG individually may make sense to help us understand either variations in the natural source or the anthropogenic sources of each gas. But - Paris targets apply only to anthropogenic sources, and it is only the anthropogenic sources that parties to the UNFCCC have control over. As far as I can see, looking at regional variations in combined CO2-eq of anthropogenic and natural source/sinks does not have obvious science or policy value. And there is lots of scope for this to be politically contentious and/or for countries to unduly focus on possible offsetting effects of natural sinks in their region. I would suggest deleting this figure and section. [Nathan Gillett, Canada]	Noted. But we believe, the inverse models can provide independent assessment on the progress of emission reduction policies each countries undertake. The countries will report total GHGs emissions, not only CO2. Combining 3 gases here is an initiative towards that. We have done further refinement of the data presented in Fig. 5.18



Comment ID	From Page	From Line	To Page	To Line	Comment	Response
43112	37	3		55	Because it treats CH4 as a long-lived gas, this section is a bit unclear regarding the relative contributions of the gases. I suggest editing the first paragraph to bring out this difference: "GHGs include short-lived gases such as ozone and black carbon, and long-lived gases with both anthropogenic sources and sinks (e.g. CO2, N2O), and sources for GHGs produced by industrial processes, also called synthetic GHGs (e.g., perfluoro-compounds, chlorofluorocarbons, HCFCs, HFCs). CH4, which has a residence time of around 10 years, is sometimes considered a short-lived pollutant and sometimes considered a long-lived pollutant. Its effects on temperature are more like those of a short-lived gas than a long-lived gas [Allen et al 2018]." [David Frame, New Zealand]	Taken into account. Thank you for the clarifying text. We have revised the text also in light with Chapter 6
24650	37	3			It is not clear that section 5.2.4 is needed. The first paragraph covers information provided in chapter 7. If kept, it must ensure consistency with chapter 7. Figure 5.17 repeats figure 2.9. If it is kept, it must use exactly the same date source as Fig 2.9 for consistency. Chapter 7 explains why the use of CO2-equivalence (GWP and GTP) are inappropriate, and instead recommends that these gases can't be directly compared but should be reported separately. Therefore the text in the third paragraph contradicts statements in section 7.7 and figure 5.18 shows comparisons that section 7.7 has assessed as being misleading. [William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We judge, it may still be worthwhile to keep Fig. 5.17 to show these 3 gases, while citing Fig. 2.9, for conveying the message clearly for the WMGHGS. We have reevaluated the weighting factors by accounting for ERFs.
47342	37	3			this whole section feels out of place in BGC chapter - would fit better in radiative forcing chapter? Need to coordinate with their LAs to find best place [chris jones, United Kingdom (of Great Britain and Northern Ireland)]	Noted. We believe this is a policy relevant discussion dealing with GHG sources. No other chapter handle emissions in such details. Coordinated with other chapters

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
32744	37	5	37	12	While what is stated here is true, what is actually needed is a chart for 2100 forcing (or better yet, integrated over the century) that shows the contributions to forcing from emissions during the 21st century, which are the forcings that can be addressed by emissions reductions. So, while comparing forcings in 2100 versus 2000 for some scenario shows the biggest change in forcing is for CO2 and this leads to a conclusion that CO2 needs to be the primary focus, this is quite misleading because all of the excess methane forcing in 2100 was emitted in the preceding 20 years or so and thus could be eliminated by emissions reductions. And for tropospheric ozone, black carbon and sulfate, all emissions causing their forcings were emitted in the last couple of months or less of 2099, so all are controllable by 21st century emissions reductions. When one accounts for the lifetimes of various species, it turns out that only about half of the 21st century forcing caused by 21st century emissions is due to CO2, with most of the rest split between methane and tropospheric ozone (calculated using the MAGICC model). It is for this reason that pushing for near-term cuts in emissions of short-lived species can have such a large effect—it is possible to eliminate much of their forcing in 2100 by cutbacks in emissions. This is not really the case for CO2 (and N2O) due to its relatively long lifetime. So, I would urge a chart be prepared of forcings in 2100 showing for each species what reductions can be made by cutting 21st century emissions. And I'd note that in presenting this material at the 2009 Copenhagen Science meeting, I got asked by an economist about why I would then want to cutback such emissions until late in the 21st century as the forcing reductions in 2100 could still be achieved and using a discount rate it would be less expensive to do later rather than earlier; I had to explain that what counts for global warming is time-integrated forcing and not the forcing in 2100 which the economists in their	Noted. We have tried to address your concerns and suggestions.
9670	37	6	37	6	change "which the three" to "and the three" [Brian Magi, United States of America]	Accepted - text revised
24648	37	7	31	7	Black carbon is not a gas! [William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account
22628	37	7	36	9	black carbon is not a short-lived gas, but a (solid) aerosol and short-lived climate forcer. I suggest to remove it from this list. [Gwenaelle GREMION, Canada]	Taken into account
13578	37	7	37	7	Black carbon is not a greenhouse gas. [Govindasamy Bala, India]	Taken into account
9672	37	7	37	7	suggest changing "such as ozone and black carbon" to "such as ozone" since black carbon is not a gas [Brian Magi, United States of America]	Accepted - text revised
55792	37	7	37	7	Greenhouse gases do not include black carbon. [Christopher Smith, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account
37742	37	7			This is another place in which methane is described as long-lived, contrary to its classification as short-lived in Chapter 6. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Taken in to account. Text corrected
36344	37	7			Black carbon is not a greenhouse gas. [Nathan Gillett, Canada]	Taken into account
13580	37	10	37	12	Linkage to Chapter 7 (on radiative forcing) could be considered. [Govindasamy Bala, India]	Taken in to account. We have revised the text also in light of Chapter 7

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
55794	37	10	37	24	Chapter 7 provides a comprehensive assessment of GHG radiative forcing. I think the authors could simply reference chapter 7 here to avoid duplication. [Christopher Smith, United Kingdom (of Great Britain and Northern Ireland)]	Taken in to account. We have revised the text also in light of Chapter 7
41818	37	12	37	12	This result appears strongly different from AR5 where radiative forcing was estimated to 1.96Wm <sup>-2</sup> for CO2 and 0.97Wm <sup>-2</sup> for methane (AR5, figure SPM5). Methane appears here to be much lesser important. How do you explain this difference? [Marc Aubinet, Belgium]	Noted. The main difference is coming from the assumption of GWP or the weighting metric in CO2-equivalent sense.
22630	37	15	37	15	The y-axis of this figure should best be changed to something along the line of 'Radiative forcing relative to 1750' to clearly show it is not an absolute value [Gwenaelle GREMION, Canada]	not applicable - figure removed
57350	37	17	37	22	It would be a lot more interesting to show warming-equivalent emissions (based on CO2-e using GWP100, but accounting for cumulative and short-lived behaviour) and temperature response as well as radiative forcing. Happy to supply a draft. [Myles Allen, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. New materials added
53480	37	17	37	22	I suggest leaving out figure 5.17. Chapter 7 is doing this and better to refer to the assessment there instead of using material from a single paper from 2006. [Jan Fuglestedt, Norway]	Noted. We have checked the Figures 7.14 and 7.15, and Figure 2.9. We shall coordinate with Chap 2 and 7
13586	37	17	37	32	Chapter 4 uses the definition of effective radiative forcing (ERF) for the estimation of radiative forcing. Here, some simple formulae are used. This may be stated in the caption. [Govindasamy Bala, India]	Noted. This is a good point. We have consolidated our discussions using ERF (please note that you mean Chap 7)
9674	37	27	37	28	shouldn't "Global Temperature Potential" be "Global Temperature-Change Potential"? [Brian Magi, United States of America]	editorial
13582	37	27	37	32	Can we say that GWP accounts only for the radiative forcing and GTP represents the total response of the climate system and hence it accounts for both radiative forcing and feedbacks? [Govindasamy Bala, India]	Noted. May be not so simply.
53482	37	27	37	32	This presentation of GWP and GTP is, in my view, not needed here. (What is written is imprecise and also incorrect; e.g. that GWP provides information on the warming path and therefore impacts). I suggest referring to ch7 for description/explanation if you decide to use these. [Jan Fuglestedt, Norway]	We have reduced the length of the section to the minim showing emissions with both GWP and GTP, and we refer to Ch7 for the value of each of these two indices.
22632	37	27	37	43	GWP100 should not be used as a metric to compare short-lived forcers with CO2. It greatly understates the long-term impact of CO2 and it is no surprise or "interestingly" (as in "Interestingly, the significance of CH4 to the global climate is dwarfed when GTP is chosen over GWP over a 100-year time horizon") that we see this effect. Currently, there is in fact no metric available that gives useful comparison for both long- and short-lived pollutants. GWP has its origins in the first IPCC report and here in AR6 it would be the right time to abandon these metrics (also GTP). Please read Pierrehumbert 2014 "Short-Lived Climate Pollution" (DOI: 10.1146/annurev-earth-060313-054843) for a more indepth and convincing line of evidence. [Gwenaelle GREMION, Canada]	Noted. We have improved this discussion, also using other metrics developed in Chap 7. Agree that GTP100 undermines the importance of CH4

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55796	37	27	37	43	The problems associated with boiling down everything into metrics have been discussed previously, particularly for methane (e.g. Allen et al., 2018, npj Climate and Atmospheric Science). Again, chapter 7 deals with metrics (as the authors have noted). I am not sure it fits here. [Christopher Smith, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Please understand that the main aim of this section is not to discuss the metric, but how the metric would be applied when regional/country level emissions accounted for in policymaking.
53484	37	27	37	56	While I see the value of discussing regional contributions by emissions of the three gases, I am not convinced about the value of figure 5.18 and the related discussion. I think you don't need metrics for this and that it would be more interesting with emissions in pure mass units. But I think this chapter could address net zero and neutrality. If this is meant as an attempt for that then it needs further development and a different introduction. [Jan Fuglestedt, Norway]	We have reduced the length of the section to the minimum showing emissions, and we refer to Ch7 for a discussion of different indices.
36346	37	27		32	Is such a comparison in scope for Chapter 5? Would it belong better in Chapter 7? [Nathan Gillett, Canada]	the issue of regional emissions of these 3 GHGs are only discussed here. We have made the section shorter and only focus on GHG emissions, that is not covered anywhere else.
56632	37	29	37	29	Replace "which measures the heat absorbed over a given period of time" with "which measures the radiative forcing over a given period of time". The former is not quite correct as representation for GWPs.. [Malte Meinshausen, Australia]	Taken in to account. Text revised
25666	37	29	37	29	"heat absorbed" is non-physical. It would seem that what is meant is "the integrated increase in net energy imbalance of Earth's climate system attributable to emissions of a given substance, normalized to emission rate" . [Stephen E Schwartz, United States of America]	Accepted. Thank you
25668	37	29	37	29	Neither this chapter nor chapter 7 presents the pertinent impulse response functions. [Stephen E Schwartz, United States of America]	Noted. Additional references cited
9676	37	34	37	43	I find this paragraph difficult to understand. The section seems to be about relative importance, but then this paragraph discusses multiple measures (GTP and GWP) and then each of these at multiple time scales and regional levels. I would suggest either simplifying the discussion perhaps by eliminating multiple regions in the discussion, or expanding the discussion to better convey any nuance that was intended (perhaps by including more citations to the literature, or to deeper discussions elsewhere in the AR6 Working Group reports). At the end of this important overall section, I find myself confused by this paragraph which I think is a poor way to end. [Brian Magi, United States of America]	Noted. We appreciate very much the encouraging message. We have improved the discussions through discussions with other chapter leads and colleagues

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
32746	37	34	37	43	It really needs to be explained that models don't use GWP--that this is a way to try to simplify the science for negotiators. The great problem is that using the GWP-100 metric hides the significant near-term benefits of cutting emissions of short-lived species (as explained in other of my comments) because their contributions to radiative forcing will quickly go down due to their short lifetimes. Another problem with GWP is that it very hard to use it to put together a strategy for meeting some goal in some year, say 2040 as each year's emissions contribute to forcing for the next 100 years. There is an effort rising out of the International Standards Organization and American National Standards Institute that uses the same radiative forcing equations and creates a metric based on radiative forcing and then integrated radiative forcing over time (so temperature change related) to enable better planning of emissions reductions. I don't think it will be published by the IPCC deadline, but it is coming--and it will overcome the shortcomings of using GWP-100 as an approximation for all aspects of climate change planning. [Michael MacCracken, United States of America]	Noted. Thank you for supporting the idea of simplifying our science for negotiators. This is the first time in IPCC AR that we are trying put three gases together. We are improving this discussion.
22634	37	35	37	37	I do not understand what this statement is so interesting, nor what consequences it may have for management of the various GHGs. Could the authors elaborate on this? And what about N2O? It seems that the same applies to N2O, why is that GHG not mentioned here? [Gwenaelle GREMION, Canada]	Taken in to account. Changed to "The high importance of CH4...". N2O has more than 100 years of lifetime, so its GWP and GTP weighted emissions are not very different. A sentence added
29510	37	37	37	37	I think here it should be specified that it is the land biosphere flux that is being referred to, and not the total net CO2 flux. [Rona Thompson, Norway]	Accepted. We have clarified that this is "total net sink". The figure text are modified using multiple inversion models
17532	37	37	37	37	Change 'sink' to 'sinks' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
17534	37	38	37	38	Delete 'and' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
22636	37	54	37	54	Year for reference Thompson missing. [Gwenaelle GREMION, Canada]	editorial
17536	37	54	37	54	Thompson et al. date missing [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account
47792	38	1	44	55	Assessment Structure: If possible, assessment conclusions should be provided in a structured traceable account of how these statements were derived. For example, sections / subsections can start with previous IPCC report conclusions (AR5 or AR6 Special Reports) and then provide an update of the more recent literature, clearly laying out the lines of evidence. Each section / subsection can then conclude with assessment statements. This section currently reads more as a review than an assessment. [WGI TSU, France]	Accepted. This is the first time we are trying assess importance of regional emissions in an integrated sense. The section is further improved

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17538	38	7	38	7	Reference(s) required after 'processes' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account – added Le Quéré C, Andrew RM, Friedlingstein P, Sitch S, Hauck J, Pongratz J, Pickers PA, Korsbakken JJ, Peters GP, Canadell JG, Arneeth A, Arora VK, Barbero L, Bastos A, Bopp L, Chevallier F, Chini LP, Ciais P, Doney SC, Gkritzalis T, Goll DS, Harris I, Haverd V, Hoffman FM, Hoppema M, Houghton RA, Hurtt G, Ilyina T, Jain AK, Johannessen T, Jones CD, Kato E, Keeling RF, Goldewijk KK, Landschützer P, Lefèvre N, Liener S, Liu Z, Lombardozzi D, Metzl N, Munro DR, Nabel JEMS, Nakaoka S-I, Neill C, Olsen A, Ono T, Patra P, Peregon A, Peters W, Peylin P, Pfeil B, Pierrot D, Poulter B, Rehder G, Resplandy L, Robertson E, Rocher M, Rödenbeck C, Schuster U, Schwinger J, Séférian R, Skjelvan I, Steinhoff T, Sutton A, Tans PP, Tian H, Tilbrook B, Tubiello FN, van der Laan-Luijkx IT, van der Werf GR, Viovy N, Walker AP, Wiltshire AJ, Wright R, Zaehle S, Zheng B (2018) Global Carbon Budget 2018. Earth Syst Sci Data 10:2141–2194. doi: 10.5194/essd-10-2141-2018
22652	38	7	38	9	The reference (Doney et al., 2009) is not suited to support this explanation (basic chemistry). (As stated in the next sentence, it was already discussed in the 1950s.) [Gwenaëlle GREMION, Canada]	Rejected – the cited paper “Doney SC, Fabry VJ, Feely RA, Kleypas JA (2009) Ocean Acidification: The Other CO <sub>2</sub> Problem. Ann Rev Mar Sci 1:169–192. doi: 10.1146/annurev.marine.010908.163834” gives full details on the carbonate chemistry in the oceans with the focus on ocean acidification. This paper is broadly cited within the Ocean Acidification scientific community. Carbonate chemistry in seawater, and the effects of increasing atmospheric CO <sub>2</sub> have indeed been discussed earlier, for instance as in Arrhenius S (1896) XXXI. On the influence of carbonic acid in the air upon the temperature of the ground. London, Edinburgh, Dublin Philos Mag J Sci 41:237–276. doi: 10.1080/14786449608620846.

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42468	38	8	38	8	Changes is a somewhat vague term in this context as CO2 uptake by the ocean will definitely decrease the carbonate concentration [Peter Croot, Ireland]	Taken into account – the text has been changed to “leading to a decrease in the concentrations of carbonate (CO3-2) ions, and increasing both bicarbonate (HCO3-) and hydrogen (H+) ions concentration, which increases the water acidity (Doney et al., 2009)
22654	38	9	38	9	I think it would be more accurate to say "which leads to a decrease of the ocean's pH" instead of "which turns the water more acidic", since the actual and projected pH is > 7. [Gwenaelle GREMION, Canada]	Accepted - text revised
33464	38	9			When discussing ocean acidification, the IPCC report should not use the words "acid" or "acidic." "Acidic" waters are considered those with pH of < 7; except for some extreme environments, the oceans are alkaline. I recommend this be worded as "increasing the concentration of H+ ions which increases seawater acidity" as called for by Gattuso et al. here: <a href="https://news-oceanacidification-icc.org/2015/08/26/a-plea-to-ocean-acidification-scientists/">https://news-oceanacidification-icc.org/2015/08/26/a-plea-to-ocean-acidification-scientists/</a> [Adrienne Sutton, United States of America]	Accepted – text revised
8318	38	12	38	12	Support for the "robust evidence" statement is needed, either in citing a previous assessment or suite of references. [Sarah Cooley, United States of America]	Taken into account - text was changed to mean that there is robust evidence for the decrease in surface seawater pH, added references for the AR5 corresponding chapter 5 and IPCC Special report Oceans and Cryosphere. We kept the statement that there is medium confidence for the effect of pH on organisms and the trophic chain, based on the findings from IPCC SROCC 2019.
22656	38	12	38	12	The use of terms "robust confidence" might be misleading with uncertainty language. [Gwenaelle GREMION, Canada]	Taken into account – here we added a reference to former IPCC assessment reports as the scientific literature point to a robust evidence.
26502	38	12	38	12	I don't think that these effects are known with "robust" confidence as they are many contradicting results available about the effect of ph on organisms. Also, this seems to contradict the "medium confidence" that follows later in the sentence [Nadine Goris, Norway]	Taken into account - text was changed to mean that there is robust evidence for the decrease in surface seawater pH, added references for the AR5 corresponding chapter 5 and IPCC Special report Oceans and Cryosphere. We kept the statement that there is medium confidence for the effect of pH on organisms and the trophic chain, based on the findings from IPCC SROCC 2019.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
42470	38	12	38	12	Robust confidence is perhaps overstating this aspect given the recent special issue by Browman et al. (2016) [Peter Croot, Ireland]	Taken into account - text was changed to mean that there is robust evidence for the decrease in surface seawater pH, added references for the AR5 corresponding chapter 5 and IPCC Special report Oceans and Cryosphere. We kept the statement that there is medium confidence for the effect of pH on organisms and the trophic chain, based on the findings from IPCC SROCC 2019 and the suggested article.
44850	38	12	38	13	The experts think that the response of marine biota to ocean acidification is diverse, I don't believe that the effects are known "robust". The result of culture experiment may change depending on culture condition even if the same species is used. Also, the culture experiment, for example, does not always capture real response. [Kaoru Kubota, Japan]	Taken into account - text was changed to mean that there is robust evidence for the decrease in surface seawater pH, added references for the AR5 corresponding chapter 5 and IPCC Special report Oceans and Cryosphere. We kept the statement that there is medium confidence for the effect of pH on organisms and the trophic chain, based on the findings from IPCC SROCC 2019.
8320	38	13	38	13	Hofmann et al. 2011 is not the most recent or exhaustive reference on this subject. AR5 has a nice box on OA with assessment, and there have been several more recent meta-analyses such as Busch and McElhany and Kroeker [Sarah Cooley, United States of America]	Taken into account - text was changed to mean that there is robust evidence for the decrease in surface seawater pH, added references for the AR5 corresponding chapter 5 and IPCC Special report Oceans and Cryosphere. We kept the statement that there is medium confidence for the effect of pH on organisms and the trophic chain, based on the findings from IPCC SROCC 2019.
8144	38	15	38	25	More information should be added to highlight that oxygen depletion (hypoxia) is a common phenomenon and increases worldwide due to eutrophication and climate change in various aquatic environments. The Friedrich et al. (2014) paper in the journal Biogeosciences ( <a href="https://doi.org/10.5194/bg-11-1215-2014">https://doi.org/10.5194/bg-11-1215-2014</a> ) should be added to refer to any details. It is not only the release of nitrous oxide, but also methane that contributes to climate warming. It should be highlighted that not only the ocean (and particularly coastal environments), but also other aquatic environments (lakes, fjords, and other land-locked water bodies) contribute to the release of greenhouse gases and must be considered in the climate balance. This paper contains several excellent references to other papers on this topic that highlight the importance of oxygen decline as a result of climate change but also as a contributor to climate warming. [Sebastian Naeher, New Zealand]	Taken into account - a) please refer to this chapter's section 5.2 for the N2O and CH4 emissions from other aquatic environments; b) please refer to subsection 5.3.3.4 for coastal oceans, where eutrophication-driven hypoxia and acidification are addressed; c) the suggested paper has been taken into account.



Comment ID	From Page	From Line	To Page	To Line	Comment	Response
26504	38	16	38	16	It has to be mentioned that the ocean does not warm uniformly, but that the most warming occurs at the surface. If the ocean would warm the same amount at the surface and in the deep, then the stratification would not change. [Nadine Goris, Norway]	Accepted - Text changed to "There is high confidence that the surface ocean heat content is increasing (IPCC, 2019)[placeholder for AR6 Chapter 9 citation] due to increasing GHG concentrations in the atmosphere, strengthening upper water column stratification.
33466	38	16	38	17	Chapter 3 section 3.6.2 notes that deoxygenation is likely due to changing ocean circulation, mixing, and/or biochemical processes, rather than the direct thermally-induced solubility effect. The conflict between these sections should be resolved. [Adrienne Sutton, United States of America]	Taken into account - text was changed and added references that refers that warming/solubility are responsible for circa 15% of the oxygen decrease while stratification may be responsible for most of the rest of the observed oxygen decline in the oceans. Warming/loss of solubility is especially strong in the upper open ocean (also stated in IPCC SROCC Chapter5 section 5.3.1). We have also added a cross reference to chapter 3 subsection, as suggested.
42472	38	17	38	17	Changes in respiration are also important here and are likely to increase with temperature. [Peter Croot, Ireland]	Taken into account - According to Breitburg et al. (2018): "warming also raises metabolic rates, thus accelerating the rate of oxygen consumption. Therefore, decomposition of sinking particles occurs faster, and remineralization of these particles is shifted toward shallower depths (Peltzer et al. 2017), resulting in a spatial redistribution but not necessarily a change in the magnitude of oxygen loss". This is discussed further in subsection 5.3.3.2 (this chapter), and in Chapter 3 (3.6.2).
55012	38	20	38	20	The phrase "also occur at the level of marine organisms" can be supported with an assessment of such references as Kawahata et al. (2019) Progress in Earth and Planetary Science 6(1),5 on "Perspective on the response of marine calcifiers to global warming and ocean acidification-Behavior of corals and foraminifera in a high CO2 world hot house." [Kilkis Siir, Turkey]	Taken into account - added the suggested reference and citation to SROCC Chapter 5.
22658	38	20	38	22	"also"? No coupling between acidification and deoxygenation was described before this sentence. [Gwenaelle GREMION, Canada]	Taken into account – "also" removed from text
8322	38	22	38	25	Cross references to AR6 WGII ch. 3 should ultimately be added on this topic and OA impacts. [Sarah Cooley, United States of America]	Taken into account - WG2 Chapter 3 FOD added.
22660	38	28	38	28	Suggestion to include approximate timeframe (as in header 5.3.1.2). [Gwenaelle GREMION, Canada]	Accepted.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
22664	38	30	38	31	A temperature increase "likely exceeding 5-8°C" is quoted for the PETM, but the citation given (Dunkley Jones et al 2013) states it was unlikely to be greater than 4-5°C. 5-8°C is the range given by McInerney & Wing (2011) (cited later in section). I suggest either adding this second reference and adjusting to "likely reaching 4-8°C" or adjusting to "likely reaching 4-5°C" to match just Dunkley Jones et al (2013) [Gwenaëlle GREMION, Canada]	Accepted.
51958	38	30	38	31	Necessary to check for consistency with and reference the chapter 2 assessment of PETM temperature. This feels lower estimate than that in chapter 2 and it is unclear which may be correct and why. [Peter Thorne, Ireland]	Accepted.
44108	38	30	38	41	Additional caveat: Lyons et al. 2019 ( <a href="https://www.nature.com/articles/s41561-018-0277-3">https://www.nature.com/articles/s41561-018-0277-3</a> ) show evidence for remobilization of sedimentary fossil carbon increased carbon delivery (release of 100 to 10,000 PgC as CO <sub>2</sub> ) to the oceans ~10-20 kyr following PETM onset, contributing to delayed climate system recovery [Sara Kahanamoku, United States of America]	Noted. This chapter doesn't pretend to provide an exhaustive review on PETM literature. Rather we focus on comparing the feedbacks associated with the PETM carbon release with feedbacks that are emerging/may emerge in the future as a result of anthropogenic CO <sub>2</sub> emissions
22662	38	30	38	41	Suggestion: shift last sentence of this paragraph to the front to motivate this subsection [Gwenaëlle GREMION, Canada]	Accepted - text revised
47748	38	30	38	53	Please check the use of this IPCC uncertainty language term. Are you able to provide a traceable account to assigning this uncertainty statement? Note that likelihood statements are quantified terms - phrases like likely and very likely have quantifiable probabilities associated with them. Please check it has been used correctly here. Please refer to the IPCC guidance note on uncertainty: <a href="https://wg1.ipcc.ch/SR/documents/ar5_uncertainty-guidance-note.pdf">https://wg1.ipcc.ch/SR/documents/ar5_uncertainty-guidance-note.pdf</a> [WGI TSU, France]	Accepted. Uncertainty language has been carefully assessed in the revised document
36350	38	30		31	Reference Section 2.3.1.1.1. [Nathan Gillett, Canada]	Accepted.
47246	38	31	38	32	Be less explicit regarding source of PETM CO <sub>2</sub> release (e.g. Gutjahr (2017) attribute PETM source to be up to 90% associated with volcanism) - there remains various hypotheses related to the source of the initial carbon pulse and explicitly attributing volcanism (related to emplacement of the North Atlantic Igneous Province) remains challenging due to chronological uncertainties etc. And if mentioning volcanism as being influential in PETM then ensure citation is attributed to Gutjahr (2017) [Katrina Nilsson-Kerr, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The point we try to make here is to compare the PETM, perhaps the best case study for rapid carbon injection scenarios in the past to the anthropogenic carbon release. In that sense discussing possible sources accounting for the carbon release during the PETM is perhaps not so relevant. Paragraph has been amended to avoid unnecessary complications
19214	38	31	38	35	please note that the 3,000 PgC estimate is not based on volcanic CO <sub>2</sub> , but rather on methane [Baerbel Hoenisch, United States of America]	Noted
17540	38	35	38	35	Subscript 2 in CO <sub>2</sub> [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
36352	38	35			Do the authors mean to refer to the RCP8.5 Extension scenario here? RCP 8.5 finishes in 2100, and has only about 2000 PgC emissions (see IPCC AR5 Fig SPM.10). RCP 8.5 has approx 5000 PgC cumulative emissions in 2300. [Nathan Gillett, Canada]	Noted

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
25468	38	37	38	40	Is there anything new to report regarding hydrate and permafrost as these statements are attributed to pre-AR5 refs. [Sharon Smith, Canada]	Accepted. References have been updated
22666	38	37	38	40	Methane emissions (from hydrates or permafrost) are presented as a feedback to the increase in atmCO2 (in order to match the full warming), but the source of the atmCO2 in the reference (Zeebe et al, 2009) is already methane from hydrates with a proposed feedback of wetland methane. I think this sentence as it stands is somewhat confusing in identifying proposed drivers and feedbacks. I would suggest rephrasing to say that some authors (e.g. Zeebe et al, 2009; Dickens, 2011, CotP, doi:10.5194/cp-7-831-2011 - a seminal review of this hypothesis) propose a methane hydrate driver while others (e.g. Gutjahr et al, 2017; Cui et al, 2011) propose some combination of a volcanic driver (potentially coupled with weakening negative feedbacks: Armstrong-McKay & Lenton, 2018, CotP, doi:10.5194/cp-14-1515-2018) with subsequent positive feedbacks on warming from organic carbon sources such as peat, permafrost (e.g. Deconto et al, 2012), or methane hydrates (Lunt et al, 2011, NatGS, doi:10.1038/ngeo1266 - pulsed clathrate releases). [Gwenaelle GREMION, Canada]	Accepted. AR6 focuses on assessing the most recent literature and not on providing an exhaustive review on the various scenarios proposed to explain the PETM. The latest literature (Gutjahr et al., 17), clearly supports a volcanic source as the proximal cause for CO2 degassing, mainly based on the duration/magnitude of the pulse. This being said, we agree that this aspect has not yet been settled. The main point here, was to provide a perspective on the rate of degassing (irrespective of its origin) and associated ecological feedbacks in the context of the anthropogenic carbon release.
6706	38	38	38	40	Schneider et al. 2019 (doi:10.1038/s41561-019-0310-1) suggests a cloud feedback mechanism for the extreme warmth of the PETM. Should be mentioned as alternative hypothesis. [Andrew MacDougall, Canada]	Noted
45674	38	38			Hydrate hypothesis is tough to reconcile with the feedback requirement as PETM hydrates were deep and the feedbacks much too slow. Nisbet, E. G., Jones, S. M., Maclennan, J., Eagles, G., Moed, J., Warwick, N., ... & Fowler, C. M. R. (2009). Kick-starting ancient warming. Nature Geoscience, 2(3), 156 [Euan Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Noted
47088	38	43	38	44	5.3.1.1.: For which reservoir was this increase calculated? [Sophie von Fromm, Germany]	Accepted. The inferred increase relate to the atmosphere. Sentence has been clarified.
22668	38	45	38	45	The likely in this sentence should be in italics (if it is referred to uncertainty language) or changed for other word to avoid confusion. [Gwenaelle GREMION, Canada]	Accepted. Sentence has been modified to avoid confusion
36354	38	45			What is the timescale associated with 'transient' here? In most of the report 'transient' refers to behaviour on timescales less than that on which the climate equilibrates with a change in forcing (under ~100 years), but I think the timescale here is much longer. [Nathan Gillett, Canada]	Accepted. Good point. Sentence has been modified as recommended
22670	38	46	38	48	"shoaling of the carbonate compensation depth" should be explained further at this point. [Gwenaelle GREMION, Canada]	Accepted

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27976	38	46	38	57	Page 38, lines 46-57: The part about shoaling of carbonate compensation depth. I found this sentence a bit confusing, the rapid shoaling reduced the depth? Also perhaps add a quick synonym for shoaling and explain a bit more in depth the carbonate compensation part. Page 39, line 3: "... PETM was much more deleterious". Florid wording, perhaps replace with detrimental or harmful. [roderik van de wal, Netherlands]	Accepted
22672	38	47	38	47	If find it unclear as if it means the carbonate compensation depth was at a depth of 2000m or if it was 2000m shallower than its actual depth. It might be usefull to explain the carbonate compensation depth. [Gwenaelle GREMION, Canada]	Accepted. Carbonate compensation is now defined more explicitly. Sentence has been modified to avoid confusion
17542	38	51	38	51	Change to '21st Century' for consistency [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised, but with the "C" not capital, following the AR6_WGI_StyleGuide ("21st century")
36356	38	51		52	Replace 'predicted for the end of the twenty-first century (RCP 8.5)' with 'projected for the end of the twenty-first century under RCP 8.5'. [Nathan Gillett, Canada]	Accepted.
16160	38	51			"0.4 pH unit decrease". Decrease from which level? [AKIHIKO MURATA, Japan]	Accepted. Sentence has been modified to avoid confusion
22674	38	52	38	53	To allow for easy comparison between PETM and current acidification rates, numbers/ranges could be included here. [Gwenaelle GREMION, Canada]	Accepted
44106	38	55	38	55	Change Honisch to Hönisch [Sara Kahanamoku, United States of America]	Taken into account
22676	39	1	39	2	The first reference given regarding diversity reductions (Bralower et al (2018) for shelf) does not actually significantly mention diversity or ecosystem impacts, instead focusing on evidence of dissolution. The second reference (Robinson 2011) actually suggests that at the study site there wasn't much evidence for unusual changes in diversity, stating that the Guyot ecosystem in question was relatively unaffected and that this may be due to consistent diversity. I would suggest using already-used references Ridgwell & Schmidt (2010) and McInerney & Wing (2011) instead here, which both mention non-benthic diversity changes being mixed despite high turnovers in some habitats. [Gwenaelle GREMION, Canada]	Accepted
22678	39	17	39	20	In this paragraph it should be clarified if the mentioned feedbacks are positive or negative. [Gwenaelle GREMION, Canada]	Accepted
22680	39	18	39	20	A minor point, but the mentions of regrowth of organic carbon stocks and global increase in marine export production could be merged here, as the primary mechanism in Gutjahr et al (2017) for organic carbon sequestration is via marine organic carbon burial, which is similar to the mechanism proposed by Bains et al (2000). An alternative way of differentiating here would be on the basis that Bowen & Zachos (2010) focus on terrestrial biosphere sequestration, while Gutjahr et al (2017) and Bains et al (2000) focus on marine biosphere sequestration. [Gwenaelle GREMION, Canada]	Noted
22682	39	25	39	31	To place the last deglaciation in context, CO2 emission rates could be compared to current values. [Gwenaelle GREMION, Canada]	Accepted. Good point.

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32748	39	25	39	31	It would be helpful to also indicate that during this transition, sea level also rose about 120 meters (well, nearly the full glacial to interglacial amount). So, all this water added to the ocean had no CO2 locked up in it when it was ice on land, so it is quite impressive that there was enough CO2 locked up in the ocean to not only increase the atmospheric concentration, but also to provide for increased vegetation and also be taken up by added water put into the ocean as ice sheets disintegrated. I'd thus urge a bit fuller discussion of what was happening, both in the interest of science and to make clear that very large changes can occur, and the human influence is of comparable magnitude (or greater, at least becoming greater). [Michael MacCracken, United States of America]	Rejected. While this is certainly an interesting observation, it does not directly relate to carbon cycle feedbacks.
22684	39	29	39	29	It would be really good to mention the timing of these rapid transitions, which are at ~16.3, ~14.8, and ~11.7 ka BP, respectively ( Marcott et al.,2014). [Gwenaelle GREMION, Canada]	Accepted
22686	39	29	39	29	Instead of few hundred years, it would be better to say on a centennial scale , or to be more quantitative, ~100-200 years [Gwenaelle GREMION, Canada]	Accepted
44852	39	30	39	30	Permafrost? If my understanding is correct, the inventory of carbon of permafrost was reduced during the LGM (thus CO2 was not sequestered, but lost). Is this line consistent with the previous description? [Kaoru Kubota, Japan]	This sentence intends to state that there was a net release of carbon from both the permafrost and ocean interior reservoirs during this interval, and this irrespective on how much carbon was previously stored in either reservoir.
22688	39	33	39	33	More specific: what geochemical proxies is this assessment based on? How does this compare to decrease in foram shells and coccolith mass mentioned later on? [Gwenaelle GREMION, Canada]	pH estimates are based on $\delta^{11}B$
19216	39	33	39	34	please note that Martinez-Boti et al. 2015b and c is the same reference. In this context, this study is inappropriate to cite because it refers to an area of CO2 outgassing, not ocean-atmosphere equilibrium. Please replace this citation with Henehan et al. 2013 [Baerbel Hoenisch, United States of America]	Accepted
42474	39	35	39	35	Citation for more than 0.1 pH in 100 years? cf p40 where -0.018 per decade is listed for sub tropical zones [Peter Croot, Ireland]	Noted
44854	39	36	39	37	I'm not sure that these are widely accepted facts (decreased shell weight of planktonic foram and coccolith during the last deglaciation). [Kaoru Kubota, Japan]	Rejected. We are not aware of any particular reason to dismiss these proxy reconstructions
43766	39	40	39	44	Yamamoto et al. (2019*) showed that deoxygenation occurred through glacial dust input in the Southern Ocean. Something like " , and glacial dust input (Yamamoto, 2019)" should be added after "...remobilisation length scale (Matsumoto, 2007)". Reference: Yamamoto et al. Climate of the Past, 15, 981-996, <a href="https://doi.org/10.5194/cp-15-981-2019">https://doi.org/10.5194/cp-15-981-2019</a> , 2019. [Michio Kawamiya, Japan]	Accepted
22690	39	44	39	44	"remobilisation length scale" needs explaining. [Gwenaelle GREMION, Canada]	Accepted

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
29368	39	44	39	44	Please also refer to Buchanan et al (2015), which quantified the impact of T-dependent remineralisation length scale on glacial deep-water deoxygenation. Matsumoto (2007) did not investigate this impact on dissolved oxygen directly. Buchanan, P. J., Matear, R. J., Lenton, A., Phipps, S. J., Chase, Z., and Etheridge, D. M.: The simulated climate of the Last Glacial Maximum and insights into the global marine carbon cycle, <i>Clim. Past</i> , 12, 2271-2295, <a href="https://doi.org/10.5194/cp-12-2271-2016">https://doi.org/10.5194/cp-12-2271-2016</a> , 2016. [Akitomo Yamamoto, Japan]	Accepted. Reference has been considered in the revised draft
29370	39	44	39	44	Recent study show that the glacial deep-water deoxygenation is also related to iron fertilization by glaciogenic dust (Yamamoto et al., 2019). Yamamoto, A., Abe-Ouchi, A., Ohgaito, R., Ito, A., and Oka, A.: Glacial CO2 decrease and deep-water deoxygenation by iron fertilization from glaciogenic dust, <i>Clim. Past</i> , 15, 981-996, <a href="https://doi.org/10.5194/cp-15-981-2019">https://doi.org/10.5194/cp-15-981-2019</a> , 2019. [Akitomo Yamamoto, Japan]	Accepted. Reference has been considered in the revised draft
22692	39	44	39	47	The term "T-dependent" is not explained. If this means temperature-dependent, it is the first time in the text when this abbreviation is used, otherwise it is only figure ledgens where this abbreviation is presented [Gwenaelle GREMION, Canada]	Accepted.
22694	39	50	39	54	Am not entirely sure whether the strengthening of NADW culation can transfer the carbon from ocean interior to the mixed layer and then to atmsohpere. Instead, I think its strengthening influence the atmospheric CO2 rather indirectly, by influencing the terrestrial carbon storage ( Kohler et al.,2005; Marcott et al.,2014). Whereas, strengthening of AABW formation can directly lead to an increase of atmsohpere CO2, due to upwelling of carbon rich deeper water in the Southern Ocean. So it would be good to be specific about how AABW and NADW circulation contributes to the concentration of atmospheric CO2. [Gwenaelle GREMION, Canada]	Technically, it is the relative strength of NADW vs. AABW, which ultimately controls to global preformed nutrient concentration and by inference the air-sea partitioning of CO2 (e.g. Ito & Follows, 2005).
22696	39	55	39	55	Should "saturation" instead be "solubility"? [Gwenaelle GREMION, Canada]	Accepted
27758	40	2	40	2	verify the following text (warming event, 14.5 kyr ago) [Poot Delgado Carlos Antonio, Mexico]	Noted
22698	40	5	40	9	Technically, this section is about ocean acidification and deoxygenation, so this paragraph about N2O could be skipped. (Yet it may be interesting/important as a feedbck mechanis, however, this is not mentioned here.) [Gwenaelle GREMION, Canada]	Accepted. The feedback associated with N2O emissions has been added in the revised draft.
22700	40	7	40	7	Please be specific about the heavier isotope used. With the N2O, studies may use d15N, d18O, both, or even their isotopologue. In this work of Schilt et al., (2014), the conclusions are basically based on d15N. [Gwenaelle GREMION, Canada]	Accepted. Indeed. This aspect has been clarified

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36358	40	16			What is the meaning of 'continuing to occur' here? As written it reads as though acidification started prior to the historical period, which is the topic of this section. Also, given that this is a quantified probabilistic statement, and is based on observations of past changes in pH, I suggest 'has occurred' instead of 'is continuing to occur', since it's not clear how you would quantify the present rate of change of pH - it has to be based on the observed rate of change over some period in the past. Also specify the period for which this 'virtually certain' assessment is being made. [Nathan Gillett, Canada]	Accepted - the sentence revised following this comment and the comment #26506.
26506	40	17	40	17	it has to be "growing CO2 uptake by the oceans" [Nadine Goris, Norway]	Taken into account - text revised in conjunction with the comment #36358.
22702	40	17	40	20	"several stations" and "several regions" is too vague. [Gwenaelle GREMION, Canada]	Accepted - text revised.
17544	40	22	40	22	Delete the negative sign, you have already said pH is decreasing (technically a decrease of a negative quantity is an increase!). [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised.
26508	40	22	40	22	it should be "rate of about" [Nadine Goris, Norway]	Accepted - text revised
42476	40	22	40	22	Potential mismatch in cited numbers, as for above p39 line 35 where 0.1 pH over 100 years is quoted. [Peter Croot, Ireland]	Taken into account - period specified.
22704	40	22	40	25	Instead of relativize all given values in this sentence with the word "about", giving ranges would be more quantitative (as in the later part of this paragraph). [Gwenaelle GREMION, Canada]	Accepted - text revised. Range is given.
33488	40	25			Reconsider significant # of digits here. I recall Bates et al. reported aragonite saturation state change upper range is 0.08 per decade. [Adrienne Sutton, United States of America]	Accepted - Range of the rates are shown.
33468	40	27	40	29	This is contrary to what we find in the central and eastern tropical Pacific where post-1998 rates in pH change are more rapid than global averages (Sutton, A.J., Feely, R.A., Sabine, C.L., McPhaden, M.J., Takahashi, T., Chavez, F.P., Friederich, G.E., Mathis, J.T. 2014. Natural variability and anthropogenic change in equatorial Pacific surface ocean pCO2 and pH. Global Biogeochemical Cycles, 2013GB004679.), therefore, it is important to note here the large influence of decadal variability in the shallow overturning circulation and the time period of the time series of choice. [Adrienne Sutton, United States of America]	Accepted - text revised.
16162	40	28	40	29	For this explanation, reference should be cited. I agree to that this is one of the processes that cause lower [H+]. But it is possible enough that increases of [H+] by uptake of anthropogenic CO2 in the tropical ocean is diluted by subsurface water. [AKIHIKO MURATA, Japan]	Accepted - reference added in the text.
44856	40	29	40	29	Add reference such as Oka et al. (2015) [Kaoru Kubota, Japan]	Taken into account - text revised: Ishii et al. (submitted) rather than Oka et al (2019) is cited here.
36360	40	35		36	Assess the overall rate and its uncertainty, rather than giving two different estimates with non-overlapping observational uncertainties. [Nathan Gillett, Canada]	Taken into account - text revised but the overall rate is still difficult to assess in the Southern Ocean.
22706	40	37	40	40	All references are from the Northamerican Arctic; suggestion to add Semiletov et al., 2016, Nature Geoscience ( <a href="https://doi.org/10.1038/ngeo2695">https://doi.org/10.1038/ngeo2695</a> ) for the Russian Arctic. [Gwenaelle GREMION, Canada]	Accepted - text revised and Semiletov et al 2016 cited.

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43750	40	53	40	53	evidence instead of evident [Carles Pelejero, Spain]	Accepted - text revised
19218	40	53	41	21	please include also the study of d11B in high latitude coralline algae by Fietzke et al. 2015 [Baerbel Hoenisch, United States of America]	Rejected. The suggested paper is an excellent work, however, this section focuses on decades-long paleo-pH records from tropical corals.
43752	40	53	41	21	I don't agree with the first sentence of this paragraph. To me, from all the paleo-pH records from tropical corals published so far, the only one that seems to unambiguously pick the anthropogenic perturbation as a decrease in pH is the one from Liu et al., 2014, Scientific Reports, from the South China Sea (note that this ref is 2014 and not 2015). In addition, the narrative of the whole paragraph should be checked so it flows better. Although the review is really thorough and detailed, I am not sure it is that important to provide all this information in this report. Maybe a summary of main trends and patterns of variability found should be enough. The last sentence, also, should be relaxed, maybe rephrased to: 'Overall, many of the records show a highly oscillating seawater pH, in some instances including a decreasing trend in d11B for the last years/decades, which may be indicative of anthropogenic ocean acidification. To be able to better detect recent decreases in pH and attribute them to human-induced pressures, further work is needed to better calibrate and constrain the use of this paleo-pH proxy in different tropical coral species and reefs'. [Carles Pelejero, Spain]	Accepted- text revised and mendeleev glitch for Liu et al. (2014) fixed.
17546	40	54	40	54	Capital C for century (for consistency elsewhere in Chapter) [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - The AR6_WGI_StyleGuide says to use "21st century"
17548	41	1	41	1	Change paleo to palaeo [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
17550	41	9	41	9	Capital C for century [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - The AR6_WGI_StyleGuide says to use "21st century"
17552	41	11	41	11	Capital C for century and superscript 'th' (for consistency elsewhere in Chapter) [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - The AR6_WGI_StyleGuide says to use "21st century" (they don't put superscript)
22708	41	11	41	14	What are the units of these values? pH unit per time ("trends")? [Gwenaelle GREMION, Canada]	Accepted - text revised. Numerical values here are the changes in pH (in the pH unit) since the mid-20th century.
44574	41	13	41	23	This number is ridiculous, ocean acidification of 0.9-1.16 pH unit? I guess the number should be 0.09-0.116, which is consistent with our expectation. [Kaoru Kubota, Japan]	Accepted - text removed.
47092	41	14	41	16	5.3.2.2.: The Suess effect describes the C-isotopic depletion in the atmosphere due to fossil fuel emission and is not something additional. [Sophie von Fromm, Germany]	Accepted - "Suess effect" removed.
17554	41	17	41	17	Capital C for century [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - The AR6_WGI_StyleGuide says to use "21st century"
44576	41	19	41	19	I don't agree that the time-lag analysis of this kind is robust, because there are only a slight decreasing trend in d11B of Shinjo et al.'s record. [Kaoru Kubota, Japan]	Accepted - text removed.
36362	41	19			Explain the reason for this two-year lag (or omit entirely if not relevant to the assessment). [Nathan Gillett, Canada]	Accepted - text removed.



Comment ID	From Page	From Line	To Page	To Line	Comment	Response
29412	41	30	41	30	I don't understand this sentence. Maybe others might feel the same? [Judith Hauck, Germany]	Taken into account - text revised so that its meaning becomes clearer. "Changes in ocean circulation" is not addressed.
22710	41	30	41	33	This sentence is unclear. [Gwenaëlle GREMION, Canada]	Taken into account - text revised so that its meaning becomes clearer. "Changes in ocean circulation" is not addressed.
22712	41	41	40	43	This sentence is lacking a reference. [Gwenaëlle GREMION, Canada]	Accepted - a reference added.
17556	41	42	41	44	Spaces required between numbers and units [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
44858	41	43	41	46	Typos (CO2, 2250m, 1000m) [Kaoru Kubota, Japan]	Accepted - text revised
36364	41	45		46	What are 'the increased impacts of the remineralization'? Explain the process, and how it is anthropogenically driven, if indeed it is. [Nathan Gillett, Canada]	Accepted - text revised
38548	41	48	41	48	Recently, Carter et al. (2019) assess the decadal changes in CO2 in the Pacific based on observations and estimated the relative large changes associated with circulation spin up in the southern Pacific. I think this is one of the first assessment of basin-scale decadal changes in ocean uptake rate, and it is worth to cite here. Carter et al., 2019 Pacific Anthropogenic Carbon Between 1991 and 2017, doi:10.1029/2018GB006154 [Shinya Kouketsu, Japan]	Taken into account - Carter et al. is cited in 5.2.1.3.3.
17558	41	49	41	49	Change 'Taken' to 'Taking' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
16164	42	6	42	42	In this section, relationships between deoxygenation and GHGs, but little for pH decline. It is better to delete "and pH decline" from the title, or add some pieces of information on pH decline. [AKIHIKO MURATA, Japan]	Taken into account - This (pH decline in relation to increase in remineralisation) has been assessed in section 5.3.3.1. We accept the suggestion and has been removed "... and pH decline" from section 5.3.3.2 title
8146	42	8	42	42	Basically same comments as for page 38 (chapter 5.3). It should be noted that oxygen decline can be a particular problem in coastal environments, lakes, fjords, lagoons, and are affected by climate change and eutrophication (therefore also to a certain component human impact). Friedrich et al. (2014), Biogeosciences ( <a href="https://doi.org/10.5194/bg-11-1215-2014">https://doi.org/10.5194/bg-11-1215-2014</a> ) demonstrates (but also reviews the existing literature) that these systems are significant contributors to greenhouse gas emissions. [Sebastian Naeher, New Zealand]	Taken into account - This comment is relevant for section 5.3.5 (coastal), so no changes were applied in this part of the document. Please also note that this chapter's section 5.2 also assesses freshwater systems.
17560	42	15	42	15	Insert , after 'However' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
22714	42	19	42	21	Suggestion: include an estimate for the last decade (since 2008 when the reference was published). [Gwenaëlle GREMION, Canada]	Accepted - Added newest trends by Schmidtko et al 2017, Ito et al. 2017, Hameau et al 2019, and kept Stramma et al 2008 citation (trend refers to the tropical ocean).
8930	42	22	42	24	Sentence not complete. Missing word or words [Benjamin Lamptey, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - text revised.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
22716	42	24	42	42	It would be interesting to see the expected change in CH4 cycling included here. [Gwenaëlle GREMION, Canada]	Taken into account - Please note that at this stage the present CH4 fluxes are highly uncertain, and most of the existing data (CH4 fluxes) come from coastal and shelf areas – section 5.2.2.4. Added flux estimate (present time) from Naqvi et al. 2010.
47094	42	26	42	29	5.3.3.2.: Instead of significant some numbers would be helpful at this point to get at least an idea about the magnitudes of such releases. [Sophie von Fromm, Germany]	Taken into account - At this stage the CH4 fluxes are highly uncertain, and most of the existing data (CH4 fluxes) come from coastal and shelf areas – section 5.2.2.4. Change the sentence in line 26-27, p. 42. Added range of fluxes for N2O and CH4 for open ocean oxygen minimum areas from Naqvi et al. 2010 paper (tables 3a and 3b)
42478	42	32	42	32	denitrification depletes nitrate in upwelling regions? The reduced nitrogen species are still bioavailable however. Also the CO2 flux from upwelling is not related to denitrification so this sentence should be reworded to separate the processes of denitrification and upwelling. [Peter Croot, Ireland]	Taken into account - Please note that denitrification (NO3- reduction to N2 by denitrifying bacteria under hypoxia) and annamox (NH4+ oxidation to N2 via bacteria under O2 depletion, mainly at open ocean OMZs) are the main pathways for nitrate and ammonium removal from the water column (references were added to the text). Here we meant that, because of nitrogen limitation in surface/subsurface waters in upwelling areas adjacent to OMZs, primary production may not be "strong enough" to capture the excess CO2 from these waters (long time not ventilated). Now it reads: "Denitrification and annamox, microbial processes occurring depletes nitrate at hypoxic and O2-depleted water column (Codispoti, 2007; Gruber and Galloway, 2008; Kuypers et al., 2005), deplete available nitrogen to primary producers and thus, when upwelled waters reach the photic zone, primary production is N-limited and CO2 from deeper water masses is emitted to the atmosphere (Tyrrell and Lucas, 2002)."

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
37744	42	32			"Denitrification depletes nitrates" could perhaps be changed to "Denitrification is a microbial process that depletes nitrates" if I am correct in my understanding. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - Text changed to "Denitrification and anammox, microbial processes occurring depletes nitrate at hypoxic and O2-depleted water column (Codispoti, 2007; Gruber and Galloway, 2008; Kuypers et al., 2005), deplete available nitrogen to primary producers and thus, when upwelled waters reach the photic zone, primary production is N-limited and CO2 from deeper water masses is emitted to the atmosphere (Tyrrell and Lucas, 2002). "
17562	42	35	42	35	Delete , after al. [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account
26510	42	35	42	38	this is already about "future projections", though the paragraph about "future projections" is right below. This mix up of topics is confusing! [Nadine Goris, Norway]	Taken into account – we have rearranged the next section 5.3.3.3 title to "future projections for Ocean acidification"
17564	42	40	42	40	References(s) required [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - References were added
17566	42	41	42	41	Reference(s) required [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - References were added
57240	42	45	43	41	Similarly to previous comments, the future projection section could benefit from a discussion of projected changes in dissolved O2 trends. Perhaps the entire chapter could benefit from an oceanic "oxygen" and "nutrient cycles" sections that delve in more detail on these important biogeochemical cycles, as they relate to anthropogenic warming. [Yassir Eddebbbar, United States of America]	Taken into account - these topics were assessed mainly in section 5.2 in this chapter, and to some extent also in section 5.7. Extra sub-sections would demand extra chapter space for 5.3. At this point, because of the reasons above, there were no substance changes to this part of the document
29414	42	54	43	8	One important result by Negrete-Garcia et al., 2019 is that there will be the onset of a second shallow aragonite saturation horizon, so the corrosive waters will not only move upwards from depth, but there will be a second corrosive layer right at the surface which might be more important for life in the ocean than the deep saturation horizon. This had previously been suggested by Hauck et al., 2010, doi:10.1029/2009JC005479, paragraph 49. This should be mentioned here. [Judith Hauck, Germany]	Accepted - text revised. Negrete-Garcia et al. (2019) cited but not detailed.
32750	43	1	43	1	Should be using "projeted" instead of "predicted" [Michael MacCracken, United States of America]	Accepted - text revised
8324	43	4	43	6	Rephrase so that "seasonal month-long undersaturation" is defined or somehow naturally made clear to the reader. As noted for the key messages, it isn't clear to someone who hasn't read the paper. [Sarah Cooley, United States of America]	Accepted - the area of the Southern Ocean that experiences aragonite undersaturation, for at least one month per year, by 2100.
22718	43	4	43	6	Unclear if the percentages refer to extent (o total area?) or probability/likelihood. [Gwenaelle GREMION, Canada]	Accepted - they refer to extent of total area. Text revised.
17568	43	5	43	5	Change 'was' to 'were' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
17570	43	6	43	6	Change 'long term' to 'long-term' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
8326	43	11	43	11	The word "modulated" carries a connotation of "made less extreme or made lower frequency"... is that what is really intended? Here and in line 29. [Sarah Cooley, United States of America]	Noted - modulated is not meant to say it is lessened by, but the impact in any one area will be by the combination of local and large-scale processes- which we often don't fully understand at the local scale e.g. Hurd et al (2018)
43754	43	12	43	12	Better 'hydrogen ion concentration seasonality increases' [Carles Pelejero, Spain]	Accepted - text revised
22720	43	22	43	24	Figure legend (and figure) should have a), b) and c) rather than "upper" and "lower" especially as there are three figures described here. [Gwenaelle GREMION, Canada]	Not applicable - the figure removed.
17572	43	24	43	24	Delete , after al. [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account
47750	43	32	43	32	Please check the use of this IPCC uncertainty language term. Are you able to provide a traceable account to assigning this uncertainty statement? Note that likelihood statements are quantified terms - phrases like likely and very likely have quantifiable probabilities associated with them. Please check it has been used correctly here. Please refer to the IPCC guidance note on uncertainty: <a href="https://wg1.ipcc.ch/SR/documents/ar5_uncertainty-guidance-note.pdf">https://wg1.ipcc.ch/SR/documents/ar5_uncertainty-guidance-note.pdf</a> [WGI TSU, France]	Accepted - changed to "may not" in the text.
22722	43	35	43	41	can more clarification be given on these two points? Otherwise rankings of certainty would be helpful [Gwenaelle GREMION, Canada]	Taken into account - The statement refers to the use of atmospheric CO2 concentrations in future projections instead of emissions. The latter would increase year by year, and that is why some feedbacks may have been overlooked. Marine ecosystem feedbacks may not have been fully considered, possibly due to lack of ecosystem or full functional types representation in biogeochemistry models. Rankings of certainty are not yet possible at present.
36366	43	35		38	Both CMIP5 and CMIP6 include some emissions-driven simulations in which CO2 concentration is simulated based on changes in anthropogenic and natural sources and sinks (C4MIP simulations in CMIP6). Is the rate of acidification higher in these free CO2 simulations than it is in prescribed concentration simulations? The text here suggests that it should be. Consider this in the assessment. [Nathan Gillett, Canada]	Accepted - as atmospheric CO2 concentrations rather than emissions often drive models used to project future changes, consequently important carbon-climate feedbacks have not been accounted for. Emissions driven simulations, show significant carbon-climate feedbacks that in turn lead to greater and more rapid rates of ocean acidification (Matear and Lenton, 2018; Zhang et al., 2018a).

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
55014	43	38	43	39	The statement "feedbacks of the marine ecosystem on ocean chemistry" can be supported by additional references, such as Henley et al. (2019) in Progress in Oceanography 173: 208-237 that emphasize "overarching cross-disciplinary priorities for future research" to resolve "atmosphere-ice-ocean-ecosystem feedbacks that control the dynamics and evolution of this complex polar system." [Kilkis Siir, Turkey]	Rejected – The paper by Henley et al is very specific for the Western Antarctic Peninsula, and in this chapter section we are dealing with larger scale feedbacks. In IPCC Special Report on Oceans and Cryosphere (SROCC), Chapter 5, it is also stated that, regarding export production (a marine ecosystem process capable of providing feedback on the carbon cycle), there is "low confidence" for the magnitude of changes "due to the medium agreement among models and the limited evidence from observations".
8328	43	48	44	5	There have been a lot of good studies on coastal acidification in the past few years; check, e.g., Sutton et al. Earth System Science Data (2014) for time series in coastal locations, and Pelletier et al. 2018 Elementa and refs therein, or refs that cite them, for insight into the coastal OA/driver attribution literature. [Sarah Cooley, United States of America]	Taken into account - the revised SOD assessment includes now a number of new articles, many published between the FOD and this review cycle, as well as the suggested literature.
22724	43	48	44	5	There is a very recent article (10.1021/acs.est.8b03655) that includes a very comprehensive review of the drivers behind pH changes in coastal ecosystems. Some of these findings may be very useful for this section (5.3.5) [Gwenaelle GREMION, Canada]	Taken into account - the reference and its findings added to the second order draft text version
31664	43	49	43	52	Here the authors state that inner seas, bays and estuaries are typically saturated with CO2. However, in the next paragraph, they mention about a case study in that CO2 are absorbed in a bay (Cotovicz et al., 2018; p44, 117-20.) I agree with Chen and Borges (2015)'s conclusion, but here we may be better to mention about existence of some exception such as "Although some exception exist, but typically....." [Tsuneo Ono, Japan]	Taken into account – added "although some exception exist"
13776	43	49	43	54	To be consistent with what is reported for the shelves, maybe also report a value for the "estuaries" (including bays) ? .... domestic and industrial sewage (Chen and Borges, 2009), AND OVERALL ESTUARIES EMIT 0.15-0.25 PgC yr-1 OF CO2 (Laruelle et al., HESS, 2013 <a href="https://doi.org/10.5194/hess-17-2029-2013">https://doi.org/10.5194/hess-17-2029-2013</a> ; Bauer et al., Nature, 2013 doi:10.1038/nature12857). [Pierre Regnier, Belgium]	Accepted - revised text includes now overall estuaries CO2 emissions (using the suggested references). Chen and Borges didn't give a CO2-emission estimate for domestic and industrial sewage, but they mean that excess organic matter brought by sewage to the coastal area undergoes degradation (heterotrophic respiration) and may be an additional CO2 source within estuaries/bays. Text was rewritten.
16166	43	49			"staturated with CO2"? [AKIHIKO MURATA, Japan]	Editorial – already revised for the new version.
13778	43	54	43	54	I would add "only" at the end of the sentence, to avoid confusion. .... considering ice-free areas ONLY. [Pierre Regnier, Belgium]	Accepted - text revised

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
42480	44	4	44	4	What is the context for the reference to metallic ions and multi stressors, which metallic ions? This is too vague. [Peter Croot, Ireland]	Taken into account - In hypoxic (negative Eh) conditions potentially toxic metallic ions such as Cu (II) or As (III) may become more available to organisms.
8148	44	8	44	55	As in my comments above, it would be useful to include some information about land-locked water bodies. [Sebastian Naeher, New Zealand]	Taken into account - Please note that freshwater ecosystems were assessed in terms of GHG emissions in section 5.2. At this point there were no changes to the document (previous reply).
33470	44	10	44	12	The data set analyzed by Lowe et al. 2019 utilizes observations of undefined quality and should not be cited by the IPCC in ocean acidification trend analyses. Due to a number of measurement and calibration issues associated with using hydrogen ion sensitive glass electrodes in seawater applications (Dickson, A.G., Sabine, C.L., Christian, J.R. 2007. Guide to Best Practices for Ocean CO2 Measurements. North Pacific Marine Science Organization, p. 176.), it is impossible to separate true pH long-term change in these coastal environments from measurement error without explicit interrogation of the methodologies, which this paper does not do. Due to the challenges of using glass pH electrodes in seawater, pH measurements using glass electrodes are considered of "undefined quality" by the United Nations in reporting to Sustainable Development Goal indicator 14.3 ( <a href="http://goa-on.org/resources/sdg_14.3.1_indicator.php">http://goa-on.org/resources/sdg_14.3.1_indicator.php</a> ). The IPCC should follow this guidance and only report results using more accurate and precise methods to measure ocean pH as defined by the oceanographic community's standard operating procedures (Dickson et al., 2007). [Adrienne Sutton, United States of America]	Taken into account - in the revised text it is now mentioned that there is a method issue for the pH observations in coastal areas, and include a reference to Carstensen et al (2019), where a number of coastal areas were assessed, and the method (glass electrode) issue also assessed. It is important also to show in this AR6 the need to improve observations to reduce uncertainty, especially for areas under multiple stress factors (eutrophication/acidification/hypoxia, warming) such as the coastal areas.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
33472	44	10	44	12	In addition to the above comment, there are plenty of other studies (using standard methodologies) in this coastal region and others that analyze the relative impact of respiration vs anthropogenic CO2 uptake on ocean acidification conditions. This paragraph should be updated to include those studies, including the following which clearly show that respiration and anthropogenic CO2 uptake (and reduced buffer capacity) both impact low pH and aragonite saturation state conditions: 1) Feely, R.A., S. Alin, B. Carter, N. Bednaršek, B. Hales, F. Chan, T.M. Hill, B. Gaylord, E. Sanford, R.H. Byrne, C.L. Sabine, D. Greeley, and L. Juranek (2016): Chemical and biological impacts of ocean acidification along the west coast of North America. <i>Estuar. Coast. Shelf Sci.</i> , 183(A), 260–270, doi: 10.1016/j.ecss.2016.08.043. 2) Cai, W.-J., Hu, X., Huang, W.-J., Murrell, M.C., Lehrter, J.C., Lohrenz, S.E., Chou, W.-C., Zhai, W., Hollibaugh, J.T., Wang, Y., Zhao, P., Guo, X., Gundersen, K., Dai, M., Gong, G.-C. (2011) Acidification of subsurface coastal waters enhanced by eutrophication. <i>Nature Geosci</i> 4, 766-770. 3) Cai, W.-J., Huang, W.-J., Luther, G.W., Pierrot, D., Li, M., Testa, J., Xue, M., Joesoef, A., Mann, R., Brodeur, J., Xu, Y.-Y., Chen, B., Hussain, N., Waldbusser, G.G., Cornwell, J., Kemp, W.M. (2017) Redox reactions and weak buffering capacity lead to acidification in the Chesapeake Bay. <i>Nature Communications</i> 8, 369. 4) Chavez, F.P., J.T. Pennington, R.P. Michisaki, M. Blum, G.M. Chavez, J. Friederich, B. Jones, R. Herlien, B. Kieft, B. Hobson, A.S. Ren, J. Ryan, J.C. Sevadjian, C. Wahl, K.R. Walz, K. Yamahara, G.E. Friederich, and M. Messié. 2017. Climate variability and change: Response of a coastal ocean ecosystem. <i>Oceanography</i> 30(4):128–145, <a href="https://doi.org/10.5670/oceanog.2017.429">https://doi.org/10.5670/oceanog.2017.429</a> . [Adrienne Sutton, United States of America]	Accepted - the suggested literature assessed for the second order draft.
22726	44	10	44	55	This subsection is poorly structured. Suggestion: either by region or phenomena (acidification, deoxygenation). [Gwenaelle GREMION, Canada]	Taken into account - The subsection has been rewritten due to important substance changes suggested by other reviewers. We have decided to keep the phenomena together and assess the drivers, which in many cases aren't related to anthropogenic GHG emissions, and show the spatial variability of these ecosystems.
17574	44	11	44	11	Change 'relates' to 'relate' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
17576	44	12	44	12	Is this all non-upwelling (as implied in the text) or 'some'? [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - yes, the examples are from non-upwelling coastal areas.
22728	44	14	44	14	Technically, a pH of 7 is defined as "neutral" not "acidic" (yet this can of course be described as "more acidic" in comparison with higher pH values...) . [Gwenaelle GREMION, Canada]	Accepted - original text was changed to "lower pH"

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33474	44	14			The Wallace et al. 2014 study also uses seawater pH methodology, in this case a glass electrode autonomous sensor, that is not considered one of the standard methodologies of the oceanographic community as defined by Dickson et al. 2007 or considered an ocean carbon sensor by the International Ocean Carbon Coordination Project ( <a href="http://www.ioccp.org/index.php/instruments-and-sensors">http://www.ioccp.org/index.php/instruments-and-sensors</a> ), and I recommend it not be included in assessments that require climate-quality research. [Adrienne Sutton, United States of America]	Taken into account - I fully agree that the reported pH measurements, and even pH scale (NBS, seawater, total) may represent a bias for assessing acidification trends in coastal areas, bays, estuaries. Here we have assessed the available literature for these ecosystems, and the conclusion is that eutrophication and biological processes are likely the key drivers of the coastal carbonate system. A few very good reviews were published between the first order draft and this review, and suggested literature has also been included here. In one of them, Carstensen et al. 2019, the authors state that "However, comparing pH across coastal ecosystem can be cumbersome due to differences in procedures and pH scales used. Coastal and estuarine monitoring programs generally use glass electrodes for measuring pH that are calibrated on a series of low ionic strength buffer solutions available from the U.S. National Bureau of Standards (NBS). The NBS scale was employed by all monitoring authorities in the long term data sets available, except for France where the seawater scale (SWS) was
33476	44	14			In addition to the above comment, "acidic" is not a pH of ~ 7, but pH of < 7. [Adrienne Sutton, United States of America]	Accepted - text was changed in this subsection, as in the beginning of section 5.3. Please note that the coastal areas subsection was modified, so you not find the text in its original place.
33478	44	21			As commented previously, "acidic" waters are considered those with pH of < 7; except for some extreme environments, the oceans are alkaline. I recommend this be worded as "increased acidity" [Adrienne Sutton, United States of America]	Accepted - text was changed in this subsection, as in the beginning of section 5.3. Please note that the coastal areas subsection was modified, so you not find the text in its original place.



Comment ID	From Page	From Line	To Page	To Line	Comment	Response
27978	44	27	44	27	<p>Page 44, line 27: "... but more severe hypoxia or anoxia." I am under the impression that they are practically the same thing (deficiency of oxygen reaching tissues), so they might not need to both be mentioned. Page 44, line 31-32: "... many times not obtainable in the peer-reviewed literature". It would be nice to elaborate on that a bit more. Deoxygenation trends can only be made through surveys and observations, but why is it not obtainable in peer-reviewed literature? Due to accessibility? [roderik van de wal, Netherlands]</p>	<p>Rejected (l. 27)/Taken into account - lp44. l. 27 -&gt; "Hypoxia" in seawater ecosystems means low levels of dissolved oxygen concentration (O2 - gas), generally below 30% of the saturation level, which in turn is dependent on temperature and salinity. Anoxia is the virtual absence of dissolved oxygen i.e. seawater O2 concentration = 0 mL L-1. In this case there is a large difference on the ability of marine organisms to live under hypoxia. Only certain groups of microorganisms live under anoxia. Oxygen levels also influence on the redox potential in seawater and in sediments interstitial water, so we couldn't consider both as the same. P.44 L 31-32 - Taken into account. In many cases regional authorities do have long term time series on oxygen and other physical-chemical parameters for coastal seas and estuaries. Unfortunately these surveys are not available online or in the peer-reviewed literature, what is often called "grey data".</p>
47752	44	28	44	28	<p>Please check the use of this IPCC uncertainty language term. Are you able to provide a traceable account to assigning this uncertainty statement? Note that likelihood statements are quantified terms - phrases like likely and very likely have quantifiable probabilities associated with them. Please check it has been used correctly here. Please refer to the IPCC guidance note on uncertainty: <a href="https://wg1.ipcc.ch/SR/documents/ar5_uncertainty-guidance-note.pdf">https://wg1.ipcc.ch/SR/documents/ar5_uncertainty-guidance-note.pdf</a> [WGI TSU, France]</p>	<p>Accepted - the phrasing was slightly changed, from likely to "high confidence" after checking in the literature but also on the SROCC assessment in this subject (SROCC Chapter 5). "Spatial distribution of hypoxic coastal areas is highly heterogeneous, but more severe hypoxia or anoxia may occur more often in highly populated coastal areas, or in regions where local water circulation, water column stratification and wind patterns lead to an accumulation of organic matter (high confidence) (Breitburg et al., 2018; Ciais et al., 2013; Rabalais et al., 2014)+[placeholder for citation of SROCC Chapter 5]. "</p>
22730	44	31	44	31	<p>change "many times" to "which are often" [Gwenaëlle GREMIION, Canada]</p>	<p>Accepted - text revised</p>

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
17578	44	35	44	35	Do you mean 'seawater from the North Sea', or 'seawater from the north' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - it was meant "seawater inflow from the North Sea into the Baltic"
27760	44	37	44	40	delete [reference missing] [Poot Delgado Carlos Antonio, Mexico]	Editorial – already revised for the new version.
31666	44	39	44	42	I think either “since 1850” or “since 1990” is duplicated here. [Tsuneo Ono, Japan]	Taken into account – rewritten to “Wang et al. (2017) showed, using a sedimentary record from the present back to 1850, that the oxygen minimum zone off southern California in the Pacific Ocean has become more intense since the 1990s,”
17580	44	40	44	41	Insert 'the' after 'sedimentary' but the text is also confusign. The two dates are contradictory. [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - a similar comment was sent. The text now is "Wang et al. (2017) showed, using the sedimentary record from the present back to 1850, that the oxygen minimum zone off southern California in the Pacific Ocean has become more intense since the 1990s, with high interannual variability due to the Southern Oscillation."
17582	44	44	44	44	Edit reference to 'Recently, Qian et al. (2017)' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial – already revised for the new version.
17584	44	46	44	46	Edit reference to 'shelf, Claret et al. (2018)' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial – already revised for the new version.
17586	44	50	44	50	Change 1950's to 1950s [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
17588	44	51	44	51	Delete , after al. [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account
17590	44	55	44	55	Delete 'decades' to remove tautology [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
47802	45	1	60	40	Assessment Structure: If possible, assessment conclusions should be provided in a structured traceable account of how these statements were derived. For example, sections / subsections can start with previous IPCC report conclusions (AR5 or AR6 Special Reports) and then provide an update of the more recent literature, clearly laying out the lines of evidence. Each section / subsection can then conclude with assessment statements. [WGI TSU, France]	Agreed.
13548	45	3	45	3	Section 5.4: Nitrogen deposition also affects the carbon uptake. Why is this effect not discussed in this section? [Govindasamy Bala, India]	Noted.
47344	45	3	45	8	I like this structure - much better than ID [chris jones, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Thanks!
36368	45	3	60	39	This section generally lacks assessment statements at the end of each subsection. Add these. I would also recommend that the authors make more reference to Figure 5.29, which is a good summary figure comparing the strengths of the different biogeochemical feedbacks, and would be a good starting point for such assessments. [Nathan Gillett, Canada]	Agreed.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
42112	45	3	60	39	<p>In section 5.4 you discuss ‘carbon-climate’ and ‘carbon-CO2 concentration’ feedbacks and calculate their values in terms of ‘Biogeochemistry-Climate’ feedbacks in W per m2 per K (so as to be easily comparable to climate feedbacks expressed in the same units).</p> <p>For the land carbon cycle, you discuss the land carbon response to CO2 in Section 5.4.1 and land carbon response to climate in section 5.4.3, and summarise both in Figure 5.29, calculating the Biogeochemistry-Climate’ feedbacks using results from the C4MIP suite of models. However, the preferred analysis of the size of the terrestrial land carbon sink in the rest of the chapter comes from the latest Global Carbon Budget analysis, as summarized in Table 5.1 (Le Quéré et al., 2018a), and not the C4MIP models. Also, there are well known non-linearity issues that make it difficult to calculate the net land carbon ‘Biogeochemistry-Climate’ feedback from the separate land carbon-CO2 and land carbon-climate responses (Schwinger et al., 2014; Arora et al., 2013).</p> <p>A recent new study (Goodwin et al., 2019 in press, doi:10.1029/2019GL082887 – see full reference below) has calculated the net ‘Biogeochemistry-Climate’ feedback for the land carbon system directly from the Global Carbon Budget reconstructions of land carbon uptake (Le Quéré et al., 2018a). Goodwin et al (2019) finds that the present day net land carbon ‘Biogeochemistry-Climate’ feedback is <math>0.31 \pm 0.09</math> W per m2 per K (where 0.31 is the best estimate and 0.09 is one standard deviation). This ‘Biogeochemistry-Climate’ feedback (<math>0.31 \pm 0.09</math> W per m2 per K: Goodwin et al., 2019) represents the net effect of the land carbon responses to both CO2 and climate, and is calculated for the terrestrial carbon sink as stated in Table 5.1 and originally analysed by the</p>	Noted.
42114	45	3	60	39	<p>Full References for above comment:</p> <p>Goodwin, P., Williams, R.G., Roussenov, V., and Katavouta, A. (2019 in press) Climate sensitivity from both physical and carbon cycle feedbacks, <i>Geophysical Research Letters</i>, gr159214, doi:10.1029/2019GL082887</p> <p>Le Quéré, C., Andrew, R. M., Friedlingstein, P., Sitch, S., Hauck, J., Pongratz, J., et al. (2018a). Global Carbon Budget 2018. <i>Earth Syst. Sci. Data</i> 10, 2141–2194. doi:10.5194/essd-10-2141-2018.</p> <p>Schwinger, J., Tjiputra, J.F., Heinze, C., Bopp, L., Christian, J.R., Gehlen, M., Ilyina, T., Jones, C.D., Salas-Méllia, D., Segschneider, J. and Séférian, R. (2014) Nonlinearity of ocean carbon cycle feedbacks in CMIP5 earth system models. <i>Journal of Climate</i>, 27(11), pp.3869-3888.</p> <p>Arora, V.K., Boer, G.J., Friedlingstein, P., Eby, M., Jones, C.D., Christian, J.R., Bonan, G., Bopp, L., Brovkin, V., Cadule, P. and Hajima, T. (2013) Carbon–concentration and carbon–climate feedbacks in CMIP5 Earth system models. <i>J. Climate</i>, 26(15), pp.5289-5314. [Philip Goodwin, United Kingdom (of Great Britain and Northern Ireland)]</p>	Noted.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
47844	45	3	60	39	Assessment Structure: If possible, assessment conclusions should be provided in a structured traceable account of how these statements were derived. For example, sections / subsections can start with previous IPCC report conclusions (AR5 or AR6 Special Reports) and then provide an update of the more recent literature, clearly laying out the lines of evidence. Each section / subsection can then conclude with assessment statements. This section currently reads more as a review than an assessment. In addition, please make sure the ES uncertainty language is clearly traceable to the underlying chapter text. [WGI TSU, France]	Noted
47098	45	3			5.4.: Emphasize the links/contradictions more between the effect of CO2, climate on land, ocean in the corresponding sections. Some of the contradictions, e.g. higher biomass production due to higher CO2 vs lower biomass production due to higher temperature are not really comment. [Sophie von Fromm, Germany]	Noted.
41820	45	5	45	5	remove "is" between "section" and "covers" [Marc Aubinet, Belgium]	Accepted - text revised
16304	45	5	45	5	delete "is" before "covers" [Wolfgang Obermeier, Germany]	Accepted - text revised
17592	45	5	45	5	Delete 'is' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
47846	45	5	45	5	Can you provide a reference here or call back to the relevant section in AR5? [WGI TSU, France]	Noted.
6824	45	11	46	18	There is no mention of leaf-level acclimation of photosynthesis to elevated CO2, which reduces leaf nitrogen content, as a result of reduced Rubisco (reviewed in Ainsworth and Long 2005, Smith and Dukes 2013). While there is some debate about the driving mechanism, this is likely primarily due to reduced leaf nitrogen demand. This is not taken into account in the CMIP6 models (Smith and Dukes 2013), and would likely reduce the simulated nutrient limitation effect. Citations: Smith NG, Dukes JS. 2013. Plant respiration and photosynthesis in global-scale models: incorporating acclimation to temperature and CO2. Global Change Biology 19: 45–63. Ainsworth EA, Long SP. 2005. What have we learned from 15 years of free-air CO2 enrichment (FACE)? A meta-analytic review of the responses of photosynthesis, canopy properties and plant production to rising CO2. New Phytologist 165: 351–372. [Nicholas Smith, United States of America]	Accepted - reference added. Note that SRCCL discusses this in detail
48136	45	13	45	15	The "associated decline in stomatal conductance" is not a cause for projected increase in land productivity: leaf-level productivity increases with conductance. Consider replacing "associated decline in stomatal conductance" with "associated increase in plant water use efficiency and leaf-area increase, particularly in semi-arid regions" [Vanessa Haverd, Australia]	Accepted - text revised. The entire paragraph has been rewritten, taking this comment into account
57760	45	15	45	15	In CMIP5 simulations started from 1860, not 1850 as in CMIP6. 2100 is usually used to mark the end of projections, not 2099 [Elena Shevliakova, United States of America]	accepted - text revised

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
57762	45	16	45	19	New mechanisms have been suggested in addition to productivity increase , e.g. increasing CO2 in water-limited forests decreases the amount of time trees spend in water limitation. Farrior, Caroline E., et al. "Decreased water limitation under elevated CO2 amplifies potential for forest carbon sinks." Proceedings of the National Academy of Sciences 112.23 (2015): 7213-7218 [Elena Shevliakova, United States of America]	accepted - text revised
29372	45	17	45	17	About "Increased productivity is one key driver of increases in vegetation carbon storage": Hajima et al. 2014, J. Clim. directly supports this message, but not only on VEGETATION carbon, but on TOTAL land carbon. They clearly showed that the sensitivity of plant productivity to elevated CO2 is likely the key to explain the large spread of concentration-carbon feedback strength among CMIP5 models. [Tomohiro Hajima, Japan]	Accepted - text revised
43770	45	17	45	19	Hajima et al. (2014) made an extensive analysis exactly on this issue, i.e., importance of productivity increase for land carbon storage. This work should be cited along with Friend et al. (2014) and Walker et al. (2019). Reference: Hajima et al., Journal of Climate, 27, 3425-3445, DOI: 10.1175/JCLI-D-13-00177.1, 2014 [Michio Kawamiya, Japan]	Accepted - text revised
31292	45	19	45	20	This gives the incorrect impression that the main consequence of carbon being stored in soils in response to CO2 fertilization is an increase in the positive climate-CO2 feedback. This is a second-order effect, however. The first-order effect is that carbon is stored, contributing to the negative CO2 concentration feedback. [Iain Colin Prentice, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised to make clear that what the reviewer said was the intended meaning
39358	45	19	45	21	Changes in plant carbon inputs to soils are one of the major causes for changing soil carbon stocks, but not necessarily "the" major cause. The change in soil C stocks is due to the balance between inputs and decomposition rates, and both can be important. [Eric Davidson, United States of America]	Accepted - text revised
17594	45	25	45	25	Delete , after 'temperature' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
16302	45	26	45	26	replace "as they are as yet" with "as they are so far" [Wolfgang Obermeier, Germany]	Accepted - text revised
9678	45	27	45	27	I suggest changing "New syntheses since AR5 corroborate" to "New syntheses since AR5, however, corroborate" or changing the paragraph altogether to better convey any contrast or comparison with findings since AR5. It's not clear what is intended in the text as it currently reads. [Brian Magi, United States of America]	Accepted - text revised

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
57764	45	29	45	31	Add new evidence suggesting that ecosystem adaptation through plant-microbe symbioses could alleviate some nitrogen limitation, e.g. Sulman, et al (2019). Diverse mycorrhizal associations enhance terrestrial C storage in a global model. Global Biogeochemical Cycles. Baskaran, P, R. et al, 2017: Modelling the influence of ectomycorrhizal decomposition on plant nutrition and soil carbon sequestration in boreal forest ecosystems. New Phytol., 213, 1452–1465, doi:10.1111/nph.14213. <a href="https://doi.org/10.1111/nph.14213">https://doi.org/10.1111/nph.14213</a> . [Elena Shevliakova, United States of America]	Accepted - text revised
29508	45	30	45	31	In fact, there is a 20-year study (Reich et al. Science, 2018) that shows there is a clear difference in the response of C3 versus C4 plants, with C4 plants initially being unresponsive to elevated CO2. However, after several years the C3 response decreased significantly (suggesting an acclimatization of C3 plants to elevated CO2). ESMs assume a positive effect of elevated CO2 on primary production, however, Reich's study strongly suggests that this effect could be short-lived. I think this uncertainty should be mentioned because it could have a very strong impact on the strength of the future carbon sink. [Rona Thompson, Norway]	Accepted - revised text states "Plant carbon allocation, changes in plant community composition, disturbance, and natural plant mortality are important processes affecting the magnitude of the response"
16306	45	31	45	31	introduce other abiotic environmental limitations on the CO2 fertilization effect: "Alongside, multiple long-term field studies under elevated CO2 have shown that the effect of elevated CO2 on plant growth strongly depends on weather conditions, and will be reduced under more extreme environmental conditions (that is e.g., drier and hotter conditions; Hovenden et al. (2019). Globally consistent influences of seasonal precipitation limit grassland biomass response to elevated CO2. Nature Plants, 5, 167-173. DOI:10.1038/s41477-018-0356-x; Reich, P. B., Hobbie, S. E., & Lee, T. D. (2014). Plant growth enhancement by elevated CO2 eliminated by joint water and nitrogen limitation. Nature Geoscience, 7, 2–6. DOI:10.1038/ngeo2284; Obermeier, W. A., Lehnert, L. W., Kammann, C. I., Müller, C., Luterbacher, J., et al. (2017). Reduced CO2 fertilization effect in temperate C3 grasslands under more extreme weather conditions. Nature Climate Change, 7, 137–141. DOI:10.1038/nclimate3191), or extreme events (e.g., Yuan et al. (2018), Extreme climatic events down-regulate the grassland biomass response to elevated carbon dioxide, Scientific Reports, DOI:10.1038/s41598-018-36157-x) [Wolfgang Obermeier, Germany]	Accepted - text revised to state "New syntheses since AR5 corroborate that the long-term effect of elevated CO2 on plant growth and ecosystem carbon storage is generally positive (high confidence), but modulated by temperature, water and nutrient availability ". Relevant references have been added
47346	45	33	45	44	Koven et al (2015, Biogeosciences) show that caution required interpreting how to compare such experiments with models as models can show a similar behaviour for different mechanisms ("false priming") [chris jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - revised text mentions this now explicitly
57766	45	39	45	39	GFDL ESM4 simulations for CMIP6 include root exudation and explicit microbial dynamics. Perhaps restate "...and very limited number of ESMs include them in CMIP6." [Elena Shevliakova, United States of America]	Accepted - text revised

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
22732	45	39	45	44	The latter sentence contradicts the previous one. [Gwenaelle GREMION, Canada]	Accepted - text revised "one global model that includes this processes suggests reduced accumulation of carbon in soils, but increased vegetation carbon storage in response to elevated CO2"
7494	45	42	45	42	Typo: Wieder et al. 2018 not 2017. [Rose Abramoff, France]	Taken into account
56620	45	46	46	11	The nitrogen and phosphorus limitation does not seem a "direct CO2 effect". I would suggest to give those effects a separate subheading. It seems important to flesh out the new research since AR5 and to what degree future projections of CO2 land carbon storage are LIKELY LOWER (and concentrations higher) when considering nitrogen limitations to explain past observations... [Malte Meinshausen, Australia]	noted - text not changed because nitrogen-phosphorus affect the atmospheric CO2 concentration by reducing the direct CO2 effect. However, the entire section has been revised to have a stronger assessment character
36370	45	47			Replace 'very strong reduction in the' with 'much lower'. The existing wording sounds like a change in sensitivity over time. [Nathan Gillett, Canada]	Accepted - text revised
39360	45	51	45	51	It is unclear why "the magnitude is likely less than in the CMIP5 ensemble." This is not clear from either Figure 5.29 or Table 5.5 and isn't explained in the text. [Eric Davidson, United States of America]	Accepted - text revised with available CMIP6 simulations, deemphasising the strong effect in CMIP5, which was only based on one model.
47754	45	53	45	55	Please check the use of this IPCC uncertainty language term. Are you able to provide a traceable account to assigning this uncertainty statement? Note that likelihood statements are quantified terms - phrases like likely and very likely have quantifiable probabilities associated with them. Please check it has been used correctly here. Please refer to the IPCC guidance note on uncertainty: <a href="https://wg1.ipcc.ch/SR/documents/ar5_uncertainty-guidance-note.pdf">https://wg1.ipcc.ch/SR/documents/ar5_uncertainty-guidance-note.pdf</a> [WGI TSU, France]	Accepted - text revised
44110	46	13	46	15	Add affirmative citation to statement prior to "but see..." [Sara Kahanamoku, United States of America]	Not applicable. Text has been shortened and this sentence been removed
47238	46	13	46	18	It might be emphasized here that the CO2 effect on water-use efficiency and CO2 fertilization are still crudely represented in Earth System Models which still suffer from serious biases in simulating land surface hydrology, land surface variability and related extremes, as well as their potential effects on vegetation mortality. [Hervé Douville, France]	Accepted. This is beyond the scope of Chapter 5. A link to chapter 8 and 11 is now added
22734	46	13	46	18	This paragraph is not directly discussing land C uptake and could be removed. [Gwenaelle GREMION, Canada]	Rejected - the CO2 interaction with the water cycle is important for the overall land response. The revised text makes this clearer, integrating more evidence and providing a link to Chapter 8

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
31294	46	15	46	15	Why "but see"? The controversy between van der Sleen et al. and Brienen et al. is not about the increase in water use efficiency, which is a generally accepted and inevitable consequence of increasing CO2. Rather, it is about whether any CO2-induced increase in growth is detectable in principle from tree-ring data. [Iain Colin Prentice, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text changed. FOD was confusing because the sequence of references and but see was changed by the reference software
31296	46	16	46	18	"Idealised simulations" are invoked here. However, there is evidence from observations (see Ukkola et al. 2015 Nature Climate Change) that vegetation cover increases due to rising CO2 in subhumid and semi-arid regions more than compensate for decreased stomatal conductance. [Iain Colin Prentice, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
29416	46	24	46	24	see my comment 11 and 14. Please use consistent numbers. It should be 22% here as above. [Judith Hauck, Germany]	Accepted - text revised.
33490	46	24			Actually, Section 5.2.2.3 refers to CH4 uptake, not CO2. And as mentioned in previous comments, an uptake of 30% conflicts with estimates reported earlier in the chapter (22% on page 7 and 25% on page 25). This chapter should use consistent CO2 uptake estimates throughout and include the time period that estimate refers to. [Adrienne Sutton, United States of America]	Accepted - text revised.
17596	46	25	46	25	Change to 'Earth System Models' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - "System Models" not capitalised.
36372	46	27	46	31	Physically ocean biogeochemistry responds to changes in atmospheric CO2 concentration, not directly to the annual anthropogenic CO2 emissions. Changes in ocean carbon uptake fraction of annual emissions are in part driven simply by changes in CO2 emissions rates in the emissions scenario. For example, if the ocean took up 1% of the excess in anthropogenic carbon every year, changes in uptake fraction of annual emissions would still occur for a scenario in which emissions increased then decreased over time, even though there were no changes in the physical processes driving carbon uptake in the model. Or for another example, if we suddenly stopped emitting CO2, the ocean would continue to take up carbon, so the uptake fraction of annual emissions would become infinite. So the uptake fraction of annual emissions in a scenario with progressive changes in emissions may not be very physically meaningful. If there is a strong desire to analyse changes in carbon uptake fraction of annual emissions across models and time, I would recommend using an idealised scenario to do this, such as 1PCTCO2. [Nathan Gillett, Canada]	Noted. The reviewer's comment is correct. This is why only the cases of 1% CO2 increase scenario and RCP8.5, in which anthropogenic CO2 emission increases over time, were mentioned here. The case of reversing the increase in atmospheric CO2 concentration is described in section 5.3.3.4.
47756	46	27	46	43	Please check the use of this IPCC uncertainty language term. Are you able to provide a traceable account to assigning this uncertainty statement? Note that likelihood statements are quantified terms - phrases like likely and very likely have quantifiable probabilities associated with them. Please check it has been used correctly here. Please refer to the IPCC guidance note on uncertainty: <a href="https://wg1.ipcc.ch/SR/documents/ar5_uncertainty-guidance-note.pdf">https://wg1.ipcc.ch/SR/documents/ar5_uncertainty-guidance-note.pdf</a> [WGI TSU, France]	Accepted - text revised. Use of uncertainty statement was revised.



Comment ID	From Page	From Line	To Page	To Line	Comment	Response
17598	46	29	46	29	Change to '21st Century' for consistency [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - The AR6_WGI_StyleGuide says to use "21st century"
36374	46	31		32	A feedback is a process in which an initial perturbation causes a change in a second quantity, which amplifies the initial perturbation. The process described here, by which the ocean carbon sink is reduced at higher levels of CO <sub>2</sub> , will increase global warming, but it isn't a feedback on global warming. Only if the change in the carbon sink was driven by the warming itself would it be a feedback. [Nathan Gillett, Canada]	Accepted - text revised.
22736	46	34	46	41	Unclear if this paragraph is meant to be specifically about the Southern Ocean? [Gwenaelle GREMION, Canada]	Taken into account - text edited because this paragraph is in principle not specific to the Southern Ocean.
36376	46	34		47	Could this change in buffering capacity and its effects on CO <sub>2</sub> uptake be explained in more detail for the benefit of non-specialists? [Nathan Gillett, Canada]	Rejected - the comment does make sense. However, we had to quit explaining for the buffering capacity in more detail in order to reduce the length of this chapter, .
17600	46	35	46	35	Reference(s) required [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text edited and the relevant reference clarified.
22738	46	37	46	40	Why H <sup>+</sup> ion concentrations here, while pH everywhere else? [Gwenaelle GREMION, Canada]	Accepted – text revised.
17602	46	38	46	38	Change 'is' to 'are' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
17604	46	39	46	39	Change to '21st Century' for consistency [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - The AR6_WGI_StyleGuide says to use "21st century"
16168	46	44			"negative feedback" probably comes from the relationship: $2\text{HCO}_3^- + \text{Ca}^{2+} = \text{CO}_2 + \text{CaCO}_3 + \text{H}_2\text{O}$ . But it is important to know where the reaction occurs. If the reaction is closed in a cell, we do not have to consider impacts on atmospheric CO <sub>2</sub> . I think this sentence is not necessary. [AKIHIKO MURATA, Japan]	Rejected - the reaction occurs in the ocean that is open to the atmosphere.
17606	46	46	46	49	Delete negative signs, you have already noted the feedback is negative [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - the negative sign unambiguously indicates that the feedback is negative. The same expression is found in Gangsto et al. (2011).
42482	46	51	46	51	Parameterization of the ballast effect - does this describe a model or a real world process? [Peter Croot, Ireland]	Noted - this is one of the effect impacting on the export fluxes of organic carbon in the real world, but its parameterization is an issue in the modelling.
45362	47	1	48	45	In 5.4.3 I'm surprised there is no review of evidence in trends from inversions as markers of feedbacks. Inversions are now long enough that you can see this happening, that's what the Rayner et al. 2015 paper (already cited) was doing but there are better (longer) inversions available [Peter Rayner, Australia]	Taken Into Account: we compare ESMS with inversions in section 5.4.5, particularly figure 5.22 and 5.23

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
57768	47	3	47	7	It's not clear if this statement applies equally to all biomes and latitudes or to global? It's not correct for boreal zone. Is warming climate refers to temperature effect in isolation from increased CO2 concentration discussed in 5.4.2. Needs a clarification. [Elena Shevliakova, United States of America]	Accepted: Text Revised
22740	47	3	47	7	The first sentence in 5.4.3 introduces several sources of land-based carbon losses derived from ESMs, which are then discussed in more details in the following paragraphs, including "changes to plant mortality and disturbance rates". While changes in mortality rates are discussed in relation to water availability at lines 30-44, disturbance is not specifically mentioned. I'd suggest this aspect should be developed, or, if disturbance is to be understood as synonymous to mortality rates, removed from the introductory sentence. [Gwenaelle GREMION, Canada]	Accepted: Have added new subsection 5.4.3.2 on fire and disturbance
17610	47	5	47	5	Change 'warmer' to 'higher' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted: Text Revised
17608	47	7	47	7	Reference(s) required [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Rejected: These are general statements here, citations will come in more specific text later in the section.
22742	47	9	47	11	The sentence reports that "ecosystem responses" to climate change will likely act as a positive feedback, this seems perhaps too general for the scope of this section and could be narrowed down to "terrestrial ecosystem responses" [Gwenaelle GREMION, Canada]	Accepted: Text Revised
17612	47	10	47	10	Italicise 'high' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
49014	47	10	47	11	"... but it is likely that ecosystem response to climate change ...", may need to give references to support this statement. [Minchao Wu, Sweden]	Accepted: Text revised to delete word "likely" here
47758	47	10	47	49	Please check the use of this IPCC uncertainty language term. Are you able to provide a traceable account to assigning this uncertainty statement? Note that likelihood statements are quantified terms - phrases like likely and very likely have quantifiable probabilities associated with them. Please check it has been used correctly here. Please refer to the IPCC guidance note on uncertainty: <a href="https://wg1.ipcc.ch/SR/documents/ar5_uncertainty-guidance-note.pdf">https://wg1.ipcc.ch/SR/documents/ar5_uncertainty-guidance-note.pdf</a> [WGI TSU, France]	Accepted: Text revised to delete word "likely" here
36834	47	13	47	17	Changes in activities of Monsoon and Typhoon as local climate change can make substantial impacts on terrestrial carbon cycle. (e.g., Kwon et al., 2010; Hong and Kim, 2011). Hong, J. and J. Kim (2011) Impact of the Asian monsoon climate on ecosystem carbon and water exchanges: A wavelet analysis and its ecosystem modeling implication, Global Change Biology, 17, 1900-1916. Kwon, H., J. Kim, J. Hong, and J. Lim (2010) Influence of the Asian monsoon on interannual variability of net ecosystem carbon exchange in two major plant functional types in Korea, Biogeosciences, 7, 1493-1504. [Jinkyu Hong, Republic of Korea]	Noted
41822	47	14	47	14	end of sentence not clear (word missing ?) [Marc Aubinet, Belgium]	Accepted: Text Revised

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
57770	47	15	47	17	what about other climate factors/phenomena , such as humidity and extreme winds ? What about fires? Could add more citations on droughts (Anderegg, William RL, et al. "Pervasive drought legacies in forest ecosystems and their implications for carbon cycle models." Science 349.6247 (2015): 528-532., Doughty, Christopher E., et al. "Drought impact on forest carbon dynamics and fluxes in Amazonia." Nature 519.7541 (2015): 78., [Elena Shevliakova, United States of America]	Accepted: Have added new subsection 5.4.3.2 on fire and disturbance
29664	47	15	47	17	These words could be additionally supported by the reference to the following work: Chang, J., Ciais, P., Viovy, N., Soussana, J.-F., Klumpp, K. and Sultan, B.: Future productivity and phenology changes in European grasslands for different warming levels: implications for grassland management and carbon balance, Carbon Balance Manag., 12(1), 11, doi:10.1186/s13021-017-0079-8, 2017. [Georgii Alexandrov, Russian Federation]	Rejected: suggested reference is largely overlapping with two that are already referenced
17614	47	19	47	19	Quantify 'warmer' and 'high' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted: Changed wording to say "tropical and temperate environments".
22744	47	20	47	22	Perhaps it is because English is not my native language, but the structure of the first half of this sentence does not seem to work. Replacing the "with" (line 22) by "or to" would solve this by putting the two mechanisms in opposition, which corresponds better to the reasoning in the second half of the sentence. [Gwenaelle GREMION, Canada]	Accepted: Text Revised
17616	47	21	47	21	Change 'exceedance' to 'exceedence' (either is correct but the latter is better English) [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
41824	47	21	47	22	Not clear. Would you mean " A key question is whether the observed relationships are due to the exceedance of temperature thresholds in photosynthetic biochemistry itself, or to higher vapour pressure deficit accompanying high temperatures"? I would agree with that. [Marc Aubinet, Belgium]	Accepted: Text Revised
49012	47	23	47	23	" ... vapour pressure deficit effects are strongest ... ", it is not clear here, may I know "strongest" here is compared to what other effects? [Minchao Wu, Sweden]	Accepted: Text Revised
6826	47	25	47	27	Smith et al. (2017) have also included both photosynthetic and leaf respiratory temperature acclimation in the LM3 model. Citations: Smith NG, Malyshev SL, Shevliakova E, Kattge J, Dukes JS. 2016. Foliar temperature acclimation reduces simulated carbon sensitivity to climate. Nature Clim. Change 6: 407–411. [Nicholas Smith, United States of America]	Accepted: Text Revised

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
6828	47	27	47	28	Many studies (e.g., Smith and Dukes 2013, Smith and Dukes 2017, Way and Yamori 2014, Yamori et al. 2014, Kattge and Knorr 2007) have found that temperature acclimation is very widespread among plant species. There may be parameteric uncertainty in the formulas, but the phenomenon is widespread and well understood. Photosynthetic temprature acclimation not only increase carbon uptake (e.g., Lombardozi et al. 2015), but also results in direct surface cooling as a result of increase transpiration (Smith et al., 2017). Citations: Smith NG, Dukes JS. 2013. Plant respiration and photosynthesis in global-scale models: incorporating acclimation to temperature and CO2. Global Change Biology 19: 45–63. Smith NG, Dukes JS. 2017. Short-term acclimation to warmer temperatures accelerates leaf carbon exchange processes across plant types. Global Change Biology 23: 4840–4853. Way DA, Yamori W. 2014. Thermal acclimation of photosynthesis: on the importance of adjusting our definitions and accounting for thermal acclimation of respiration. Photosynthesis Research 119: 89–100. Yamori W, Hikosaka K, Way DA. 2014. Temperature response of photosynthesis in C3, C4, and CAM plants: temperature acclimation and temperature adaptation. Photosynthesis Research 119: 101–117. Kattge J, Knorr W. 2007. Temperature acclimation in a biochemical model of photosynthesis: a reanalysis of data from 36 species. Plant, Cell & Environment 30: 1176–1190. Lombardozi DL, Bonan GB, Smith NG, Dukes JS, Fisher RA. 2015. Temperature acclimation of photosynthesis and respiration: A key uncertainty in the carbon cycle-climate feedback. Geophysical Research Letters 42: 8624–8631. Smith NG, Lombardozi D, Tawfik A, Bonan G, Dukes JS. 2017. Biophysical consequences of photosynthetic temperature acclimation for climate. Journal of Advances in Modeling Earth Systems 9: 536–547. [Nicholas Smith, United States of America]	Taken Into Account: at least one of these papers was already assessed in FOD.
17618	47	30	47	30	move 'better' to after 'understand' to remove split infinitive [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
57772	47	30	47	44	somewhere in this paragraph, there needs to be an acknowledgement that many CMIP6 ESMs are still using prescribed vegetation not dynamic vegetation models and cannot predict changes in vegetation composition. Most CMIP6 models represent plant mortality only due to carbon starvation. [Elena Shevliakova, United States of America]	Accepted: Have added new subsection 5.4.3.2 on fire and disturbance
47848	47	30	47	44	Section 8.4.2.5 assesses modes of variability and their impact on the water cycle - would it be appropriate to cross-referece to this section here? [WGI TSU, France]	Accepted: cross-referenced section 8.4.2.7 (current modes of variability section in ch. 8 interim draft)
17620	47	33	47	33	Change 'extend' to 'extent' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
7496	47	33	47	34	Typos: Extent not extend; remove "the" from "in the interannual rainfall"; missing "be" in "appear to [be] an important driver" [Rose Abramoff, France]	Accepted - text revised
16308	47	34	47	34	change wording e.g. replace "to an" with "as an" or make "[...] as important drivers" or "appear to be important drivers" [Wolfgang Obermeier, Germany]	Accepted - text revised
22746	47	35	47	35	The reference (Korth et al., 2015) does not make sense in this context. [Gwenaelle GREMIION, Canada]	Accepted: Text Revised to cite Ahlstrom 2016 rather than what had been there.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
22748	47	41	47	43	The "additional ecological processes" should be specified (at least some examples). [Gwenaëlle GREMION, Canada]	Accepted: Text Revised
47096	47	46	47	55	5.4.3.: Enhanced weathering due to climate change may change mineralogy and carbon storage of soils; see e.g. Doetterl et al. 2018 ( <a href="https://doi.org/10.1038/s41561-018-0168-7">https://doi.org/10.1038/s41561-018-0168-7</a> ) [Sophie von Fromm, Germany]	Rejected: This is a long-term process as per the reference cited, may include discussion in section 5.4.9
6726	47	48	47	48	Remove the question mark from the middle of this sentence. [Andrew MacDougall, Canada]	Accepted - text revised
41826	47	48	47	48	remove question mark [Marc Aubinet, Belgium]	Accepted - text revised
6830	47	48	47	48	Out of place question mark. I believe "positive" is correct in this context. [Nicholas Smith, United States of America]	Accepted - text revised
17622	47	48	47	48	There is a rogue ? in the text, and there is something missing after 'feedback' ('mechanism'/'source'/'driver') [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted: Text Revised
13650	47	48			"positive ?" --> clear question mark [Lena Boysen, Germany]	Accepted - text revised
17624	47	50	47	50	Poor expression, 'include' implies there are other things, no listed. I suggest replacing 'include' with 'are' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
22750	47	50	47	51	Including a coarse estimate of what fraction of worldwide soil carbon these "large amounts of potentially decomposable soil carbon" represent could help understanding the importance given to permafrost systems on page 48. [Gwenaëlle GREMION, Canada]	Accepted: have included estimate of permafrost carbon stocks.
57774	48	2	48	3	it would be good to remind a reader what is the difference between the carbon-concentration and carbon- climate feedbacks [Elena Shevliakova, United States of America]	Accepted: Text Revised
22752	48	2	48	3	"carbon-concentration" = atmospheric CO2 concentration? Unclear in this context. [Gwenaëlle GREMION, Canada]	Accepted: Text Revised
36378	48	2		3	Give the sign of these contributions to the feedbacks. [Nathan Gillett, Canada]	Accepted: Text Revised
47850	48	3	48	3	'large uncertainty' is not an official IPCC confidence term. Please refer to the IPCC guidance note on uncertainty for correct use of terms: <a href="https://wg1.ipcc.ch/SR/documents/ar5_uncertainty-guidance-note.pdf">https://wg1.ipcc.ch/SR/documents/ar5_uncertainty-guidance-note.pdf</a> [WGI TSU, France]	Accepted: Text Revised
36380	48	3		5	Are there any observational constraints on the soil contributions to these feedbacks, or are the models the only thing on which we can base an assessment? If the model range is large, but there are good observational constraints, then the overall uncertainty could still be small. Also this sentence refers to 'this feedback', but the previous sentence refers to two different feedbacks (carbon-concentration and carbon-climate). Which of these is being referred to here? [Nathan Gillett, Canada]	Accepted: Reworded and added references to He et al 2016 and Varner et al (in review), which try to use observational constraints to narrow ranges of model projections.
7498	48	6	48	8	"Changes ... were ... weak" is not very clear. Suggest "Changes were small" or "Changes were not well-represented", depending on what the author means here. [Rose Abramoff, France]	Accepted: Text Revised
22754	48	9	48	11	Perhaps Guenet et al. 2018 (already cited p45) should be referred to in that sentence, as their main conclusion was the importance to include such interactions in future ESMs. [Gwenaëlle GREMION, Canada]	Accepted: Text Revised

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
15184	48	13	48	21	Another considerable uncertainty with regard to permafrost is the role that fire will have in promoting the release of carbon. See: Mack, Michelle C., et al. "Carbon loss from an unprecedented Arctic tundra wildfire." <i>Nature</i> 475.7357 (2011): 489. This comment would similarly apply to FAQ 5.2. [Richard Vachula, United States of America]	Accepted: Have added new subsection 5.4.3.2 on fire and disturbance
57776	48	13	48	21	this section will benefit from an update on estimates (with uncertainty) of C stocks in permafrost [Elena Shevliakova, United States of America]	Accepted: Text revised to cite Hugelius et al 2014 estimate of permafrost soil C stocks.
32226	48	13	48	21	A postdoc in my group has a synthesis article currently in review on the use of radiocarbon to detect respiration of old permafrost soil carbon into CO <sub>2</sub> and CH <sub>4</sub> following permafrost thaw. The conclusion is that field data at this time show that permafrost thaw has been shown to cause respiration of old soil C under some conditions (e.g. active layer deepening of aerobic soils), while other settings such as wetlands where thaw leads to inundation of recently thawed soils does not seem to lead to rapid mineralization into CO <sub>2</sub> . Hence I think this paragraph can state that experiments and field data have shown that permafrost thaw can lead to increased respiration of aged soil C in certain settings, but not uniformly. The citation is: Estop-Aragonés C, Olefeldt D, Abbott BW, Chanton JP, Czimczik CI, Dean JF, Egan JE, Gandois L, Garnett MH, Hartley IP, Hoyt A, Lupascu M, McClelland JW, Natali SM, O'Donnell JA, Raymond PA, Tanentzap AJ, Tank SE, Schuur EAG, Turetsky MR, Walter Anthony K (In review) A synthesis of <sup>14</sup> C measurements from the northern permafrost region: assessing the potential for mobilization of old soil carbon after permafrost thaw. Submitted April 2019 to <i>Global Biogeochemical Cycles</i> . [David Olefeldt, Canada]	Taken Into Account
22756	48	13	48	42	These paragraphs lack of any term about the confidence or likelihood of the assertions and including them would be very helpful for the readers. [Gwenaelle GREMION, Canada]	Taken Into Account
47348	48	13			could add whether or not permafrost included to table 5.5? [chris jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted: Text of table 5.4 revised.
22758	48	17	48	21	Since rhizosphere priming is mentioned earlier (p.45 and end of previous paragraph) and will likely be integrated in future ESMs, it could be worthwhile to acknowledge that plant-microbe interactions could substantially increase the permafrost carbon source magnitude and its uncertainty (Frida Keuper and others, "Rhizosphere priming doubles carbon loss from northern circumpolar permafrost soils", manuscript under review). Alternatively, this could be mentioned within section 5.4.8.2 at page 58. [Gwenaelle GREMION, Canada]	Rejected: insufficient space to go into such detail.
36382	48	19			If these interactions weaken the feedback, they must partly offset it. Do the authors mean 'do not completely offset'? [Nathan Gillett, Canada]	Accepted: Text Revised
17626	48	21	48	21	This needs more detail and/or quantification [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted: Text of table 5.4 revised.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
22760	48	23	48	24	This sentence does not make sense. Do you mean something like "Soil microbial dynamics shift in response to changes in temperature, with more complex ecological alterations occurring over longer-term temperature changes."? [Gwenaelle GREMION, Canada]	Accepted: Text Revised
49016	48	32	48	37	This paragraph may have some overlap information with the section 5.4.1 "Direct CO2 effects on land carbon uptake", lines 46-55, may be good to harmonize them. In addition, the statement "In CMIP5, only one land model included nutrient dynamics, and it was an outlier in its feedback strength as compared to models that did not include nutrients." may be not clear enough. It is not clear if these CMIP5 simulations discussed here were driven by climate only, or the combination of climate and CO2 effects, or say, the different feedback strength is in response to climate or the elevated CO2? If it is the later, the main idea here is also overlapped with 5.4.1. May also be better to clarify and give supporting references. [Minchao Wu, Sweden]	Accepted: Text Revised
36384	48	35			An outlier in which direction? [Nathan Gillett, Canada]	Accepted: Text Revised
41828	48	39	48	39	what do you mean by "plant stoichiometry"? [Marc Aubinet, Belgium]	Accepted: Text Revised
39362	48	41	48	42	It is unclear why "the overall effect of nutrients is weaker than was inferred in AR5." This not only needs an update, but that update needs evidence and explanation. [Eric Davidson, United States of America]	Accepted: Text Revised
27762	48	42	48	42	delete [[Placeholder: needs update]]. [Poot Delgado Carlos Antonio, Mexico]	Accepted: Text Revised
47760	48	49	48	49	Please check the use of this IPCC uncertainty language term. Are you able to provide a traceable account to assigning this uncertainty statement? Note that likelihood statements are quantified terms - phrases like likely and very likely have quantifiable probabilities associated with them. Please check it has been used correctly here. Please refer to the IPCC guidance note on uncertainty: <a href="https://wg1.ipcc.ch/SR/documents/ar5_uncertainty-guidance-note.pdf">https://wg1.ipcc.ch/SR/documents/ar5_uncertainty-guidance-note.pdf</a> [WGI TSU, France]	Not applicable - sentence changed
22764	48	49	48	49	Is the "likely" based on the following paragraph? If so, I would use it in a summarising statement after presenting the evidence [Gwenaelle GREMION, Canada]	Not applicable - sentence changed
22762	48	49	48	50	This sentence isn't entirely clear as to what the common pattern is in (i.e. Hindcast models) or what the common pattern itself is (i.e. Increased storage, biggest increase in mid-high latitude thermoclines and lowest in equatorial Pacific & Indian Oceans). [Gwenaelle GREMION, Canada]	Taken into account - text revised
22766	48	49	48	50	It is confusing that heat and carbon uptake show a similar pattern, yet warming reduces CO2 uptake which would indicate opposite patterns (increased heat vs. decreased carbon). This seems incoherent to me. [Gwenaelle GREMION, Canada]	Rejected - CO2 and heat show the same pattern, but with opposite sign. This is explained already in the first sentence in this paragraph
22768	48	50	48	50	The reference Frölicher et al. discusses ocean heat uptake and not ocean carbon storage (there is only one figure on ocean carbon in the supplements). Perhaps this reference could even be omitted, as the subsequent Randerson reference covers the statement quite well. [Gwenaelle GREMION, Canada]	Rejected - the reference <a href="https://journals.ametsoc.org/doi/10.1175/JCLI-D-14-00117.1">https://journals.ametsoc.org/doi/10.1175/JCLI-D-14-00117.1</a> discusses extensively carbon storage as well as heat

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
13546	48	51	48	53	In which scenario? It may be better to discuss the % changes here. [Govindasamy Bala, India]	Taken into account - text revised
22770	48	51	48	53	It isn't stated which scenario this projection is from (rcp8p5). The rcp8p5 projection for ocean CO2 uptake from the previous reference (Randerson et al., 2015) is -44 PgC by 2100, but this isn't mentioned in either sentence. I'd consider reorganising the first couple of sentences to have the first discuss the nature of the common patterns of change, and the second that future warming projections are for reduced ocean CO2 uptake (with -20 & -44 PgC given as two example rcp8p5 projections). [Gwenaelle GREMION, Canada]	Taken into account - text revised
22772	48	52	48	52	by year 2100 [Gwenaelle GREMION, Canada]	Accepted - text revised
22774	48	52	48	52	use "decrease" instead of "reduce". Reduce would only work in the passive form here [Gwenaelle GREMION, Canada]	Accepted - text revised
22776	48	53	48	53	Would appreciate a brief explanation of what buoyancy flux is. What is meant with "heat, freshwater" in parentheses? [Gwenaelle GREMION, Canada]	Taken into account - text revised
22778	48	53	48	54	This sentence partially repeats the information from the preceding one, that increased ocean warming decreases CO2 uptake. Perhaps the sentence can be combined or the explanation of the buoyancy fluxes simply expanded more in the second sentence. [Gwenaelle GREMION, Canada]	Taken into account - this paragraph refers explicitly to circulation-driven changes in co2 uptake, not just temperature driven. The text on buoyancy fluxes has been expanded.
22780	48	54	48	54	Ito et al (2015) show a global decrease but a notable increase in Southern Ocean, so it might be more accurate to change to ", which decrease global CO2 uptake". [Gwenaelle GREMION, Canada]	Accepted - "global" has been added
22784	48	55	48	56	This chapter concerns physical drivers of ocean carbon uptake. This sentence does not quite fit in with the topic. However, it is still of interest, but would fit better if for example the drivers of ocean carbon distribution were mentioned. [Gwenaelle GREMION, Canada]	Not applicable - sentence changed
22782	48	55	49	1	The given reference (Khatiwala et al, 2018) is entirely focused on C14 distribution, and I can't find any discussion of general ocean carbon distribution changes (and their main model doesn't feature temperature impacts or allow circulation changes). On closer reading it is clearer that this sentence is actually following on from the previous two references (Bernardello et al, 2014; & Ito et al, 2015), but as currently drafted the sentence seems to imply it refers to Khatiwala et al (2018). [Gwenaelle GREMION, Canada]	Taken into account - text revised
22786	49	3	49	6	This statement (decreased CO2 uptake in high vs low latitudes) somewhat contradicts the results of Ito et al (2015), which finds strengthened uptake and storage in the high-lat Southern Ocean (although both agree on a global net weakening). This contrast should be discussed. [Gwenaelle GREMION, Canada]	Taken into account - text revised
22788	49	3	49	6	The sentence could be restructured to read "At high latitudes,..., at low latitudes..." to achieve an emphasis on the difference between latitudes [Gwenaelle GREMION, Canada]	Taken into account - text revised



Comment ID	From Page	From Line	To Page	To Line	Comment	Response
22790	49	6	49	8	This sentence is a bit unclear and repeats previous statements The past several sentences have been focused on how and why warming and circulation changes will drive a weakening in the ocean CO2 uptake, so it seems unnecessary to repeat that here. Unless something different is meant by CO2 uptake fraction in this context? If so, it needs clarifying. [Gwenaelle GREMION, Canada]	Rejected - these sentences discuss the ocean CO2 uptake over different timescales so present new information
17628	49	8	49	8	Change to 'Earth System Model' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - "System Model" not capitalised.
22794	49	8	49	10	It is not crystal-clear if and how these 20% relate to the 20PgC mentioned earlier (p.48 L52) [Gwenaelle GREMION, Canada]	Rejected - the 20PgC refers to the total reduction in cumulative ocean CO2 uptake by both solubility and biological pumps, plus circulation changes. The 20% decrease in ocean CO2 uptake refers explicitly to the solubility and circulation changes. We believe the text is sufficiently clear.
22792	49	8	49	13	No details are in the reference list for the citation for this sentence, so I can't check or vouch for its content. I'd recommend including a PgC value here as well as the % decline value so that it can easily be compared to the 20 PgC reduction stated on pg.48.ln.52. "region... at high surface ocean pCO2 conditions" is also a little unclearly phrased, but I can't check its context against the reference. [Gwenaelle GREMION, Canada]	Not applicable - the text in this section has changed and the reference no longer appears
17630	49	9	49	9	Change to '21st Century' for consistency [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - The AR6_WGI_StyleGuide says to use "21st century"
22796	49	12	49	13	"Rodgers and other no title" - Is that a missing reference? [Gwenaelle GREMION, Canada]	Noted - Rodgers et al., citations are now correctly linked.
27764	49	12	49	13	check bibliographic citation (Rodgers and others No title). [Poot Delgado Carlos Antonio, Mexico]	Noted - Rodgers et al., citations are now correctly linked.
17632	49	13	49	13	Not clear what 'No title' means [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account
22798	49	16	49	16	Dissolved organic carbon should be change to dissolved inorganic carbon [Gwenaelle GREMION, Canada]	Accepted - text revised
47100	49	16	49	16	5.4.4.1.: Usually DIC = dissolved inorganic carbon and DOC = dissolved organic carbon. [Sophie von Fromm, Germany]	Taken into account - DIC was meant here
22800	49	17	49	19	Rysgaard et al (2013) states that this DIC/TA-enriched brine increased CO2 uptake as a result of being exported through and below the mixed layer, and does not discuss the brine staying in the mixed layer and driving CO2 release as implied. This latter statement is closer to the results of Grimm et al (2016) cited in the next sentence, who find that regions of net sea ice growth have CO2 outgassing. I suggest either moving the Rysgaard reference to the previous sentence and replacing with Grimm, or adapting this sentence to feature both the possibility of enriched-brine staying in the mixed layer and driving CO2 release (Grimm) or sinking below the mixed layer and driving CO2 uptake (Rysgaard). [Gwenaelle GREMION, Canada]	Taken into account - text revised

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
22802	49	19	49	21	Do you mean the sea-ice induced CO2 uptake by the atmosphere is only a small fraction of the oceanic uptake? Not clear to me which uptakes are meant when considering the previous sentence mention flux to the atmosphere [Gwenaelle GREMION, Canada]	Taken into account - this refers to oceanic uptake
19220	49	24	50	30	please also consider Pavia et al. 2019: global warming feedback on subtropical gyres and oxygen minimum zones [Baerbel Hoenisch, United States of America]	Rejected - we concluded that this regional study did not have sufficient relevance at the global, climate scales that this chapter deals in
22804	49	26	49	26	"sign of the response" - I understand what is meant, but perhaps "direction" or something similar works better [Gwenaelle GREMION, Canada]	Accepted - text revised
22806	49	26	49	26	Start this section with a sentence on the direct relationship between PP and ocean carbon uptake [Gwenaelle GREMION, Canada]	Taken into account - text added
33480	49	26	49	26	The is contrary to the result reported in Chapter 3 first paragraph of page 50. [Adrienne Sutton, United States of America]	Taken into account - Chapter 3 page 50 refers to trends from observations over the period 1997-2013. The trends discussed in this section are the future projections of trends from 2006-2100. We have added a couple of words to make this clear.
22808	49	26	49	44	The paragraph lacks of any term about the confidence or likelihood of the assertions and including them would be very helpful for the readers. [Gwenaelle GREMION, Canada]	Taken into account
22810	49	29	49	30	"..., including the rate of PP,..." [Gwenaelle GREMION, Canada]	Accepted - text revised
22812	49	29	49	30	This statement is too simplified. Light reactions are increasing in their rates, while RuBisCO does. Furthermore, PP and respiration are not simply increasing with increasing temperatures, but temperature stress has to be considered. Especially for PSII this can have severe affects on the photophysiology and eventually on CO2 fixation rates.  Reference: Mathur, S., Agrawal, D., & Jajoo, A. (2014). Photosynthesis: response to high temperature stress. Journal of Photochemistry and Photobiology B: Biology, 137, 116-126. [Gwenaelle GREMION, Canada]	Taken into account - text added
22814	49	29	49	30	One important point in the review by Boscolo Halazzo et al. is that metabolic rates increase, but 2 times faster for respiration than PP rates, which is very important to consider. In the current formulation it sounds like both rates increase equally, which would have very different consequences for marine CO2 fixation. [Gwenaelle GREMION, Canada]	Taken into account - text added
6832	49	29	49	30	Respiration is a metabolic process, so this sentence needs reworded. [Nicholas Smith, United States of America]	Accepted - text revised
22816	49	30	49	30	"the respiration rate" [Gwenaelle GREMION, Canada]	Accepted - text revised

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
22818	49	30	49	31	<p>Increased stratification does not necessarily reduce PP. Increased stratification has two effects; I) decreasing nutrient supply due to decreasing upwelling (reduction of PP) and II) shallower mixed layer depth increases the time phytoplankton spends in the euphotic zone, which decreases light limitation (increase in PP). The overall effect depends on the region. E.g. the southern Ocean has currently a deep mixed layer depth limiting PP by light limitation, while PP in other regions is already more limited by nutrient supply via upwelling.</p> <p>In the Southern Ocean, for example, PP is expected to increase with increasing stratification.</p> <p>Reference Bopp L, Resplandy L, Orr JC et al. (2013) Multiple stressors of ocean ecosystems in the 21st century: projections with CMIP5 models. Biogeosciences Discussions, 10, 6225–6245. [Gwenaëlle GREMION, Canada]</p>	Taken into account - text added
31668	49	30	49	32	<p>Increased atmospheric transport of nitrogen to the ocean may counteract reduced vertical nutrient supply (e.g., Jung et al., 2011: J. Atmos. Chem. 68, 157-181, Kim et al., 2014: Science 346, 1102-1106, Yasunaka et al., 2016: Geophys. Res. Lett. 43, 3389-3397), but there are no mention about this neither in this palagpagh nor Chapter 9. [Tsuneo Ono, Japan]</p>	Taken into account - text added
22820	49	32	49	34	<p>If place is an issue, this sentence can be shortened to give more place to allow mentioning the positive effects of shallowed mixed layer depths e.g. [Gwenaëlle GREMION, Canada]</p>	Taken into account - text revised
22822	49	32	49	34	<p>The metabolic rates in the reference are measured in 3 phytoplankton species during a short term cultivation experiment (no evolution possible) under oligotrophic conditions. I am not convinced that this statement can be generalized to all metabolic rates in the ocean and should include some terms of uncertainty. [Gwenaëlle GREMION, Canada]</p>	Taken into account - uncertainty language added
22826	49	34	49	34	<p>delete "through" [Gwenaëlle GREMION, Canada]</p>	Accepted - text revised
7500	49	34	49	34	<p>Missing "mechanisms" or similar noun after "direct (metabolic rates)" [Rose Abramoff, France]</p>	Accepted - text revised
17634	49	34	49	34	<p>Looks like there is text missing at the end of the sentence [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]</p>	Accepted - text revised
22824	49	34	49	37	<p>No reference appears to be given for the enhanced dust/iron input due to desertification leading to increased PP, or for circulation/stratification-induced changes in iron and PP. [Gwenaëlle GREMION, Canada]</p>	Taken into account - references added
31976	49	34	49	39	<p>It would be useful to compare this result with paleo results showing the dust impacts of the ocean in the past [Marie-France Loutre, Switzerland]</p>	Taken into account - text added
36386	49	34		35	<p>References? [Nathan Gillett, Canada]</p>	Taken into account - reference to Mahowald et al. (2017) has been added

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
22828	49	35	49	39	<p>I am missing the input of bioavailable iron by ice bergs. Is has been shown to be the major input of iron in polar regions, exceeding the input from dust or upwelling. With climate change calving of glacier fronts will increase, which will increase the input of iceberg derive iron and eventually increase PP.</p> <p>Reference: Duprat, L. P., Bigg, G. R., &amp; Wilton, D. J. (2016). Enhanced Southern Ocean marine productivity due to fertilization by giant icebergs. Nature Geoscience, 9(3), 219.</p> <p>Raiswell, R., Hawkings, J. R., Benning, L. G., Baker, A. R., Death, R., Albani, S., ... &amp; Tranter, M. (2016). Potentially bioavailable iron delivery by iceberg-hosted sediments and atmospheric dust to the polar oceans. Biogeosciences, 13(13), 3887-3900. [Gwenaelle GREMION, Canada]</p>	<p>Accepted - text added. Iron input from icebergs only exceeds input from dust and upwelling at the local scale (sometimes), not at the Southern Ocean-wide scale. We have noted that icebergs may become more frequent in future</p>
13886	49	35	49	39	<p>It would be relevent to mention here that there is evidence from paleoclimate records supporting a significant impact of dust-borne iron fertilization in the subantarctic Southern Ocean and possibily other HNLC areas, during the Last Glacial Maximum; global ocean biogeochemistry model simulations indicate that ~20 ppmv CO2 could be removed from the atmosphere due to this mechanism (e.g. Albani et al. 2018 and references therein).</p> <p>Albani S., Balkanski Y., Mahowald N., Winckler G., Maggi V., Delmonte B.: Aerosol-climate interactions during the Last Glacial Maximum. Curr. Clim. Change Rep., 4, 99-114, doi:10.1007/s40641-018-0100-7, 2018. [Samuel Albani, Italy]</p>	<p>Accepted - text revised. More information on the paleo aspects are given in section 5.2.2.1</p>
22832	49	37	49	37	"CO2 concentration" [Gwenaelle GREMION, Canada]	Accepted - text revised
13282	49	37	49	37	why are units by volume (ppmv) suddenly used rather than by mass ones (ppm) ? Also in 5-53-32 and 5-58-34. [Frederic Chevallier, France]	Accepted - text revised
17636	49	37	49	37	Delete , after al. [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account
22830	49	37	49	38	A mixture of ppm and uatm is used for pCO2 in this sentence, which may confuse some readers and makes it read less consistently. [Gwenaelle GREMION, Canada]	Accepted - text revised
22834	49	37	49	39	It is not clear to me how 2 ppmv and 9.6 uatm compare due to the different units. The sentence could contain a statement as to the meaning of the different effects of dust deposition and light-iron limitation. [Gwenaelle GREMION, Canada]	Accepted - text revised
22836	49	38	49	38	Iron-light colimitation is an important mechanism. However, in the current form it is unclear why light limitation plays a role. I would suggest introducing the role of lighth limitation based on shallower mixed layer depths earlier in this paragraph. [Gwenaelle GREMION, Canada]	Accepted - text revised
36388	49	38			Why is CO2 concentration reported in these units (micro atm) here, when the more usual ppmv is used earlier in the same sentence? [Nathan Gillett, Canada]	Accepted - the value has been changed to 9.9 micro atm

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
22842	49	39	49	41	This is not true for all Oceans. The cited paper found opposite trends in the Southern Ocean and some tropical regions, where large diatom contributions are increasing. [Gwenaëlle GREMION, Canada]	Accepted - text revised
22838	49	39	49	42	Fu et al (2016) doesn't specifically consider ocean acidification - its only mention is that it's not well represented and may have an impact (but impact direction or magnitude is not stated). I suggest adding Finkel et al (2010, J. Plankton Res., doi:10.1093/plankt/fbp098 - a review on climate impacts on plankton) as a reference here as well, which discusses many of these changes including the potential impact of OA. I'd also consider adding "and export" or similar after "both are expected to reduce PP", as PP and export are not always coupled (and Fu2016 show export declines more than PP). [Gwenaëlle GREMION, Canada]	Accepted - text revised
22840	49	39	49	42	The addition of extra information at the end of the sentence ("and also as a result...", ";both are expected...") makes it difficult to read. It is not clear what the result of ocean acidification refers to due to the unclear sentence structure. Better to make one or two coherent sentences. [Gwenaëlle GREMION, Canada]	Taken into account - text revised
22844	49	43	49	43	This statement sounds as if it is not clear whether there is any potential at all for synergistic or antagonist effects - I am sure is potential, but the unclarity lies in whether and to which extent the effects will be synergistic or antagonistic [Gwenaëlle GREMION, Canada]	Taken into account - text revised
22846	49	43	49	44	Nutrient limitations by changes in the nitrogen cycle are not discussed. Since it is an indirect effect i might be enough to link to another chapter discussing impacts of climate change on the nitrogen cycle (e.g. loss of nutrients by denitrification in OMZs, increasing N2 fixation with increasing iron inputs) [Gwenaëlle GREMION, Canada]	Taken into account - a sentence has been added on this point and call-outs to more information in this chapter and also the SROCC
22848	49	48	49	50	So, here it seems that most models predict that temperature drives the reduction of PP. In the previous paragraph (line 34), it was stated that the effect of warming is mostly through indirect effects on nutrient supply. This appear contradicting. [Gwenaëlle GREMION, Canada]	Accepted - text revised. Strong nutrient limitation occurs in all of the models, but in 4 of them there is the additional temperature-driven increase in loss rates
22850	49	50	49	50	What is sinking? The effect of grazing and mortality on primary production could be explained more. It is not clear to me how grazing and mortality affect PP as such. As far as I am aware, PP is the amount of carbon fixed in organic molecules, regardless of whether this gets eaten or deceases afterwards. [Gwenaëlle GREMION, Canada]	Accepted - text revised. This refers to net PP (i.e. carbon fixed - carbon lost via grazing, mortality, or sinking of particles out of the euphotic zone)
22852	49	51	49	52	Maybe add "and spatial coverage" after "due to insufficiently long records", as Henson et al (2016) discuss spatial coverage as a major issue as well (with only 9-15% of the global ocean area considered to be well covered by observations). [Gwenaëlle GREMION, Canada]	Rejected - spatial coverage is not an issue for PP, as satellite ocean colour data provides global estimates
42484	49	54	49	54	Potentially increased light levels? Stratification in summer may be increased but winter deep mixed layers also. However deep chlorophyll maxima already exist so unclear at present how this will impact ecosystems seasonally and annually. [Peter Croot, Ireland]	Taken into account - Light levels may be higher in high latitudes, extending the phytoplankton growing season (information added to 1st paragraph of section). A mention of DCMs has also been added.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
22854	49	54	50	7	This paragraph deals with nutrient limitation which was already mentioned in the first paragraph. It can be integrated into the first paragraph. The effect of nutrient limitation on PP is very interesting and good to specify. It would be interested if latitudinal differences in nutrient limitation are known, such as is known for terrestrial ecosystems. [Gwenaelle GREMION, Canada]	Rejected - this paragraph actually deals with changes to nutrient stoichiometry of organic matter and the influence of allowing variable stoichiometry on modelled projections of ocean C uptake.
22856	49	54	50	7	The paragraph lacks of any term about the confidence or likelihood of the assertions and including them would be very helpful for the readers. [Gwenaelle GREMION, Canada]	Taken into account
44112	49	55	49	55	Elser et al. 2008 offers a good overview of biological (ecological) stoichiometry ( <a href="https://onlinelibrary.wiley.com/doi/full/10.1111/j.1461-0248.2000.00185.x">https://onlinelibrary.wiley.com/doi/full/10.1111/j.1461-0248.2000.00185.x</a> ) [Sara Kahanamoku, United States of America]	Rejected - this section addresses the projected changes in stoichiometry, which this paper does not mention.
22858	50	1	50	3	What is a "soft tissue pump" and what is the meaning of this? A brief description could be given since it is used in the following paragraph. Do these ratios refer to the phytoplankton or the ocean? [Gwenaelle GREMION, Canada]	Taken into account - section 5.2.4.9 describes the soft tissue pump. Variable stoichiometry refers to the phytoplankton ratios.
42486	50	9	50	9	New important particle flux papers by Boyd et al (2019) [10.1038/s41586-019-1098-2] and Cavan et al (2019) [10.3389/fevo.2018.00230] [Peter Croot, Ireland]	Accepted - text revised
22860	50	9	50	15	This section could do with some references - the first three sentences have none. I would again suggest Finkel et al (2016, J. Plankton Res., doi:10.1093/plankt/fbp098) as a good review. Another useful modelling study is Segschneider & Bendtsen (2013; Global Biogeochem. Cycles; doi:10.1002/2013GB004684), who modelled the temperature influence on POC remineralisation. For ballasting, there is some relevant discussion in John et al (2014; P <sup>3</sup> ; doi:10.1016/j.palaeo.2014.05.019) and Wilson et al (2012; GBC; doi:10.1029/2012GB004398). It's not yet submitted (but will be within the next few months, so hopefully within the IPCC acceptance timeframe), but I'll also soon submit work recently presented at EGU on modelling the interaction of temperature effects on remineralisation, stoichiometry, and plankton community structure and implications for future ocean C uptake in an EMIC (ecoGENIE), which might be of relevance for this section as well (provisional citation details: Armstrong McKay et al, 2019/2020?, Incorporating ecological and metabolic dynamics in modelling of ocean carbon cycle feedbacks). [Gwenaelle GREMION, Canada]	Accepted - additional references have been added
22862	50	9	50	17	Perhaps these sentences can be summarised. Warming and community structure are mentioned at least twice. Are any results known concerning the effect of community structure, metabolic rates etc on ocean CO2 uptake? [Gwenaelle GREMION, Canada]	Taken into account - some additional references have been added
22864	50	9	50	30	There is a very recent article (10.1038/s41586-019-1098-2) that includes a comprehensive review of the the particle carbon pump on the ocean. Some of these findings may be very useful for this section (5.4.4.2) [Gwenaelle GREMION, Canada]	Accepted - text revised
22866	50	9	50	30	The paragraph lacks of any term about the confidence or likelihood of the assertions and including them would be very helpful for the readers. [Gwenaelle GREMION, Canada]	Taken into account

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
22868	50	10	50	14	With the current structure it is unclear, which mechanisms would decrease POC export (e.g less calcifiers, higher respiration rates) and which increase it (altered CN stoichiometry) without backgroundn knowledge. [Gwenaelle GREMION, Canada]	Rejected - how alterations to (for example) the community structure may affect POC export is currently unclear, hence the first sentence in this paragraph. Where the direction of effects has more certainty (e.g. reduction in ballasting), this is specified in the text already.
42488	50	11	50	11	Neglects microbial communities, remineralization could be considered mostly a microbial process [Peter Croot, Ireland]	Accepted - text revised
22870	50	11	50	12	Trophic mismatches because of changes in the timing (phenology) of phytoplankton blooms are missing here:  e.g. Edwards, M., & Richardson, A. J. (2004). Impact of climate change on marine pelagic phenology and trophic mismatch. Nature, 430(7002), 881.  Thackeray, S. J. (2012). Mismatch revisited: what is trophic mismatching from the perspective of the plankton?. Journal of Plankton Research, 34(12), 1001-1010. [Gwenaelle GREMION, Canada]	Rejected - relevance to efficiency of the soft tissue pump not clear.
47102	50	12	50	12	5.4.4.2.:What does export mean in this context? Export out of the ocean or down to ground? [Sophie von Fromm, Germany]	Accepted - text revised
17638	50	13	50	13	Don't use 'etc', give all details [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - space limitations preclude giving all the details
22872	50	13	50	17	Fu et al (2016) do not discuss deoxygenation (beyond commenting that the impact of e.g. oxygen minimum zones isn't modelled but may have an impact). [Gwenaelle GREMION, Canada]	Accepted - references changed
22874	50	13	50	17	I think this sentence should read "carbon pump" rather than "carbonate pump", as it isn't specifically about calcium carbonate. [Gwenaelle GREMION, Canada]	Accepted - text revised
7502	50	15	50	15	If ballast here means to provide stability or structure, then you can drop the quotes. [Rose Abramoff, France]	Taken into account - text revised
22876	50	16	50	17	It is unclear how deoxygenation and warming affects communities and how the communities affect the POC export. [Gwenaelle GREMION, Canada]	Accepted - text and reference added
22878	50	17	50	23	The discussion of an increased POC export under RCP8.5. is inconsistent with the decline of POC export by 1-12% mentioned above. A short statement, why the simulation by Matear and Lenton expect an increase would help to make the structure more clear. [Gwenaelle GREMION, Canada]	Accepted - text revised
13780	50	22	50	22	This should read "particulate INORGANIC carbon (PIC)" [Pierre Regnier, Belgium]	Accepted - text revised
46046	50	22	50	22	"organic" should be "inorganic"? [Kaoru Tachiiri, Japan]	Accepted - text revised
17640	50	24	50	24	Insert space between number and units (38 ppm) [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
22880	50	26	50	30	A potentially relevant recent model improvement in this area is the incorporation of trait-based plankton ecosystem dynamics into the EMIC cGENIE (ecoGENIE; Ward et al, 2018; Geosci. Model Develop.; doi:10.5194/gmd), which allows shifts in plankton community structure and stoichiometry to occur in response to climate change. [Gwenaelle GREMION, Canada]	Rejected - if papers are published using this approach to predict future plankton dynamics before the AR6 is finalised, they will be included
57778	50	35	50	35	Rephrase the sentence : "This section analyses the future projections of land and ocean carbon sinks, and of atmospheric CO2, from the 35 latest Earth System Models (ESMs)." The report assesses the literature not analyzes [Elena Shevliakova, United States of America]	Accepted. Text revised to "This section summarises future projections..."
57780	50	35	50	54	It would be good to highlight how CMIP6 experimental design is different from CMIP5 in respect to carbon cycling projections, including scenarios, C4MIP and LUMIP simulations [Elena Shevliakova, United States of America]	Noted.
17642	50	36	50	36	Earth System Models' is already defined, although your aim here may be to remind the reader what ESMs mean [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Yes, that was indeed the intention.
22882	50	43	50	45	Friedlingstein et al (2006) state that eleven rather than six models were compared in C4MIP (seven OAGCMs and four EMICs). [Gwenaelle GREMION, Canada]	Noted, but this sub-section is specifically about the Earth System Models used in the projection chapter. We have made it clearer in our next revision.
22884	50	44	50	45	Could expand on the uncertainties that have been found [Gwenaelle GREMION, Canada]	Agreed. We mention the key uncertainties (CO2 fertilization of photosynthesis, climate effects on soil respiration and forest dieback, carbon uptake in the southern ocean).
47350	50	44			C4MIP had 11 models (7 ESMs and 4 EMICs) [chris jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
22886	50	48	50	49	I believe this citation should be Friedlingstein et al (2014b), as there are two Friedlingstein et al (2014) papers. [Gwenaelle GREMION, Canada]	Agreed - figure label corrected.
26512	50	53	50	53	will there actually be results from CMIP6 included in the IPCC AR6? Is it not too late for that, as probably new text would have to be added concerning their performance, while at the same time the review of AR6 is already done? Also, several model groups are not ready yet and that might lead to a smaller model spread just because there are less models available (hence misleading the reader) [Nadine Goris, Norway]	Noted. We have included results from CMIP6 models
6834	50	54	50	54	How were these quantified as improvements? This could maybe more accurately read as "along with many other added processes". [Nicholas Smith, United States of America]	Agreed - reworded
57782	51	4	51	7	It may be useful to split table 5.4 into two parts, one describing ocean components and another describing land. Right now it only has names of land and ocean components and 2 columns fro land on N and LU. Does not say anything about ocean features or other land features, e.g. permafrost or vegetating dynamics. [Elena Shevliakova, United States of America]	Agreed. Table revised for CMIP6.
41830	51	6	51	6	check references : not all are in the reference list (Dufresnes a.o.) [Marc Aubinet, Belgium]	Taken into account



Comment ID	From Page	From Line	To Page	To Line	Comment	Response
27766	51	6	51	7	improve the resolution of the table [Poot Delgado Carlos Antonio, Mexico]	Accepted. Table has been updated and improved to describe CMIP6 models.
17644	51	6	51	7	Table text is fuzzy and difficult to read [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Table has been updated and improved to describe CMIP6 models.
36390	51	13			This section should cross-reference Section 3.6 which includes evaluation of model biogeochemistry. [Nathan Gillett, Canada]	Agreed - cross-refer to Section 3.6.
57784	51	15	51	21	Section 5.4.5.1 needs to assess the literature beyond ILAMB on the evaluation of CMIP6 ESMs, which is not available yet. However, there are numerous papers on evaluation of CMIP5 ESMs which are not covered in this section - I think that literature has to be assessed. ILAMB does not do seasonal evaluation - is there a plan to identify and assess ESMs seasonal features? [Elena Shevliakova, United States of America]	Agreed.
36392	51	15		16	I would suggest that just comparing to a wide variety of observational benchmarks isn't enough to have confidence in a model's projections. Which benchmarks are relevant depends on the variable whose projections are being considered - fitness for purpose is important. And for the comparison to be useful in informing projections, ideally some relationship between the projection and an observable quantity should have been demonstrated. If there is no correlation between e.g. projected ocean carbon uptake by 2100 and some observable metric across models, then just comparing that metric with observations won't change the projection or increase confidence in it. [Nathan Gillett, Canada]	Noted - indeed this is why we focus specifically on emergent constraints (which are based-on observables which are correlated to future projections).
29374	51	17	51	17	In addition to Friedlingstein et al., 2003, 2006, and 2014b, Arora et al. 2013, J. Clim. should be referred here since this is just the work directly analyzing carbon cycle feedbacks in CMIP5 ESMs. [Tomohiro Hajima, Japan]	Agreed.
29376	51	17	51	19	About "Land models within ESMs should be compared to multiple different datasets of processes": Hajima et al. 2014 J. Clim. claim the necessity to constrain multiple processes of land carbon cycle to increasing CO2, like "we demonstrated that CO2 increases stimulate several carbon cycle processes (such as plant production, litter fall, and heterotrophic respiration), and the degree of the responses are different among the models." This conclusion is visualized in Fig.3 of their work. [Tomohiro Hajima, Japan]	Noted.
41832	51	18	51	18	do you mean "leaf area"? [Marc Aubinet, Belgium]	Accepted - text revised
46048	51	18	51	18	"leaf are" should be "leaf area" [Kaoru Tachiiri, Japan]	Accepted - text revised
8932	51	18	51	18	Something wrong with sentence "physical predictions such as leaf are and carbon" [Benjamin Lamptey, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
17646	51	18	51	18	Change 'are' to 'area' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
32752	51	18	51	18	Change to "leaf area" [Michael MacCracken, United States of America]	Accepted - text revised
17648	51	21	51	21	Define ILAMB [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Agreed - iLAMB described.
13656	51				Table 5.4 outdated CMIP5 description [Lena Boysen, Germany]	Accepted. Table has been updated and improved to describe CMIP6 models.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
22888	52	1	52	6	The figure is not explained in detail and not integrated into the text well. What are the scores and how were they obtained? [Gwenaelle GREMION, Canada]	Accepted. Additional text has been added to explain the significance of this figure.
57786	52	1	52	56	Is there an evaluation of ESMS ability to predict atmospheric CO2 features ( e.g. seasonal amplitude in tropics vs. boreal zone, latitudinal gradients) in addition to annual mean concentration from FF emissions? And the climate itself? Right now figures and placeholders are only for ocean and land sinks and for mean CO2 and only for RCP8.5, What about other SSPs? [Elena Shevliakova, United States of America]	Noted. The seasonal and interannual variability of CO2 is covered in subsection 5.4.6 on emergent constraints.
27768	52	2	52	2	attend the placeholders [Poot Delgado Carlos Antonio, Mexico]	Accepted. Placeholders have been attended to.
22890	52	2	52	6	The [[Placeholder]] is reasonable but indeed provisional. The "Soil carbon" scores shown in the figure and on the ILAMB website, both for HWSD and NCSCDV2, are worse for "Mean CMIP6" than "Mean CMIP5", which might be due to artefacts and/or missing values in three CMIP6, but would deserve discussion if it is still true once more final data are available. [Gwenaelle GREMION, Canada]	Noted.
22892	52	11	52	11	In Figure 5.21, a clearer division line between the CMIP5 left-hand side and CMIP6 right-hand side would be useful - at the moment it's hard to pick out the division between them. [Gwenaelle GREMION, Canada]	Noted.
22894	52	11	52	16	I don't understand what is meant by left and right hand side of the table. Also, the meaning of grey squares should be in the figure legend. [Gwenaelle GREMION, Canada]	Noted.
27770	52	13	52	13	attend the placeholders [Poot Delgado Carlos Antonio, Mexico]	Accepted. Placeholders have been attended to.
26514	52	20	52	21	This gives a wrong impression of ESMS performing well for the historical ocean carbon sink. I would prefer: "ocean carbon cycle models reproduce the ANNUAL MEAN historical carbon uptake well, with a ...". Also, please add something like "Despite this agreement for the average ocean carbon uptake, there is little agreement in the annual cycles of the ocean carbon uptake, especially in the high latitude North Atlantic and the Southern Ocean (Mongwe et al., 2018, Goris et al., 2018), pointing towards the fact that some fundamental mechanisms are misrepresented in these models." [Nadine Goris, Norway]	Noted. However, evaluation of the zonal variation in the carbon sink is covered elsewhere in Section 5.4.
36672	52	20	52	23	The content looks not naturally connected with the above-mentioned paragraph, which is mainly focused on the benchmarking of CMIP land models. [Jiafu Mao, United States of America]	Noted.
25670	52	20	52	23	The apparent spread in the land models in Figure 5-23 of about 200 Pg is on its face about 77% of the increase to date. I suggest this be pointed out. [Stephen E Schwartz, United States of America]	Noted.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
6491	52	20	52	29	As I understand it the GCP ocean carbon uptake estimate is based on ocean model calculations, so this is -- to some degree -- a model/model comparison. For a comparison of CMIP5 model ocean carbon uptake to primary observations from Sabine and Khatiwala, Bronselaer et al (2017; GRL; "Agreement of CMIP5 Simulated and Observed Ocean Anthropogenic CO2 Uptake") could be referenced instead or in addition. [Michael Winton, United States of America]	Noted. I GCP estimates are based-on a combination of data and models. We consider this the best internally consistent estimate of changes in atmosphere, ocean and land carbon.
29418	52	21	52	21	"current day" --> which year(s) is that precisely? [Judith Hauck, Germany]	Accepted - "current day" replaced by "2005".
36394	52	22		23	Specify the observational uncertainties. Is 25 PgC the observational uncertainty? The key question is whether or not models are consistent with obs to within the obs uncertainty. [Nathan Gillett, Canada]	Noted.
22896	52	23	52	29	Are the ESM results here from CMIP5 or CMIP6? I assume it will be CMIP6, but might be a placeholder at the moment? [Gwenaelle GREMION, Canada]	Agreed. SOD now includes CMIP6 model results.
22898	52	26	52	31	The lines in the plot are overlapping, hiding some lines in the background. This makes it not possible to see all models. Thinner lines or transparency could help to show all lines. [Gwenaelle GREMION, Canada]	Accepted. Figure has been reworked.
47352	52	26	52	50	I like these two figures - could you consider combining into one figure? (still multi panel) - would be nice to synthesise across land/ocea instead of keeping them separate [chris jones, United Kingdom (of Great Britain and Northern Ireland)]	Agreed.
22900	52	28	52	28	Where is the data in the figure from? [Gwenaelle GREMION, Canada]	Agreed. it is from the Global Carbon Project (GCP). Now spelled-out in the figure caption.
9532	52	34	52	36	It would be good to define how this land carbon storage (quoted in PgC) is defined: i.e. does it include land use change? or is it the net atmosphere-land carbon flux (i.e the 'nbp' CMIP5 variable?) [Katarzyna (Kasia) Tokarska, United Kingdom (of Great Britain and Northern Ireland)]	Noted . Land carbon storage includes the impacts of land-use change (as does the net flux).
22902	52	34	52	36	Might be worth mentioning here the observational estimate of -30 PgC for immediate context. [Gwenaelle GREMION, Canada]	Noted
13600	52	34	52	39	The individual contribution of CO2-fertilization, N deposition, climate change and LULCC for the land-atm carbon flux during the historical period was assessed comprehensively in a single model by Devaraju et al. 2015 Climate Dynamics: DOI 10.1007/s00382-015-2830-8. This study showed that the land was a net source of carbon to the atmosphere during 1850-2005 and the value of the cumulative source was ~45 PgC. There is the only study that has the assessed the individual contributions and should be cited. [Govindasamy Bala, India]	Noted, but we are interested here in assessing results from multiple CMIP5/6 models.
9534	52	34	52	41	Another important challenge that could be mentioned here are difficulties in like for like comparison of CMIP5 models' output with observations -i.e. some output variables sometimes are not directly observed (e.g. the net air-land carbon flux 'nbp' in CMIP5 models cannot be easily compared with the observations), so it is difficult to compare like for like with observations. [Katarzyna (Kasia) Tokarska, United Kingdom (of Great Britain and Northern Ireland)]	Noted.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
25672	52	34	52	41	On its face Fig 5.23 indicates absolutely no skill in land uptake modeling, showing differences even in sign, with a spread of about 200 Pg. The text attributes this to the fact that some models report land uptake and land use change separately and some report the sum. This would seem to be readily corrected so that the figure would show only land uptake, with land use change not included. I would urge the authors to separate these out so that there is not an appearance of such absence of skill in the models. once that gets sorted out I would recommend that the terrestrial sink rate (i.e. not including the land use emissions) be plotted against the excess atmospheric CO2 (mixing ratio, or better, stock) from which slope one can infer a transfer coefficient and whether that is constant with increasing CO2, indicative of a fertilization effect, or decreasing with increasing CO2. This would be value added to the assessment. [Stephen E Schwartz, United States of America]	Noted
9536	52	36	52	39	Another challenge that could be mentioned here is how land use change is defined (Pongratz et al., 2014), and that it is difficult to diagnose it directly from modelled output (in case of CMIP5 models).  Ref: Pongratz, J., Reick, C. H., Houghton, R. A., and House, J. I.: Terminology as a key uncertainty in net land use and land cover change carbon flux estimates, Earth Syst. Dynam., 5, 177-195, <a href="https://doi.org/10.5194/esd-5-177-2014">https://doi.org/10.5194/esd-5-177-2014</a> , 2014 [Katarzyna (Kasia) Tokarska, United Kingdom (of Great Britain and Northern Ireland)]	Noted, but the range shown in Figure 5.23b is the simulated change in land carbon storage. It is not affected by difficulties in diagnosing the land-use change flux.
38168	52	39	52	41	If possible, the transition of uncertainty after C4MIP should be summarized. [Hiroaki Kondo, Japan]	Noted, but the comment is unclear.
7504	52	44	52	55	Figure 5.23 colors of some of the model runs look the same to me (BNU-ESM & MRI-ESM1; CanESM2 & NorESM1-ME); in (b) I do not see the two GFDL ESMs, are they not plotted?; The following sentence has a long, multi-clause subject, I would suggest rearranging this sentence to make it easier to read. [Rose Abramoff, France]	Accepted. Figure has been reworked.
22904	52	53	53	1	Figure 5.24a should read Figure 5.24, as the sentence refers to both panels, and a citation to Figure 5.24a included on pg.53.In.1 after "relatively small model spread" (as that sentence is specifically about panel a). [Gwenaelle GREMION, Canada]	Agreed.
27980	52	55	52	55	Page 52, line 55: "... state-of-the-art atmospheric inversion" I think the wording "state of the art" is a big questionable here. Perhaps the word "modern" could be used instead. Page 53, line 5, 6: Explain a bit more why there is a wide range in the latitudinal distribution of net land carbon uptake. Why NH is higher than tropics? [roderik van de wal, Netherlands]	Accepted. We replace "state-of-the-art" with "recent".
13276	52	55	52	55	Up to this point, four atmospheric inversions have been used and now only one is kept (actually the legend of Fig 5.24 suggests that there are more than one). How was it selected? [Frederic Chevallier, France]	Accepted. We have updated the figure and analysis to use results from 3 different atmospheric inversions.
29420	52	55	52	55	"state-of-the-art atmospheric inversion"; which inversion? A reference would be most helpful [Judith Hauck, Germany]	Accepted. References have been added.
22906	52	56	52	56	"sink" is missing after "mid-latitude CO2". [Gwenaelle GREMION, Canada]	Accepted - text revised

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
22908	53	1	53	2	Why could the carbon sink in the ensemble mean be weaker in the Southern Ocean compared to the inversion estimate? Could it be caused by considering/not considering the effects of decreased light limitations or increased iron input from icebergs? [Gwenaelle GREMION, Canada]	Noted, but we should avoid speculation.
55848	53	4	53	9	Large spread in the latitudinal distribution of land carbon uptake in CMIP5 models may also be due to inaccurate representations of the distribution of forest biomass (Yang et al., 2018). For DGVMs, this may be a result of inaccurate representation of dynamic responses to climate, soils, nutrients, etc. [Yang, Cheng-En, Jiafu Mao, Forrest M. Hoffman, Daniel M. Ricciuto, Joshua S. Fu, Chris D. Jones, and Martin Thurner (2018), Uncertainty quantification of extratropical forest biomass in CMIP5 models over the Northern Hemisphere, Sci. Rep., 8(1), 10962, doi:10.1038/s41598-018-29227-7.] [Forrest Hoffman, United States of America]	Noted, but these errors also exist in models with prescribed vegetation (as is the case for CMIP5 models).
47354	53	4	53	9	discussion of errors in the simulated land sink requires caution - it's not at all clear from the figure that ESMs "overestimate" the sink in the tropics - the plot spans zero and includes the inversion estimate - that's certainly not an overestimate. For the higher latitudes, it's hard to tell if this is ESM underestimate or inversion overestimate - can you at least plot error estimates on the inversion results? Would also be useful to see multi-ESM mean as well as just the range [chris jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - we now include a range of inversion estimates.
22910	53	5	53	6	Perhaps it is not necessary, but the second part of the sentence ("Most ESMs tend to [...] underestimate the northern hemisphere land carbon sink") could be supported by reference to Ciais et al 2019 (doi:10.1038/s41586-019-1078-6) [Gwenaelle GREMION, Canada]	Accepted. Relevant citations have been added to support this statement.
17650	53	5	53	6	For consistency elsewhere, Tropics and Northern Hemisphere should be capitalised [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
28144	53	5	54	6	You might want to cite a recent publication on the underestimation of plant productivity in earth system models based on a satellite-data driven constraint: Winkler, A. J., Myneni, R. B., Alexandrov, G. A. & Brovkin, V. Earth system models underestimate carbon fixation by plants in the high latitudes. Nat. Commun. 10, 885 (2019). [Alexander Winkler, Germany]	Noted.
27772	53	6	53	7	attend the placeholders [Poot Delgado Carlos Antonio, Mexico]	Accepted. Placeholders have been attended to.
45364	53	7	53	8	The comment on N-deposition seems unduly speculative [Peter Rayner, Australia]	Agreed.
47356	53	8			avoid speculation over the cause of any changes. Maybe nutrients, may also be land-use or different climat responses. I don't think we know yet [chris jones, United Kingdom (of Great Britain and Northern Ireland)]	Agreed.
22912	53	14	53	16	It would be good if data labels could be included in panel a) as well to facilitate post-publication sharing of just that single panel. [Gwenaelle GREMION, Canada]	Accepted. Key now included in panel (a).

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
13654	53	21	54	36	What are the references here? None given. Eg. Land carbon changes in RCP.5 see Boysen et al., 2014 [Lena Boysen, Germany]	Accepted. Relevant citations added to support this statement.
23458	53	23	53	23	Will these results be replaced with CMIP6 in due course? And I'm assuming the concentration-driven rcp2p6 and 8p5 runs are currently CMIP5 as well? [Gwenaelle GREMION, Canada]	Agreed. CMIP6 results now included.
22914	53	23	53	25	Will these results be replaced with CMIP6 in due course? And I'm assuming the concentration-driven rcp2p6 and 8p5 runs are currently CMIP5 as well? [Gwenaelle GREMION, Canada]	Agreed. CMIP6 results now included.
22916	53	24	53	24	Either "CMIP5" here should be "CMIP6", or I'm very confused. [Gwenaelle GREMION, Canada]	Noted. "CMIP5" has been updated to "CMIP6" now the latest models are included.
46050	53	25	53	25	What "cleaner comaprison" means is unclear. [Kaoru Tachiiri, Japan]	Noted - the comparison is cleaner in concentration driven runs as in these runs models have the same prescribed evolution of atmospheric CO2.
27774	53	30	53	33	attend the placeholders [Poot Delgado Carlos Antonio, Mexico]	Accepted. Placeholders have been attended to.
25674	53	31	53	44	The spread in the ESM models for atmospheric CO2 at present shown in Fig 5.25a and b is 103 Pg or about 40% of the increase to date. I suggest this be pointed out and that the Assessment voice low confidence in such models at present. Or at least in those outliers. If these models are so erroneous at present what confidence can be placed in their predictive value? [Stephen E Schwartz, United States of America]	Noted.
22918	53	32	53	32	The spread in Figure 5.25a looks more like 300 ppm - does 250 ppm refer to provisional CMIP6 results instead? [Gwenaelle GREMION, Canada]	Agreed. No - the range is not based-on CMIP6. We now ensure that ranges and figures are consistent.
17652	53	38	53	38	Change 'carbon dioxide' to 'CO2' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
22920	53	41	53	41	Figure 5.25 only presents the 2060 emergent relationship, when Hoffman et al (2014) present both a 2060 and 2100 graph. While I understand having an intermediate timeframe emergent constraint is useful (especially as it has less uncertainty), given that the paragraph below discusses emergent relationships in the context of higher CO2 by 2100 and this and other reports mostly use 2100 as the common future timeframe I would have thought 2100 would be the clearer choice here. Is there a reason for presenting the 2060 rather than 2100 emergent relationship (assuming only one can be shown)? [Gwenaelle GREMION, Canada]	Noted. We choose to focus on 2060 because the constraint is stronger than for 2100. The figure caption now corrected to reflect that.
8934	53	47	53	47	The word "concentration" is missing in "Models that tend to have higher CO2 ..." [Benjamin Lamptey, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
17654	53	47	53	47	Delete hyphen [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
47358	53	47	53	51	others have tried to do this as well as Hoffman. Friedlingstein et al (2014, J. Clim) reached a different conclusion about the strength of a constraint and Booth et al (2016, J. Clim) also included uncertainty in the land-use emissions to get a constraint. [chris jones, United Kingdom (of Great Britain and Northern Ireland)]	Noted.
22922	53	48	53	51	I would add a word of caution to this statement, as these correlations between predictions may not hold [Gwenaelle GREMION, Canada]	Noted.
25676	53	49	53	51	Rather than draw correlations between past and future CO2 calculated with erroneous models and call them an emergent constraint, reject the models that are shown to be erroneous. Emergent constraints are useful only when the models cannot be rejected as erroneous, for example the emergent constraint between sensitivity and forcing. [Stephen E Schwartz, United States of America]	Noted, but rejected. Emergent constraints use the ensemble of models to relate contemporary observations to future projections. They are a good example of the whole being more than the sum of the parts with regard to multi-model ensembles.
36396	53	53			Delete 'primarily'. [Nathan Gillett, Canada]	Agreed.
22924	53	54	53	56	Fig 5.26 is not easy to follow. Is b) derived from the rate in a)? Is net uptake meant? Same in 5.27 [Gwenaelle GREMION, Canada]	Noted - these are future projections equivalent to the historical projections shown in Figures 5.22 and 5.23
22926	53	55	53	55	"(left panel)" could be "Figure 5.26.a" and "(right panel)" could be "Figure 5.26.b" [Gwenaelle GREMION, Canada]	Accepted - the way these panels are referred to changed as suggested.
22928	53	56	54	1	This sentence could be removed as this information is already in Figure 5.26 caption. [Gwenaelle GREMION, Canada]	Accepted - text revised
47360	54	4	54	29	as earlier - these are nice figures. You could consider combining so land and ocean are treated together [chris jones, United Kingdom (of Great Britain and Northern Ireland)]	Agreed.
47362	54	4	54	29	chapters are being asked to be consistent on use of scenarios, so please show all 5 of SSPs: 1-1.9, 1-2.6, 2-4.5, 3-7.0,5-8.5 [chris jones, United Kingdom (of Great Britain and Northern Ireland)]	Noted, but rejected. Showing all 5 SSPs is unlikely to add any additional insight and will make figures much less readable.
9538	54	6	54	9	How is +/- 1 standard deviation defined? Is it based on the CMIP5 models spread, and from how many models? (a bit more detail regarding this and a list of models used would be useful in Supplementary material) [Katarzyna (Kasia) Tokarska, United Kingdom (of Great Britain and Northern Ireland)]	Noted.
9540	54	6	54	9	Perhaps the fact that radiative forcings (and especially aerosol forcing) is much different in RCP 2.6 compared to 8.5 should be mentioned, since some effects may be cancelling out for the wrong reasons. Perhaps a cleaner way would be to also show it for CO2-only simulations with no aerosols? [Katarzyna (Kasia) Tokarska, United Kingdom (of Great Britain and Northern Ireland)]	Noted, but showing results from all model runs is not possible given the space constraints.
22930	54	6	54	9	Why does Figure 5.26 use 2090 as the final timeframe instead of 2100? As per my previous comment, it seems like a good idea to maintain a common timeframe of 2100 where possible in order to facilitate cross-comparisons, unless there's a clear reason not to. [Gwenaelle GREMION, Canada]	Noted.
22932	54	6	54	9	As with Figure 5.24, it'd be useful to have data labels in both panels of Figure 5.26 to facilitate individual sharing and re-use post-publication. [Gwenaelle GREMION, Canada]	Agreed.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
41686	54	10	54	20	<p>The lead in sentence for the TCRE is rather weak referring to “are governed by complex mechanisms that evolve in time (Gregory et al., 2009).” There is now a more precise understanding of the controlling mechanisms, which is not fully conveyed in the present text. The below insert provides a more up to date mechanistic viewpoint.</p> <p>Surface warming may be directly connected to carbon emissions via a single theoretically-derived equation (Goodwin et al. 2015, Williams et al, 2016), where the increase in global-mean surface temperature is connected to the carbon emissions via a product of terms depending upon a thermal response, carbon response and a non-CO2 radiative response. The thermal response dependence on climate sensitivity and ocean heat uptake (Froelicher et al., 2014). The carbon response may be related to the air-borne fraction (Matthews et al., 2009) and the ratio of the ocean saturated and atmospheric carbon inventories (Katavouta et al., 2018).</p> <p>References                      Froelicher, T. L., M. Winton, and J. L. Sarmiento (2014), Continued global warming after CO2 emissions stoppage, Nature Climate Change, 4, 40–44, doi:10.1038/nclimate2060.                      Goodwin, P., Williams, R. G. &amp; Ridgwell, A. Sensitivity of climate to cumulative carbon emissions due to compensation of ocean heat and carbon uptake. Nat. Geosci. 8, 29–34 (2015).                      Katavouta, A., R.G. Williams, P. Goodwin and V. Roussenov, 2018. Reconciling atmospheric and oceanic views of the Transient Climate Response to Emissions. Geophysical Research Letters, 45, 6205-6214, doi.org/10.1029/2018GL077849.                      Matthews, H. D., Gillet, N. P., Stott, P. A. &amp; Zickfield, K. The proportionality of global warming to cumulative carbon emissions. Nature 459, 829–832 (2009).</p>	Noted. We extend the discussion of TCRE where we can.
22934	54	14	54	19	<p>The paragraph lacks of any term about the confidence or likelihood of the assertions and including them would be very helpful for the readers.</p> <p>[Gwenaelle GREMION, Canada]</p>	Accepted. Confidence and likelihood statements now included in Section 5.4.
27776	54	16	54	16	<p>attend the placeholders [Poot Delgado Carlos Antonio, Mexico]</p>	Accepted. Placeholders attended to.
47364	54	16			<p>this might be redundant if you use updated SSP scenarios, but I don't recall a CO2 stabilisation under RCP8.5 - can you clarify what you mean here? (stabilised _emissions_ perhaps?) [chris jones, United Kingdom (of Great Britain and Northern Ireland)]</p>	Agreed. Text reworded.
36398	54	17			<p>Usually 'saturation' of a sink means that the sink is not taking up any more carbon - for example 'saturation' of the land carbon sink in future simulations usually means this. But in this sentences the meaning is just that the flux into the ocean is constant. I suggest using a word other than 'saturation' for this. [Nathan Gillett, Canada]</p>	Agreed. Text reworded.
22936	54	24	54	27	<p>As with Figure 5.26, I think it'd be useful to justify why Figure 5.27 uses 2090 rather than 2100 as the endpoint, and to have data labels in both panels to enable post-publication figure usage. [Gwenaelle GREMION, Canada]</p>	Noted.
41834	54	32	54	32	<p>"once" again [Marc Aubinet, Belgium]</p>	Accepted - text revised [text modified according to comment 9680]



Comment ID	From Page	From Line	To Page	To Line	Comment	Response
9680	54	32	54	32	Change "One again, we see a much larger range" to "As discussed above (REFER TO SECTIONS?), there is much more uncertainty" [Brian Magi, United States of America]	Accepted - text revised
13602	54	32	54	36	The individual contribution of CO2-fertilization, N deposition, climate change and LULCC for the land-atm carbon flux for three future scenarios was assessed comprehensively in a single model by Tharammal et al. 2018 Climate Dynamics: <a href="https://doi.org/10.1007/s00382-018-4388-8">https://doi.org/10.1007/s00382-018-4388-8</a> . This study showed that land is a source of carbon in the RC8.5 and RCP2.6 scenarios mainly because of LULCC and climate change, but afforestation and CO2 fertilization in the RCP4.5 scenario facilitate the land to be a sink. There is the only study that has the assessed the individual contributions and should be cited. [Govindasamy Bala, India]	Noted, but we are interested here in assessing results from multiple CMIP5/6 models.
13652	54	32			"Once again" (not One again) [Lena Boysen, Germany]	Accepted - text revised [text modified according to comment 9680]
36400	54	33		34	Are these 5-95% ranges? If not, I suggest quoting 5-95% ranges. [Nathan Gillett, Canada]	Noted - these are standard deviations across the models.
46870	54	39	54	39	I think a more comprehensive assessment of the C feedbacks might be useful here. First, several papers indicate (e.g. Humphrey et al. 2018) that tracking warming is not sufficient to track climate impact of C sinks. Then several other papers such as (Schwinger et al. 2014 and Scwinger et al. 2018) suggests that there are non-linear terms that are not taken into account in this framework. While I support it use for tracking model generation (AR5 and now AR6) I think further details and discussion about the use of this framework migh be relevant here. Besides, further discussion might be required when comparing Beta and gamme with offline estimates (Huntzinger et al., 2017) or beta and gamme estimates from CDR and SRM experiments (see Plazzotta et al., 2019 and Schwinger et al. 2018) [Roland Séférian, France]	Noted.
47366	54	39			this section feels a bit out of place here - youalready described individual beta/gamm processes in 5.4.1/2/3/4. then you go into projections. Then back to feedbacks would 5.4.5.3 be better earlier? [chris jones, United Kingdom (of Great Britain and Northern Ireland)]	Noted , but this structure has been arrived at after much discussion. We do not want to waste more time restructuring again.
47368	54	39			this whole section seems to me to have potential for more detail - would be nice to dive into the details which have driven the changes in feedbacks - esp. on land if more models have a N-cycle. [chris jones, United Kingdom (of Great Britain and Northern Ireland)]	Noted - but that requires analysis of model results that are not yet available.
38170	54	41	54	45	Are $\Delta CL$ and $\Delta T$ global average (or total)? Give clearer definition. [Hiroaki Kondo, Japan]	Agreed.
13604	54	41	54	49	The papers Devaraju et al. 2015 (Climate Dynamics) and Tharammal et al. 2018 (Climate Dynamics) also include a term that corresponds to changes in N deposition rate in the equation for the terrestrial carbon stock chage. The sensitivity to N deposition was represented by the parameter $\delta$ . Although this additional term can be estimated in only a few models today (as most models lack N cycle), this point could be discussed here. As shown in these papers, the magnitude of the individual effect of N deposition is of the same as climate change. [Govindasamy Bala, India]	Noted.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
9682	54	41	54	49	I think the equation needs to either be written more generally, or a separate equation written for the linear decomposition of the Change in Land Carbon Storage and the Change in Ocean Carbon Storage. Currently, deltaC_O is declared but not defined. Similarly, Beta_O and Gamma_O are only described in Table 5.5 [Brian Magi, United States of America]	Accepted. Land and ocean equation now both listed, and the meanings of the symbols is more clearly defined.
57788	54	41	55	3	background material on linear feedback analysis should be in a box, it's not an assessment [Elena Shevliakova, United States of America]	Noted. This was discussed at the LA2 meeting, where it was decided that (a) that the linear feedback analysis needs to be explained in the chapter (as it is the basis for our assessment here), and (b) it wasn't necessary to put this in a box.
13862	54	41	55	18	I wish I had more time to read this entire chapter but for now I am only able to provide comments on section 5.4.5.3 (Linear feedbacks analysis). This section is fairly smaller than what we had in AR5's Chapter 6. AR5's section 6.4.2 was fairly substantive. We are currently working to write a paper summarizing carbon feedbacks using results from CMIP6 models. We hope to be able to submit our paper by 31 Oct, 2019 (two months before the Dec 31, 2019 deadline). This section (5.4.5.3) should refer to the AR5's section 6.4.2. (it doesn't do this, at the moment) since there is no point in repeating all that info but I feel it should still bring out the basics a bit more. I realize it's been difficult for authors this time around to write chapters with no new CMIP6 results. The carbon feedbacks analysis for CMIP6 runs is based on 1pctCO2 runs, just like it was for AR5 making the comparison much cleaner. This section (5.4.5.3) also says that the feedback values will be available for global but also tropical and high northern latitudinal bands. We are working on global values only so at least our analysis will not provide feedbacks values selected latitudinal bands from CMIP6 models. I am willing to contribute to help expand this section. I am not sure what the process is to do this. Perhaps, I need to contact the lead authors. I would have pasted some new results but this Excel spreadsheet doesn't allow to do that. [Vivek Arora, Canada]	Noted.
25678	54	46	54	46	It is not even clear whether the equation refers to stock or flux. One would hope flux, because that would seem to be the quantity that would be affected by changing temperature or the like. Unfortunately the table at page 55, line 17 suggests it is stock. [Stephen E Schwartz, United States of America]	Noted.
22938	54	46	54	46	The equation presented is only the land version - either the ocean version (as prepared for with ocean carbon storage defined in pg.54.ln.44) should be included as well, or the equation should be made generic and not specific to either land or ocean. [Gwenaëlle GREMION, Canada]	Agreed.
41684	54	46	54	46	Add equivalent equation for the ocean or rephrase text. [Ric Williams, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Land and ocean equation now both listed, and the meanings of the symbols is more clearly defined.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
26516	54	51	54	51	It is also know that the ocean carbon cycle feedbacks are non-linear and that should be mentioned here (Schwinger et al., 2014; DOI: 10.1175/JCLI-D-13-00452.1) [Nadine Goris, Norway]	Noted.
29782	54	53	54	53	The values of beta and gamma has been shown to vary between transient equilibrium simulations. The equilibrium values are much larger than transient values: G. Bala, Sujith Krishna, Devaraju Narayanappa, Long Cao, K. Caldeira, R. Nemani, 2012: An estimate of equilibrium sensitivity of terrestrial carbon cycle using NCAR CCSM4, Climate Dynamics, DOI 10.1007/s00382-012-1495-9 [Govindasamy Bala, India]	Noted, but we are interested here in assessing results from multiple CMIP5/6 models.
29378	54	54	54	54	About "the confounding effect of the scenario dependence Beta and Gamma": For scenario dependency of Beta (dependency of Beta on the rate of atmospheric CO2 increase), Hajima et al. (2014) J. Clim. clearly show the dependency in their Fig. 7, leading a conclusion that "a time lag of terrestrial carbon with atmospheric CO2 increase is the crucial process for the large inter-scenario spread of concentration-carbon feedback; a high rate of CO2 increase makes the terrestrial carbon amount much less than the equilibrium carbon amount for a given CO2 concentration." [Tomohiro Hajima, Japan]	Noted. We choose to focus on 2060 because the constraint is stronger than for 2100. The figure caption now corrected to reflect that.
46052	55	1	55	3	In AR5 (or Arora et al., 2013), gamma was calculated by radiatively coupled and full coupled runs, wasn't it? [Kaoru Tachiiri, Japan]	Noted - yes.
13606	55	5	55	7	Bala et al. 2012 Climate Dynamics (DOI 10.1007/s00382-012-1495-9) provided a similar list of $\beta$ and $\gamma$ vaues in their table 2. The interesting conclusion in this paper is that the equilibrium values are much larger than the transient values. [Govindasamy Bala, India]	Noted.
9542	55	6	55	8	is it subject to assumptions that the feedbacks are not state-dependent (i.e do not change at higher levels of warming)? [Katarzyna (Kasia) Tokarska, United Kingdom (of Great Britain and Northern Ireland)]	Agreed.
9684	55	7	55	8	Best to actually describe what is meant by how emergent constraints affect these sensitivities, or refer to a specific sub-section (5.4.6?). It is painfully vague as a stand-alone statement. [Brian Magi, United States of America]	Accepted. Reference to section 5.4.6 added - "Emergent constraints have been suggested for both of these sensitivities (see Section 5.4.6)".
25680	55	17	55	17	Suggest not mix units Pg and ppm. Suggest stay with Pg throughout. [Stephen E Schwartz, United States of America]	Noted. However, ppm is a much more meaningful unit for CO2 for the non-expert.
25682	55	17	55	17	Would be much more valuable to see the dependence of uptake rate on CO2 or Temperature.The stock in the ocean or terrestrial biosphere is the integral of the prior uptake rate, which would have occurred at lower CO2 or temperature, muddying any dependence on those quantities. But even better, define transfer coefficient as uptake rate divided by atmospheric stock and examine transfer coefficient (an intensive variable) as a function of temperature or CO2. [Stephen E Schwartz, United States of America]	Noted.
45366	55	22	56	30	Papers constraining parameters in simplified models using observations (e.g. Bodman et al., doi:10.1038/NCLIMATE1903) also belong here [Peter Rayner, Australia]	Noted - but that is distinct from the concept of emergent constraints.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
32754	55	23	55	23	I'm surprised there is no discussion of the permafrost situation, where thawing, and presumably release of CO2 and/or CH4, is increasing far faster than model simulations, at least as I understand the situation. [I see that CH4 aspects of permafrost are discussed on page 57, and CO2 on page 59--perhaps reference ahead to these discussions] I'm also curious about what will happen when emissions go to zero and what effects this may have. [Michael MacCracken, United States of America]	Noted - permafrost feedback are indeed covered elsewhere in the chapter.
9686	55	23	56	28	I am unclear what the overall importance of this sub-section is. Many times, it seems like the text is going to clarify what an emergent constraint is, but then nothing is specified. For example, explain WHY snow-albedo feedback is an "archetypal" example. I really do not understand what the goal of this sub-section is, and I think it needs a careful re-write with specific goals and/or quantifiable evidence clearly stated. For example, can the strength of feedback be defined in a modeling scenario with and without an emergent constraint? Another example, is it even possible to separate out the magnitude of an emergent constraint if it is emergent, or is a modeling exercise simply going to contain this limitation inherently? [Brian Magi, United States of America]	Noted, but this is covered in section 1.4.5.2 of Chapter 1.
47370	55	23			see Goris et al (2018, J.Clim) for another emergent constraint approach - this one for N Atlantic carbon uptake [Chris Jones, United Kingdom (of Great Britain and Northern Ireland)]	Agreed.
36402	55	23			This paragraph is missing an overall assessment statement. [Nathan Gillett, Canada]	Agreed.
9544	55	25	55	33	In addition to a reference to another chapter, I think an essential point to mention here is that for an emergent constraint to be robust, there needs to be an explanation of the possible mechanisms, rather than just a correlation between the two variables (e.g. Caldwell et al., 2018). Also, it would be better to evaluate the emergent constraint/relationship against more than one observational data set.  (Ref. Caldwell, P.M., M.D. Zelinka, and S.A. Klein, 2018: Evaluating Emergent Constraints on Equilibrium Climate Sensitivity. J. Climate, 31, 3921–3942, <a href="https://doi.org/10.1175/JCLI-D-17-0631.1">https://doi.org/10.1175/JCLI-D-17-0631.1</a> ) [Katarzyna (Kasia) Tokarska, United Kingdom (of Great Britain and Northern Ireland)]	Noted, but this is covered in section 1.4.5.2 of Chapter 1.
22940	55	29	55	32	I'm unable to confirm if these are definitely the first coining and archetypal usage of emergent constraints, but the citations are as referenced. [Gwenaëlle GREMION, Canada]	Noted.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
57790	55	30	55	30	the archetypal example of an emergent constraint on snow-albedo feedback (Hall and Qu, 2006) has been disputed as more observation become available. See Xiao et al 2017 Remote Sens. 2017, 9(9), 883; <a href="https://doi.org/10.3390/rs9090883">https://doi.org/10.3390/rs9090883</a> . [Elena Shevliakova, United States of America]	Noted. However, a change in the observational constraint does not undermine the emergent relationship across models, which in this case has been seen in both CMIP5 and CMIP6. Caveat added to explain that the constraint depends both on the emergent relationship across models and the observational constraint - if either changes the emergent constraint will change.
27778	55	33	55	33	verify this paragraph [Poot Delgado Carlos Antonio, Mexico]	Noted, but not sure what the reviewer means here?
47852	55	33	55	33	The correct section in chapter 1 is 1.4.5.2. [WGI TSU, France]	Agreed - we now point to 1.4.5.2
28146	55	33			You might want to cite a recent study on the applicability of emergent constraints in the context of a carbon cycle case study: Winkler, A. J., Myrneni, R. B. & Brovkin, V. Investigating the Applicability of Emergent Constraints, Earth System Dynamics Discussions, 1-33 [Alexander Winkler, Germany]	Agreed.
22942	55	35	55	41	The concept of emergent constraints could be explained in detail here. What does it mean, what is the significance for predictions etc. The example in l. 35 onwards is not that clear. How can emergent constraint be used to improve future projections? [Gwenaelle GREMION, Canada]	Noted, but this is covered in section 1.4.5.2 of Chapter 1.
28148	55	35	55	41	You differentiate between different types of emergent constraints. You write that the simplest type is based on an emergent relationship of simulated values in time (present vs. future) of an observable variable (here CO2 concentration). You miss to clearly mention the more complex types of emergent constraint. The next level of complexity is to establish an emergent linear relationship between two variables, where only one is observable. The non-observable is the entity of interest which can be of the present state of the system or at a potential future state (e.g. Wenzel et al., 2016: observable CO2 amplitude is a predictor of change in non-observable GPP, or Winkler et al. 2019: observable LAI sensitivity to CO2 is a predictor of changes in GPP). In summary, there are three levels of complexity for emergent constraints. First, the emergent relationship can be established in time for one observable variable, or between two causally linked variables (predictor-predictand relationship), or both. [Alexander Winkler, Germany]	Noted, but this is covered in section 1.4.5.2 of Chapter 1.
9546	55	40	55	41	Is it subject to assumptions that feedbacks are not state-dependent (i.e do not change at higher levels of warming)? If so, perhaps this could be mentioned as a caveat. [Katarzyna (Kasia) Tokarska, United Kingdom (of Great Britain and Northern Ireland)]	Agreed.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
28150	55	43	56	15	The authors describe the different approaches of how to determine a meaningful predictor, i.e. short-term variability as predictor for long-term sensitivity, changes in the seasonal cycle as predictor of longterm changes. The robustness of the linkage between the predictor and predictand is generally a critical issue. Predictor and predictand must have a physically or physiologically connection - this aspect is discussed in length in literature (e.g. Hall et al., Nature, 2019, doi:10.1038/S41558-019-0436-6). Another aspect concerns the logical linkage which is discussed in Winkler et al., (ESSD, 2019, doi:10.5194/ESD-2018-71). For instance, Wenzel et al. (2016) established a linear relationship between relative changes in the predictand taking the initial state into account (changes in GPP for doubling of CO2 relative to the initial pre-industrial state), and a predictor neglecting the initial state (historical sensitivity of CO2 amplitude to rising CO2). This statistical relationship can be spurious, because the model skill of simulating an accurate initial state and a plausible sensitivity to a forcing are not connected. Hence, this predictor-predictand set lacks a logical connection (Figure 5.28c). I suggest to include a statement about the caveats of emergent constraints in general and maybe in particular about Wenzel et al. (2016) or exclude the Figure (Figure 5.28c). [Alexander Winkler, Germany]	Noted, but this is covered in section 1.4.5.2 of Chapter 1.
22944	55	44	55	44	Reference to the theorem doesn't does not add meaningful content if it is not explained further [Gwenaelle GREMION, Canada]	Noted, but this is why we include the Leith (1975) reference.
26518	56	11	56	11	please add Goris et al., 2018 [Nadine Goris, Norway]	Noted.
22946	56	11	56	11	Could mention here Piao et al 2008 Net carbon dioxide losses of northern ecosystems in response to autumn warming Nature letters. [Gwenaelle GREMION, Canada]	Noted.
13610	56	12	56	15	The text and what is shown in Figure 5.28c are not consistent. The y-axis label in Fig. 5.28c should be $GPP(2xCO_2)/GPP(1xCO_2)$ [Govindasamy Bala, India]	Accepted. Y-axis label of Figure 5.28c has been corrected.
31298	56	12	56	15	It is not mentioned here, but it should be, that there is an open question about this (Wenzel et al.) "emergent constraint" on the CO2 fertilization effect – because Wenzel et al. found that the "true" effect lies within the range of CMIP5 models whereas previous work by Graven et al. (2013, Science) had shown that the observed seasonal cycle amplification is nowhere near to being simulated by any of these models. [Iain Colin Prentice, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Graven et al. (2013) looked at snapshots at altitude, whereas

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
7506	56	18	56	28	Typo: In the Figure 5.28 figure description, it should read 2060 not 2100; The y-axis for (c) should have (betaLM) in the axis label like in (b) [Rose Abramoff, France]	Accepted - Figure caption corrected from "2100" to "2060". Noted, but it is the figure caption that is in error - this should read: "(c) sensitivity of Gross Primary Production (GPP) to a doubling of atmospheric CO2 against the sensitivity of the amplitude of the CO2 seasonal cycle at Point Barrow, Alaska to a change in global mean atmospheric CO2 concentration".
22948	56	18	56	28	Panel a) the y-axis label (2060) doesn't correspond to the description in the caption (2100). Readability: panel a) the "Obs constraint" legend could be moved so it is less ambiguous whether it refers to the dashed line, red dot or both; panels a-b-c) the caption does not describe what the dots represent; panels a-b-c) the ellipses and associated numbers are not defined in the figure caption, and the numbers tend to overlap making them hard to read, giving a colour-coded key of the ellipses with corresponding numbers could improve readability. [Gwenaelle GREMION, Canada]	Accepted - figure caption corrected from "2100" to "2060". Agreed - "Obs constraint" removed from all panels. Agreed - figure caption made more descriptive. Agreed - numbers removed from contours and number of contours reduced to one (to show +/- 1 stdev).
13608	56	20	56	26	The caption for panel a) is not consistent with what is shown in the Figure. The caption says 2100 concentration but the figure shows 2060 values. The x-axis is also not consistent between the caption and figure. [Govindasamy Bala, India]	Accepted - Figure caption corrected from "2100" to "2060".
22950	56	20	56	26	In the caption of Figure 5.28 providing a quick definition of gamma and beta factors for b) and c) (using the y-axis definitions for example) would help with clarity. [Gwenaelle GREMION, Canada]	Noted, no space for full definitions here but we point to the text where gamma and beta factors are described.
22952	56	20	56	26	The graph title for panel c) ("Mid-latitude CO2 fertilisation") does not match the caption ("mid and high-latitude...") - in the paper it's Northern Hemisphere Extratropical regions (30N-90N), so the panel title could read "Extratropical" or similar instead. [Gwenaelle GREMION, Canada]	Noted, but it was the figure caption that was in error - now reads: "(c) sensitivity of Gross Primary Production (GPP) to a doubling of atmospheric CO2 against the sensitivity of the amplitude of the CO2 seasonal cycle at Point Barrow, Alaska to a change in global mean atmospheric CO2 concentration".
22954	56	20	56	26	I suspect that the y-axis for panel c) should be $2xCO_2 \text{ GPP} / 1xCO_2 \text{ GPP}$ (rather than $1x/2x$ as shown) to match the cited paper. [Gwenaelle GREMION, Canada]	Agreed - figure label corrected.
28152	56	20	56	26	Figure 5.28: Here, I suggest to also include an example figure of an emergent constraint on the land carbon cycle in ESMs which is not only based on CO2 concentration observations. For example, Winkler et al. (2019, Nature Communications, doi:10.1038/S41467-019-08633-Z) use remote sensing data of longterm changes in surface reflectances (in from of LAI for comparability reasons to ESMs) as predictor of GPP increase for doubling of CO2 (Fig 2c). [Alexander Winkler, Germany]	Noted.
22956	56	31	56	31	This section would fit better after 5.4.5, i.e. first carbon cycle feedbacks then other feedbacks. [Gwenaelle GREMION, Canada]	Rejected – Sect 5.4.6 deals with emergent constraints for CO2 feedbacks, which is followed by section on non-CO2 feedbacks

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22958	56	31	56	31	The feedback effects between CO2, CH4, N2O and climate could be very nicely illustrated in a figured or flow chart. [Gwenaelle GREMION, Canada]	Rejected – impact of CH4 and N2O on carbon cycle is a modulation of feedbacks rather than feedbacks
49018	56	31	57	48	With my best regards and concerns of this aspect I would like to comment here: Compared to AR5, value for “Fire CO2-flux response to climate” becomes larger this time, probably due to the inclusion of three more estimates (as seen in Fig. 5.29), this is significant. But it is not clear if such change implies increasing robustness for the overall estimates ? If yes, a question related to this may be: the plausible improvement is because of the increased number of the estimates, or the diverse research methods? and what is the new advances compared to AR5 ? I believe it would be convenient for the readers to add statements/paragraphs to assess such changes. [Minchao Wu, Sweden]	Accepted – Chapter is extended by discussion of natural fires
49020	56	31	57	48	Similar to previous question, the value for “permafrost CO2 response to climate” shown in Fig. 5.29 is considerably smaller compared to Fig. 6.20 in AR5. It is noticed that at least two estimates in AR5 with the feedback strength larger than 0.4 W/m2/K are no longer available in Fig. 5.29 in FOD. What is the implication here for such change? I also believe that improving clarity with addition description in this section, or the caption of the figure would help a better understanding. [Minchao Wu, Sweden]	Accepted - the figure is revised
22960	56	33	56	37	Mentioning the effect of CH4 and N2O on CO2-climate feedbacks right in the beginning makes it seem as if the direct effects of CH4 and N2O on climate change are secondary. The interaction of these non-CO2 greenhouses gases with CO2 concentration could be mentioned later and also needs to be explicated, otherwise this sentence is not very informative. [Gwenaelle GREMION, Canada]	Accepted - the sentence 'both directly and indirectly' is added after 'respond'
24652	56	35	56	35	These non-CO2 processes are now being routinely included in ESMs, but are not routinely coupled to give a feedback. E.g. wetland methane is routinely diagnosed, but as the simulations are mostly driven by methane concentrations rather than emissions the feedback isn't included. [William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Accepted
27982	56	35	56	35	Page 56, line 35: “These feedbacks are not yet routinely included in ESMs”, add a quick sentence on why they are not taken into account. Figure 5.29, page 168: In the caption, say briefly why there is a such a wide spread of the single estimates, especially the permafrost CO2 response to climate [roderik van de wal, Netherlands]	Part 1: Taken into account - combined with comment 24652. Part 2: Rejected - already discussed in Sect. 5.4.7
47762	56	35	56	35	Please check the use of this IPCC uncertainty language term. Are you able to provide a traceable account to assigning this uncertainty statement? Note that likelihood statements are quantified terms - phrases like likely and very likely have quantifiable probabilities associated with them. Please check it has been used correctly here. Please refer to the IPCC guidance note on uncertainty: <a href="https://wg1.ipcc.ch/SR/documents/ar5_uncertainty-guidance-note.pdf">https://wg1.ipcc.ch/SR/documents/ar5_uncertainty-guidance-note.pdf</a> [WGI TSU, France]	Accepted - the word 'likely' is removed



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45676	56	35			Should also mention methane giving methane feedbacks - both via impact on OH and also via warming wetlands etc. [Euan Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - this is already included in sentence 'Sources and sinks of non-CO2 greenhouse gases such as methane (CH4) and nitrous oxide (N2O)'
22962	56	40	57	4	The figure caption should mention what the error-bars represent in the last two bars of the upper panel ("non-C4MIP feedback (lower panel)" and "non CO2 feedback (lower panel)") [Gwenaelle GREMION, Canada]	Accepted - it is stated that errors bars correspond to 1.s.d
22968	56	42	56	42	"climate expressing non-climate feedbacks" is too long a term to really understand what is meant [Gwenaelle GREMION, Canada]	Accepted
22970	56	42	56	56	What are filled versus striped bars in the figure? [Gwenaelle GREMION, Canada]	Accepted - difference in bars is described in caption.
22964	56	42	57	2	In Figure 5.29, it would be useful to have side labels (maybe on the left-hand side) that clearly signpost what each section represents, i.e. the top part is the C4MIP CO2 feedbacks, the middle is comparing the combined C4MIP CO2 feedbacks with non-C4MIP & non-CO2 feedbacks, and the bottom panel the non-C4MIP/CO2 feedbacks in detail. [Gwenaelle GREMION, Canada]	Accepted
22966	56	42	57	2	In Figure 5.29, it might also be worth spelling out exactly what the bar graphics represent (i.e. blue=negative, red=positive, hatched=w/ N cycle feedbacks) for clarity. [Gwenaelle GREMION, Canada]	Accepted
24654	56	42			Figure 5.29: This is a useful figure that people might want to take values from. Could the data be provided as a table in the text or an appendix? [William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - all data in figure are already published in peer-reviewed literature or may be subject to change because of uncertainties in CMIP6
32228	57	7	57	7	Text states "CH4 feedbacks may arise from wetland and permafrost CH4 emissions" I think this can be framed better, as permafrost per se does not emit methane. "may arise from changing wetland CH4 emissions, and from new CH4 sources following permafrost thaw" may be a better description. [David Olefeldt, Canada]	Accepted
47374	57	7	57	24	some more recent references on CH4 feedbacks would include: Comyn-Platt et al (2018, Nature Geosci.); Burke et al (2018, ERL) [chris jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - combined with comment 24660
22972	57	7	57	24	As land-use change was mentioned earlier, this paragraph could at least mention CH4 emissions from farming [Gwenaelle GREMION, Canada]	Accepted
22974	57	7	57	24	What about freshwater and ocean CO2 which is mentioned as other large sources of CH4 in chapter 5.2.2.4? [Gwenaelle GREMION, Canada]	Rejected - unclear what is to be revised
22976	57	7	57	24	The paragraph lacks of any term about the confidence or likelihood of the assertions and including them would be very helpful for the readers. [Gwenaelle GREMION, Canada]	Rejected - all statements are already in the last paragraph of Sect 5.4.7

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36824	57	7	57	24	There may be climatic feedback between biomass burning and terrestrial CH4 exchange. For example, in dry years, lower CH4 emissions from wetlands would be compensated by higher CH4 emissions from biomass burning, as observed in 2010 Amazon drought (Saito et al., 2016; Ribeiro et al., 2018). Saito, M., Kim, H.S., Ito, A., Yokota, T., Maksyutov, S., 2016. Enhanced methane emissions during Amazonian drought by biomass burning. Plos One 11, 10.1371/journal.pone.0166039. Ribeiro, I.O., Andreoli, R.V., Kayano, M.T., de Sousa, T.R., Medeiros, A.S., Guimarães, P.C., Barbosa, C.G.G., Godoi, R.H.M., Martin, S.T., de Souza, R.A.F., 2018. Impact of the biomass burning on methane variability during dry years in the Amazon measured from an aircraft and the AIRS sensor. Science of Total Environment 624, 509–516. [Akihiko Ito, Japan]	Rejected - unclear what is to be revised
32236	57	7	57	24	I think this paragraph could include a sentence or two about the differences in level of knowledge between methane emissions from tropical versus higher latitude wetlands. With differences in vegetation composition and other physical characteristics, wetlands have been found to vary in terms as to what factors they are most sensitive to with regards to methane emissions. Methane emissions from temperate and boreal wetlands are probably best understood, given a longer history of research compared to tropical and arctic wetlands. A great paper to cite here could be "Bridgham et al., Methane emissions from wetlands: biogeochemical, microbial, and modeling perspectives from local to global scales, 2013, Global Change Biology". [David Olefeldt, Canada]	Rejected - the discussion would become too lengthy
24656	57	7	57	44	This text reads as if all these methane and N2O processes are too uncertain to say anything, yet numerical values have been provided in figure 5.29. This paragraph should include an assessment of the evidence used to create figure 5.29 and how these differ from AR5 (their figure 6.20). Note for methane feedbacks the updated assessment of the methane forcing efficiency in Table 7.A.1 should be used as it is 25% larger than used previously (TAR, AR4 & AR5). The N2O forcing efficiency is slightly changed too for AR6. [William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - the sentence 'However, insufficient studies are available to assess the effect of the CO2-wetland CH4 on radiative forcing' is removed; we note that low qualitative understanding of these feedbacks is consistent between the indicated text and Fig. 5.29
7508	57	8	57	8	Typo: positively, not positive [Rose Abramoff, France]	Accepted - text revised
17656	57	8	57	8	Change 'positive' to 'positively' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
47372	57	8	57	10	this statement (CH4 emissions rise with T) is true in a "per square metre" sense for wetlands, but rememebr that the wetland extent itself may change. In fact previous studies fond it more likely to decrease with T than increase (see AR5 fig 6.37). Is there any updat on the NET effect of wetlands due to both extent and flux density changes? CO2 itself may be the bigger driver than climate change [chris jones, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - no additional information is available

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32232	57	8	57	10	Wetland methane emissions, at least in temperate, boreal, and arctic biomes are highly sensitive to soil temperature but also water table position. A good reference for this is "Turetsky MR, Kotowska A, Bubier J, Dise NB, Crill P, Hornibrook E, Minkinen K, Moore TR, Myer-Smith IH, Nykänen H, Olefeldt D, Rinne J, Saarnio S, Shurpali N, Waddington JW, White J, Wickland K, Wilming W (2014) A synthesis of methane emissions from 70 northern, temperate, and subtropical wetlands, <i>Global Change Biology</i> , 20, 2183-2197, doi:10.1111/gcb.12580". It should also be stated that these relationships are less well established for tropical wetlands, which are the main sources of natural wetland emissions, although see Pandey et al., 2017, <i>Scientific Reports</i> , on how tropical wetland emissions increased during La Nina years which increased wetland inundation. [David Olefeldt, Canada]	Rejected - water table position is implicitly taken into account in term 'precipitation anomalies' in the next sentence; it is undesirable to replace 'precipitation anomalies' by 'water table anomalies' because precipitation also affects vegetation primary production and, thus, the amount of high-quality substrate
22978	57	9	57	10	For completeness, the whole feedback loop should be described. [Gwenaëlle GREMION, Canada]	Rejected - described in the referenced literature
32230	57	10	57	10	I don't think the Groeningen citation is appropriate here, as it is a synthesis of the impacts of rising CO2 on CH4 and N2O emissions - temperature effects on CH4 is not the main focus. [David Olefeldt, Canada]	Accepted - the citation is removed
22980	57	10	57	13	Could explicitly mention here that an example of this interannual variability & climate anomalies in the citations is El Nino, which then connects to mentions of El Nino in N2O section. [Gwenaëlle GREMION, Canada]	Accepted
44214	57	13	57	16	Under CMIP5, the GISS model included climate-sensitive methane emissions from wetlands in their RCP-driven simulations, and so should be cited here (Shindell et al., <i>Interactive ozone and methane chemistry in GISS-E2 historical and future climate simulations</i> , <i>ACP</i> , 13, 2653–2689, 2013). That study, which also shows impacts on methane lifetime, could also be added to the results in Fig 5.29 (sections f and g of lower panel). [Drew Shindell, United States of America]	Accepted
44216	57	13	57	16	It would be more useful to readers to quantify the strength of the feedback rather than just say that the effect on radiative forcing is 'low'. What is considered low? In the GISS CMIP5 simulations, the feedback due to enhanced emissions from wetlands led to an additional radiative forcing of about 0.1-0.2 W/m2, depending on the RCP; see Fig 23 of Shindell et al., <i>Interactive ozone and methane chemistry in GISS-E2 historical and future climate simulations</i> , <i>ACP</i> , 13, 2653–2689, 2013. Is that 'low' or not? Fairly small for RCP8.5, but even 0.1 W/m2 not that small under RCP2.6. Hence more useful to give values across the cited literature. [Drew Shindell, United States of America]	Rejected - assessed in Fig. 5.29
41836	57	15	57	15	Explain why the impact of methane build up in the atmosphere on radiative forcing is considered as low. [Marc Aubinet, Belgium]	Accepted - sentence is revised
41840	57	15	57	15	apparently this process would be different from those resulting from permafrost thawing (discussed below on page 58-59) This should be specified and the difference between the two processes clarified. [Marc Aubinet, Belgium]	Rejected - at this line it is stated only about the CH4 build up in the atmosphere

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36404	57	15			Why is the effect on methane radiative forcing low? Just because the emissions of methane from this feedback are small? [Nathan Gillett, Canada]	Accepted - the sentence is clarified
28424	57	16	57	19	Rephrase as follows: Model simulations further suggest that the feedback between climate and wetland CH4 emissions is weaker than the feedback between rising atmospheric CO2 and wetland CH4 emissions due to the effect of increased plant productivity on methane production in wetlands (Melton et al., 2013; Ringeval et al., 2011). [Claude-Michel Nzotungicimpaye, Canada]	Accepted
45678	57	16	57	19	These are all now rather elderly studies and many depend on very imprecise Q10 assumptions. I'd suggest this section needs a further look and maybe a significant update. [Euan Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - (Denisov et al., 2013 and Shindell et al., 2013) were not assessed in AR5. Other references are added for completeness. No other relevant information is available
22982	57	17	57	19	CO2 is mentioned a couple of times but it is not clear what role it plays in the CH4-climate feedback [Gwenaëlle GREMION, Canada]	Accepted - it is clarified that enhanced plant production increases amount of available litter
41838	57	17	57	20	not clear: could you better explain the process discussed here ? (I don't see clearly in what an increased plant productivity could affect methane production?) [Marc Aubinet, Belgium]	Taken into account - combined with 22982
22984	57	18	57	18	Could it be explained further how plant productivity affects methane production? [Gwenaëlle GREMION, Canada]	Taken into account - combined with 22982
28426	57	19	57	20	Rephrase as follows: However, insufficient studies are available to assess the effect of the feedback between atmospheric CO2 and wetland CH4 emissions on the radiative forcing. [Claude-Michel Nzotungicimpaye, Canada]	Taken into account - combined with comment 24656
24658	57	19	57	20	Sentence "However insufficient studies...radiative forcing": It is not clear whether this sentence implies that the Melton et al and Ringeval et al. studies are insufficient, or whether the issue is these studies didn't specifically calculate a radiative forcing. If it is the latter, it is very simple to convert emission changes to radiative forcing changes. [William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - combined with comment 24656
25470	57	20	57	20	Right terminology? - Permafrost thaw leads to thermokarst so perhaps rewrite. Maybe also mention thawing of organic terrain to indicate permafrost terrain that is carbon rich? [Sharon Smith, Canada]	Taken into account - combined with comment 32234
32234	57	20	57	20	Text states "methane emissions from thermokarst may further contribute" which needs to be rephrased to "methane emissions from thermokarst wetlands and lakes may further contribute" - just saying "thermokarst" doesn't make sense. [David Olefeldt, Canada]	Accepted
36406	57	20		22	The magnitude of the permafrost methane feedback is something readers will expect to be assessed in detail in the chapter, and something which is high profile in the literature and public debate. More discussion and assessment on this topic should be added. [Nathan Gillett, Canada]	Rejected - the assessment is in Sect. 5.4.8.2.

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28428	57	22	57	22	A paper by Turetsky et al. is cited in the sentence but the publication year is not specified (maybe 2015?). This information cannot be retrieved (verified) from (in) the list of references. [Claude-Michel Nzotungicimpaye, Canada]	Accepted - two instances of Turetsky et al. In the list of references are merged; the paper is under review and, thus, it is still incomplete
17658	57	22	57	22	Date missing from Turetsky reference [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account – combined with comment 28428
22986	57	22	57	25	Not clear from the reference list which Turetsky citation this is (one has no title/date, the other entry is incomplete and I can't find a paper of the listed partial-name). Is it meant to be the recent Turetsky et al (2019) in Nature (doi: 10.1038/d41586-019-01313-4)? If not, this would be a good reference here, as would the review of permafrost and climate change impacts by Schuur et al (2015; Nature; doi:10.1038/nature14338). This latter reference proposes some constraints on magnitude/rate of 130-160 GtC by 2100 under high emission scenarios, which could be mentioned even if uncertain? [Gwenaelle GREMION, Canada]	Taken into account – combined with comment 28428
22988	57	23	57	23	Voulgarakis et al (2013) also indicate that the negative feedback of reduced CH4 atmospheric lifetime gets saturated by using up OH under high emission scenarios. [Gwenaelle GREMION, Canada]	Rejected - only the sign of the feedback is discussed in this paragraph
47764	57	26	57	26	Please check the use of this IPCC uncertainty language term. Are you able to provide a traceable account to assigning this uncertainty statement? Note that likelihood statements are quantified terms - phrases like likely and very likely have quantifiable probabilities associated with them. Please check it has been used correctly here. Please refer to the IPCC guidance note on uncertainty: <a href="https://wg1.ipcc.ch/SR/documents/ar5_uncertainty-guidance-note.pdf">https://wg1.ipcc.ch/SR/documents/ar5_uncertainty-guidance-note.pdf</a> [WGI TSU, France]	Accepted - the statement is added that the uncertainty is assigned due to the assessment in the previous two paragraphs
22990	57	26	57	28	The citation given (Thompson et al, 2014) does not discuss the impacts of climate anomalies such as El Nino on N2O emissions, instead primarily focusing on seasonal-annual variations in atmospheric transport and exchange. The subsequent citations (Tian et al, 2019; Xu-Ri et al, 2012) do discuss climate impacts some more and so may serve as alternative references here, but still don't mention El Nino. Has the potential impact of El Nino on N2O emissions been studied or hypothesised in a published paper, or is the inference made only here? [Gwenaelle GREMION, Canada]	Accepted - misspelled reference to (Thompson et al., 2013) is corrected, and other two references are added
17660	57	28	57	28	Change Nino to Niño [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
22992	57	32	57	33	The citations given sufficiently describe how increases in N2O emissions are constrained by N availability, but I can't find any mentions of N-poor ecosystems being liable to a decline in N2O emissions due to warming (rather than land cover change). [Gwenaelle GREMION, Canada]	Accepted - the sentence is revised
17662	57	36	57	36	Change to '21st Century' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - The AR6_WGI_StyleGuide says to use "21st century"

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
22994	57	41	57	44	I suggest making it clearer here that the magnitudes of the marine and terrestrial N2O emissions are similar but opposite sign and so may mostly cancel each other out, and that marine emission changes are likely to be more important if sustained beyond 2100, adding Martinez-Rey et al (2015) as a citation supporting these points. [Gwenaelle GREMION, Canada]	Accepted
24660	57	46	47	48	While the feedbacks may be small the impacts on carbon budgets may be important. Comyn-Platt et al. 2018 suggest that the methane feedback may reduce allowable carbon budgets by up to 10% for a 1.5 degree limit. [William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - already discussed in Sect. 5.5
39364	57	46	57	48	This statement that the climate-CH4 feedback will be small (with medium to high confidence) seems to be contradicted by section 5.4.8.2. In that section, the argument is made that there is high confidence that thawing permafrost will lead to C releases, "but low confidence in the timing, magnituded and relative roles of CO2 versus CH4 feddback processes." That says to me that we don't have "medium to high confidence" that the climate-CH4 feedback will likely be small.at the multidecadal and centennial time scale. [Eric Davidson, United States of America]	Rejected - no apparent contradiction
22996	57	46	57	48	The confidence statement is fine (High Agreement on Medium Evidence). [Gwenaelle GREMION, Canada]	Rejected - unclear what is to be changed
22998	57	46	57	48	The authors should include here the references used to draw the conclusion so readers can check them. [Gwenaelle GREMION, Canada]	Rejected - all references in previous paragraphs of this section
36408	57	46		48	Can you also quantify or constrain the effects of these feedbacks on multi-centennial timescales. [Nathan Gillett, Canada]	Taken into account - combined with comment 22998
45680	57	46			"medium to high" confidence - very stong statement about very poorly understood phenomena, We really have so little dedcent field understanding of what is happening in the tropics. In places like the Sudd, or even the Pantanal, there's virtually no detailed flux quantification by experimental measurement - not modellng but real data are needed. Until then, to clim high confidence is dangerous. [Euan Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - there is a high model agreement on relationships between the relative role of natural feedbacks and anthropogenic forcings in all existing model projections: anthropogenic forcing dominates, climate-carbon cycle feedbacks modulates the response by at most several tens per cents, and climate-CH4 and climate N2O interactions modulate this feedback
17664	57	48	57	48	Capital C for century [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
23000	57	51	57	51	Sea ice melting/ice sheet loss may not be directly related to biochemical cycles, but it would fit in this section. All the tipping points should be mentioned together somewhere either in this section or in another section that is not exclusive to biogeochemical cycles [Gwenaelle GREMION, Canada]	Rejected - beyond the scope of the Chapter

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55016	57	51	58	9	Section {5.4.8} on "Abrupt changes and tipping points" or other related sections can benefit from an assessment of "Trajectories of the Earth System in the Anthropocene" by Steffen et al. (2018) in PNAS 115 (33) 8252-8259 that contains schematic illustrations that distinguishes a hothouse Earth pathway based on self-reinforcing feedbacks. [Kilkis Siir, Turkey]	Accepted
44942	57	51			Section 5.4.8 on abrupt changes and tipping points. Here's one example of where the it makes make sense to integrate information from paleo observations with other evidence, in this case, to derive a more complete understanding of abrupt changes. [Darrell Kaufman, United States of America]	Rejected - covered by Chapter 1
26520	57	53	57	55	The existence of emergent constraints does not suggest that large-scale biogeochemical feedbacks are approximately linear in the forcing from changes in CO2 and climate. Emergent constraints only suggest that there is a quasi-linear relationship between across an ensemble of models, typically between (i) a certain aspect of long-term changes in ESM and (ii) an observable trend or variation in the contemporary climate. That is very general and not the same as this very concrete example chosen here. [Nadine Goris, Norway]	Accepted - the reference to emergent constraint is removed
23002	57	53	57	55	It might be worth mentioning here that both the emergent constraints and linear feedback frameworks are both based on Earth System Models analysis which may not capture all of the rare and not-yet-observed tipping points that might exist, and so it's not entirely surprising that these frameworks yield fairly linear relationships. Of course this doesn't prove or disprove the existence of tipping points, but the framing of this section opening could clarify this by mentioning that ESMs are overall fairly linear (and this is reflected by emergent constraints and feedback analysis) but may be missing some poorly resolved tipping points (such as the following...). [Gwenaelle GREMION, Canada]	Taken into account - combined with comment 26520
45368	57	54	57	54	"require" rather than "suggest", there is little observational evidence of linearity [Peter Rayner, Australia]	Rejected -the note on emergent constraint removed (see response to comment 26520) , thus the revised statement is, '...utility of the linear feedback framework ... suggests'; this is a precise statement
27780	58	1	58	1	verify this paragraph [Poot Delgado Carlos Antonio, Mexico]	Rejected - unclear what is to be revised
23004	58	1	58	3	Some additional more recent seminal references here following up on proposed tipping elements and their interactions could include Lenton & Williams (2013, TREE, doi:10.1016/j.tree.2013.06.001), Schellnhuber et al (2016, NatCC, doi:10.1038/nclimate3013), and probably most relevant update is the recent "Hothouse Earth" paper (Steffen et al, 2018, PNAS, doi:10.1073/pnas.1810141115). [Gwenaelle GREMION, Canada]	Accepted
17666	58	3	58	3	Change 'Sahara' to 'Saharan' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
23006	58	3	58	4	The greening is of the Sahara rather than sub-Sahara (at least in Lenton et al, 2008). [Gwenaelle GREMION, Canada]	Rejected - the sub-Sahara is more precise term

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
23008	58	4	58	8	It might help to reiterate and clarify what is meant here and across the report by 'tipping point', as there are many definitions in use. In Chapter 1, the IPCC's usage of tipping point is defined when "abrupt change occurs because the current state becomes unstable, such that the subsequent rate of change is actually independent of the forcing". This is similar to key citation Lenton et al's (2008) definition, in which they broaden it from the narrow classical definition (of mathematical bifurcations and irreversible changes due to hysteresis) to when small deviations maintained above a critical value of a key variable result in large qualitative changes in the system (and aren't necessarily irreversible). Lenton, 2013 (ARER, doi:10.1146/annurev-environ-102511-084654) further explains his 2008 tipping point definition as when "a steady change in some control parameter ultimately leads to a qualitative change in the system state when some threshold is passed", and can be either reversible or irreversible, and either forcing-, noise-, or rate-induced. Both imply a critical threshold beyond which changes become self-perpetuating independent of the original forcing, with large impacts that are likely (but not necessarily) abrupt and/or irreversible. I think it would be worth restating the IPCC's tipping point definition, Lenton et al's, or both again in this section introduction to ensure clarity, and to make sure that each subsequent proposed tipping element/point is measured against this yardstick of there being a candidate critical threshold followed by self-perpetuating change independent of forcing leading to a substantial shift. As it stands, some of the tipping points mentioned sound more like repeated mentions of linear feedbacks with no proposed critical threshold or self-perpetuation mechanism clearly stated [Gwenaelle GREMION, Canada]	Accepted - the respective text is added in the beginning of Sec. 5.4.8
17668	58	8	58	8	Change 'gases' to 'gas' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
47376	58	11			Some work by Peter Good has developed a metric of "dry season resilience" as an indicator of tropical forest resilience to climate change. They have shown the existence of a threshold in this phase space which can be diagnosed from observations, some forests are closer to this threshold for dieback than others (notably the Amazon) and that CO2 fertilisation (in models) changes where this threshold is, and hence affects whether or not abrupt dieback may occur. Good et al (2011, J. Clim; 2013 J. Clim) [chris jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted
36410	58	11			To what extent are the forest dieback processes assessed in this section included in CMIP6 ESMs, and therefore included in projections discussed elsewhere in the report? [Nathan Gillett, Canada]	Accepted
23010	58	13	58	38	No information on temperate forest dieback was given. What reference is the statement based on that boreal forest dieback is unlikely to change atmospheric perturbation? [Gwenaelle GREMION, Canada]	Accepted



Comment ID	From Page	From Line	To Page	To Line	Comment	Response
23012	58	14	58	14	Brando et al (2014, PNAS, doi:10.1073/pnas.1305499111) might be a useful additional citation for observational evidence of abrupt self-perpetuating forest dieback, as would Jones et al (2009, NatGS, doi:10.1038/ngeo555) a seminal citation for Amazon dieback including a proposed critical threshold (~2°C) and self-perpetuating processes (as change is committed for many decades into future). This is useful, as to be included in a tipping point section there should be some sort of discussion of a potential threshold for tropical forest dieback along with a self-perpetuating process independent of driver, here potentially provided by Zemp et al, 2017 through rainfall feedbacks, leading to a substantial state shift (here a sharp drop in tree cover). This should be emphasised a bit more in this section. [Gwenaelle GREMION, Canada]	Accepted
17670	58	15	58	15	Delete 'diebacks' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
7510	58	15	58	15	Typo: one too many "dieback" in this line. [Rose Abramoff, France]	Accepted - text revised
23014	58	15	58	15	The word dieback is written two times on a row. [Gwenaelle GREMION, Canada]	Accepted - text revised
17672	58	18	58	18	Space required between , and ( and delete second 'to' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
17674	58	19	58	19	Delete 'to' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
23016	58	19	58	19	Scheffer et al (2012) and Zemp et al (2017) from the previous sentence can be added to the climate extremes references as they also posit extremes as important, whereas I can't find significant mention of climate anomalies or extremes in Staver et al (2011) which mostly focuses on general fire trends and feedbacks. [Gwenaelle GREMION, Canada]	Accepted
17676	58	22	58	22	Italicise 'low' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
23018	58	22	58	22	"confidence is low" should be in italics. [Gwenaelle GREMION, Canada]	Accepted - text revised
17678	58	23	58	23	Change 'forests' to 'forest' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
23020	58	23	58	24	"...partly due to climate changes which are insufficient for crossing climate thresholds" doesn't quite capture the key points of Boulton et al (2017) and Huntingford et al (2013) - my key takeaways from them are that dieback only occurs in a certain physiological parameter space, which makes models in that space feature thresholds. Rephrasing to clarify/emphasise dieback in models being parameter-dependent might be clearer. An additional references for models not featuring tropical dieback could include Lucht et al (2006) from pg.58.In.17. [Gwenaelle GREMION, Canada]	Rejected - 'parameter-dependent' is very unclear term - it might be a confusion between external and internal parameters; 'climate thresholds' is easier to follow
7512	58	23	58	27	Adding (i) (ii) (iii) to the clauses would make this sentence easier to follow. [Rose Abramoff, France]	Accepted - text revised
23022	58	25	58	25	I'd consider changing "environmental heterogeneity" to "ecosystem heterogeneity" to better reflect Levine et al's (2016) terminology and emphasise it's the diversity in the ecosystem itself which is most important here. [Gwenaelle GREMION, Canada]	Rejected - wording would become awkward

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23024	58	26	58	27	Reduced competition features heavily alongside acclimation in Lloret et al (2012) as a stabilising process, and could be worth mentioning too if there's space. [Gwenaelle GREMION, Canada]	Rejected - lack of space
47766	58	27	58	34	Please check the use of this IPCC uncertainty language term. Are you able to provide a traceable account to assigning this uncertainty statement? Note that likelihood statements are quantified terms - phrases like likely and very likely have quantifiable probabilities associated with them. Please check it has been used correctly here. Please refer to the IPCC guidance note on uncertainty: <a href="https://wg1.ipcc.ch/SR/documents/ar5_uncertainty-guidance-note.pdf">https://wg1.ipcc.ch/SR/documents/ar5_uncertainty-guidance-note.pdf</a> [WGI TSU, France]	Taken into account - combined with comment 26964
17680	58	30	58	30	Edit reference to Anadon et al. (2014) [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted
26962	58	30	58	34	This is a very crude and rough extrapolation and should be replaced by a better estimate. [Joachim Rock, Germany]	Rejected - it is clearly stated that the assessed estimate is an upper limit which assumes that all carbon currently present in the vegetation is released in the atmosphere
57792	58	30	58	38	5.4.8.1 Forest dieback section could benefit from including assessment of studies on droughts and fires, perhaps some discussion of mega fires in paleoclimates, other drivers of mortality ( e.g. insect outbreaks) [Elena Shevliakova, United States of America]	Rejected - the focus of the section is to assess the impact of forest diebacks rather than to link it with particular mechanisms, especially with paleoclimate events with qualitatively uncertain release of greenhouse gases
17682	58	31	58	31	Change to '21st Century' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - The AR6_WGI_StyleGuide says to use "21st century"
32756	58	31	58	31	"may be" needs to be changed into the IPCC lexicon as "may" can mean anything from 1 to 99%. [Michael MacCracken, United States of America]	Accepted - 'may' is replaced by 'might'

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
23026	58	32	58	38	I think there may be a mistake in the extrapolated estimate of total tropical forest carbon release by 2100. Currently, the estimated max. 39% reduction in Amazon C storage from Anadon et al (2014) is extrapolated across the 247 GtC figure from Saatchi et al (2011), giving ~96 GtC total release. This is then translated to 0.1 ppm per year, but by my calculation 96 GtC gives ~45 ppm CO2 (at 2.13 GtC per 1 ppm CO2), which over 2000-2070 (the final date in Anadon et al, 2014) would yield ~0.6 ppm CO2/year, rather than 0.1. However, the total C release calculation assumes that all of the C storage lost from rainforest would go to the atmosphere, when in fact the rainforest is replaced by savannah which stores less but still some carbon. In Steffen et al (2018, PNAS, doi:10.1073/pnas.1810141115 - supplementary information), a conversion rate of ~65% is used for carbon stored in savannah versus rainforest (although they use a far higher estimate of 150-200 GtC in the Amazon and 547 GtC global tropical forest). Recalculating using this then, ~100 GtC in the Amazon rainforest would yield ~35 GtC in a total dieback scenario (with the rest transferred to savannah storage), 39% of which lost by 2070 being ~14 GtC. Scaling this calculation for the global tropical forest of 247 GtC would give a total dieback release of ~86 GtC, 39% of which is ~34 GtC (which would translate as 16 ppm / 70 years = ~0.2 ppm/year). Even if Steffen et al's (2018) higher tropical forest C (547 GtC) is used, 39% of the total rainforest-savannah dieback difference of 191 GtC (53-70 GtC from Amazon) would yield ~76 GtC by 2070. Based on this, I suspect 100 GtC is too high an estimate for ~40% dieback, even with uncertainty on total C storage. For the confidence on this estimate, there is certainly high agreement of no more than the proposed upper bound (even if adjusted for recalculation), but it's potentially debatable as to whether the evidence base is fully robust yet (but CMIP6 may help in this), which might	Accepted - the sentence 'This estimate becomes even smaller taking into account the carbon stored in savannah grasses which replaces tropical forests (Steffen et al., 2018)' is added
17684	58	33	58	34	Don't split numbers and units across a line [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. Please note that the final report will undergo a professional copy-edit before publication. This sort of issue will be fixed then.
13612	58	34	58	34	The upper bound for the carbon release is 1 PgC per year, not 0.1 PgC per year. [Govindasamy Bala, India]	Accepted - changed from 'O(0.1 ppm/yr)' to ' $\leq 1$ ppm/yr'
46054	58	34	58	34	Is "dCO2/dt" correct? [Kaoru Tachiiri, Japan]	Taken into account – combined with comment 13612
26964	58	34	58	35	This statement needs to be supported by references, it should also be stated what is "substantially" in this context. [Joachim Rock, Germany]	Rejected - this statement is a conclusion from the previous part of this Section
23028	58	34	58	35	I think there should be a bit more detail on why boreal forest dieback is unlikely to have a significant impact, as having mentioned it before the greater detail on tropical dieback there's not much follow-up discussion as to why boreal dieback might be minimal. Presumably this is because of northward forest colonisation counteracting southern losses to savannah/steppe? If so, it'd be worth clarifying. [Gwenaelle GREMION, Canada]	Taken into account - combined with comment 23010

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
17686	58	36	58	37	Wrap line [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. Please note that the final report will undergo a professional copy-edit before publication. This sort of issue will be fixed then.
57794	58	37	58	38	"There is a high confidence that forest dieback will not release more than 100 Pg of carbon over the 21st 37 century." ESMs and DGVMs do not simulate forest dynamics - vegetation dynamics is crudely parameterized with a few vegetation types or PFT. They do not represent many mortality and establishment processes known to determine the fate of future forests. I don't think one can make high confidence statements about how much carbon will be released with all known models' limitations, including fire and plant mortality. [Elena Shevliakova, United States of America]	Rejected - it is clearly stated that the assessed estimate is an upper limit which assumes that all carbon currently present in the vegetation is released in the atmosphere
23030	58	37	58	38	How many Pg carbon are needed to create a tipping point? [Gwenaelle GREMION, Canada]	Rejected - no relevant information is available
32758	58	37	58	38	Does this not need to be qualified by how large the temperature change is - would not going to 4 C, for example so stress systems that more would be released, and then perhaps gradually taken up as new ecosystems emerge - which may require time for evolution to occur. [Michael MacCracken, United States of America]	Rejected - discussed in previous paragraph
17688	58	38	58	38	Capital C for century [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - The AR6_WGI_StyleGuide says to use "century"
25472	58	41	58	41	Section 5.4.8.2 title - Do you mean Biogenic emissions from thawing permafrost? [Sharon Smith, Canada]	Taken into account - combined with 32238
32238	58	41	58	41	"Biogenic emissions from permafrost" I suggest this should be change to "Biogenic emissions following permafrost thaw", as permafrost does not emit ghg's, its only once permafrost thaws that the organic matter can decompose and be released as GHG's. [David Olefeldt, Canada]	Accepted
23032	58	41	59	10	N2O emissions from thawing permafrost systems would be a good addition to the "Biogenic emissions from permafrost" section (see for instance Voigt et al 2017, doi:10.1073/pnas.1702902114). [Gwenaelle GREMION, Canada]	Accepted - reference to (Voigt et al., 2017) is added (but in Sect.5.4.7 because this paper does not quantify future N2O emissions)
32240	58	43	58	43	"The permafrost region has acted either as a weak carbon sink or source historically.." - this statement is really dependent on what you mean with historically - what time-frame? Over the Holocene, the permafrost region has been a very significant C sink due to development of peatland soils and other soils which become incorporated into permafrost. The statement is more accurate if you only consider only a much more recent period - last few decades to centuries. [David Olefeldt, Canada]	Accepted
6708	58	43	58	47	Add estimate from MacDougall and Knutti 2016 (doi:10.5194/bg-13-2123-2016) for RCP 8.5 at 2100 of 27 to 199 PgC (5th to 95th percentile range). Also estimate from Schneider von Deimling et al. 2015 (doi:10.5194/bg-12-3469-2015) for RCP 8.5 at 2100 of 42 to 141 (68% range). There may be more estimates available. [Andrew MacDougall, Canada]	Accepted - two references are added, all estimates are combined into single range 11-205 Pg (taking into account comment 23038)

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26674	58	43	58	48	You may want to include the dependence of net permafrost-region emissions on emission pathway here, see Kleinen & Brovkin (2018) (Kleinen, T. & Brovkin, V. (2018). Pathway-dependent fate of permafrost region carbon. Environmental Research Letters, 13: 094001. doi:10.1088/1748-9326/aad824) [Thomas Kleinen, Germany]	Accepted
12832	58	43	59	7	Note that permafrost is a source of methane, which is released into the atmosphere as the permafrost thaws and has a far greater impact on warming than CO2, especially in the near-term, because of its larger GWP. Schuur E. A. G., et al. (2015) Climate Change and the Permafrost Carbon Feedback, NATURE 520: 171–179, 171 (“At the proposed rates, the observed and projected emissions of CH4 and CO2 from thawing permafrost are unlikely to cause abrupt climate change over a period of a few years to a decade. Instead, permafrost carbon emissions are likely to be felt over decades to centuries as northern regions warm, making climate change happen faster than we would expect on the basis of projected emissions from human activities alone.”). Permafrost thaw can also be a source of N2O, a greenhouse gas that can contribute additional warming. Wilkerson J., et al. (2019) Permafrost nitrous oxide emissions observed on a landscape scale using the airborne eddy-covariance method, ATMOS. CHEM. PHYS. 19:4257–4268, 4257 (“The microbial by-product nitrous oxide (N2O), a potent greenhouse gas and ozone depleting substance, has conventionally been assumed to have minimal emissions in permafrost regions. This assumption has been questioned by recent in situ studies which have demonstrated that some geologic features in permafrost may, in fact, have elevated emissions comparable to those of tropical soils. However, these recent studies, along with every known in situ study focused on permafrost N2O fluxes, have used chambers to examine small areas (< 50 m2). In late August 2013, we used the airborne eddy-covariance technique to make in situ N2O flux measurements over the North Slope of Alaska from a low-flying aircraft spanning a much larger area: around 310 km2. We observed large variability of N2O fluxes with many areas exhibiting negligible emissions. Still, the daily mean averaged over our flight campaign was 3.8 (2.2–4.7)	Rejected - CO2 and CH4 emissions are already assessed in Sect. 5.4.8.2, and the note on N2O emissions are not quantified for the whole 21st century - see reply to comment 23032
49480	58	43	59	7	If I remember correctly, the estimates in line 46 do not include fluxes during the non growing season? These contribute greatly to the uncertainty in their estimates and may be worth mentioning. Susan Natali presented on this at AGU in 2018 ( <a href="http://adsabs.harvard.edu/abs/2018AGUFM.B23A..01N">http://adsabs.harvard.edu/abs/2018AGUFM.B23A..01N</a> ). She also has papers describing the potential importance of winter emissions: <a href="https://doi.org/10.1890/13-0602.1">https://doi.org/10.1890/13-0602.1</a> [Seth Spawn, United States of America]	Accepted - the note on winter emissions is added
42338	58	43	59	7	Note that permafrost is a source of methane, which is released into the atmosphere as the permafrost thaws and has a far greater impact on warming than CO2, especially in the near-term, because of its larger GWP. Schuur E. A. G., et al. (2015) Climate Change and the Permafrost Carbon Feedback, NATURE 520: 171–179, 171 [Gabrielle Dreyfus, United States of America]	Rejected - it is already assessed

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
12666	58	43	59	7	Note that permafrost is a source of methane, which is slowly released into the atmosphere as the permafrost thaws and has a far greater impact on warming than CO <sub>2</sub> , especially in the near-term, because of its larger GWP. Schuur E. A. G., et al. (2015) Climate Change and the Permafrost Carbon Feedback, NATURE 520: 171–179, 171 (“At the proposed rates, the observed and projected emissions of CH <sub>4</sub> and CO <sub>2</sub> from thawing permafrost are unlikely to cause abrupt climate change over a period of a few years to a decade. Instead, permafrost carbon emissions are likely to be felt over decades to centuries as northern regions warm, making climate change happen faster than we would expect on the basis of projected emissions from human activities alone.”). Permafrost thaw can also be a source of N <sub>2</sub> O, a greenhouse gas that can contribute additional warming. Wilkerson J., et al. (2019) Permafrost nitrous oxide emissions observed on a landscape scale using the airborne eddy-covariance method, ATMOS. CHEM. PHYS. 19:4257–4268, 4257 (“The microbial by-product nitrous oxide (N <sub>2</sub> O), a potent greenhouse gas and ozone depleting substance, has conventionally been assumed to have minimal emissions in permafrost regions. This assumption has been questioned by recent in situ studies which have demonstrated that some geologic features in permafrost may, in fact, have elevated emissions comparable to those of tropical soils. However, these recent studies, along with every known in situ study focused on permafrost N <sub>2</sub> O fluxes, have used chambers to examine small areas (< 50 m <sup>2</sup> ). In late August 2013, we used the airborne eddy-covariance technique to make in situ N <sub>2</sub> O flux measurements over the North Slope of Alaska from a low-flying aircraft spanning a much larger area: around 310 km <sup>2</sup> . We observed large variability of N <sub>2</sub> O fluxes with many areas exhibiting negligible emissions. Still, the daily mean averaged over our flight campaign was 3.8 (2.2–4.7)	Rejected - it is unclear what is to be changed
23034	58	44	58	44	McGuire et al (2012) does not appear to contain any historical estimates of permafrost (as it's entirely future scenario modelling-based) and Belshie et al (2013) report that permafrost is mostly a net source throughout their study period (1980-2010), so as it stands the sink statement isn't supported unless an additional citation is added. [Gwenaelle GREMION, Canada]	Taken into account - combined with comment 32240; note that McGuire et al. (2012) report estimates for 1990-2006
23036	58	44	58	48	If the range of uncertainty in the cited literature is similar in the CMIP6 ESMs outcomes, it would be interesting to briefly mention what the main sources of this uncertainties are, and thus which parameters should be further constrained in future research (e.g. SOC distribution, water table depth, thermokarst events, biotic interactions, plant fertilization from permafrost N, etc.). [Gwenaelle GREMION, Canada]	Accepted
23038	58	45	58	46	Another model range (of 42-141 GtC) is available from Schneider von Deimling et al (2015), cited on pg.58.in.51, as well as from Schaefer et al (2014, ERL, doi:10.1088/1748-9326/9/8/085003) of 120 GtC +/-85 for rcp8p5. It might also be worth mentioning that Schuur et al (2015) go on to give an expert judgment of 130-160 GtC at risk by 2100, so tending towards the top end of the range estimates. [Gwenaelle GREMION, Canada]	Accepted - references to (Schaefer et al., 2014) and (Schuur et al., 2015) are added; the comment is combined with comment 23034

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
23040	58	46	58	47	McGuire et al (2012) can also be added here instead of In.44 as support for further strengthening by 2300. [Gwenaelle GREMION, Canada]	Accepted - the reference to (McGuire et al., 2018) is added (because (McGuire et al., 2012) focusses on historical period) as well other relevant references
27782	58	48	58	48	attend the placeholders [Poot Delgado Carlos Antonio, Mexico]	Accepted – placeholders have been replaced by values in SOD
23042	58	48	58	50	Schuur et al (2015) provide an estimate of ~2.3% of C being released as methane, which may be a useful estimate to add here. [Gwenaelle GREMION, Canada]	Rejected - Schuur et al.'s (2015) estimate is an expert assessment with an unknown uncertainty
51554	58	50	58	50	I suggest include "This is in particular true because the respired CO2:CH4 ratio from thawing permafrost has been measured to be 0.92 +/- 0.18 (Knoblauch et al., 2018, doi: 10.1038/s41558-018-0095-z). [Christian Beer, Germany]	Rejected - this paper focusses on incubation experiments, which results may be different from estimates in field measurements (Schuur et al., 2015); CO2:CH4 ratio is very uncertain
23044	58	50	58	51	The methane emissions of 663-2440 TgCH4 by 2100 given from Schneider von Deimling et al (2015) is actually for rcp6p0, whereas I think for consistency the rcp8p5 figure of 836-2614 TgCH4 should be used. This is also for all permafrost methane emissions, rather than just thermokarst lakes (although they are a large source of this methane). [Gwenaelle GREMION, Canada]	Accepted
23046	58	50	58	52	There seems to be some replication - but with more detail - between here and section 5.4.7, where permafrost CH4 emissions are described in a sentence but not quantified. This goes back to the potential issue of some repetition between carbon cycle feedbacks and tipping points described in an earlier comment - should linear C sources and feedbacks be explained and quantified in full in previous sections and then this section reserved only for further evidence of feedback processes fitting the tipping point criteria (i.e. with thresholds and self-perpetuation)? Or is some repetition OK? [Gwenaelle GREMION, Canada]	Rejected - objectives of Sect. 5.4.7 is to describe overall mechanisms leading to non-CO2 feedbacks, while the scope Sect.5.4.8 (and its subsections) is to quantify these feedbacks
23048	58	51	58	52	No details are given in the reference list for Turetsky and others, and having checked her recent papers I cannot find one advocating 9000 TgCH4 - is this in review somewhere at the moment? [Gwenaelle GREMION, Canada]	Taken into account – combined with comment 28428
17690	58	51	58	52	Capital C for century x 2 [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - The AR6_WGI_StyleGuide says to use "century"
36700	58	51	58	52	"Cumulative release of CH4 due to thermokarst may contribute to increase in global mean air temperature by 0.11 degree C (Yokohata, Saito et al. Impact of GHG releases by ice-rich pemafrst degradations on the climate system)" can be added. The manuscript is in preparation, and to be submitted by summer 2019. [Kazuyuki Saito, Japan]	Rejected - it is not possible to cite non-submitted papers; however this paper may be added if it will become available before the AR6 deadline

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45682	58	51			Maybe cite Anthony, Katey Walter, et al. "Methane emissions proportional to permafrost carbon thawed in Arctic lakes since the 1950s." Nature Geoscience 9.9 (2016): 679. Note also though that France, James L., et al. "Measurements of $\delta^{13}C$ in $CH_4$ and using particle dispersion modeling to characterize sources of Arctic methane within an air mass." Journal of Geophysical Research: Atmospheres 121.23 (2016) are not seeing current big Arctic emissions. Nor are Fisher, Rebecca E., et al. "Measurement of the $^{13}C$ isotopic signature of methane emissions from northern European wetlands." Global Biogeochemical Cycles 31.3 (2017): 605-623. [Euan Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - these papers do not allow to assess future release of greenhouse gases at the global scale
28430	58	52	58	52	The cited Turetsky et al. paper should be cited correctly (missing publication year) and included in the list of references. [Claude-Michel Nzotungicimpaye, Canada]	Taken into account – combined with comment 28428
32242	58	52	58	52	After the sentence that ends with "increasing carbon loss", I think there should be a sentence about how permafrost thaw in the long term (scale of centuries) is likely to lead to increased soil carbon accumulation - this occurs in thermokarst wetlands and thermokarst basins (drained lakes). Some recent papers on this include "Walter-Anthony et al., 2014, A shift of thermokarst lakes from carbon sources to sinks during the Holocene epoch, Nature", and "Jones et al., 2016, Rapid carbon loss and slow recovery following permafrost thaw in boreal peatlands, Global Change Biology". [David Olefeldt, Canada]	Rejected - the topic is out of the scope of the chapter (should be covered in Chapter 9)
23050	58	52	59	2	This is an important point to emphasise, as internal heat generation provides a potential mechanism for abrupt self-perpetuating processes that could justify permafrost carbon release being considered as a tipping element/point rather than a quasi-linear feedback. A clear threshold for permafrost isn't stated in Holleson et al (2015), but for some regions in Greenland a warming of $5.2^{\circ}C$ from 2012-2100 (on a high-end rcp4p5 scenario) pushing local mean annual air temperature to $\sim +1.2^{\circ}C$ led to a strong positive feedback from decomposition providing an average of $3^{\circ}C$ extra heat that rapidly thawed the entire 3m deep permafrost layer and lost up to 40% of the carbon. This implies that localised permafrost tipping points exist when temperatures mean annual temperatures consistently exceed around $0.5^{\circ}C$ (when decomposition begins in the model), providing both a candidate threshold and self-perpetuating process afterwards - although with some significant uncertainty attached. I suggest emphasising these elements if permafrost is to be included as a tipping point as well as featuring it in the feedbacks/uptake sections 5.4.3 and 5.4.7. [Gwenaelle GREMION, Canada]	Accepted



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23052	58	52	59	2	I think some sentence clarification is needed in this section as the internal heat production of Holleson et al (2015) and the compost-bomb instability of Luke & Cox (2011) both refer to the same processes of warming & drying of organic-rich soils triggering decomposition-driven warming feedbacks, with the only difference being the former's focus on the special case of permafrost thawing dynamics. At the moment the two sentences slightly repeat each other (e.g. mentioning "carbon-rich permafrost" again after "organic permafrost" in the previous sentence) and imply they're somewhat different processes. [Gwenaelle GREMION, Canada]	Accepted
23054	58	52	59	2	Another potential line of evidence for some sort of a potential permafrost threshold that could be added to this section is from palaeoclimate records, in the form of speleothem evidence of larger-scale Siberian permafrost thawing in the Eemian when global mean temperature hit ~1.5°C warmer (Vaks et al; 2013; Science; doi:10.1126/science.1228729). However, there wasn't much more warming beyond this point, so it's not evidence of a significant self-perpetuating process afterwards. [Gwenaelle GREMION, Canada]	Rejected - the topic is out of the scope of the chapter (should be covered in Chapter 9)
51556	58	52	59	2	I find a discussion about heat production by biogenic CH4 production is too detailed and uncertain for this report. I suggest delete these lines. [Christian Beer, Germany]	Rejected - this discussion focusses on important and still unquantified process; nonetheless, the sentence is clarified - see also reply to comment 23052
25474	59	4	59	4	In permafrost regions, gas hydrates would be mostly found at depth >200m (require permafrost >200m thick to have temperature conditions for their stability). The timing of release of methane would be different then that from peatlands - longer time scales. Shouldn't hydrates be discussed in Secion 5.4.8.3 which covers clathrates (same as gas hydrate). Also there are hydrates in ocean bed where pressure and temperature combination is sufficient for their stability (i.e don't necessarily need permafrost and associated colder conditions as high pressure is also a factor in hydrate occurrence along with the supply of gas) [Sharon Smith, Canada]	Accepted - the references on possible existence of methane hydrates at much shallower depth is added
23056	59	4	59	7	There doesn't appear to be much direct observational confirmation provided of relict methane hydrates existing below permafrost (with Arzhanov et al, 2016; 2017 both being modelling studies) beyond methane gas detection in the craters, so I'd consider there to be quite a high uncertainty on this possibility. [Gwenaelle GREMION, Canada]	Taken into account - combined with comment 25474
23058	59	9	59	10	I agree with high confidence on permafrost carbon release, but low confidence on timings and magnitude seems slightly underconfident - there seems to be at least some agreement amongst the model study release ranges by 2100 quoted earlier in this section, even if they're relatively wide ranges with some processes not fully resolved yet. [Gwenaelle GREMION, Canada]	Rejected - while overall emissions basically agree among different estimates, their timing and pathways are uncertain
24662	59	9	59	10	It would be useful to give an assessed range for the permafrost carbon releases. [William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account (rejected) - see reply to comment 36412

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
36412	59	9		10	Can you provide an assessed range for the amount of CO2 and CH4 released by thawing permafrost by e.g. 2100 under a given scenario? [Nathan Gillett, Canada]	Rejected - the ranges for RCP8.5 are already provided in the first paragraph of this Section
17692	59	10	59	10	Insert blank line(s) [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
23060	59	13	59	13	The 10 TgCH4 figure for clathrates is actually from Sauniois et al (2016a) - 2016b doesn't directly discuss hydrates. 2016a also revises down the figure to 0-5 TgCH4 from all hydrates (not just shelf, although shelf dominates) in their budget, so <5 TgCH4 from all clathrates might be more accurate. [Gwenaelle GREMION, Canada]	Rejected - (Sauniois et al., 2016a) considers oceanic clathrates (their Sect. 3.2.6), and their estimate includes CH4 flux from these source; the figure in our Chapter is kept unchanged because of large uncertainty in the (Sauniois et al., 2016b) estimate
24664	59	13	59	13	What evidence is this clathrate release based on? Do Sauniois et al. review several studies or is this just their best guess? The last sentence of this paragram suggests a flux of less than 5 Tg/yr. This paragraph should assess the evidence and either come up with an AR6 estimate or state that a best estimate isn't possible. [William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - Sauniois et al. (2016b) review several sources and come up with the best estimate which is possible at the date; estimates in the last and first sentences in this paragraph are, formally, consistent
23062	59	13	59	15	A further citation for the long response time and millennial timescale release (not mentioned in Sauniois 2016a/b directly) might also be useful in the second sentence (extra suggested citations are in the next comments). Alternatively, this second sentence could be moved until after the next few sentences discussing slow release rates and magnitudes with citations, so that this statement is already supported. [Gwenaelle GREMION, Canada]	Accepted - the references to (Malakhova, Eliseev, 2017; Minshull et al., 2016) are added
27984	59	13	59	16	Page 59, lines 13 - 16: There is use of Petagrams and then it switches to Teragrams. Might be good to remain consistent between the two. [roderik van de wal, Netherlands]	Rejected - it is difficult to unify the units because of large separation of the orders of magnitudes for estimates of different fluxes
47768	59	13	59	24	Please check the use of this IPCC uncertainty language term. Are you able to provide a traceable account to assigning this uncertainty statement? Note that likelihood statements are quantified terms - phrases like likely and very likely have quantifiable probabilities associated with them. Please check it has been used correctly here. Please refer to the IPCC guidance note on uncertainty: <a href="https://wg1.ipcc.ch/SR/documents/ar5_uncertainty-guidance-note.pdf">https://wg1.ipcc.ch/SR/documents/ar5_uncertainty-guidance-note.pdf</a> [WGI TSU, France]	Accepted - the wording is checked and clarified
25476	59	14	59	15	See previous comment - Subsea hydrates are not just found in relict shelf permafrost but can be found in deeper ocean where pressure high enough for their existnece. [Sharon Smith, Canada]	Accepted - the respective sentence is added
23064	59	16	59	17	This was initially confusing, as Archer et al (2009) project 450 PgC in methane for 1000 PgC fossil fuels (with 600 PgC in methane for 5000 PgC fossil fuels), but I see that you've converted 450 PgC in methane to 600 PgCH4, which is fair enough. However, it might be worth adding that this is over a 10 Ky simulation, so as to clarify its a slow non-catastrophic methane burden and to support the next statement on multi-millennium timescales. [Gwenaelle GREMION, Canada]	Accepted

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
23066	59	17	59	18	In response to Zeebe (2013), a paper of ours further analysed the scenario in this paper with a more realistic methane hydrate model, and found that methane release would likely be even slower than he projected (Minshull et al, 2016; GRL; doi:10.1002/2016GL069676). This further reinforces the case for multi-millennium releases. [Gwenaelle GREMION, Canada]	Accepted - the reference to (Minshull et al., 2016) is added; in other respects the existing text ('plausible at multi-millennium and longer scales') does not contradict to the comment
28432	59	18	59	18	Missing key word(s) after "associated with a slow" [Claude-Michel Nzotungicimpaye, Canada]	Not applicable - the sentence is removed
23068	59	18	59	19	This sentence seems misplaced - having just discussed how centennial timescale releases are unlikely for clathrates, suddenly here multi-decadal to centennial timescales are mentioned as possible again and referred to as "slow" with no examples or citations given. If multi-decadal timescales are to be discussed, a good reference would be Marín-Moreno et al (2013; GRL; doi:10.1002/grl.50985) in which future shelf clathrate release for anthropogenic warming up to ~2300 is modelled and found to be small and gradual. [Gwenaelle GREMION, Canada]	Taken into account - combined with comment 28432
23070	59	19	59	20	2000 PgC is said to be smaller than previously estimated (N.B. Ruppel & Kessler, 2017 actually have a best estimate of ~1800 PgC, excluding uncertain Antarctica), but the widely used and previously cited Archer et al (2009) estimate 1600-2000 PgC total global clathrates, so this mostly just confirms that figure. Other studies mentioned in Ruppel & Kessler (2017) and similar to their estimate are Piñero et al 2013 (500-2300 PgC), Johnson 2011 (~1800 PgC), and Boswell and Collett 2011 (~1500 PgC), while subsequently cited Kretschmer et al (2015) model ~1200 PgC. Perhaps this sentence could then simply be rephrased as "studies using multiple different methods estimate a total global clathrate reservoir of approximately 1500-2000 PgC (Archer et al, 2009; Ruppel & Kessler, 2017)" or similar, and could even be moved to the start of the section to provide context of how much carbon is stored versus how slowly it comes out. [Gwenaelle GREMION, Canada]	Accepted
23072	59	23	59	23	The figure from Kretschmer et al (2015) (~4.73 TgCH <sub>4</sub> /year) strongly supports the <5 TgCH <sub>4</sub> range from Sauniois et al (2016a), so it might be worth reciting the latter again here, or citing Kretschmer et al (2015) at the start alongside Sauniois et al (2016a) instead. [Gwenaelle GREMION, Canada]	Accepted

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
23074	59	24	59	24	I would again emphasise the importance of clarifying whether or not each element in this section meets the criteria for being a tipping point, specifically if there's evidence of some sort of critical threshold beyond which self-perpetuating processes are activated leading to a substantial state shift. Clathrates have been described as a slow tipping point of the Earth system before (notably by Archer et al (2009), Lenton et al (2008), and in the context of the PETM), although it is not yet clear if there is a specific global threshold beyond which significant dissociation occurs or whether it is better thought as a simple feedback response on global warming (with greater warming triggering more dissociation in the future in a quasi-linear relationship). It's also not yet clear to what extent clathrate dissociation might become self-perpetuating, or whether the long lag between atmospheric warming and warming reaching clathrates inevitably creates a long-term committed dissociation period. However, given that many studies have discussed clathrates in terms of a carbon cycle capacitor that discharged after some warming threshold and destabilised the carbon cycle during palaeo hyperthermal events (e.g. Dickens, 2003, EPL, doi:10.1016/S0012-821X(03)00325-X; Lunt et al, 2011, NatGS, doi:10.1038/ngeo1266; Armstrong-McKay & Lenton, 2018, CotP, doi:10.5194/cp-14-1515-2018) and that even if atmospheric warming ceased just passed such a threshold one would expect gradual ocean and sediment warming to carry on driving hydrate dissociation independent of the original forcings current behaviour, I believe one can indeed propose clathrates as a potential slow tipping point under the briad definition. But I think the reasoning why needs to be clarified and emphasised, perhaps with an additional sentence describing the potential threshold behaviour and long-term dissociation commitments, for example: "Although there is no known global warming threshold for large-scale clathrate dissociation,	Accepted
23076	59	24	59	24	I think this final statement needs to have "over the next few centuries" or similar added at the end to clarify the timescale this confidence/likelihood statement is for. The uncertainty itself is about right, although it could be argued that confidence in clathrates not emitting much this century is potentially bordering on medium-to-high (with high agreement on limited-to-medium evidence). [Gwenaelle GREMION, Canada]	Accepted - see also reply to comment 36414
36414	59	24			Is this true just on centennial timescales, or for the anthropogenic response on all timescales? [Nathan Gillett, Canada]	Accepted - the note that it is valid over the next few centuries is added
47104	59	27	59	39	5.4.8.4.: Conclusive statment written in cofidence language is missing. [Sophie von Fromm, Germany]	Not applicable - section is removed
23078	59	27	59	39	The increased PP due to a shallower mixed layer depth, increasing light availability, which has been discussed to be important for the Southern Ocean should be discussed.  Bopp L, Resplandy L, Orr JC et al. (2013) Multiple stressors of ocean ecosystems in the 21st century: projections with CMIP5 models. Biogeosciences Discussions, 10, 6225–6245. [Gwenaelle GREMION, Canada]	Not applicable - section is removed

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
23080	59	27	59	39	The increased iron fertilisation by ice bergs due to increased calving is missing as an important factor affecting PP here.  Duprat, L. P., Bigg, G. R., & Wilton, D. J. (2016). Enhanced Southern Ocean marine productivity due to fertilization by giant icebergs. Nature Geoscience, 9(3), 219. [Gwenaëlle GREMION, Canada]	Not applicable - section is removed
13360	59	27	59	39	It should be added that the Southern Ocean carbon sink has weakened again since 2012 due to shifts in regional wind patterns (see Keppler & Landschuetzer, 2019; doi:10.1038/s41598-019-43826-y). [Lydia Keppler, Germany]	Not applicable - section is removed
36416	59	27		39	This proposed tipping point due to carbon uptake suppression in the Southern Ocean does not appear to be well founded in the literature. The studies cited discuss changes in ocean carbon uptake associated with wind stress changes, but not a tipping point. Why would such changes constitute a tipping point? Are they abrupt and/or irreversible? Also, wouldn't the relevant processes already be included in ESMs? In which case I recommend also assessing the evidence that these models show the kind of abrupt and irreversible changes in Southern Ocean carbon uptake and climate characteristic of a tipping point. [Nathan Gillett, Canada]	Taken into account - combined with comment 47378
47378	59	27			can you clarify why this is a threshold or tipping point? It reads like an important, but not non-linear or abrupt, process which affects the sink. Is it really a tipping point in the context of the other sections here? Or should this go under the process/feedbacks section? [Chris Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - the section is removed because it is not enough evidence that these processes constitute a tipping point
23082	59	29	59	39	The paragraph lacks of any term about the confidence or likelihood of the assertions and including them would be very helpful for the readers. [Gwenaëlle GREMION, Canada]	Not applicable - section is removed
29422	59	33	59	35	I would be careful here with the mechanism. Maybe it is just my limited understanding, but I can't see how you can have reduced upwelling while the westerly winds keep increasing. Is there any physical model of the Southern Ocean that reproduce this? [Judith Hauck, Germany]	Not applicable - section is removed
17694	59	35	59	35	Delete hyphen [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
13358	59	36	59	37	The referred to "Section 5.2.2.3.1" does not exist. [Lydia Keppler, Germany]	Not applicable - section is removed
47786	59	38	59	38	IPCC uncertainty language used incorrectly: a confidence statement (e.g., high/medium/low confidence) is made up of 2 clauses (evidence and agreement), which must be used together. Please refer to the IPCC guidance note on uncertainty for correct use of terms: <a href="https://wg1.ipcc.ch/SR/documents/ar5_uncertainty-guidance-note.pdf">https://wg1.ipcc.ch/SR/documents/ar5_uncertainty-guidance-note.pdf</a> [WGI TSU, France]	Not applicable - section is removed
29424	59	38	59	38	one might or might not want to add a sentence: However, a reduction of Southern Ocean physically-driven CO <sub>2</sub> -uptake leads to stronger CO <sub>2</sub> uptake elsewhere, reaching 90% compensation after 200 years (Hauck et al., 2018, <a href="https://doi.org/10.1029/2018GB005977">https://doi.org/10.1029/2018GB005977</a> ) [Judith Hauck, Germany]	Not applicable - section is removed

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
32760	59	44	59	44	Need to change "Predicting" to "Projecting" as it is a forecast conditioned on human choices and not inevitable. [Michael MacCracken, United States of America]	Accepted
23084	59	50	59	50	A couple of more recent references for these metrics being used palaeoclimate records could be added here, as 2008 is a little while ago now for a 2021 report and to bolster the statement that they've been found in multiple records. Some examples to choose from include: Lenton (2011) as a review with several examples; Lenton et al (2012a, RS-TA, doi:10.1098/rsta.2011; 2012b, CotP, doi:10.5194/cp-8-1127-2012) for more advanced deglaciation examples; & Armstrong-McKay & Lenton (2018, CotP, doi:10.5194/cp-14-1515-2018) for resilience metrics across the PETM. However, there have also been some reasonable critiques of this method, e.g. Ditlevsen & Johnsen (2010, GRL, doi:10.1029/2010GL044486) & Boettiger & Hastings (2012a, JRSI, doi:10.1098/rsif.2012.0125; 2012b, PRSB, doi:10.1098/rspb.2012.2085), so it might also be worth stating that there is some uncertainty on these findings (even if the critiques are not cited, as it's too method discussion specific). [Gwenaelle GREMION, Canada]	Accepted
7514	59	50	59	50	Typo: "temperature or CO2" not "temperature of CO2"? [Rose Abramoff, France]	Accepted - text revised
17696	59	51	59	51	Edit to '..the AR5 there was, however, no...' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
42490	59	54	59	54	What are these Ocean tipping points in the context of the ocean? Figure 5.30 does not include an ocean tipping point. [Peter Croot, Ireland]	Not applicable - the section on oceanic tipping points is removed
23086	59	54	59	55	Technically Drijfhout et al (2015) sought out abrupt changes (defined using the AR5 definition of "A large-scale change in the climate system that takes place over a few decades or less, persists (or is anticipated to persist) for at least a few decades, and causes substantial disruptions in human and natural systems") with their method rather than directly identifying tipping points. Abrupt shifts and tipping points are linked and often overlap, but some tipping points can be too slow to be abrupt in this definition (e.g. West Antarctic Ice Sheet) and not all abrupt shifts may be self-perpetuating (i.e. if the forcing was released some abrupt shifts may be able to stop, in contrast to the earlier IPCC tipping point definition). To identify tipping points from these abrupt shifts would require mechanistic diagnosis of critical thresholds and subsequent self-perpetuating mechanisms for each as well as the substantial state shift. While this subsection is titled "Abrupt changes detected in ESM projections" rather than specifically about tipping points, I think it's worth making sure that it's clear that they're not exactly the same thing by updating it to something like "...identified a number of abrupt changes in the CMIP5 ensemble which may be the result of tipping points being reached". It's important to make sure that definitions are maintained and clear throughout the whole tipping point section, as it's a field with many alternative and sometimes unclear definitions which the IPCC can help clarify. [Gwenaelle GREMION, Canada]	Accepted

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
32762	60	1	60	1	Are not losses of Greenland and Antarctica the most obvious potential bifurcations? Paleo evidence makes clear it takes perhaps ten times as long to build up an ice sheet as for it to deteriorate, plus it would take other favorable conditions (e.g. rebound of land, specific orbital parameters, etc.). [Michael MacCracken, United States of America]	Rejected - the topic is out of the scope of the chapter (should be covered in Chapter 9)
27784	60	5	60	5	attend the placeholders [Poot Delgado Carlos Antonio, Mexico]	Not applicable – the figure is excluded
17698	60	6	60	6	Italicise 'medium confidence' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
23088	60	6	60	8	While the uncertainty language is fine (medium agreement on medium evidence), I'd perhaps clarify that while the magnitude of the impact on atmospheric GHG concentrations has limited evidence, the overall evidence suggests that the biogeochemical abrupt shifts would tend to lead to an increase in GHG and act as positive feedbacks. [Gwenaelle GREMION, Canada]	Rejected - insufficient evidence
23090	60	6	60	8	The uncertainty language in this sentence ("medium confidence" and "limited evidence") should be italicized. [Gwenaelle GREMION, Canada]	Accepted
47784	60	7	60	7	IPCC uncertainty language used incorrectly: a confidence statement (e.g., high/medium/low confidence) is made up of 2 clauses (evidence and agreement), which must be used together. Please refer to the IPCC guidance note on uncertainty for correct use of terms: <a href="https://wg1.ipcc.ch/SR/documents/ar5_uncertainty-guidance-note.pdf">https://wg1.ipcc.ch/SR/documents/ar5_uncertainty-guidance-note.pdf</a> [WGI TSU, France]	Accepted - 'evidence' place by 'confidence' in the second instant
7516	60	11	60	18	Maybe this figure isn't properly rendered but I do not see abrupt changes. [Rose Abramoff, France]	Not applicable – the figure is excluded
17700	60	13	60	13	Change 'system' to 'System' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
23092	60	13	60	16	I know this is a placeholder figure before CMIP6 (and it's a good idea for a figure), but as it currently stands the result curves shown in Figure 5.30 don't show very many obviously abrupt shifts (especially for boreal and tropical forests), with many tracking quite linear or gradual paths. Perhaps the more abrupt ones could be highlighted somehow, maybe in a different colour? Drijfhout et al (2015) picked out some convincing CMIP5 abrupt shifts for vegetation (types 11-13 in Fig.3) although most of these are just from HadGEM2-ES which suggests model-dependence. Hopefully CMIP6 shall resolve some of this when complete... [Gwenaelle GREMION, Canada]	Not applicable – the figure is excluded
47380	60	13			figure 5.30 looks unconvincing in terms of tipping points. None of those time series (esp the bottom two) look like tipping points, thresholds or irreversible in anyway. Are there better examples? (see aforementioned Good et al 2011, J, Clim paper for an example of a diagnosed threshold in tropical forest existence in terms of dry season length - their figures 2 or 3) [chris jones, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable – the figure is excluded
27786	60	16	60	16	attend the placeholders [Poot Delgado Carlos Antonio, Mexico]	Not applicable – the figure is excluded

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
47382	60	21			great to see a section on long-term response beyond 2100. Some thoughts on what else this could include: long-term committed changes in vegetation (Jones et al. 2009, Nature Geosci.; Jones et al 2010, Tellus and more recently Pugh et al 2018; Earths Future) - these all show that veg dynamics play a role possibly as important as permafrost for long-term response. Davies Barnard et al (2015, Glob Biogeochem Cycles) showed natural veg cover changes can be at least as big as those from anthro land-use, and this will become progressively more the case on longer timescales. Also, do you include anything on reversibility in this section? there will (eventually!) be new CMIP6 runs out to 2300 for SSP5-85 and SSP5-34 both fully coupled and biogeochemically coupled, so that carbon cycle feedbacks and benefits of mitigation can be assessed [Chris Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted: Some discussion of these references made in 5.4.3.2, and some analysis of SSP5.8.5-ext and SSP5-3.4-overshoot-ext now here.
47854	60	23	60	33	Is 'Sequestration potential' the same as 'mitigation potential' used in chapter 2 and 6 of the SRCL? For example please see Figure 2.24 in Section 2.7 of the SRCL? Could a cross-reference for clarity be included here? [WGI TSU, France]	Not Applicable to this section
23094	60	23	60	37	The paragraph lacks of any term about the confidence or likelihood of the assertions and including them would be very helpful for the readers. [Gwenaelle GREMION, Canada]	Accepted: have added confidence / likelihood statements
23096	60	24	60	24	1% of what? [Gwenaelle GREMION, Canada]	Accepted: Text Revised
26522	60	24	60	24	this should be 1% CO2 [Nadine Goris, Norway]	Accepted: Text Revised
36418	60	26		29	Other studies have looked at this. See for example Figures 1c and d in Gillett et al. (20011; <a href="https://www.nature.com/articles/ngeo1047">https://www.nature.com/articles/ngeo1047</a> ) which show ocean and land carbon in zero emissions simulations run to year 3000 from CanESM. [Nathan Gillett, Canada]	Rejected: ZEC and ZECMIP now discussed in section 4.7.2 and 5.5.1.2, and a pointer to those assessments is now here.
23098	60	33	60	37	It might be worth moving the Schneider von Deimling et al (2015) citation from line 36/37 to the end of the sentence on line 35, as at the moment it read to me like the permafrost release values for rcp8p5 came from McGuire et al (2018) with only the rcp2p6 values coming from Schneider von Deimling et al. [Gwenaelle GREMION, Canada]	Accepted: have clarified the text
23100	60	35	60	35	The 0.23K increase is the maximum possible increase under rcp8p5, with the whole range being 0.11-0.23 and median of 0.16K - either the range should be used or "up to" inserted before "a 0.23K increase". [Gwenaelle GREMION, Canada]	Accepted: Text Revised
27788	60	39	60	39	attend the placeholders [Poot Delgado Carlos Antonio, Mexico]	Accepted: Text Revised
35402	60	42	61	15	Be more specific about the kind of temperature change you are referring to here. This is CO2-induced temperature change only. Especially on page 60, line 45, and page 61 line 13, this should be made explicit. Otherwise this metric can be misunderstood. [Nadine Mengis, Canada]	Taken into account - Partially correct, because studies (Stocker et al, 2013; Meinshausen et al, 2009) showed that this is even the case when taking into account all greenhouse gases. We have edited the statement to make sure this is clear.



Comment ID	From Page	From Line	To Page	To Line	Comment	Response
26414	60	42	68	30	<p>From a methodological perspective, the assessment of the remaining carbon cycle strikes me as very sophisticated and excellent. However, I miss a crucial piece of assessment concerning the changes that occurred from the AR5 to the SR1.5. As correctly stated here (5-66, lines 8,9), the SR1.5 did a comparison of more recent results with the AR5, but neither in the SR1.5 nor here is a proper explanation given of WHY the CMIP5 models underestimated the remaining carbon budget. The SR1.5 was under severe time constraints and might hence have been unable to present this explanation. But I think it's imperative that the current chapter provides it.</p> <p>Footnote 14 of SR1.5 SPM says: "Irrespective of the measure of global temperature used, updated understanding and further advances in methods have led to an increase in the estimated remaining carbon budget of about 300 GtCO<sub>2</sub> compared to AR5 (medium confidence) [2.2.2]." But this number does not show up in the underlying pre-Incheon SR1.5 report; substantial material, including this number, was added post-Incheon (trickle-back). Moreover, there is no straightforward way to construct the 300 GtCO<sub>2</sub> from any of the numbers given for individual contributions, meaning that the 300 GtCO<sub>2</sub> have unclear provenance. This leaves even an informed climate researcher who, however, is not an expert on carbon budgets scrambling for an explanation.</p> <p>Qualitatively, the main SR1.5 text does say that the models assessed in AR5 showed discrepancy to observations in their relationship between cumulative emissions and warming. The simple fact that referencing the remaining budget to the most recent observations makes a difference demonstrates that something is not quite right with the connection</p>	Accepted - A dedicated box on how the assessment has been updated from AR5 to AR6 has been included.
26416	60	42	68	30	<p>(continued) There are thus sufficiently many question marks left in the AR5 and the SR1.5 that a proper joint evaluation is required: Do CMIP5 models overestimate the airborne fraction? Is the subsample of models that have an interactive carbon cycle biased toward high TCR? Has any of that changed in CMIP6? This chapter should provide clear answers to these questions. [Jochem Marotzke, Germany]</p>	Accepted - A dedicated box on how the assessment has been updated from AR5 to AR6 has been included.
56634	60	42			<p>Congratulations on a great section overall on the remaining carbon budgets. [Malte Meinshausen, Australia]</p>	Accepted

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
35192	60	42			This is a particular framing of budgets. Budgets can also be calculated from emission pathways, such as ESMs in WG1 or IAM scenarios linked to a simple climate model or emulator in WG3. The TCRE approach is an alternative. The text did not convince why this approach was superior, and it also includes elements of the WG3 approach for nonCO2. There is also some circular argument, run ESM on pathway to get TCRE, deduct nonCO2, then use budget to calculate pathways. I am not sure this approach does not remove any statistical dependencies either. Basically, there is nothing wrong with the text, but there is no "this literature shows that method X is inferior, therefore, in this section we take method Y as these studies have shown is superior". Of course, you could use multiple approaches, instead of just ESM / TCRE approaches? [Glen Peters, Norway]	Rejected - This discussion was already included in "5.5.2.1 Framework and earlier approaches" and we have included more text that section explaining why the chosen approach is preferred to other approaches in the literature. The reviewer seems to have misunderstood that TCRE is not simply derived from running ESMs "on pathways", but is the result of an assessment of multiple lines of evidence. Typically, TCRE is not derived from pathways that include non-CO2 contributions except from individual cases where CO2 warming is isolated from the historical observational record.
23112	60	45	60	46	Also MacDougall, A.H., Swart, N.C., and Knutti, R. (2017). The uncertainty in the transient climate response to cumulative CO2 emissions arising from the uncertainty in physical climate parameters,, J. Climate 30(2), 813-827, doi:10.1175/JCLI-D-16-0205.1 [Gwenaelle GREMION, Canada]	Rejected - A study published in 2017 does not qualify as "Science at the time of the IPCC AR5", which was published in 2013/2014.
42116	60	45	60	52	Section 5.5: Remaining Carbon Budgets. There is a Nature Geoscience study calculating remaining carbon budgets not currently cited in the following sentence:  "This relationship is now used to estimate the amount of CO2 emissions that would be consistent with limiting global average temperature increase to specific levels (Allen et al., 2009; Collins et al., 2013; Knutti and Rogelj, 2015; Matthews et al., 2009, 2012; Meinshausen et al., 2009; Rogelj et al., 2016; Stocker et al., 2013b; Zickfeld et al., 2009) noting that this relies on more than CO2 emissions only (Meinshausen et al., 2009; Mengis et al., 2018; Rogelj et al., 2015a, 2015b, 2016; Simmons and Matthews, 2016; Tokarska et al., 2018)"  The study Goodwin et al. (2018) should be cited here as a significant study that uses the TCRE to constrain carbon budgets to restrict warming to a given target (and also this study should be used in the TCRE estimates, see next point below).  Full reference:  Goodwin, P., A. Katavouta, V.M. Roussenov, G.L. Foster, E.J. Rohling and R.G. Williams, (2018) Pathways to 1.5 and 2 °C warming based on observational and geological constraints, Nature Geoscience 11, 102-107, doi:10.1038/s41561-017-0054-8. [Philip Goodwin, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - This study has been included in this overview.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
36420	60	46		47	TCRE is the metric defined as the ratio of warming to cumulative emissions, usually at doubled CO2 in 1PCTCO2. It is not the name of the proportional relationship between warming and cumulative emissions. [Nathan Gillett, Canada]	Accepted - The use of the term TCRE has been checked for consistency throughout the entire section.
23114	60	47	60	47	Removed "or" and have TCRE in brackets [Gwenaelle GREMION, Canada]	Accepted - text revised
28434	60	50	60	51	I think that the words "more than" need to be deleted in this sentence [Claude-Michel Nzotungicimpaye, Canada]	Rejected - They are necessary as total global average temperature increase relies on more than CO2 emissions only. The sentence has been edited to result in less confusion.
9548	60	52	60	54	I found this sentence unclear, presenting an opaque and confusing definition of the remaining carbon budgets. I would suggest to define it more straightforwardly. [Katarzyna (Kasia) Tokarska, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - The sentence was edited, but the definition was unchanged. We don't understand what would be opaque or confusing about the definition: "The remainder of CO2 emissions that would be in line with limiting warming to a specific temperature threshold is referred to as the remaining carbon budget."
24666	60	52	60	54	Chapter 7, figure 7.12 suggests that only 60-65% of the GHG warming to date has come from CO2. [William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - the sentence has been edited to read that most of the warming to date is due to cumulative emissions of CO2, with a reference to Chapter 7.
6710	61	6	61	6	TCRE was defined in AR5 as the 'Transient Climate Response to cumulative CO2 Emissions'. Not 'Transient Climate Response to cumulative carbon Emissions'. Using the second definition is inconsistent with previous reports and annoys those who study atmospheric methane. [Andrew MacDougall, Canada]	Accepted - This has been edited consistent with the definition in AR5.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
25684	61	6	68	34	<p>The TCRE concept, increase in global temperature <math>\Delta T</math> proportional to integrated emissions, or constant ratio of <math>\Delta T</math> to Integrated emissions, would seem to be an artifact of emissions increasing approximately exponentially, typically characterized as 1% per year increase in emissions. It cannot be expected to hold for a different emissions trajectory or if the emissions trajectory substantially changes. Consider an emission of a pulse of CO<sub>2</sub> of short duration followed by zero emissions. <math>\Delta T</math> will gradually increase and then, as CO<sub>2</sub> decreases, <math>\Delta T</math> will ultimately decrease. As integrated emissions is a constant, TCRE will likewise first increase, then decrease. So TCRE cannot be considered a constant. QED.</p> <p>The non-constancy of TCRE was demonstrated, for example by Zickfeld et al (ERL, 2016) in which they examined various profiles of emissions including cessation and negative emissions (the latter decreasing the integrated emissions). In the cessation case <math>\Delta T</math> continues to increase for a time; ultimately <math>\Delta T</math> would start to decrease once CO<sub>2</sub> is taken up by sinks. But the abscissa, the total emissions is a constant. So temp response cannot be a function solely of total prior emissions. Figure 1 d of Zickfeld shows TCRE varying by as much as a factor of 2 for the scenarios examined. So the concept has no foundation in science and has no utility for any planning of climate futures in a reduction scenario.</p> <p>I suggest replace entire section with a brief statement of the above and that the report state that TCRE has no predictive value for situations in which the pattern of emissions would substantially change.</p> <p>Zickfeld, K., MacDougall, A. H., and Matthews, H. D. (2016). On the proportionality between global temperature</p>	<p>Rejected – The TCRE concept has been demonstrated in simulations with exponential (e.g. Matthews et al. 2009), linear (e.g. Krasting et al. 2014), and bell shaped emission trajectories (e.g. MacDougall et al. 2019), as well as for RCP scenarios (e.g. Tokarska et al. 2016). The concept has been shown to be consistent with the observational record of temperature and CO<sub>2</sub> emissions (Gillett et al. 2013). A deep theoretical understanding of the origins of the linearity and path-independence of the TCRE has been established (MacDougall and Friedlindstein, 2015, Goodwin et al. 2015, MacDougal 2017, Sheshadri, 2017). One of the very first papers that examined TCRE, Matthews et al. 2009, examined pulse experiments and showed that after an adjustment period of a few years the near constant TCRE relationship emerges. In transient simulations TCRE is expected to hold except for very high and very low emission rates (MacDougall, 2017, Sheshadri, 2017).</p> <p>The reviewer conflates several concepts that describe the relationship between</p>
23424	61	10	61	15	<p>In this paragraph, it is commented for the first time in this section the radiative forcing from CO<sub>2</sub>. However, for a non-expert in the subject and considering that this concept is taken up several times throughout the section, I suggest adding at least one or two lines to briefly explain the concept of radiative forcing or just to emphasize its importance when considering the TCRE relationship taking the radiative forcing from CO<sub>2</sub> only and/or the radiative forcing for other non-CO<sub>2</sub> greenhouse gases. [Gwenaëlle GREMION, Canada]</p>	<p>Rejected - Radiative forcing is a term that is defined in the glossary and is discussed extensively in Chapter 7. We choose not to duplicate this effort.</p>

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
41694	61	10	61	53	<p>No mention is made here of the mechanisms leading to inter-model differences in the TCRE. This omission should be addressed somewhere in this section or linked to Table 5.6</p> <p>By exploiting a mechanistic framework separating the TCRE into contributions from thermal changes and carbon changes, the inter-model uncertainty in the TCRE based on diagnostics of 10 CMIP5 models for 1% CO2 increase is found to be dominated by the uncertainty in the thermal contribution for the first 80 years and then subsequently also affected by the uncertainty in the carbon contribution (Williams et al., 2017); the uncertainty in the thermal contribution is dominated by the uncertainty in the climate sensitivity. When this uncertainty analysis is repeated for 9 CMIP5 Earth system models following RCP8.5, the uncertainty in the surface warming dependence on cumulative carbon emissions is found to be dominated again by the uncertainty in the thermal contribution until year 2020 and then there are comparable contributions from the uncertainty in the carbon contribution (Williams et al., 2017). The uncertainty in the thermal contribution is dominated initially by the uncertainty in ocean heat uptake (for the first 15 years) and then by the uncertainty in climate sensitivity.</p> <p>Reference: Williams, R. G., V. Roussenov, P. Goodwin, L. Resplandy, and L. Bopp (2017), Sensitivity of global warming to carbon emissions: Effects of heat and carbon uptake in a suite of Earth system models, <i>Journal of Climate</i>, 30(23), 9343–9363, doi:10.1175/JCLI-D-16-0468.1. [Ric Williams, United Kingdom (of Great Britain and Northern Ireland)]</p>	Accepted – A number of studies have now used analytical frameworks to assess the sources of uncertainty in TCRE. A paragraph has been added to section 5.5.1.3
13590	61	12	61	15	<p>This is an interesting but hardly surprising result for equilibrium changes. Temperature change is proportional to radiative forcing (RF) which is proportional to the log of the increase in atmospheric carbon dioxide. For small emissions, if we assume RF is directly proportional to CO2 concentrations, then I can see clearly why T change should be proportional to cumulative emissions if the air-borne fraction is constant. For transient scenarios, suppose 1000 PgC of carbon is emitted. This should mean 250 ppm increase in atmospheric CO2 assuming air borne fraction of 0.5. Again, if we assume linearity between CO2 and RF for this small increase, a linear relation between transient warming and CO2 increase is immediately implied. Am I missing something? [Govindasamy Bala, India]</p>	Noted – While such an argument holds for small changes in CO2, it does not explain a linear relationship that holds to 2000 (Gillett et al. 2013) or 5000 PgC of cumulative CO2 emissions (Tokarska et al. 2016). Without other factors changing the logarithmic forcing from CO2 should make the cumulative emissions versus temperature curve logarithmic. However, ocean heat uptake (MacDougall and Friedlingstein, 2015, Goodwin et al. 2015) and the airborne fraction of carbon (Goodwin et al. 2015, MacDougall, 2017) change in such a way as to produce a linear relationship independent of the pathway of emissions.
17702	61	14	61	15	<p>Move references to end of sentence [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]</p>	Accepted - text revised

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
23116	61	22	61	23	Also Herrington, T. and Zickfield, K. (2014). Path independence of climate and carbon cycle response over a broad range of cumulative carbon emissions. <i>Earth Syst. Dynam.</i> 5, 409-422. doi:10.5194/esd-5-409-2014 [Gwenaëlle GREMION, Canada]	Accepted - This reference was included for completeness.
17704	61	23	61	23	Change 'Earth-system' to 'Earth System' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - This change would remove the clarifying nature of the capitalized characters for the EMIC acronym
41690	61	23	61	26	The Williams et al. (2017) study should be included in the list of studies diagnosing the response of Earth system models. This study identifies the thermal and carbon controls of the TCRE for 10 CMIP5 Earth system models using 1% annual increase in CO2 and identifies the additional effect of non-CO2 radiative forcing for 9 CMIP5 models using RCP forcing. Reference Williams, R. G., V. Roussenov, P. Goodwin, L. Resplandy, and L. Bopp (2017), Sensitivity of global warming to carbon emissions: Effects of heat and carbon uptake in a suite of Earth system models, <i>Journal of Climate</i> , 30(23), 9343–9363, doi:10.1175/JCLI-D-16-0468.1. [Ric Williams, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - This reference was included for completeness.
17706	61	24	61	24	Change 'Model' to 'Models' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
41688	61	24	61	25	Mention is made of the analytical methods to decompose and examine Earth system model output by Goodwin et al. (2015) and Williams et al. (2016), but their studies should refer to the single equation derived for the TCRE based on theory that is used to diagnose the Earth system models. References Goodwin, P., Williams, R. G. & Ridgwell, A. Sensitivity of climate to cumulative carbon emissions due to compensation of ocean heat and carbon uptake. <i>Nat. Geosci.</i> 8, 29–34 (2015). Williams, R. G., P. Goodwin, V. M. Roussenov, and L. Bopp (2016), A framework to understand the Transient Climate Response to Emissions, <i>Environmental Research Letters</i> , 11, doi:10.1088/1748-9326/11/1/015003. [Ric Williams, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - These studies have been characterized as suggested in this comment.
9292	61	26	61	30	This sentence supplies key explanations to the near-constancy of the TCRE and therefore to the physical validation of the Carbon Budget concept. Would not it be appropriate to give to it a bit more visibility? Also, possible references to other parts of the WG1 that would support said explanations would be welcome. [philippe waldteufel, France]	Taken into account - cross-references have been included. Visibility can be ensure in the ES.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
9626	61	30	61	30	<p>Perhaps include also references to Gregory et al., 2015; Frölicher, T. L. &amp; Paynter, 2015, and Tokarska et al., 2016, who also mention the mechanisms of diminishing ocean heat content in ESMs at high levels of warming.</p> <p>Refs:</p> <p>Gregory, J. M., Andrews, T. &amp; Good, P. The inconstancy of transient climate sensitivity under increasing CO<sub>2</sub>. <i>Phil. Trans. R. Soc. A</i> 373, 20140417 (2015).</p> <p>Frölicher, T. L. &amp; Paynter, D. J. Extending the relationship between global warming and cumulative carbon emissions to multi-millennial timescales. <i>Environ. Res. Lett.</i> 10, 075002 (2015).</p> <p>Tokarska K.B., Gillett N.P., Weaver A.J., Arora V.K., and Eby, M. The climate response to five trillion tonnes of carbon. <i>Nature Climate Change</i>, 6, 851–855 (2016). [Katarzyna (Kasia) Tokarska, United Kingdom (of Great Britain and Northern Ireland)]</p>	Accepted - these additional studies have been added to the supporting list of evidence.
23118	61	31	61	31	The word "decomposes" not appropriate for this context; consider "deconstructs", "dissects", or "breaks down" the TCRE [Gwenaelle GREMION, Canada]	Taken into account - The sentence was edited and doesn't use this word anymore.
17708	61	33	61	33	Change 'to verify' to 'verification of' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
6728	61	33	61	33	Change 'allow to verify' to 'allow verification' [Andrew MacDougall, Canada]	Accepted - text revised
23120	61	33	61	33	Insert "scientists" between "allow" and "to" [Gwenaelle GREMION, Canada]	Accepted - text revised [text modified according to comment 6728 or 17708]

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
27986	61	33	61	33	Page 5-61, line 33: "allow to verify internal consistency between the TCRE assessment and assessment of other factors in this report (Section 5.5.1.3)." It is not immediately clear to me to what other kind factors are referred here. For readability and clarity maybe provide an example or elaborate shortly on these factors. Page 5-61, line 37: "Both studies conducted to date agree that pathway independence is sensitive to the rate of CO2 emissions,..." The part: "conducted to date" feels unnecessary. Since in the previous sentence there is already reference to both studies I would suggest: "Both studies agree that...". Page 5-61, line 19 and line 38. Line 19: " ii) why the relationship is independent of the pathway of CO2 emissions" and line 38: "Both studies conducted to date agree that pathway independence is sensitive to the rate of CO2 emissions, such that pathway independence is expected to breakdown at both very high and very low CO2 emission rates". These two sentences seem to contradict. Maybe add a side note at line 19 that the pathway independence does not hold for all emission rates, or that for most emission rates the relationship is independent of the pathway of CO2 emissions. Page 5-61, line 51-52: "if such feedbacks significantly contribute carbon to the atmosphere." Please rephrase. Maybe a suggestion to change to: "if such feedbacks significantly contribute to the atmospheric carbon content." [roderik van de wal, Netherlands]	Accepted - This sentence was edited so that it becomes clearer what is meant here.
32764	61	36	61	40	It should be mentioned what the effect would be for negative emissions-- would pulling the CO2 concentration back down to 300-350 ppm pull the temperature back to near pre-industrial? Would using SRM to reduce effective forcing bring the temperature back down with similar conditions? [Michael MacCracken, United States of America]	Rejected - This is highlighted in Section 5.6 as well as Chapter 4, Section 4.6 and 4.7.
9688	61	38	61	39	I think saying a "rate of emissions" is redundant and confusing. Emissions itself is a rate (mass per unit time) so please clarify the language here. [Brian Magi, United States of America]	Rejected - While correct that emissions are often understood as being mass per unit time, this doesn't necessarily be the case. In the context of this section, the distinction between annual rates of emissions and total cumulative emissions is essential and hence this slight redundancy in the text is maintained.



Comment ID	From Page	From Line	To Page	To Line	Comment	Response
13592	61	40	61	42	The TCRE applies only to CO2. Is this because of the long life of CO2 and more importantly an almost constant air-borne CO2 fraction on decadal and centennial timescales? Again, this works for equilibrium changes. [Govindasamy Bala, India]	Accepted - Theoretical work suggests that TCRE relationships do not exist for CH4 or N2O due to their relatively short atmospheric lifetimes and the limited solubility of these gases in ocean water (MacDougall, 2017, Sheshadri, 2017). Modulation of the airborne fraction of CO2 emissions by ocean carbon uptake is critical for path-independence of the TCRE relationship (MacDougall and Friedlingstein, 2015, MacDougall, 2017). The statement has been edited for clarity.
36422	61	40		41	Some studies do find that a cumulative emissions metric can be used to a good approximation for other long-lived greenhouse gases - see for example Smith et al. (2012; 10.1038/nclimate1496). [Nathan Gillett, Canada]	Accepted - This was also already highlighted in AR5 Chapter 12, which is referenced here.
9550	61	42	62	29	A conceptual figure or a diagram would be useful here to illustrate the mechanisms behind the linearity of TCRE and how some of them approximately cancel each other out. Perhaps something similar to Box 5.1 Figure 1 but for TCRE instead (and including ocean heat uptake). [Katarzyna (Kasia) Tokarska, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - due to space constraints no such diagram was included here as the text was considered sufficiently clear.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
36424	61	42		43	<p>Even if ocean heat and carbon uptake were driven by the same physical processes, it does not follow that warming will be proportional to cumulative emissions. First, cumulative emissions do not just depend on ocean carbon uptake - land carbon change and change in the carbon content of the atmosphere are also important and both vary over time. Second, while the rate of ocean heat uptake is related to the temperature in an energy balance equation, cumulative emissions include the total ocean carbon anomaly - i.e. emissions are related to cumulative ocean carbon uptake, but temperature is related to the first derivative of cumulative ocean heat uptake - and the first derivative of a function is not in general proportional to the function itself. So just because the ocean is involved in carbon and heat uptake, it doesn't automatically follow that warming is proportional to cumulative emissions. Finally the statement is made without any citation of underlying literature. [Nathan Gillett, Canada]</p>	<p>Accepted – 1) The theoretical work of MacDougall, 2017 and Seshadri, 2017 assume that the land-borne fraction of carbon is constant in time. Large deviations in the land-borne fraction from constant values have been shown by other work (e.g. MacDougall and Friedlingstein) to disrupt the path-independence of TCRE and hence are unlikely to be the origin of the path-independence of TCRE.                  2) In the framework of MacDougall, 2017 ocean heat and carbon uptake are treated as diffusing into a semi-infinite half-space. This leads to an ocean heat uptake which is a function proportional to <math>1/\sqrt{t}</math>, while ocean carbon uptake is proportional to the integral of a function proportional to <math>1/\sqrt{t}</math>. The integral of <math>1/\sqrt{x}</math> is <math>\sqrt{x}</math>. These two functions are multiplied together leading to the origin of the rate cancelation in the approximation of MacDougall, 2017. So in this approximation the integral of the function and the function do cause cancelation. Sheshadri, 2017 examines two layer approximation of the ocean with a denser mathematical description</p>

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
41692	61	49	61	50	<p>“The land carbon cycle does not appear to play a fundamental role in the physical origin of the TCRE.” This sentence is rather imprecise. The TCRE depends upon a product of thermal and carbon terms, and the carbon term depends on the change in the saturated ocean carbon inventory and the changes in the actual ocean and land carbon inventories (Goodwin et al., 2016; Williams et al., 2016). In practice on a centennial timescale, the change in the land carbon inventories only makes a relatively small contribution to the TCRE, and instead the TCRE is controlled more by change in the carbon term involving the change in the saturated ocean carbon inventory; see Earth system model diagnostics by Williams et al. (2017). This viewpoint can be equivalently expressed in terms of the TCRE being controlled by the air-borne fraction and the ratio of the saturated ocean and atmospheric carbon inventories (Katavouta et al., 2018). References: Goodwin, P., Williams, R. G. &amp; Ridgwell, A. Sensitivity of climate to cumulative carbon emissions due to compensation of ocean heat and carbon uptake. Nat. Geosci. 8, 29–34 (2015). Katavouta, A., R.G. Williams, P. Goodwin and V. Roussenov, 2018. Reconciling atmospheric and oceanic views of the Transient Climate Response to Emissions. Geophysical Research Letters, 45, 6205-6214, doi.org/10.1029/2018GL077849. Williams, R. G., P. Goodwin, V. M. Roussenov, and L. Bopp (2016), A framework to understand the Transient Climate Response to Emissions, Environmental Research Letters, 11, doi:10.1088/1748-9326/11/1/015003. Williams, R. G., V. Roussenov, P. Goodwin, L. Resplandy, and L. Bopp (2017), Sensitivity of global warming to carbon emissions: Effects of heat and carbon uptake in a suite of Earth system models, Journal of Climate, 30(23), 9343–9363, doi:10.1175/JCLI-D-16-0468.1. [Ric Williams, United Kingdom (of Great Britain and Northern Ireland)]</p>	Accepted – The paragraph has been rewritten to account for this and other comments.
47384	61	49			<p>as per previous comment on earlier section - I dispute this claim that ocean carbon uptake dominates. TCRE depends on heat and carbon response being similar. Heta uptake is clearly dominated by the ocean, but carbon uptake is joint between land and ocean and it is the total which matters. It happens that land uptake tends to behave similarly to ocean uptake (both being driven by CO2 increase). But if the land uptake was double, or zero, then TCRE would be very different. Liekwise if land uptake is very non-linear and departs from the similar behaviour of ocean uptake then TCRE would breakdown. It's dangerous to suggest that TCRE only depends therefore on ocean carbon uptake. Jones and Friedlingstein (in prep - I will provide a copy of the manuscript) explore this explicitly and find contributions to TCRE uncertainty are dominated by LAND carbon uptake - especially the land response to co2 ("beta" in C4MIP terminology) [chris jones, United Kingdom (of Great Britain and Northern Ireland)]</p>	Accepted – Paragraph has been modified to clarify that the ocean is the origin of the linearity and path independence of TCRE while the land carbon feedbacks contribute to its magnitude.
9690	61	50	61	50	<p>Perhaps parenthetically refer to exactly which carbon cycle feedbacks have the greatest potential to break the linearity (or refer to the figure/illustration that is set up as a placeholder currently) [Brian Magi, United States of America]</p>	Taken into account – The sentence has been modified to include a reference to the permafrost carbon feedback, which is the most well studied of terrestrial feedback that affects the value of TCRE.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
23122	61	50	61	51	Are these "carbon cycle feedbacks" here in reference to terrestrial carbon cycle feedbacks? If so, specify in the text. [Gwenaelle GREMION, Canada]	Accepted - It has been specified that this refers to terrestrial carbon cycle feedbacks in particular
27790	62	2	62	2	attend the placeholders [Poot Delgado Carlos Antonio, Mexico]	Noted
35404	62	5	62	5	Heading should probably read: "Sensitivity to cumulative emissions" [Nadine Mengis, Canada]	Rejected - This suggestion removes the reference to CO2 and to the amount of cumulative emissions for not particular reason
44114	62	5	62	5	Section header does not make sense [Sara Kahanamoku, United States of America]	Accepted - text revised
23124	62	5	62	5	5.5.1.2.1 title doesn't make sense; perhaps "Sensitivity to quantifying cumulative CO2 emissions" [Gwenaelle GREMION, Canada]	Taken into account - The title has been changed to "5.5.1.2.1 Sensitivity to amount of cumulative CO2 emissions"
27988	62	5	62	5	This subsection title is not really clearly formulated. I suggest: "Sensitivity to cumulative CO2 emissions", it is shorter and thus more readable and I think it illustrates the section better. [roderik van de wal, Netherlands]	Accepted - This was a typo and has been changed to "5.5.1.2.1 Sensitivity to amount of cumulative CO2 emissions"
41842	62	5	62	5	rewrite title [Marc Aubinet, Belgium]	Accepted - text revised
53470	62	5	62	5	I guess "quantify" should be changed to "quantity" on the title of section 5.5.1.2.1. (Alternatively: amount) [Jan Fuglestedt, Norway]	Accepted - This was a typo and has been changed to "5.5.1.2.1 Sensitivity to amount of cumulative CO2 emissions"
9552	62	5	62	14	This section on sensitivity to emission rate should also indicate whether emissions are increasing monotonically, or if there is a "peak and decline" in emission rate (i.e. an overshoot). In case of scenarios with high levels of overshoot (e.g. MacDougall et al. 2015), the post-overshoot budgets are smaller than overshoot budgets. [Katarzyna (Kasia) Tokarska, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - The section referred to is about the sensitivity to the amount of cumulative CO2 emissions, while the section discussing the rate is situated further down in the chapter. A "peak and decline" in emission rate is not of interest here, but rather the difference between net emissions and net removals of CO2. The latter is discussed in a dedicated section "5.5.1.2.3 Reversibility and Earth system feedbacks"
23126	62	6	62	7	The "for cumulative emissions" stated here isn't quite clear; consider revising to "AR5 assessed that the TCRE remains approximately constant for scenarios with increasing CO2 emissions up to 2000 PgC (Collins et al., 2013)." [Gwenaelle GREMION, Canada]	Taken into account - The sentence was edited to improve clarity.
9554	62	9	62	9	Please note that Tokarska et al. 2016 show that TCRE is linear in some CMIP5 models even up to 5000 PgC (not 3000 PgC as written here) Perhaps edit the sentence to: "at least 3000 PgC, and even up to 5000 PgC (Tokarska et al. 2016)"? [Katarzyna (Kasia) Tokarska, United Kingdom (of Great Britain and Northern Ireland)]	Noted - This is correct, but in the context of this assessment that looks at the remaining carbon budget for policy-relevant temperature levels "at least 3000 PgC" provides more than enough information. The specification for higher levels has not been made explicit.
36426	62	9		10	Tokarska et al. (2016) do not find a decrease in TCRE beyond 3000 PgC emissions in CMIP5 ESMs. [Nathan Gillett, Canada]	Accepted - The sentence has been edited to reflect this variation between studies.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
9556	62	10	62	14	I found this section confusing. The numbers of 360 PgC and 1560 PgC could be put in context (e.g. compared with the total carbon budget, for reference of the order of magnitude). Also, it is unclear what the 95% of peak TCRE value is referring to. It would be useful to clarify that the TCRE window is independent of the emission rate, and whether the emissions need to be monotonically increasing, or if it holds valid for overshoot scenarios too. Perhaps a conceptual/illustrative figure showing the TCRE window/range would be helpful here. [Katarzyna (Kasia) Tokarska, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - The text was revised for clarity. However, no additional figure was added, as this is a minor issue that would not warrant an additional visual item.
9558	62	13	62	14	The referenced paper of Tokarska et al., 2016 shows approximate linearity of TCRE up to 5000 PgC, so I found it confusing being cited here since the "TCRE window" discussed in this section is only until 1560 PgC. Also, since that paper does not include "window" terminology, perhaps delete this reference here to avoid confusion. [Katarzyna (Kasia) Tokarska, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - Tokarska et al (2016) show that TCRE in CMIP5 models can be constant for emissions up to 5000 PgC. The reference is hence adequate to support the statement that "models with a more sophisticated ocean representation support expanding the high-end of this window further". To avoid confusion, we have reworded the text.
9618	62	13	62	14	Please note that the referenced paper of Tokarska et al., 2016 shows approximate linearity of TCRE up to 5000 PgC, so I found it confusing being cited here since the "TCRE window" discussed in this section is only until 1560 PgC. Also, since that paper does not include "window" terminology, perhaps delete this reference here to avoid confusion. [Katarzyna (Kasia) Tokarska, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - Tokarska et al (2016) show that TCRE in CMIP5 models can be constant for emissions up to 5000 PgC. The reference is hence adequate to support the statement that "models with a more sophisticated ocean representation support expanding the high-end of this window further". To avoid confusion, we have reworded the text.
41696	62	16	62	17	The weakening of ocean carbon uptake may be explained in terms of how cumulative carbon emissions leads to an acidifying effect on the ocean: this acidifying effect decrease the ratio of the ocean saturated carbon inventory and the atmospheric carbon inventory, which decreases the carbon contribution to the TCRE (Katavouta et al., 2018). Reference: Katavouta, A., R.G. Williams, P. Goodwin and V. Roussenov, 2018. Reconciling atmospheric and oceanic views of the Transient Climate Response to Emissions. Geophysical Research Letters, 45, 6205-6214, doi.org/10.1029/2018GL077849. [Ric Williams, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - This is one contribution to weakened carbon sinks. Another is simply the increase temperature of the ocean caused by CO2-induced warming. These processes for carbon sink changes should be explained elsewhere in the chapter (e.g. sections 5.4.4 and 5.4.5) so need not be addressed in detail here. A cross-reference was included.
23426	62	16	62	29	I would suggest adding a confidence range to this paragraph. [Gwenaelle GREMION, Canada]	Rejected - This paragraph doesn't mention any range or central value, so the inclusion of a confidence range does not seem warranted. A confidence statement is included where the overall assessment of TCRE is provided.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
32766	62	18	62	18	It is not really the saturation of the CO2 bands that matters--after all, the bands were saturated at preindustrial CO2 level (indeed, this was a criticism of Arrhenius hypothesis). What matters is the altitude at which saturation occurs as this defines the temperature of the back radiation to space and the outward radiation to space. And this only matters because the atmospheric temperature profile is not uniform, but because there is a lapse rate such that changing altitude of saturation changes the temperature radiation occurs (not meaning to imply that the emission is only from exactly where saturation occurs, but over a band as work toward saturation). [Michael MacCracken, United States of America]	Accepted - This is a very technical comment, which is not very relevant. "Saturation of radiative forcing" need not refer specifically to the saturation of absorption bands, but rather is used here to indicate a general tendency for smaller increases in radiative forcing per unit increase of atmospheric CO2 concentration. Nevertheless, the statement was edited to avoid this confusion.
23428	62	18	62	20	Related to the sentence: "At high values of ... carbon sinks". Could this imply that increasing CO2 emissions into the atmosphere could serve as negative feedback for global warming? [Gwenaelle GREMION, Canada]	No, this is not a feedback, which would require some change in response to CO2-induced climate changes. It is just a non-linear forcing relationship as a function of CO2 concentration.
9568	62	26	62	29	Perhaps one sentence of potential mechanisms behind the differences in TCRE in ESMs and EMICs at high levels of warming would be useful here.  e.g. from Tokarska et al. 2016: "ESMs tend to exhibit a reduced rate of warming as cumulative emissions increase, which is largely associated with stronger decreases in the efficiency of ocean heat uptake with warming in the ESMs, consistent with other recent studies (Gregory et al., 2015; Frölicher, T. L. & Paynter, D. J. 2015)"  ESMs tend to exhibit a reduced rate of warming as cumulative emissions increase, is largely associated with stronger decreases in the efficiency of ocean heat uptake with warming in the ESMs, consistent with other recent studies (Gregory et al., 2015; Frölicher, T. L. & Paynter, D. J. 2015).  see also Gregory et al., 2015 and Frölicher, T. L. & Paynter, D. J. 2015 for a discussion of potential mechanisms and differences between EMICs and ESMs at high levels of warming.  Refs: Gregory, J. M., Andrews, T. & Good, P. The inconstancy of transient climate sensitivity under increasing CO2. Phil. Trans. R. Soc. A 373, 20140417 (2015).  Frölicher, T. L. & Paynter, D. J. Extending the relationship between global warming and cumulative carbon emissions to multi-millennial timescales. Environ. Res. Lett. 10, 075002 (2015). [Katarzyna (Kasia) Tokarska, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - The text was revised to reflect this suggestion.
23128	62	29	62	29	Concluding sentence at the end of this section would be appropriate to highlight the level of uncertainty for TCRE models [Gwenaelle GREMION, Canada]	Accepted - a high-level concluding statement was added

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
9570	62	32	62	52	Perhaps it should be clarified that this section refers to monotonically increasing emission rates and not to overshoot scenarios, in which post-overshoot budgets are smaller than pre-overshoot budgets.  I think a paragraph on overshoot budgets would be useful here. [Katarzyna (Kasia) Tokarska, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - Section "5.5.1.2.3 Reversibility and Earth system feedbacks" addresses the question of overshoot budgets.
32768	62	33	62	33	Time scale response of what? Temperature? Redistribution of the CO2? What [Michael MacCracken, United States of America]	Accepted - It has been clarified that this is the response timescale of the resulting global average temperature increase.
23130	62	47	62	48	Sentence "This finding is support [...] be affected by model drift" worth taking out due to lack of certainty [Gwenaëlle GREMION, Canada]	Taken into account - This sentence was not providing further strong evidence as the point it was trying to support was weakened by the caveat of this study potentially experiencing model drift. Given that this is a pre-AR5 study, it was not included here anymore.
27990	62	48	62	48	Page 5-62, line 48: "Ultimately, Tachiiri et al. (2015) found that the uncertainty in TCRE increased in cases in which CO2 concentrations were stabilised". For readability maybe change "in which" to "where". [roderik van de wal, Netherlands]	Accepted - The sentence was edited
23430	62	48	62	49	Related to the sentence: "Ultimately, Tachiiri et al. (2015) ... hence gradually decline". Could this imply that in an CO2 reduction emissions scenario, the TCRE would no longer be a confidence method to interpret the relationship between cumulative CO2 emissions and temperature increase? [Gwenaëlle GREMION, Canada]	Taken into account - When CO2 emissions decline, the TCRE is slowly combined with the ZEC (assessed in Chapter 4). This is now clarified.
17710	63	1	63	1	Change 'system' to 'System' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - no reason is given to change this otherwise grammatically correct sentence.
41698	63	2	63	3	The text should more fully address the issue of the delayed warming after carbon emissions cease (before addressing the valid points made about the effects of negative CO2 emissions). There may be an overshoot in surface temperature after emissions cease as illustrated by a 1000 year experiments with an Earth system model (Frölicher 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). References 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)]	Accepted - Specific text on the interplay with ZEC and cross-referencing Chapter 4 has been included.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
53472	63	3	63	3	you may insert "emissions" after "declining" (even if the word appears later in the sentence) [Jan Fuglestedt, Norway]	Accepted - Sentence has been edited.
9562	63	3	63	6	however, if we consider dimensions other than global mean temperature rise, climate change is not reversible in overshoot scenarios -I think it may be good to emphasize this point here. [Katarzyna (Kasia) Tokarska, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - While correct, this section speaks to TCRE and thus the relationship between cumulative CO2 emissions and global mean temperature increase.
23132	63	10	63	10	Should read "The AR5-assessed range" otherwise the sentence doesn't read correctly [Gwenaelle GREMION, Canada]	Accepted - text revised
36428	63	10			The AR5 assessed range of TCRE also considered observational constraints. Replace 'was based on' with 'was based in part on'. [Nathan Gillett, Canada]	Accepted - Has been updated.
13594	63	16	63	19	This is expected, right? Permafrost carbon is external to the system. If warming leads to its release, you can expect that TCRE would go up. [Govindasamy Bala, India]	Noted - Indeed this is expected.
17712	63	17	63	17	Change to '21st Century' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - The AR6_WGI_StyleGuide says to use "21st century"
36430	63	19		20	Replace 'increase' with 'increase or decrease'. Missing processes could increase or decrease TCRE. [Nathan Gillett, Canada]	Accepted - Has been updated.
27992	63	24	63	24	The subsection title is a pleonasm, since the IPCC report is already entirely based on written literature. I suggest therefore to prevent confusion to leave out the word literature and just use "Estimates of TCRE". [roderik van de wal, Netherlands]	Accepted - The section title was updated.



Comment ID	From Page	From Line	To Page	To Line	Comment	Response
42118	63	24	64	17	<p>Chapter 5 section 5.5.1.3 Literature estimate of the TCRE and Table 5.6: There are other estimates of the TCRE from the literature that are not present in Table 5.6 that should be.</p> <p>Firstly, the estimate of the TCRE from theoretically derived equations, mentioned in the text (Goodwin et al., 2015) in section 5.5.1.3, is not inserted into Table 5.6. Secondly, an estimate from a large number of observation-constrained simulations in Goodwin et al (2018) built around this theoretical equation should also be included.</p> <p>The estimate from Goodwin et al. (2018) refers to the gradient of the warming against cumulative carbon emissions plot from figure 4a in that study, including shaded uncertainty ranges. I think this is an important estimate to add to Table 5.6 given the context of the other estimates published since AR5: while the Goodwin et al. (2018) estimate of 1.8 to 2.6 K per EgC fits within the minimum to maximum ranges of the other estimates also published since AR5 (0.9 to 2.7 K per EgC) the Goodwin et al. (2018) estimate is the only one whose median or best estimate is towards the upper end of this range (2.2 K per EgC).</p> <p>Below I detail example entries for these TCRE estimates that could be placed into Table 5.6, (all in the section for literature estimates published since IPCC AR5). Feel free to include less detail on precisely which observational reconstructions are used in the notes section – this is easily found within the Goodwin et al. (2018) reference itself.</p> <p>1st study: Study: Goodwin et al. (2015); Estimate (K per EgC): 1.1±0.5 K per EgC; Notes: Theoretically derived equation for the TCRE constrained by</p>	Accepted - estimates have been included in table and assessed.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
42120	63	24	64	17	<p>Full references for above comment:</p> <p>Goodwin, P., A. Katavouta, V.M. Roussenov, G.L. Foster, E.J. Rohling and R.G. Williams, (2018) Pathways to 1.5 and 2 °C warming based on observational and geological constraints, Nature Geoscience 11, 102-107, doi:10.1038/s41561-017-0054-8.</p> <p>Goodwin, P., R.G. Williams and A. Ridgwell (2015), Sensitivity of climate to cumulative carbon emissions due to compensation of ocean heat and carbon uptake, Nature Geoscience, Vol. 8, p29-34, doi:10.1038/ngeo2304.</p> <p>References for air-sea temperature reconstructions used in notes:</p> <p>Morice, C. P., Kennedy, J. J., Rayner, N. A. &amp; Jones, P. D. Quantifying uncertainties in global and regional temperature change using an ensemble of observational estimates: the HadCRUT4 dataset. J. Geophys. Res. 117, D08101 (2012).</p> <p>GISS Surface Temperature Analysis (GISTEMP) (NASA Goddard Institute for Space Studies, accessed 19 January 2017); <a href="https://data.giss.nasa.gov/gistemp/">https://data.giss.nasa.gov/gistemp/</a></p> <p>10. Hansen, J., Ruedy, S., Sato, M. &amp; Lo, K. Global surface temperature change. Rev. Geophys. 48, RG4004 (2010).</p> <p>Smith, T. M., Reynolds, R. W., Peterson, T. C. &amp; Lawrimore, J.</p>	Noted.
41700	63	26	63	38	<p>No explanation is provided for the intermodal differences in the TCRE. Based upon 10 CMIP5 models following 1% CO2 increase and 9 CMIP5 Earth system models following RCP8.5, the inter-model uncertainty in the surface warming dependence on cumulative carbon emissions is found to be dominated by the inter-model uncertainty in the thermal contribution from climate sensitivity and ocean heat uptake, and then after several decades there are comparable contributions from the inter-model uncertainty in the carbon contribution (Williams et al., 2017). Reference:</p> <p>Williams, R. G., V. Roussenov, P. Goodwin, L. Resplandy, and L. Bopp (2017), Sensitivity of global warming to carbon emissions: Effects of heat and carbon uptake in a suite of Earth system models, Journal of Climate, 30(23), 9343–9363, doi:10.1175/JCLI-D-16-0468.1. [Ric Williams, United Kingdom (of Great Britain and Northern Ireland)]</p>	Accepted - explanation has been included.
17714	63	43	64	1	<p>References in left column of table need editing for correct style, and if the table is to be split over two pages then please duplicate the column headings at the top of the second page [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]</p>	Taken into account - references in the left column are correct style

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
41702	63	44	64	1	<p>Two additional independent estimates of the TCRE should be included. Goodwin et al. (2015); Estimate (K per EgC): 1.1±0.5 K per EgC; Notes: Theoretically derived equation for the TCRE constrained by evaluations of surface warming, radiative forcing and historic ocean and land carbon uptake adopted by IPCC AR5. Assumes no radiative forcing from other sources.</p> <p>Goodwin et al. (2018); Estimate (K per EgC): range = 1.8 to 2.6, median = 2.2; Notes: 2.5 to 97.5 % range. Efficient model ensemble constrained by blended surface-air and sea-surface temperature records, ocean heat uptake reconstructions and the evaluations of historic ocean and land carbon uptake adopted in IPCC AR5. Estimate for future carbon emissions of up to 800 PgC, and evaluated from simulated warming and cumulative carbon emissions for scenarios RCP2.6, RCP4.5, RCP6.0 and RCP8.5. References:</p> <p>Goodwin, P., A. Katavouta, V.M. Roussenov, G.L. Foster, E.J. Rohling and R.G. Williams, (2018) Pathways to 1.5 and 2 °C warming based on observational and geological constraints, Nature Geoscience 11, 102-107, doi:10.1038/s41561-017-0054-8.</p> <p>Goodwin, P., R.G. Williams and A. Ridgwell (2015), Sensitivity of climate to cumulative carbon emissions due to compensation of ocean heat and carbon uptake, Nature Geoscience, Vol. 8, p29-34, doi:10.1038/ngeo2304. [Ric Williams, United Kingdom (of Great Britain and Northern Ireland)]</p>	Accepted - estimates have been included in table and assessed.
23134	64	5	64	5	The title of 5.5.1.4 is vague. What is "combined" referring to? Is this in reference to pre- and post-AR5 TCRE assessments? It may be better to spell that out explicitly [Gwenaelle GREMION, Canada]	Taken into account - "Combined assessment of TCRE" indicates that this section presents the assessment of all lines of evidence supporting TCRE and its value/range.
47386	64	5			Jones and Friedlingstein paper (in prep - I will provide a copy) perform a breakdown of TCRE magnitude and uncertainty into C4MIP feedback terms of alpha, beta, gamma (split by land and ocean) - which helps understand uncertainty and distribution of TCRE [chris jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - this breakdown has been used to verify internal consistency of the TCRE assessment across WG1 chapters.
47388	64	5			just a note that this section may be updated if Ch.7 come up with constrained range of ECS, so the two must be consistent [chris jones, United Kingdom (of Great Britain and Northern Ireland)]	Noted - However, the TCR is more closely linked to the TCRE and the Chapter 7 assessment has here been taken into account.
28826	64	7	67	17	Can you assess the carbon sensitivity separately and combine it with chapter 7's estimate of TCR, otherwise TCRE and TCR assessments may end up inconsistent? [Piers Piers Forster, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - combined with an assessment of the airborne fraction, this can be combined with TCR to verify internal consistency across WG1 chapters.
23432	64	11	64	12	Related to the sentence: "Taking into account ... less likely". In the guide there is not a specific probability range to less likely. I interpret the authors want to express a low probability range, so I would suggest to say "unlikely". [Gwenaelle GREMION, Canada]	Taken into account - "Likely" in this context was not referring to the calibrated uncertainty language, which resulted in this confusion. This has been avoided in the revised chapter text.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
47770	64	12	64	12	"Less likely". Please check the use of this IPCC uncertainty language term. Are you able to provide a traceable account to assigning this uncertainty statement? Note that likelihood statements are quantified terms - phrases like likely and very likely have quantifiable probabilities associated with them. Please check it has been used correctly here. Please refer to the IPCC guidance note on uncertainty: <a href="https://wg1.ipcc.ch/SR/documents/ar5_uncertainty-guidance-note.pdf">https://wg1.ipcc.ch/SR/documents/ar5_uncertainty-guidance-note.pdf</a> [WGI TSU, France]	Taken into account - "Likely" in this context was not referring to the calibrated uncertainty language, which resulted in this confusion. This has been avoided in the revised chapter text.
9566	65	2	65	4	Perhaps it would be good to clarify and specify which methodological and definition choices is this referring to? e.g. choices of the temperature definition (blended or not, full coverage or masked) and temperature target definition (does not exceed the given level at all)? [Katarzyna (Kasia) Tokarska, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - This sentence already included a reference to the section below which explicitly speaks to this issue. Repeating them here without further context has thus been deemed unnecessary as it would probably rather confuse than help the reader understanding these aspects.
17716	65	3	65	3	Replace 'include' with 'are' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - more contributions can be thought of but would be of second order and are hence not listed here
9564	65	8	65	8	Perhaps Tokarska&Gillett, 2018 could be cited here: they showed no significant differences between the RCP 4.5 and 8.5 scenario despite different radiative forcing (which is only true in RCP scenarios, due to random cancellation) [Katarzyna (Kasia) Tokarska, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - As indicated by the reviewer. This paper does not provide additional evidence for the influence of non-CO2 species, but rather report an accidental relative zero effect between two scenarios.
23136	65	21	65	21	For clarity, change "Here" to "In this report" [Gwenaelle GREMION, Canada]	Accepted - text revised
9572	65	22	65	23	Perhaps it would be good to specify the baseline. [Katarzyna (Kasia) Tokarska, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - It is unclear what is meant with "baseline" in this context. The sentence already specifies that the remaining carbon budget is expressed relative to today (or a point in the recent past). This is what the literature in this context would refer to as baseline.
36432	65	22			Tokarska et al. (2018) calculated TEBs directly from CMIP5 simulations - they did not rely on TCRE. [Nathan Gillett, Canada]	Taken into account - On the indicated line there is no reference to Tokarska et al (2018). Assuming this is referring to page 65, line 52 (the first instance of Tokarska implying the least typo's by the reviewer) Tokarska et al 2018 still implicitly rely on TCRE as they use CMIP5 simulations to derive TEBs and then suggest in their text that these are useful for policymakers to achieve targets (hence implicitly relying on TCRE being pathway independent).

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
56636	65	25	65	25	This sentence is slightly confusing, as a) whether the TCRE relationship between cumulative emissions and temperatures is near-linear or linear does not matter for the net zero levels conclusion.. Just needs to be strictly monoton... Secondly, the conclusion of whether today or another point is taking as the starting point also does not affect the conclusion. Maybe replace with something like "Irrespective of the exact definition of the remaining carbon budget, the finding that temperatures continue to increase with higher cumulative carbon emissions implies that net CO2 emissions have to decline to zero levels in order to halt global warming, whether at 1.5C or 2C or another level."... [Malte Meinshausen, Australia]	Taken into account - The explanation by the reviewer is deemed correct and the sentence has been edited following the reviewer's suggestions.
9574	65	26	65	26	Perhaps clarify by adding: "by the time when the given budget is used up." [Katarzyna (Kasia) Tokarska, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - The sentence has been edited for clarity and to include this suggestion.
17718	65	30	65	30	Change to 'Exceedence' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - This concept has been defined in British English, see Rogelj et al (2016, Nature Climate Change).
17720	65	38	65	38	Capital C for century [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - The AR6_WGI_StyleGuide says to use "century"
23138	65	42	65	44	The "Drawbacks" at the start of the sentence seems to only refer to one; perhaps make it singular to "A drawback of TEBs..." [Gwenaelle GREMION, Canada]	Accepted - Sentence has been edited.
9576	65	46	65	47	also, TEBs do not account for ZEC or assume that ZEC is near zero. Maybe this caveat could be mentioned here too? [Katarzyna (Kasia) Tokarska, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - The sentence already refers to lags in warming, without making the more technical point related to the Zero Emission Commitment (ZEC), which is discussed in Section "5.5.2.2.4 Adjustments due to other not represented feedbacks and potential limitations of TCRE"
41704	65	49	65	52	The study by Goodwin et al. (2018) should be cited here as using the TCRE to constrain carbon budgets to restrict warming to a given target. Reference: Goodwin. P., A. Katavouta, V.M. Roussenov, G.L. Foster, E.J. Rohling and R.G. Williams, (2018) Pathways to 1.5 and 2 °C warming based on observational and geological constraints, Nature Geoscience 11, 102-107, doi:10.1038/s41561-017-0054-8. [Ric Williams, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - This reference was included for completeness.
23140	65	50	65	51	The end of this sentence is abrupt ("for example") [Gwenaelle GREMION, Canada]	Accepted - Edited for clarity.
27994	65	50	65	52	Page 5-65, line 50-52: "for example (Friedlingstein et al., 2014a; Matthews et al., 2017; Millar et al., 52 2017; Tokarska et al., 2018)" maybe here some actual descriptive examples can be given to show how these approaches indirectly rely on the TCRE. Just to complement this part and give an idea how carbon budgets can be estimated, apart from the framework that is applied here for AR6. [roderik van de wal, Netherlands]	Taken into account - A clarification has been added as to how these estimates implicit rely on a pathway independent TCRE in the formulation of their findings.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
9578	65	51	65	52	Perhaps it could be clarified that these two references to Millar et al. 2017 and Tokarska et al. 2018 do not use TCRE (from the TCRE range) to calculate carbon budgets, but rather calculate budgets directly from the model responses (individual models' cumulative CO2 emissions), which is different from the AR5 TCRE range. [Katarzyna (Kasia) Tokarska, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - These two papers indeed directly use the model responses, but based on the assumption that the concept of TCRE is robust, and that the cumulative CO2 emissions emitted until the time of crossing a temperature thresholds are also representative of the CO2 emissions that would be consistent with holding warming to a specific temperature threshold under strongly decline CO2 emissions to net zero.
23142	65	52	65	54	The "Here" at the beginning of this sentence is vague; replace with "In this report," [Gwenaelle GREMION, Canada]	Accepted - text revised
9580	66	1	66	2	Please note that Tokarska and Gillett 2018 could be also added here as a relevant reference (which is different from Tokarska et al., 2018 reference).  References:  Tokarska, K.B and Gillett, N.P. Cumulative carbon emissions budgets consistent with 1.5 °C global warming. Nature Climate Change, 8, 296–299 (2018).  Tokarska, K.B., Gillett, N.P., Arora, V.K., Lee, W.G., and Zickfeld, K. The influence of non-CO2 forcings on cumulative carbon emissions budgets. Environmental Research Letters, 13, 034039 (2018). [Katarzyna (Kasia) Tokarska, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - This list of references intends to give some examples of studies that use the TCRE as their core metric, but does not intend to be exhaustive.
17722	66	3	66	3	Change 'system' to 'System' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - no reason is given to change this otherwise grammatically correct sentence.
23144	66	18	66	19	Add text to reiterate the confidence in this uncertainty [Gwenaelle GREMION, Canada]	Rejected - This sentence restates a range that was assessed in an earlier section (Section 5.5.1.4) where a confidence statement is provided.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
9560	66	26	66	27	<p>Please note that the relevant reference here is Tokarska and Gillett 2018, and not Tokarska et al., 2018 (these are two different papers, and only the first one makes use of changing the baseline and the present level of warming in carbon budget calculations).</p> <p>References:</p> <p>Tokarska, K.B and Gillett, N.P. Cumulative carbon emissions budgets consistent with 1.5 °C global warming. Nature Climate Change, 8, 296–299 (2018).</p> <p>Tokarska, K.B., Gillett, N.P., Arora, V.K., Lee, W.G., and Zickfeld, K. The influence of non-CO2 forcings on cumulative carbon emissions budgets. Environmental Research Letters, 13, 034039 (2018). [Katarzyna (Kasia) Tokarska, United Kingdom (of Great Britain and Northern Ireland)]</p>	Accepted - This reference was corrected.
51960	66	26	66	36	I assume consistency with chapter 2 and not 3 here. In chapter 2 estimates for both GMST and GSAT are planned to be provided. Would you use both or just one? [Peter Thorne, Ireland]	The remaining C budget is based on GSAT, not on both.
17724	66	29	66	29	change 'like' to 'such as' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
23146	66	29	66	31	Is shown to be important in the estimation of remaining carbon budgets? Add text to specify [Gwenaelle GREMION, Canada]	Accepted - A reference was made to Cross-chapter Box 2.3 in Chapter 2 on the topic of surface temperature metrics. This box clarifies the impact of temperature metrics on the quantification of global warming.
42992	66	32	66	36	<p>This passage appears to be a misunderstanding of the method employed in chapter 2 of SR1.5 to assess historical warming to date. The actual calculation there was based on HadCRUT4 rise from 1850-1900 to 2006-2015 (0.84C), upwardly adjusted by the difference between CMIP5 SAT (0.99) and blended SAT-SST/masked CMIP5 (0.86C) over the same period. (0.84C + (0.99C - 0.86C) = 0.97C). A similar calculation of "GSAT" can be found in AR6 Ch 2, but this time based on SST-SAT adjustment only as all series are expected to be spatially more complete by the time of SOD. The "human-induced" warming estimate in SR1.5 was not in any way involved in the calculation; rather it was based on adjusted observations - an approach that AR6 should follow as outlined above. [David Clarke, Canada]</p>	<p>Taken into account - IPCC SR15 Ch1 states that "In all cases, since 2000 the estimated combined contribution of solar and volcanic activity to warming relative to 1850–1900 is found to be less than ±0.1°C (Gillett et al., 2013), while anthropogenic warming is indistinguishable from, and if anything slightly greater than, the total observed warming, with 5–95% confidence intervals typically around ±20%." With observed and anthropogenic warming indistinguishable over the time period of interest here. The assessment of the remaining carbon budget will continue to use the anthropogenic warming estimates, as temperature limits of interest (i.e. 1.5°C and 2°C relative to preindustrial) are also understood to refer to anthropogenic warming.</p>

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
53474	66	33	66	33	You may write GSAT (instead of SAT) to be consistent. [Jan Fuglestedt, Norway]	Accepted - text revised
49128	66	33	66	33	The choice of GSAT needs to be debated within and across WGs. Especially as WMO reports GMST in the annual state of the global climate report. There is great confusion among policymakers as a result of SR1.5. [Jim Skea, United Kingdom (of Great Britain and Northern Ireland)]	Noted - Consistency with the warming definition used to inform the target levels in the Paris Agreement is of the essence.
48744	66	33	66	34	Inconsistent with Chapter 7p 5, line 26-27: Human induced surface temperature rise for the period 1750-2017 is 1.1 °C [0.9 to 1.3 °C 5% to 95% range]. And inconsistent with chapter 2 p 37 line 33-37: SR1.5 reported warming from 1850-1900 to 2006-2015 of 0.87 °C, with an 1880-2012 trend of 0.86°C and an 1880-2015 trend of 0.92°C.. A table with the different historic warming estimates, an explanation of the terms, why each is used where and the origine of the differences would help. [Birgit van Munster, United Kingdom (of Great Britain and Northern Ireland)]	Noted - This falls within the scope of Chapter 2 with title "Changing State of the Climate System".
9638	66	33	66	34	The authors describes that they "here apply a historical warming expressed in global average surface air temperatures (SAT) of 0.97 degree C --- based on the assessment of human-induced global warming by the IPCC Special Report on Global Warming of 1.5 degree C". In page 42 line31 of Chapter 1, there is a description that remaining carbon budgets are larger than than those estimated in AR5 because SR 1.5 used GMST as a measure of surface temperature" instead of SAT. Please clarify these differences. [Mitsutsune Yamaguchi, Japan]	Noted - The statement in Chapter 1 SR15 is incorrect. The definition of global warming is not one of the reasons remaining carbon budget are larger in SR15 compared to AR6. This has been clarified in a dedicated box on comparing AR5 to AR6 remaining carbon budget estimates.
56640	66	34	66	34	To the extent that the mean 2006-2015 period will be biased due to natural variability, the assessment should consider using the D&A estimates of human-induced warming for that period, rather than simply the observed warming over the period (although they could be close). [Malte Meinshausen, Australia]	Taken into account - IPCC SR15 Ch1 states that "In all cases, since 2000 the estimated combined contribution of solar and volcanic activity to warming relative to 1850–1900 is found to be less than ±0.1°C (Gillett et al., 2013), while anthropogenic warming is indistinguishable from, and if anything slightly greater than, the total observed warming, with 5–95% confidence intervals typically around ±20%." With observed and anthropogenic warming indistinguishable over the time period of interest here. The assessment of the remaining carbon budget will continue to use the anthropogenic warming estimates, as temperature limits of interest (i.e. 1.5°C and 2°C relative to preindustrial) are also understood to refer to anthropogenic warming.



Comment ID	From Page	From Line	To Page	To Line	Comment	Response
56638	66	34	66	36	It is important that the historical warming section 5.5.2.2.2 also discusses the sensitivity to assumptions of starting relative to 1850-1900 versus a time around pre-industrial, i.e. 1750. The remaining warming gap towards a 1.5C target will be (best estimate) around 0.1K different (Hawkins et al. etc, see Chapter 1), which means a roughly 20% difference in the remaining carbon budget. [Malte Meinshausen, Australia]	Taken into account - the potential variation due to historical base period choice is highlighted and cross-referenced with Chapter 1.
27792	66	38	66	38	attend the placeholders [Poot Delgado Carlos Antonio, Mexico]	Noted
23434	66	42	67	8	From this paragraph, I am not sure to understand the uncertainty or certainty given so far by literature about the climate transient response for the non-CO2 emissions. I would suggest if is possible to include in this paragraph a confidence or probability range of what exist so far about the climate response to other greenhouse gases when the zero CO2 emissions be achieved. [Gwenaëlle GREMION, Canada]	Rejected - It is unclear what the reviewer comment tries to request. The uncertainty in non-CO2 warming response is included in the discussion of the remaining carbon budget and so is the non-CO2 scenario variation.
57348	66	42	67	11	Non-CO2 forcing can be accounted for in remaining carbon budgets by converting it into CO2-forcing-equivalent emissions, or if that is considered too complicated, using the following formula: the warming ( $\Delta T$ ) resulting from a combination of cumulative CO2 emissions G and non-CO2 radiative forcing F over a given time-interval is $\Delta T = \text{TCRE} \times [G + \gamma H (\epsilon \text{Fbar} \Delta t + \Delta F)] / \text{AGWP\_H}$ where Fbar is the average and $\Delta F$ the change in non-CO2 radiative forcing over that time-interval, $\gamma$ is a constant less than but of order unity while $\epsilon$ is small, representing the fractional rate of decline of radiative forcing required to maintain stable temperatures (around -0.3% per year, depending on the ECS, TCR and long thermal adjustment time). [Myles Allen, United Kingdom (of Great Britain and Northern Ireland)]	Noted - However, it is unclear which literature basis underpins this comment. Also without the provision of a clear way to translate non-CO2 emissions into forcing and an (uncertainty) quantification of the constant "?" smaller but of the order of unity, this method cannot be directly applied.
24668	66	42	67	11	Section 5.5.2.2.3: Chapter 7, section 7.7.2.4 now provides an explicit framework for converting changes in the rate of SLCF emissions to the cumulative carbon emissions needed for TCRE. This could be referred to and used here. [William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - This has been referred to with cross-reference to Chapter 7. Depending on whether an adequate robustness and uncertainty analysis is available in Chapter 7, it can also be applied.
53476	66	42	67	11	You may say that the approach taken for non-CO2 is the same as in SR1.5 (and also refer to the Sup Mat there which explains the approach well. But I hope you will have a similar explanation/documentation here too) [Jan Fuglestad, Norway]	Noted - Also for AR6 documentation will be provided for both the emulator setup developed by Ch1-Ch7 and its use to estimate the impact on the remaining carbon budget. However, reproducing any of the documentation already available in SR1.5 does not seem to be very useful.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
41706	66	43	66	47	The effect of non-CO2 radiative forcing in modifying the surface temperature response and the dependence of surface warming on cumulative carbon emissions is set out in a framework by Williams et al. (2016) and diagnosed by Williams et al. (2017). References Williams, R. G., P. Goodwin, V. M. Rousenov, and L. Bopp (2016), A framework to understand the Transient Climate Response to Emissions, Environmental Research Letters, 11, doi:10.1088/1748-9326/11/1/015003. Williams, R. G., V. Rousenov, P. Goodwin, L. Resplandy, and L. Bopp (2017), Sensitivity of global warming to carbon emissions: Effects of heat and carbon uptake in a suite of Earth system models, Journal of Climate, 30(23), 9343–9363, doi:10.1175/JCLI-D-16-0468.1. [Ric Williams, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - These references indeed further support this statement and have been included.
56384	67	1	67	3	I think the assesemnt of the climate effects of specific CDR methods could be expanded as there is a lot of new literature out there. CDR-MIP also includes ocean alkalization and afforestation experiments. [Kirsten Zickfeld, Canada]	Wrongly assigned - Comment seems to be misplaced
27794	67	8	67	8	attend the placeholders [Poot Delgado Carlos Antonio, Mexico]	Noted
27796	67	13	67	15	attend the placeholders [Poot Delgado Carlos Antonio, Mexico]	Noted
27996	67	19	67	19	As for the feedbacks only the thawing of permafrost is discussed, where it is stated that this is the most important one. How significant are other feedbacks that can alter the estimated carbon budgets? Furthermore for the statement (line 44-45): “estimated unrepresented Earth system processes to result in a reduction of remaining carbon budgets of about 100 GtCO <sub>2</sub> ”, which unrepresented Earth system processes are referred to? Or is this only for the feedbacks related to permafrost thawing? This is not directly very clear. [roderik van de wal, Netherlands]	Accepted - The section now draws on the full feedback assessment of Section 5.4, going beyond permafrost thawing only.
56642	67	19			It would be a valuable advancemet of AR6 over IPCC SR1.5, if those additional unrepresented Earth system feedbacks would at least be presented as a linearly scaled addition (XtC/warming or similar) rather than a flatrate offset (100 GtC) across all temperature levels.... [Malte Meinshausen, Australia]	Taken into account - While the permafrost feedback results in a steeper TCRE curve, the difficulty is in the time-scale of this increase in steepness. The permafrost feedback is not instantaneous and continues to add carbon to the atmosphere also after net zero CO <sub>2</sub> emissions are achieved and cumulative CO <sub>2</sub> emissions hence do not further increase. Including this factor as a time-bound correction is thus necessary.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
35406	67	24	67	30	There should be a mention on the fact that the ZEC is also expected to depend on the level of non-CO2 emissions. [Nadine Mengis, Canada]	Taken into account - The ZEC in this context is defined as the committed warming from CO2 only. The warming from non-CO2 emissions is assessed through the non-CO2 contribution, which in SR15 has been assessed to be stabilized or declining for pathways that bring net CO2 emissions down to zero.
13596	67	24	67	35	I believe the value of ZEC would depend on the rate at which we approach net zero emissions, implying a dependence on time scale because of the thermal and carbon inertia of the system. [Govindasamy Bala, India]	Accepted - a clear definition of the ZEC is indeed necessary to unambiguously quantify it. Here, the ZEC is defined as the remaining warming after a cessation of emissions following a 1% CO2 concentration increase concentration, at the time the diagnosed CO2 emissions reach 1000 PgC.
17726	67	32	67	32	Change to Pre-Industrial [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - The same spelling as in the IPCC SR1.5 has been applied
41708	67	33	67	33	The peak in surface warming after carbon emissions cease diagnosed by Froelicher et al., (2014) is due to a decline in ocean heat uptake and climate feedback, which combine to increase the proportion of radiative forcing used to increase surface temperature (Williams et al., 2017). Reference: 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)]	Noted - The assessment of the processes and magnitude of the ZEC can be found in Section 4.7.2. A direct reference to this section was included in this sub-section on the remaining carbon budget.
24670	67	33	67	35	What is the assessed range on the ZEC for AR6? [William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - See Chapter 4.7.2 for the assessed range for the ZEC.
41710	67	33	67	35	The assumption that ZEC of CO2 is considered zero appears implausible. A 1000 year experiments with an Earth system model ESM2M (Froelicher et al., 2014) reveals a delayed warming after emissions cease. There is a peak in surface warming after carbon emissions cease due to a decline in ocean heat uptake. In this experiment, there is a progressive decline in the climate feedback parameter after emissions cease, which then increases the surface warming response (Williams et al., 2017, Supplementary Figure 1d). References 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 - The ZEC is being considered over a policy relevant time frame of half a century, while also the potential variation over multi-centennial time horizons is highlighted. .

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
36434	67	33		35	There is zero probability that ZEC is exactly zero. So rather than assessing that there is low confidence that ZEC is precisely zero, better to assess with medium or high confidence that it is smaller than x K in magnitude, with a best estimate centred on zero. [Nathan Gillett, Canada]	Accepted - Based on the ZEC assessment carried out in Chapter 4, a range with confidence interval/statement was included.
17728	67	37	67	37	Change 'systems' to 'Systems' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - no reason is given to change this otherwise grammatically correct sentence.
23436	67	40	67	41	Related to the sentence: "The most important .... thawing permafrost". I interpret with this sentence that the thawing of permafrost is important because of carbon release, but maybe could also be included here other greenhouse gases release (eg. methane) which could have a higher radiative forcing than CO2? [Gwenaelle GREMION, Canada]	Noted - This seems to be a misinterpretation, which additional clarifications to this section now are trying to avoid. The unrepresented Earth system feedbacks include all feedbacks assessed in Section 5.4, which include CO2 and other greenhouse gas feedbacks.
47390	67	45			Pugh et al (2018, Earth's Future) show the importance of long-term committed carbon sinks due to vegetation dynamics. This can rival permafrost thaw in terms of magnitude and may reduce any need for offsetting the carbon budget for missing processes. [chris jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - This sections integrates the overall Earth system feedback assessment that was presented in Section 5.4. Whatever the overall assessment of permafrost feedbacks concludes will be reflected here.
36436	67	45			Replace 'about 100 GtC' with 'up to 100 GtC'. The distinction is important. [Nathan Gillett, Canada]	Accepted - This correction was implemented.
17730	67	46	67	46	Change 'system' to 'System' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - no reason is given to change this otherwise grammatically correct sentence.
24672	67	49	67	50	What is the range of the potential magnitude of permafrost thawing? [William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - The impact of unrepresented Earth system feedbacks is based on the assessment of Section 5.4 and now comes with uncertainty ranges and a confidence statement.
17732	67	51	67	51	Change 'to overall' to 'overall to' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
17734	67	52	67	52	Change 'system' to 'System' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - no reason is given to change this otherwise grammatically correct sentence.
27798	67	55	67	55	attend the placeholders [Poot Delgado Carlos Antonio, Mexico]	Noted
27998	68	3	68	3	Is it possible to visualize the data about the remaining carbon budgets in Table 5.7 in the form of a figure? This will make it easier to interpret the data and will highlight the assessment of the remaining carbon budgets, which I think is a very important takeaway message of this section. [roderik van de wal, Netherlands]	Rejected - The concept can be visualized, but the data itself is best accessible in the table.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
56006	68	3	68	21	These carbon budgets are significantly different from the ones in AR5. Now: for +2°C @50th percentile: --> 1500 GtCO <sub>2</sub> . In AR5: {790 GtC cumulative emissions @ +2°C} minus {580 GtC cumulative emissions until 2016} = 210 GtC --> 770 Gt CO <sub>2</sub> . I.e. a factor of 2 different!!! Please explain the reasons for the large differences. [Urs Ruth, Germany]	Accepted - a dedicated box on how the assessment has been updated from AR5 to AR6 is included. These carbon budgets are very consistent with the updates provided in IPCC SR15.
47782	68	11	68	11	IPCC uncertainty language used incorrectly: a confidence statement (e.g., high/medium/low confidence) is made up of 2 clauses (evidence and agreement), which must be used together. Please refer to the IPCC guidance note on uncertainty for correct use of terms: <a href="https://wg1.ipcc.ch/SR/documents/ar5_uncertainty-guidance-note.pdf">https://wg1.ipcc.ch/SR/documents/ar5_uncertainty-guidance-note.pdf</a> [WGI TSU, France]	Accepted - The use of IPCC uncertainty language has now been consistently applied throughout the section.
23438	68	12	68	15	Related to the sentence: "Combined with the .... carbon budget estimates". Since the remaining carbon budgets implies the amount of CO <sub>2</sub> that could be still emitted to holding the global warming below a specific temperature threshold, maybe I would suggest to change the "medium confidence" to "low confidence" since there is a limited evidence about the effects of the Earth system feedbacks, leaving more uncertainty about the CO <sub>2</sub> emissions budget that could be achieved only by human activity. [Gwenaelle GREMION, Canada]	Accepted - reasoning and confidence statement was included.
17736	68	13	68	14	Change 'system's' to 'System's' and 'system' to 'System' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - no reason is given to change this otherwise grammatically correct sentence.
27800	68	17	68	17	attend the placeholders [Poot Delgado Carlos Antonio, Mexico]	Noted
9628	68	19	68	54	Another source of uncertainty is the role of internal variability on the carbon budgets, which is especially noticeable for the land carbon uptake. [Katarzyna (Kasia) Tokarska, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - It seems that this is rather a question and source of uncertainty to determine whether the remaining carbon budget is being exhausted. There are multiple methods that allow to isolate the forced signal from internal variability.
13598	68	24	67	33	Unit for column 3 in table 5.7 is not written [Govindasamy Bala, India]	Accepted
9582	68	24	68	30	Maybe the 10th and 90th percentiles of TCRE could be included in this table too? (as an estimate of the spread of the model responses) [Katarzyna (Kasia) Tokarska, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - Higher percentiles have been considered, but can only be given if the TCRE assessment can provide a 90% range (or 10th to 90th percentile range).
47106	68	24	68	33	Table 5.7: A graph might be easier to read than the table. [Sophie von Fromm, Germany]	Rejected - Given the various contributions, a table is considered to be a more useful way to communicate these data.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
56008	68	24	68	33	Specify start year and end year for the carbon budget (due to the continued uptake of CO2 by the ocean, the end-year should matter). [Urs Ruth, Germany]	Rejected - The end year for carbon budgets is when global net CO2 emissions become zero. This is time independent. The continued uptake of CO2 by the ocean and land compensates the transition from transient to equilibrium warming, with deviations from this captured in the ZEC.
27802	68	26	68	27	attend the placeholders [Poot Delgado Carlos Antonio, Mexico]	Noted
38172	68	30	68	30	"(2)" is missing in the Table 5.7. [Hiroaki Kondo, Japan]	Accepted
41844	68	30	68	30	Column 3 (remaining carbon budget) : specify the unit; specify at which date the budget is established. [Marc Aubinet, Belgium]	Accepted
41846	68	30	68	30	The coherence between the table numbers and the text could be improved. For example a computation of the remaining emissions on the base of a TCRE range of 0.8-2.5°C/EgC and a temperature increase of 0.53 as suggested by the text leads to 777-2429 GtCO2, much above the range given in the table (420-840 on 4th line). Why is there a difference ? Is it due to non CO2 warming contributions ? [Marc Aubinet, Belgium]	Accepted - Indeed, the non-CO2 contribution has to be subtracted from the remaining allowable warming. A stylized schematic was added that clarifies how all parts fit together.
17738	68	30	68	31	Poor quality table, with small text which is difficult to read [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Noted
35408	69	1	69	20	Solar Radiation Mangement (SRM) is not the appropriate umbrella term, and according abbreviation, for all proposed radiation management measures. As explained in section 4.6.3.3, cirrus cloud thinning is targeting long wave radiation, making the 'solar' inappropriate. I think that the IPCC report should make an effort to correct the predominant abbreviation of SRM as an umbrella and introduce a more appropriate term & abbreviation, such as Radiation Management (RM), or RMM (radiation management measure), which is used in Chapter 4 page 70 line 10 already. Accordingly the definition of radiation management in lines 13-14 has to be adjusted. [Nadine Mengis, Canada]	Rejected. It has been decided to use Solar Radiation Management for consistency with the AR6 scoping document. We have clarified that SRM is used in this report to refer to all direct interventions on the planetary radiation budget, including cirrus cloud thinning.
29346	69	1	71	28	Multiple instances of Keller et al.- this section needs to be strengthened with references. [Minal Pathak, India]	Taken into account. Additional references were added to this paragraph.
47804	69	1	82	7	Assessment Structure: If possible, assessment conclusions should be provided in a structured traceable account of how these statements were derived. For example, sections / subsections can start with previous IPCC report conclusions (AR5 or AR6 Special Reports) and then provide an update of the more recent literature, clearly laying out the lines of evidence. Each section / subsection can then conclude with assessment statements. [WGI TSU, France]	Taken into account. Suggested assessment structure was followed in most sections.
13496	69	5	69	6	One of important features of climate intervention is "large" or "planetary" scale. This is missing in the current text. [Govindasamy Bala, India]	Taken into account. "Planetary" scale is a feature of SRM, not necessarily CDR, and is introduced in conjunction with SRM.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
9694	69	5	69	20	the introduction paragraphs are fine, but I would suggest including citations to the literature that support these summaries of CDR and SRM, or cross-referencing with where in the IPCC AR6 a fuller introduction supported by relevant literature can be found. [Brian Magi, United States of America]	Accepted. References for definitions and cross-references to other report sections have been added.
55566	69	7	69	8	The implied definition of 'mitigation' used here is in conflict with the definition of mitigation used in IPCC SR15 as well as the mitigation definition under the UNFCCC convention text 1992 and its 2015 Paris Agreement! Refer to section 5.6.2.1.2 for a correct description of the relationship between the terms 'mitigation', 'emissions', and 'removals'. [Matthisa Honegger, Germany]	Accepted. The wording in question has been removed.
29342	69	7	69	8	Not sure if these contrast. According to SR1.5, large scale SRM could be used to supplement mitigation actions in overshoot scenarios. [Minal Pathak, India]	Not applicable. Wording in question has been deleted.
17740	69	10	69	10	Move 'directly' to after 'emissions' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
29344	69	10	69	11	CDR does not seek to reverse emissions. Suggestion to delete the first part of the sentence.CDR, also referred to as “negative CO2 emissions”, refers to interventions that remove CO2 from the atmosphere and durably store it in geological, terrestrial or ocean reservoirs. [Minal Pathak, India]	Accepted. Wording in question has been deleted as suggested.
23152	69	10			...seems to directly reverse GHG emissions maybe better "reverse the effects or the impact of GHG emissions" [Gwenaelle GREMION, Canada]	Not applicable. Wording in question has been deleted.
23148	69	12			Can you give an example for „direct removal“. As readers with a lower level of knowledge would wonder how that is possible and ask where the difference between direct removal and enhancing carbon sinks would be, as carbon sinks should be removing CO2 directly as well. [Gwenaelle GREMION, Canada]	Accepted. We have clarified that we refer to direct CO2 capture from the air and storage. Direct capture is achieved by chemico-technological methods as explained in the subsequent section.
17742	69	13	69	13	deleat second RM [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised [text modified according to comment 13498]
7518	69	13	69	13	Typo: SRM is repeated in this sentence. [Rose Abramoff, France]	Accepted - text revised
41848	69	13	69	13	"SRM" appears twice [Marc Aubinet, Belgium]	Accepted - text revised
13498	69	13	69	13	"SRM" is written twice. [Govindasamy Bala, India]	Accepted - text revised
9692	69	13	69	13	change "SRM, on the other hand," to "On the other hand," [Brian Magi, United States of America]	Accepted - text revised
8936	69	13	69	13	"SRM, on the other hand, SRM attempts to offset the ..." Delete the second SRM [Benjamin Lamptey, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised [text modified according to comment 13498]
55568	69	13	69	14	This definition of SRM excludes cirrus cloud thinning (to permit more amounts of longwave radiation escape the atmosphere). Is this done deliberately so, and if so, is cirrus cloud thinning addressed elsewhere? [Matthisa Honegger, Germany]	Accepted. We have clarified that SRM is used in this report to refer to all direct interventions on the planetary radiation budget, including cirrus cloud thinning.
28070	69	13	69	14	Page 5-69, line 13: “SRM, on the other hand, SRM attempts to offset the climate effects of greenhouse gas emissions” please rephrase, I suggest to leave out the second SRM to get: “SRM on the other hand, attempts to offset the climate effects of greenhouse gas emissions,...”. [roderik van de wal, Netherlands]	Not applicable. Wording in question has been deleted.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
23150	69	13			Repetition of the term SRM [Gwenaelle GREMION, Canada]	Accepted. Duplicate term has been removed.
32770	69	14	69	14	There also is a proposal to increase long-wave loss that is often included within SRM, so this statement might be generalized. [Michael MacCracken, United States of America]	Accepted. We have clarified that SRM is used in this report to refer to all direct interventions on the planetary radiation budget, including cirrus cloud thinning.
55570	69	16	69	18	This sentence is a bit fuzzy regarding the physical basis of the climate system: Reaching any temperature target i.e. stabilizing the climate system means balancing sources and sinks of CO <sub>2</sub> , which means that removals equal (remaining). Among removals, some are occurring naturally (at a diminishing rate), others could be induced via human intervention. The latter type of action is referred to as CDR. CDR has been included in the vast majority of pathways to limiting warming to 1.5 to 2°C within this century and are regarded as necessary forms of action alongside conserving and enhancing ecosystems acting as natural sinks. [Matthias Honegger, Germany]	Noted. Unclear what revision is requested here.
36438	69	16		18	The use of CDR to reach low warming targets is in part a feature of the cost optimisation in IAMs i.e. in the framework of the IAMs it is the lowest cost way to reach these targets. But there may be other ways to reach Paris targets without CDR. I believe Joeri Rogelj has a draft paper on this. [Nathan Gillett, Canada]	Noted. Unclear what revision is requested here.
47772	69	17	69	17	Please check the use of this IPCC uncertainty language term. The SR1.5 does not say there is a 90-100% chance of using CDR. Are you able to provide a traceable account to assigning this uncertainty statement? Note that likelihood statements are quantified terms - phrases like likely and very likely have quantifiable probabilities associated with them. Please check it has been used correctly here. Please refer to the IPCC guidance note on uncertainty: <a href="https://wg1.ipcc.ch/SR/documents/ar5_uncertainty-guidance-note.pdf">https://wg1.ipcc.ch/SR/documents/ar5_uncertainty-guidance-note.pdf</a> [WGI TSU, France]	Accepted. Use of uncertainty language was not used appropriately and has been removed.
23154	69	18			will be required in order to meet the Paris... [Gwenaelle GREMION, Canada]	Not applicable. Wording in question has been deleted.
57278	69	23	80	21	ch5 should decide - together with ch4 (and maybe even with WGIII) - on a consistent terminology around "carbon dioxide removal" and "negative emissions", which is able to clearly 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 CO <sub>2</sub> , while negative emissions might work better when describing global aggregates, first and foremost the state of net negative emissions [Oliver Geden, Germany]	Accepted. Terminology is now used consistently as suggested by the reviewer.



Comment ID	From Page	From Line	To Page	To Line	Comment	Response
23194	69	23			The title says "responses to the CDR" but in the end it is only written about the methods of CDR but not really any outcomes of those methods. With this title I would expect more information on how the removal affects the CO <sub>2</sub> - the ecosystems involved (i.e. Ocean fertilization), since nothing is done without causing other reactions. Maybe also threats and other effects that come with the applied methods. It says in line 33-34, that the focus is on the sequestration per unit deployment. Show this information the table --> How much carbon is sequestered per unit deployment for each method. [Gwenaëlle GREMION, Canada]	Rejected. Effects of CDR methods on biogeochemical cycles and climate are discussed in section 5.6.2.2
28000	69	25	69	25	Page 5-69, line 25: "CDR methods seek to remove CO <sub>2</sub> from the atmosphere either directly or by enhancing terrestrial, marine or geological carbon sinks to accelerate removal of CO <sub>2</sub> from the atmosphere." The same was a few sentences ago already mentioned in the introduction and thus feels unnecessary. I would either remove it here or in the introduction. Page 5-69, line 37: "The assessment emphasizes literature published since AR5; the IPCC Special Reports on the global warming of 1.5 degrees (SR1.5) and on climate change and land (SRCL) assessed CDR potentials and side effects but did not address the effects of CDR on carbon and 40 other biogeochemical cycles in detail." This sentence contains too much information, whereby the message is not clear. Maybe it is better to split this into two sentences. Page 5-69, line 37: "The assessment emphasizes literature published since AR5; the IPCC Special Reports on the global warming of 1.5 degrees (SR1.5) and on climate change and land (SRCL) assessed CDR potentials and side effects but did not address the effects of CDR on carbon and 40 other biogeochemical cycles in detail." This sentence contains too much information, whereby the message is not clear. Maybe it is better to split this into two sentences. Page 5-69, line 45: "The main methods suggested are summarised in Table 5.8." A repetition of line 27. Maybe it is a bit excessive to refer twice to the table so close together. [roderik van de wal, Netherlands]	Taken into account. Repetitions have been deleted.
23156	69	25	69	26	The CO <sub>2</sub> removal is mentioned repeatedly. Better: CDR methods seek to remove CO <sub>2</sub> directly or accelerate its removal from the atmosphere by enhancing terrestrial or marine carbon sinks. [Gwenaëlle GREMION, Canada]	Taken into account. Repetitions have been deleted.
9696	69	25	69	46	literature support for this discussion would make the discussion stronger [Brian Magi, United States of America]	Accepted. Additional references have been included.
23158	69	26			If you say geological, doesn't that include terrestrial already? [Gwenaëlle GREMION, Canada]	Rejected. Terrestrial refers to biological reservoirs on land, as explained further down in the text.
23162	69	30	69	36	Move the sentence starting with "Instead.." in line 33 to become the second sentence of the paragraph and remove the word "instead" because it makes this sentence disagree with the first sentence of the paragraph, and also "determining their effectiveness in reducing atmospheric CO <sub>2</sub> " to not repeat yourself and keep the sentences short and simple. [Gwenaëlle GREMION, Canada]	Accepted. Sentence was moved up.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
23164	69	31	69	33	Put the sentence starting with "Global carbon sequestration potentials" at the end of this paragraph. [Gwenaelle GREMION, Canada]	Accepted. Sentence was moved down.
17744	69	33	69	33	Insert , after 'Instead' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
23160	69	34			There is something wrong with the first part of this sentence. "... the focus is on sequesrtation biophysical potentials...". What do you want to say here? "The focus is on the sequestration of biophysical potentials" maybe? [Gwenaelle GREMION, Canada]	Not applicable. Wording in question has been deleted.
17746	69	42	69	42	Insert 'to' before 'four' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
17748	69	42	70	1	References needed in the text or the table. Also, if the table is to be split over two pages then please duplicate the column headings at the top of the second page [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. References are included in the text and/or in the table. Former Table 5.9, now Table 5.10, does not include side effects anymore, which is not included in a more comprehensive way in Figure 5.34. It now fits easily in one page.
23166	69	42			Considering the carbon cycle processes that result in CO2 removal, CDR methods may be divided into four major categories: ... [Gwenaelle GREMION, Canada]	Accepted. Sentence has been rephrased as suggested.
44116	69	45	69	46	Redundant statement: "main [CDR] methods suggested are listed in Table 5.8." [Sara Kahanamoku, United States of America]	Accepted. Sentence has been removed
36440	69	45			Insert 'which have been' before 'suggested'. This is important, otherwise it sounds like IPCC is suggesting methods of CDR, and IPCC should not be policy-prescriptive. [Nathan Gillett, Canada]	Taken into account. Sentence has been rephrased.
23168	69	45			For more fluent reading, switch wording to "The suggested main methods are..." [Gwenaelle GREMION, Canada]	Not applicable. Wording in question has been deleted.
23170	69	45			Why "suggested"? Aren't these methods existing and applied? Shouldn't statistics of how frequently the individual methods are used clearly reveal which ones are the main methods?! [Gwenaelle GREMION, Canada]	Taken into account. Sentence has been rephrased.
23192	69	49	69	52	Add a colum with the literature sources. [Gwenaelle GREMION, Canada]	Rejected. We decided to not include references in the table for sake of space. References are provided in the text.
35410	69	49	70	2	How is the CO2 capture and storage process from emissions from bioenergy crops (BECCS) different from the CO2 capture and storage process from emissions from other sources (DACCS)? The storage process is specified for DACCS but not for BECCS. If its the same basic process then the storage form for BECCS is the same as for DACCS, so inorganic. This should be made more explicit, especially when then in table 5.9 storage for BECCS is given as potentially permanent, as for DACCS. [Nadine Mengis, Canada]	Accepted. We agree that the storage form of BECCS, as well as DACCS, is in inorganic form. Labelling it as organic was a typo, and it was corrected.
49120	69	51	69	51	WG III Note - there is a need to look for links to WG III chapter 7 (AFOLU) and Chapter 12 (x-sectoral). These methods will also be addressed but with additional information on potentials and costs [Jim Skea, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. References to specific chapters/sections of WGIII will be included once a more advanced draft of the WGIII report becomes available.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
13500	69	51	70	8	Can direct Injection of CO2 into deep ocean be considered as a CDR option? [Govindasamy Bala, India]	Taken into account. Deep ocean included as a possible destination in the description of DACCS.
47392	69	51			I would query the replacing of BECCS in this table which is neither "enhanced production" nor "storage on land". It is offsetting fossil emissions with existing production and then storing geologically. [Chris Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The enhanced production in the case of BECCS refers to future increases in bioenergy crops, similar to afforestation. We slightly modify the table structure to include "storage on land (or geologically)".
23172	69	51			Table 5.8: Description of "Ocean fertilization" change the beginning to "Fertilization of upper ocean..." (because the main sentence is "Fertilization via XXX and carbon storage through XXX") [Gwenaelle GREMION, Canada]	Rejected. To keep the style of this column of this table, all CDR methods are described as actions (store, use, pyrolyze, capture, fertilize, etc.)
23174	69	51			Table 5.8 - Descriptions: Use terms consistently and do not always switch between "carbon" and "C" [Gwenaelle GREMION, Canada]	Accepted. C was replaced by carbon throughout the table.
23176	69	51			Table 5.8: Description of "Ocean alkalisation": Increased CO2 uptake via increased alkalinity by deposition of alkaline minerals (e.g. olivine). [Gwenaelle GREMION, Canada]	Accepted. We point out, however, that the style of the description column in Table 5.8 may vary to increase understanding of the method. In this case, we agree the reviewer suggestion improves the description of the process.
23178	69	51			Table 5.8: Description of "Direct air capture": The description should say HOW the CO2 gets directly removed and not repeat the type of method [Gwenaelle GREMION, Canada]	Accepted. Description was rewritten in a more complete way.
23180	69	51			Table 5.8: Can you add a column to indicate to show the type of method, which were mentioned earlier in the text "Direct removal", "enhancing terrestrial sink", "enhancing marine sink". [Gwenaelle GREMION, Canada]	Taken into account. The four categories are originally described in the first column of Table 5.8. However, since it apparently was not clear to this reviewer, we modified the first column and the earlier text to make them match more closely.
16216	69		70		Table: the removal process and storage form of BECCS should not be classified as biological and organic; monoculture trees have very little to do with natural ecosystems and forests, and storing CO2 in geological formations via CCS is a technological, not an organic approach. [Linda Schneider, Germany]	Taken into account. Please note that the third column synthesizes two types of information, nature of CO2 removal process, and after the slash (/), the storage form. We agree that the storage form of BECCS, as well as DACCS, is in inorganic form. Labelling it as organic was a mistake, and it was corrected. However, monoculture trees, even if having little to do with natural ecosystems, are still of biological nature. With respect to the category of DACCS, we have renamed it to technological (chemical).
48128	69		79		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]	Accepted. Overlap with Chapter 4, section 4.6.3 has been largely eliminated.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
33368	69				This section 5.6 is in part a repeat and overlap of the section in Chapter 4, 4.6.3 (Climate Response to Mitigation, Carbon Dioxide Removal, and Solar Radiation Modification) [Michael Schwabe, Uruguay]	Accepted. Overlap with Chapter 4, section 4.6.3 has been largely eliminated.
23190	69				General comment on introduction of 5.6.2 : as explicitly said in the introduction of 5.6.3. for SRM, this subsection should have a sentence aiming to warn about negative effects caused by of most of these methods. If just reading this introduction, one may think that CDR methods are the perfect solutions without any consequences [Gwenaelle GREMION, Canada]	Rejected. The introduction is meant to lay out the scope of the section. Side-effects of specific CD methods are discussed in section 5.6.2.2.
23186	70	5	70	7	Group into removal from atmosphere and removal from seawater --> These methods include biomass burial, ocean downwelling, and cloud alkalisation, as well as the removal of CO2 from seawater with storage (Keller et al., 2018b). [Gwenaelle GREMION, Canada]	Rejected. We find it more logical to group ocean-based methods together.
47108	70	5	70	8	5.6.2.: Wood harvesting can have indeed effects on other biogeochemical cycles - it depends on the amount of harvesting probably and the health of the remaining forest/plantation. [Sophie von Fromm, Germany]	Not applicable. Sentence in question was deleted.
23182	70	5	70	8	Keep this paragraph attached to the previous paragraph on page 69, line 42-46. This explains, the word "suggested" here. [Gwenaelle GREMION, Canada]	Accepted. Paragraph was moved before table.
23184	70	5			Change wording: Many other CDR options have been suggested, but their assessments are not possible, due to insufficient literature. [Gwenaelle GREMION, Canada]	Rejected. We prefer our original wording.
13530	70	6	70	7	There is no reference to "removal of CO2 from seawater with storage, and cloud alkalisation" in Keller et al. 2018b. [Govindasamy Bala, India]	Taken into account. Additional references have been included.
23188	70	8			Change wording at the end of the sentence: "... has no other biogeochemical implications, and therefore its potential is not discussed in the context of this report. [Gwenaelle GREMION, Canada]	Accepted. Wording was changed as suggested.
17750	70	19	70	19	Change to 'number' and 'exist' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
6712	70	20	70	20	Add citation to MacDougall 2013 (doi:10.1002/2013GL057467) [Andrew MacDougall, Canada]	Rejected. In this section we only consider emissions-driven CDR studies.
23196	70	25	71	3	The wording "This section..." has been used in the beginning of the entire section 5.6.2.1. It is strange to use again here, because you already said what this section is for, and here the scope of the section is changed. Bring this complete last sentence (p. 70, line 25 to p.71, line 3) up to join the introductory paragraph (p.70, line 16), where you talk about the focus of this section. [Gwenaelle GREMION, Canada]	Taken into account. Section has been replaced with subsection. We prefer to keep the subsection outlook here rather than moving it to the introduction.
16218	70		70		Ocean fertilization is prohibited under the London Protocol of the London Convention. Why does the IPCC continue to discuss a technology that has been banned by the international community due to its excessive adverse impacts on the marine environment? [Linda Schneider, Germany]	Taken into account. Ocean fertilization is discussed for completeness. We have included mention that ocean fertilization is prohibited under the London Protocol.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
28028	71	1	71	1	In this figure it is not directly clear if the 'Carbon reservoir size increase' includes or excludes the 'Natural carbon redistribution in response to a perturbation' and the 'Carbon reservoir size increase due to CDR storage'. Maybe these distinctions and definitions can be more elaborately explained in the caption. Furthermore for 'where carbon has been deliberately removed from a reservoir', is there a difference between the grey or yellow dotted lines? [roderik van de wal, Netherlands]	Accepted. Elements of the detail are explained in more detail.
47394	71	6			make sure numbers in this box are consistent with earlier paragraph (p.13) and figure 5.3 [chris jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We have ensured consistencies with earlier text and figures in this chapter.
23198	71	8	71	12	To better see the difference and comprehend each time frame's impact on CO2 cycle, can you group the information concerning the industrial era first (line 9-10, 13-16) and add information about the percentage of CO2 in the atmosphere, ocean, biosphere, as mentioned for today's time. And then continue with all the information about today (line 11-12, 16-19). [Gwenaelle GREMION, Canada]	Rejected. Information in lines 9-10 and 13-16 refers to different periods/situations.
17752	71	9	71	9	Change to 'Industrial Era' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
17754	71	10	71	10	Change to 'geological' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
37746	71	11	71	12	Please see comments 231 and 238, which apply to the wording here. It would be correct to write that the amount of CO2 stored in the ocean was equal to 30% of the amount of CO2 emitted by humans, but it is incorrect to write that 30% of the emitted CO2 was taken up by the oceans. Also, why is the decade 2007-2016 here, but 2008-2017 in the text referred to in comments 233 and 238? [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Unclear what revision is requested here. The decade 2007-2016 has been made consistent with the decade 2008-2017 used earlier in the chapter.
47498	71	11	71	12	Inconsistent with chapter 5 page 6, line 41-43: The fate of the carbon emitted from human activities during the decade of 2008–2017 (annual average 10.9 PgC yr-1) was: 44% accumulated in the atmosphere (4.7 PgC yr-1), 22% was taken up by the ocean (2.4 PgC yr-1) and 29% was removed by terrestrial ecosystems (3.2 PgC yr-1). [Birgit van Munster, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We have ensured consistencies with earlier sections in this chapter.
16034	71	11	71	12	Suggest aligning the data period (2007-2016) and the apportionment of CO2 in the atmosphere, ocean and terrestrial ecosystem with that shown in the Executive Summary (P.6, Lines 41-43). [SAI MING LEE, China]	Accepted. Periods have been aligned.
17756	71	16	71	16	Change to 'Industrial Era' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
36442	71	16		19	It isn't the case that as soon as CDR exceeds CO2 emissions, the land and ocean will immediately begin to outgas. For example, consider a scenario in which emissions increase, and then CDR starts instantaneously which just exceeds emissions. This is analogous to a zero emissions experiment. Land and ocean will initially continue to take up carbon. Land and carbon will only begin to outgas with some lag from the start up of CDR, which depends on how large net negative emissions are and on the emissions pathway prior to starting CDR. [Nathan Gillett, Canada]	Accepted. We have revised the text accordingly.
7520	71	22	71	30	Cut the alternate figure heading that is embedded in the figure. [Rose Abramoff, France]	Accepted. Duplicate caption has been deleted.
17758	71	25	71	26	Change to 'System' x3 [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - The same spelling as in the IPCC SR1.5 / SROCC has been applied ("system" has been adopted throughout the chapter for consistency)
47396	71	37			see Schwinger et al (2018, GRL) for a nice analysis of reversibility (or not) of different components of ocean carbon store [chris jones, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Reversibility of climate system components, including carbon stores, is discussed in Chapter 4
23202	71	44	71	44	i.e. if the responses were symmetric [Gwenaelle GREMION, Canada]	Accepted - text revised
16220	71	46	71	50	What exactly did these simulations find? How is the response increasingly asymmetric for pulse emissions/removals >100 PgC? The paragraph is very abstract and some more detail on how individual components of the carbon cycle respond would be helpful, especially since many IAM scenarios assume deployment of CDR >100 PgC. [Linda Schneider, Germany]	Accepted. More details have been provided and the implications of these results have been discussed.
27804	71	52	71	52	attend the placeholders [Poot Delgado Carlos Antonio, Mexico]	Taken into account. Placeholder has been replaced with actual text.
13502	72	3	72	11	The Figure 5.31 has no labels and hence it is hard to interpret the figure. [Govindasamy Bala, India]	Accepted. Original figure had labels etc. but was not correctly reproduced in pdf.
47398	72	3			yes, I like this figure - can you show behaviour on a shorter timescale than 1000 years? (100 is of clear relevance too for this chapter) [chris jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We have redrawn the figure for a 100 year timescale.
27806	72	11	72	11	attend the placeholders [Poot Delgado Carlos Antonio, Mexico]	Taken into account. Placeholders have been eliminated where data has become available.
17760	72	16	72	16	Insert , after 'AR5' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
9698	72	16	72	16	Change "Since AR5 studies" to "Since AR5, studies" [Brian Magi, United States of America]	Accepted - text revised
7522	72	16	72	19	There is no main clause in this sentence. [Rose Abramoff, France]	Accepted. Sentence has been rephrased.
23200	72	16	72	19	The sentence seems too long and is not clear [Gwenaelle GREMION, Canada]	Accepted. Sentence has been rephrased.
17762	72	17	72	17	Capital C for century [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - The AR6_WGI_StyleGuide says to use "century"
13504	72	22	72	22	"Source-sink" should be "sink-source". Same issue on lines 26 and 34. [Govindasamy Bala, India]	Accepted - text revised

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
23204	72	23	72	25	It sounds strange that "...carbon sinks respond to the history of atmospheric CO2...". Better: "...as carbon sinks respond much later to then historic atmospheric CO2 rates, which were emitted decades earlier, and not to the instantaneous emissions rates. The CO2 uptake can continue for decades to centuries even after emissions have become net negative." [Gwenaelle GREMION, Canada]	Accepted. The wording has been revised.
32772	72	24	72	25	What is the uncertainty of this statement (i.e., that net uptake will continue for such a long time, especially given that FACE experiments, at least what I recall of them, suggest that ecosystems largely come into balance with increased CO2 concentrations within a decade, and even less)? While the following sentences suggest results are uncertain, it would be helpful to have an indication of its magnitude. [Michael MacCracken, United States of America]	Accepted. More details about the uncertainty in the timing of the sink-to-source transition have been included.
23208	72	27	72	29	What about the response from the ocean and biosphere? Why is it here less uncertain than the land sink? Even if there is no research on this, add the information, that there is missing data. [Gwenaelle GREMION, Canada]	Noted. Models generally have a larger spread in the land than in the ocean carbon cycle response.
23206	72	27			Can you specify what the uncertainty means? e.g. add information about what Jones et al found about the timing: ... particularly for the land sink, where responses vary from xxx PgC uptake within xxx years to yyy PgC uptake after yyy years/centuries, while emissions decreased/increased from xxx PgC to yyy PgC respectively. [Gwenaelle GREMION, Canada]	Accepted. More details about the uncertainty in the timing of the sink-to-source transition have been included.
27808	72	31	72	31	attend the placeholders [Poot Delgado Carlos Antonio, Mexico]	Taken into account. Placeholders have been eliminated where data has become available.
57280	72	31	72	35	In future version, a section of effects for low stabilization scenarios should be included [Oliver Geden, Germany]	Rejected. The lowest SSP with an extension beyond 2100 is SSP1-2.6.
13506	72	40	72	43	It is really interesting that the ocean does not become a source even by 2300. Will be good to know how long does it take for ocean to outgas CO2 in the CDR scenario. Hopefully, this will be addressed in SOD. [Govindasamy Bala, India]	Taken into account. We are planning to analyse simulations that extend beyond 2300 if the become available on time.
27810	72	43	72	43	attend the placeholders [Poot Delgado Carlos Antonio, Mexico]	Taken into account. Placeholders have been eliminated where data has become available.
23210	72	49			Shouldn't it be "... the responses of land and ocean carbon sinks are sensitive..." sinc the land and ocean carbon sinks respond differently, so it is more than one response? [Gwenaelle GREMION, Canada]	Rejected. "Response" includes suite of processes.
23212	73	3	73	4	It seems rather odd to repeat the source at the end of the sentence, just for the model's name. Can you not include the model's name before and just cite one time? [Gwenaelle GREMION, Canada]	Rejected. It is common practice to cite the reference that coined a term.
23214	73	7			Again strange to read about the "history of CO2". Could you write "...from the response to the historic atmospheric CO2 rates..."? [Gwenaelle GREMION, Canada]	Accepted. The wording has been revised.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
23216	73	12	73	14	CDR and "effectiveness" repetition makes it hard to read. Better: "... the effectiveness of CDR is found to be rather insensitive to the removal's rate and amount (source), while it is strongly dependant on the applied emissions scenario (source). In scenarios with higher background atmospheric CO2 concentrations, the effectiveness of CDR is found to be larger, due to state dependencies and climate-carbon cycle feedbacks, that lead to a weaker overall response to CO2 removal (source). [Gwenaelle GREMION, Canada]	Noted. We adopted part of the suggested edits.
36444	73	14		16	The text states that the effectiveness of CDR is larger in scenarios with higher background CO2 concentration. This is true in terms of the unit change in atmospheric CO2 per unit carbon removed, and is a consequence of the higher airborne fraction at high CO2. But I would not expect it necessarily to be true for the climate response, because the GSAT response per unit change in atmospheric CO2 is smaller at high background CO2 (due to the dependence of the radiative forcing on the logarithm of the CO2 concentration perturbation). Is this accounted for in the cited in prep Zickfeld (2019) reference? Without having looked at model experiments, I would expect to first order that these effects would cancel, and that in terms of the climate response the effectiveness of CDR would be approximately independent of the background level of CO2, consistent with the approximate proportionality of warming to cumulative carbon emissions. [Nathan Gillett, Canada]	Accepted. This point has been clarified in the text.
7524	73	15	73	15	Typo: missing "to" in "due [to] state dependencies" [Rose Abramoff, France]	Accepted - text revised
23218	73	15	73	15	Missing "to" within "... due state dependencies..." [Gwenaelle GREMION, Canada]	Accepted - text revised
9700	73	15	73	15	Change "due state" to "due to state" [Brian Magi, United States of America]	Accepted - text revised
27812	73	17	73	17	attend the placeholders [Poot Delgado Carlos Antonio, Mexico]	Taken into account. Placeholders have been eliminated where data has become available.
17764	73	24	73	24	Delete 'the' and 'period' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
17766	73	26	73	26	Delete , after al. [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account
14474	73	37	73	45	One of the largest anthropogenic emissions are due to landfills, probably the greatest in regards to carbon absorption in soil. It is important to extend the CDR discussion (Section 5.6.2.2) to tackle landfills as it is still the major wastemanagement approach in sub-saharan Africa, contributing to biogeochemical cycles. [Ivan Lule, Uganda]	Rejected. Landfill management is not a CDR method, in the sense that it does not remove CO2 already present in the air. Instead, it may be considered a mitigation option, to avoid future emissions. This subject is the mandate of WG3.



Comment ID	From Page	From Line	To Page	To Line	Comment	Response
49492	73	37			A discussion of the time scales over which land-based CDR effective is notably missing here. For example, Houghton et al. 2015 ( <a href="https://doi.org/10.1038/nclimate2869">https://doi.org/10.1038/nclimate2869</a> ) argue that forest-based CDR is a bridge, stabilizing atmospheric growth of CO2 while low carbon technologies are established to replace those relying on fossil fuels. Griscom et al. 2017 similarly note that the efficacy of their suite of land-based CDR methods begins to saturate around 2060, thus implying that many are only a short term solution meant to usher in an error of carbon free energy and technology. [Seth Spawn, United States of America]	Accepted. Second paragraph of Section 5.6.2.2.1 address the time scale discussion.
47240	73	37			While I agree that it is important to discuss the potential impacts of (land-based) CDR techniques on climate, what about the limitations of CDR techniques due to climate constraints (water limitations and extreme events for instance) ? It might be also an important issue about the effectiveness of CDR which may need to be considered following the risk assessment framing proposed by Rowan Sutton (ESD 2018 or BAMS 2019). [Hervé Douville, France]	Taken into account - discussed shortly in the text, and partly in the table column "termination effects", which replaces the old column "CDR reversibility"
47400	73	39			this section is very important - will need to be checked against SRCLL when it is released to ensure consistency with the special report which has much coverage of land-based mitigation and consequences [chris jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. This section was revised after the SRCLL SOD was released and is now consistent with the SRCLL final version. In particular, most CDR potential numbers in Table 5.10 are consistent with SRCLL.
23250	73	39			In this entire chapter, there is specific information missing on how much each method is actually adding/changing to the CO2 sequestration rates and in what time frame. Does it take/lasts decades or years for this change and effect? [Gwenaelle GREMION, Canada]	Specifically for the CDR method, the information is present in Table 5.8 as well as in Figure 5.34.
28002	73	41	73	41	Page 5-73, line 41. First time that the abbreviation GPP is used in this chapter, please explain. Page 5-73, line 41. It is not clear how via 'disturbances' CO2 is returned to the atmosphere. Maybe clarify this a bit more or provide an example. Page 5-73, line 41. It is not clear to me how via 'disturbances' CO2 is returned to the atmosphere. Maybe clarify this a bit more or provide an example. [roderik van de wal, Netherlands]	Accepted. Text changed for clarification of these points.
17768	73	41	73	42	Surely biological decomposition also contributes [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. The term "biological decomposition" is too generic, as it includes not only the heterotrophic respiration (loss of carbon by organisms other than primary producers) but also other forms of decomposition, like mineralization, that is not related to the carbon cycle. We prefer to keep heterotrophic respiration here, to keep it focused on the carbon cycle.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
16222	73	47	71	51	More importantly, Co2 sequestration rates are much higher in biodiverse, natural ecosystems/forests than in monoculture tree plantations that are very susceptible to pests and fires. I.e. natural ecosystems/forests are far more resilient as carbon stocks. Generally, natural ecosystems are currently far below their natural carrying capacity due to e.g. wood harvest rates and ecosystem destruction. [Linda Schneider, Germany]	Taken into account. Text was modified to include some of the points. See second paragraph of Section 5.6.2.2.1.
35412	73	47	74	1	Large-scale afforestation can change the water recycling in the atmosphere as well as the runoff and thereby the freshwater input into the ocean (Mengis et al., 2019). 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 - the reference has been added and the text has been revised
23246	73	47		48	What are the typical rates for these different forests? Add specific numbers. [Gwenaelle GREMION, Canada]	Taken into account. Uncertainty in the literature is large, so we limit ourselves to the qualitative statement that restoring degraded tropical forests is the most effective approach to maintain carbon stocks from depletion and sustaining CO2 sequestration rates.
47774	73	49	73	49	Please check the use of this IPCC uncertainty language term. Are you able to provide a traceable account to assigning this uncertainty statement? Note that likelihood statements are quantified terms - phrases like likely and very likely have quantifiable probabilities associated with them. Please check it has been used correctly here. Please refer to the IPCC guidance note on uncertainty: <a href="https://wg1.ipcc.ch/SR/documents/ar5_uncertainty-guidance-note.pdf">https://wg1.ipcc.ch/SR/documents/ar5_uncertainty-guidance-note.pdf</a> [WGI TSU, France]	Accepted. Since we have not assessed probabilities, the text was modified by replacing "is likely to" by "may".
49482	73	50	73	50	Fargione et al. 2018 (DOI: 10.1126/sciadv.aat1869) report that avoided grassland conversion is a particularly effective way of maintaining existing C stocks and sequestration rates in temperate areas where perennial grasslands are particularly vulnerable to land use change (see Lark et al. 2015 [ <a href="https://doi.org/10.1088/1748-9326/10/4/044003">https://doi.org/10.1088/1748-9326/10/4/044003</a> ] and Spawn et al. 2019 [ <a href="https://doi.org/10.1088/1748-9326/ab0399">https://doi.org/10.1088/1748-9326/ab0399</a> ]). These effects could also be noted in table 5.9. [Seth Spawn, United States of America]	Taken into account. Partially included in the third paragraph of Section 5.6.2.2.1. Side effects are now presented more clearly in Figure 5.34.
23248	73	51		52	What mean "substantial variation"? Specific numbers/examples of the rates and change of rates would help understand better. [Gwenaelle GREMION, Canada]	Taken into account. Actual numbers are presented in Table 5.10 and Figure 5.34.
17770	73	52	73	52	Insert 'the' after 'of' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
23220	73	52			the age of the forest or "the forest's age" [Gwenaelle GREMION, Canada]	Accepted. Text changed.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
23222	73	54	74	55	Northward shift of the tree line will favour carbon sequestration in vegetation but feedbacks to the climate system it would cause might be dominated by other processes leading to an increased warming. In particular due to changes in albedo (Arctic greening), evapostranspiration (see "Shifts in Arctic vegetation and associated feedbacks under climate change" (Pearson et al. 2013) and larger sources of CH4 (see for example "Tundra shrubification and tree-line advance amplify arctic climate warming: results from an individual-based dynamic vegetation model" (Whang et al.2013) ). [Gwenaelle GREMION, Canada]	Taken into account - this is an important remark, being addressed in the text and in a new table "CDR methods references". The larger sources of methane in the referred paper are due to higher temperature and the earlier onset of growing season, not to afforestation or shift of treeline per se. Pearson et al. 2013 reference added.
13534	73	56	74	1	Large scale Afforestation can also have remote effects such as shifting the ITCZ in the tropics. Reference: 30. Devaraju, G. Bala and A. Modak, 2015: Effects of large scale deforestation on precipitation in the monsoon regions: Remote versus local effects, Proceedings of the National Academy of Sciences, doi.10.1073/pnas.1423439112 [Govindasamy Bala, India]	Accepted - added a mentioning of the remote effects in a new table "CDR methods references"
13532	74	1	74	1	A good reference for the biophysical effect is a 2015 review paper in Plant Cell and Environment: 32. Devaraju, G. Bala, and R. Nemani, 2015: Modeling the influence of land-use changes on biophysical and biochemical interactions at regional and global scales, Plant, Cell and Environment, doi: 10.1111/pce.12488 [Govindasamy Bala, India]	Taken into account - added this reference in a new table "CDR methods references"
32774	74	3	74	13	There are also suggestions for genetic modification of plants to increase root mass, or root mass fraction, etc. In my view, such genetic modification raises serious risks over and beyond other considerations, but such research is apparently underway. [Michael MacCracken, United States of America]	Rejected - no scientific evidence/publication provided to support changes suggested by the reviewer.
12834	74	3	74	13	Building up soil carbon and re/afforestation are both critical strategies, but have very different beneficial and adverse side-effects, from increased crop yields from soil carbon increases to increased biodiversity from reforestation. Recommend distinguishing between them. [Durwood Zaelke, United States of America]	Taken into account - the text has been revised so that a separate assessment on the side affects is done
49484	74	3	74	13	Fargione et al. 2018 builds on Griscom et al. 2017 by reporting a more detailed analysis for the USA, a potential bellweather of land-based CDR potential in the temperate zone. On top of the well known sequestration effects of forestry methods, they report a potentially significant effect of avoided grassland conversion, cover crops, alley cropping, biochar, and cropland nutrient management. In many cases the estimated effects are greater than those reported by the global analysis by Griscom et al. 2017 per unit area. These effects could also be noted in table 5.9. [Seth Spawn, United States of America]	Accepted. The natural capital solution with the highest potential for carbon removal in Fargione et al. (2018) were added to the discussion.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
12668	74	3	74	13	Soil Carbon and re/afforestation have very different beneficial and adverse side-effects, from increased crop yields from soil carbon increases to increased biodiversity from reforestation. Recommend distinguishing between them. [Kristin Campbell, United States of America]	Taken into account. Former Table 5.9, now Table 5.10, does not include side effects anymore, which are now included in a more comprehensive way in Figure 5.34. The figure separates biogeochemical and biogeophysical effects of CDR from other side effects (e.g. on biodiversity, food, water security). New information on side effects has been added.
49486	74	10	74	10	I agree that the soil carbon sequestration potential of any land-based CDR has "medium confidence" however, it seems in the literature that, in comparison to the effects of re-/afforestation as mentioned here, there is greater confidence about the sign and even magnitude of soil carbon sequestration potential of grassland restoration on a per unit area basis. See Sanderman et al. 2017 ( <a href="https://doi.org/10.1073/pnas.1706103114">https://doi.org/10.1073/pnas.1706103114</a> ), Don et al. 2011 ( <a href="https://doi.org/10.1111/j.1365-2486.2010.02336.x">https://doi.org/10.1111/j.1365-2486.2010.02336.x</a> ), Poeplau et al. 2011 ( <a href="https://doi.org/10.1111/j.1365-2486.2011.02408.x">https://doi.org/10.1111/j.1365-2486.2011.02408.x</a> ), Li et al. 2018 (DOI: 10.1111/gcb.14328), Guo and Gifford 2002 ( <a href="https://doi.org/10.1046/j.1354-1013.2002.00486.x">https://doi.org/10.1046/j.1354-1013.2002.00486.x</a> ). These effects could also be noted in table 5.9. [Seth Spawn, United States of America]	Accepted. A distinction was made between the grassland restoration and generic carbon sequestration methods.
23224	74	10	74	13	Both have several beneficial (low confidence) and adverse (low confidence) side effects. With high confidence, deployment of both methods will decrease bioiversity unless wisely adopted. Isn't loss of biodiversity a substantial adverse side effect ? [Gwenaelle GREMION, Canada]	Not applicable - the table has been revised. The biogeochemical and biophysical effects have been separated from the other effects (biodiversity, food, etc) and moved to an annex table "CDR methods references" . The sentences have been revised accordingly.
23226	74	12	74	13	... unless wisely adopted makes it sound as if we are aiming for less biodiversity. Maybe change to: "... oftentimes unwise deployment of both methods will decrease biodiversity, but can be maintained/enhanced by proper/adjusted management. [Gwenaelle GREMION, Canada]	Accepted - wording was changed
36446	74	12		13	Why will reforestation decrease biodiversity? I would have expected reforestation to increase biodiversity. Also, aren't affects on biodiversity out of scope for WGI? [Nathan Gillett, Canada]	Taken into account. Reforestation may decrease biodiversity if it is done by replacing original, pristine ecosystem (i.e., not adopted wisely). This issue is now better addressed in the table. We have been asked by the WGI co-chair to make links to IBPES assessments, i.e. biodiversity.
23228	74	17	74	18	Can you add examples for how the different feedstock is affecting the sequestration potential with specific numbers? [Gwenaelle GREMION, Canada]	The SOD no longer discusses carbon potential per unit deployment, which has been discussed extensively by the SRCCL.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
12836	74	20	74	33	BECCS is not carbon negative in the critical window of the next couple of decades, and leaves a carbon deficit for 44 to 104 years, depending on the source material. John D Sterman et al., Does replacing coal with wood lower CO2 emissions? Dynamic lifecycle analysis of wood bioenergy, ENVIRON. RES. LETT. 13 (18 January 2018); Mary S. Booth, Not carbon neutral: Assessing the net emissions impact of residues burned for bioenergy, ENVIRON. RES. LETT. 13 (21 February 2018) (“Though oxidizing 100 EJ of biomass [the amount of bioenergy assumed in some models] would emit about 9 Gt of CO2 each year, most mitigation models assign bioenergy zero net emissions.”) To understand the error Booth describes, she proposes an approach to carbon accounting of biomass that avoids some of the pitfalls of zero pollution assumptions by listing net emissions (direct emissions minus alternative fate emissions) as a percent of direct emissions (called Net Emissions Impact (NEI)). In other words, NEI calculates the percent of direct biomass CO2 emissions that contribute additional warming effects over a 50-year period. Since direct emissions from biomass can be higher than coal, Booth’s findings of 20-95% NEI from biomass burning shows the significant carbon impact of bioenergy, even with replanting and utilizing leftover biomass waste and even over 50 years. In addition, BECCS requires a massive amount of land, and risks competing with food security. Anderson K. & Peters G. (2016) The trouble with negative emissions, SCIENCE 354:182–183, 183. Moreover, the CCS part of BECCS is not yet developed either as a technology that can scale, nor as a technology that is socially acceptable. Vaughan & Gough, Expert assessment concludes negative emissions scenarios may not deliver, Environmental Research Letters (August 2016). [Durwood Zaelke, United States of America]	Taken into account. Discussion on BECCS has been considerably expanded, including some of the references suggested.
49488	74	20	74	33	I feel that this section needs to make a further distinction between annual and perennial bioenergy crops. Soil carbon depletion associated with bioenergy crops is often noted for annual crops but the opposite trend is often reported for perennial crops which, when planted on "marginal lands" have the potential to increase soil organic carbon (see Robertson et al. 2018 [DOI: 10.1126/science.aal2324] for an overview) and decrease net ecosystem exchange (see for example Abraha et al. 2019 [https://doi.org/10.1088/1748-9326/aafc10]). Moreover, while annual monocultures may decrease biodiversity, perennial biofuel crops may in many cases represent a biodiversity benefit (see for example Werling et al. 2013 [ https://doi.org/10.1073/pnas.1309492111]). These effects could also be noted in table 5.9. [Seth Spawn, United States of America]	Taken into account. Woody and herbaceous bioenergy crops are distinguished in the discussion.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
16224	74	20	74	33	Given the prominent role that BECCS plays in IAM modelling, the findings in this paragraph should be lifted to the summary on page 9; both the 50-90% reduction in CDR potential of BECCS and the adverse side effects. Also: why is this statement assigned „low confidence“? – there is ample real-world experience with the adverse effects of bioenergy/agrofuels on communities and ecosystems which would be exacerbated many times over when scaling up BECCS to industrial levels. [Linda Schneider, Germany]	Taken into account. The carbon losses along the BECCS chain was raised to the summary. This chapter assesses only biogeochemical feedbacks. Effects on communities are the mandate of WG3.
39770	74	22	74	27	Richards et al 2017 could add more detail to both statements here <a href="http://onlinelibrary.wiley.com/doi/10.1111/gcbb.12360/abstract">http://onlinelibrary.wiley.com/doi/10.1111/gcbb.12360/abstract</a> [Dagmar Henner, Austria]	Taken into account. Included in the discussion.
39772	74	22	74	33	Bioenergy crops are currently mostly planted in agricultural soils. For completeness of this paragraph, you should add some information about woody bioenergy crops compared with agricultural crops. Henner, PhD thesis 2019, University of Aberdeen. There will also be an article on this topic in the next months. [Dagmar Henner, Austria]	Taken into account. Woody and herbaceous bioenergy crops are distinguished in the discussion.
23230	74	22			In the beginning of the paragraph it seemed to be focusing on the carbon sequestration potential, while the 2nd sentence starts, focusing on DGVMs. For a better flow and connection of the information, change beginning of 2nd sentence to: "An increase in the net carbon uptake is simulated by DGVMs, if marginal land is replaced by woody bioenergy plants... [Gwenaelle GREMION, Canada]	Taken into account. Text changed, following comments 23230 and 23232.
23232	74	24			Sentence too long. Finish sentence after the source (... Smith et al 2012b). Continue with new sentence: Depletion of the soil-carbon stocks and a reduction of the additional sink capacity of standing forests on the other hand, could be triggered by replacing carbon-rich ecosystems with herbaceous bioenergy plants (source). [Gwenaelle GREMION, Canada]	Taken into account. Text changed, following comments 23230 and 23232. We use a different sentence structure, since the one suggested was disconnecting the second result from DGVM simulations.
23234	74	27			EITHER "Further (no comma) carbon losses..." OR "Furthermore, (comma) carbon losses..." [Gwenaelle GREMION, Canada]	Accepted. Text changed.
23236	74	30			Check source ANDREAS Krause et al ----> Erase first name [Gwenaelle GREMION, Canada]	Accepted. Citation changed.
26968	74	32	74	33	Please elaborate what adverse side effects are meant here and explain why BECCS in general should decrease biodiversity. If managed properly and / or if restrained to land already under agricultural management, especially the latter is not necessarily true. [Joachim Rock, Germany]	Taken into account - these effects are explained in a new annex table "CDR methods references". BECCS is likely to decrease biodiversity if done by establishing rapidly growing crops or monocultures, as in typical scenarios. The impacts on BD are greatly dependent on the grown species. See also replies to review comments #36446 and #26972
23238	74	32		33	Add examples for "adverse side effects" [Gwenaelle GREMION, Canada]	Taken into account. Main side effects are now presented more clearly in Figure 5.34.
23242	74	40	78		Please repeat the first row of the table with the row's title on each page, to make it easier to read on. [Gwenaelle GREMION, Canada]	Taken into account

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
49122	74	42	74	42	WG III Note - there is a need to look for links to WG III chapter 7 (AFOLU) and Chapter 12 (x-sectoral). These methods will also be addressed but with additional information on costs [Jim Skea, United Kingdom (of Great Britain and Northern Ireland)]	Noted - agree
13536	74	42	78	2	In the last column for the afforestation, this reference may be included: Bala, G., K. Caldeira, M. Wickett, T. J. Phillips, D. Lobell, C. Delire, and A. Mirin, 2007: Combined climate and carbon cycle effects of global deforestation, Proceedings of the National Academy of Sciences, 104(16), 6550-6555 [Govindasamy Bala, India]	Rejected - the reference list cannot be exhaustive, and this is already an old paper.
13538	74	42	78	2	In the last column for ocean fertilization: The 2nd point should be enhanced "deep" ocean acidification [Govindasamy Bala, India]	Rejected - the potential enhanced uptake of CO2 by increased primary production would decrease pH of surface waters. If artificial fertilisation continued for decades, then potentially deep waters could be affected (the long timescales of ocean circulation suggest any effect would not be immediate)
13544	74	42	78	2	The upper limit of CDR potential of weathering is listed as 26 PgC per year. This appears too large. Is there a typo? Similarly the CDR potential of ocean alkalization is listed as <27 PgC per year. This also appears too high. [Govindasamy Bala, India]	About enhanced weathering: These are still very uncertain numbers. This upper rate of 95 GtCO2/yr; 26 PgC yr-1 was taken from Strefler et al. 2018, which was also included in the Fuss et al assessment. We agree that the addition of silicate minerals to soils is still highly uncertain in terms of its potential, which is the same conclusion of SRCCCL chapter 4. However, we have decided to use in the AR6 the numbers for land-based CDR in agreement with the technical potentials in SRCCCL, which is more recent. However, SRCCCL is inconclusive about the technical potential of enhanced weathering. Quoting from SRCCCL 2.6.1.6: "While the geochemical potential is quite large, agreement on the technical potential is low due to a variety of unknown parameters and of limits such as rates of mineral extraction, grinding, delivery, and challenges with scaling and deployment." About ocean fertilization (OF): This upper limit was probably left due to a mistake while revising the FOD. This number came from Oeschles 2009 paper, which is an outlier (98 PgCO2/yr; 27 PgC/yr), and was included in an earlier version of the FOD.
23240	74	44			What means BGC? I believe it is the first time, this abbreviation is used, even though you talked about biogeochemical cycles before. [Gwenaelle GREMION, Canada]	Accepted. BGC was spelled out.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
47402	74	45			"asterisks indicate confidence levels" - I couldn't see asterisks on the table? But this is a good idea to show confidence levels [chris jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Asterisks are shown in a new table "CDR methods references" (where part of the columns from a former Table 5.9 were moved). Confidence levels will also be shown in the next version of the Figure 5.34 in the next draft.
17772	74	46	78	1	If the table is split over two pages then please duplicate the column headings at the top of each page (I have flagged this as a clarity issue!) [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable - the size of the table has been reduced so that it fits into one page
41850	74	46			Table 5.9 : line "soil carbon sequestration", last column : the sentence "N2O emissions if fertilised (Gu et al.,2017), reduction of N2O emissions" ,appears contradictory. Please clarify. [Marc Aubinet, Belgium]	Taken into account.
36448	74				Table 5.9. The meaning of the column 'Feedbacks on CO2 sequestration potential' is not clear. [Nathan Gillett, Canada]	Not applicable - the table has been revised and this column no more exists
26970	75	1	75	1	Table 5.9, first row (afforestation, reforestation and forest management): Please explain the statements in column "Feedbacks on CO2 Sequestration Potential": how should A/R or FM lead to warming? How should they reduce soil moisture when compared with alternative land uses or land cover? How should they lead to warming that increases the occurrence of pests etc.? If you do not refer to feedbacks from A/R and FM to other "BGC cycles and climate", but to influences on A/R / FM, please rephrase the text to make this clearer. [Joachim Rock, Germany]	Not applicable - the table has been revised and this column no more exists
26972	75	1	75	1	Table 5.9, first row (afforestation, reforestation and forest management): Please prove the statements in column "Impacts on Other BGC Cycles, Climate and Biodiversity (BD)" in a more comprehensive way: first, A/R and FM can have very different impacts as the first two usually mean a change in vegetation cover and vegetation type, whereas FM usually means small changes in existing vegetation. In addition, whether transpiration or soil moisture change with regard to the alternative vegetation depends on species planted and / or the type and intensity of management that is applied. The impact on biodiversity also depends on species choice and management which can easily also increase BD. [Joachim Rock, Germany]	Taken into account - it is true that the implications can vary widely depending on the vegetation type, the choice of species, intensity of management, etc. The text was very compact due to space limitations. The text has been elaborated to indicate more this high variability in the impacts of the measures.
45792	75	1	76		fire risk: not consistent with IPCC definition of risk [Katja Mintenbeck, Germany]	Not applicable - the table has been revised and this column no more exists
23244	75				Biochar --> column 6: What is the initial reason of this feedback? Warming (?) to reduce soil moisture and increase fire risk. [Gwenaelle GREMION, Canada]	Not applicable - the table has been revised and this column no more exists
7526	76		76		Second row, fourth column of table: Extra "e" after MgC [Rose Abramoff, France]	Rejected. Numbers are indeed in MgC equivalent, or MgCe (Ref. Griscomm et al. 2017 supplementary material, Table S1, page 11, peatland restoration line)



Comment ID	From Page	From Line	To Page	To Line	Comment	Response
16244	76		76		Ocean fertilization is prohibited under the London Protocol of the London Convention. Why does the IPCC continue to discuss a technology that has been banned by the international community due to its excessive adverse impacts on the marine environment? [Linda Schneider, Germany]	Noted - the mandate of WGI was to assess the technical potential of the commonly described CDR's. However, a sentence was added in the text stating "Large-scale OF has been prohibited under international treaty since 2008"
42492	76				Decreased productivity in unfertilised regions - the concept of Nutrient robbing was first raised by Dutkiewicz et al. 2005 [10.1209/2004GB002342] [Peter Croot, Ireland]	Rejected - Dutkiewicz's paper discussed nutrient robbing, but not in the context of CDR methods
29426	77	40	77	40	lowest right cell of the table: "decreased ocean acidification (surface waters only)". I don't agree, see my comment no. 3 [Judith Hauck, Germany]	Taken into account
56036	78	5	79	5	there is no consideration of other options for CDR by means of mariculture, eg seaweeds or microalgae as in NEDO devices. [Roque Pedace, Argentina]	Taken into account - we added a reference to the SROCC report which provides a comprehensive assessment of CDR by mariculture
47790	78	5	87	5	IPCC assessments are policy relevant but not policy prescriptive. Please avoid using terms like should, must, need in the text when referencing actions or decisions. [WGI TSU, France]	Taken into account - text has been checked and altered where necessary
13542	78	7	78	9	Yes, it is correct that ocean circulation cannot be easily modified. However, ocean circulation can be used to sequester more carbon in the ocean. For example, anthropogenic carbon has penetrated into the deep ocean in the downwelling regions of the North Atlantic and the Southern Ocean. I believe the ocean downwelling methods propose to inject CO <sub>2</sub> into these regions. [Govindasamy Bala, India]	Noted
16228	78	11	78	19	what about real-world experiments and their findings regarding effectiveness? Also: What is the resource consumption and CO <sub>2</sub> emissions impact of 7 million pipes to 1000m depth? Would any CO <sub>2</sub> be effectively removed from the atmosphere? [Linda Schneider, Germany]	Taken into account - we are not aware of any AOU <sub>pw</sub> real-world experiments published in peer-reviewed literature. CO <sub>2</sub> emission impacts are discussed in this paragraph and in table 5.9, as are the side effects of the approach.
31670	78	11	78	19	This process simultaneously brings DIC-rich water to the ocean surface and hence increase surface pCO <sub>2</sub> . As the present paragraph only mention to positive effect brought by nutrient transport, it is unclear for readers whether the presented figure of CO <sub>2</sub> removal (4.3 PgC/y at first decade and 0.4-1 PgC/y afterwards) involves negative effect of DIC transport or not. [Tsuneo Ono, Japan]	Accepted - a sentence has been added to highlight this. The model results quoted include this effect.
44118	78	19	78	19	Expand on the side effects of AOU <sub>pw</sub> --what kinds of effects will be widespread? [Sara Kahanamoku, United States of America]	Taken into account - side effects are in table 5.9
16226	78	21	78	33	Ocean fertilization is prohibited under the London Protocol of the London Convention. Why does the IPCC continue to discuss a technology that has been banned by the international community due to its excessive adverse impacts on the marine environment? [Linda Schneider, Germany]	Taken into account - a sentence concerning the London Protocol has been added

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
16230	78	21	78	33	this paragraph should draw not just on model simulations but also on the findings of real-world outdoor experiments of OF, which found a very low effectiveness of this approach, but significant side effects on the marine environment that led to OF being prohibited under LP/LC. [Linda Schneider, Germany]	Taken into account - text revised
42494	78	26	78	26	Decreased productivity in unfertilised regions - the concept of Nutrient robbing was first raised by Dutkiewicz et al. 2005 [10.1209/2004GB002342] [Peter Croot, Ireland]	Rejected - Dutkiewicz's paper discussed nutrient robbing, but not in the context of CDR methods
47776	78	28	78	38	Please check the use of this IPCC uncertainty language term. Are you able to provide a traceable account to assigning this uncertainty statement? Note that likelihood statements are quantified terms - phrases like likely and very likely have quantifiable probabilities associated with them. Please check it has been used correctly here. Please refer to the IPCC guidance note on uncertainty: <a href="https://wg1.ipcc.ch/SR/documents/ar5_uncertainty-guidance-note.pdf">https://wg1.ipcc.ch/SR/documents/ar5_uncertainty-guidance-note.pdf</a> [WGI TSU, France]	Taken into account - language has been checked and changed where necessary
17774	78	41	78	41	Delete 'increase' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
39290	79	8	79	11	Some citation of (paleo-based and modern) understanding of weathering necessary here. I.e. work by Berner, Maher, West, Hartmann. [Daniel Ibarra, United States of America]	Accepted - added citation of Hartmann et al. 2013, enhanced chemical weathering as a geoengineering strategy to reduce atmospheric CO2, supply nutrients, and mitigate ocean acidification.
28004	79	9	79	15	Page 5-79, line 9-15. Is there not a quantitative estimate available of the CO2 sequestration rate of enhanced weathering? For other CDR methods an estimate was provided. Page 5-79, line 31-40 Again, it would be nice if a quantitative estimate of the CO2 sequestration rate was provided for the BECCS/DACCS methods in the text, as was done for the other CDR methods. [roderik van de wal, Netherlands]	The rates of CDR for enhanced weathering are still very uncertain, reason why they have not been included in the text. Although they were included in Table 5.9 in the FOD, in the SOD, it is rated as highly uncertain, for compatibility with the SRCLL. Rates for BECCS/DACCS have been included in the text, as well as in the table.
16232	79	9	79	15	The quantity of resources/rock material required for EW as well as the CO2 emissions associated with mining, processing, transporting, etc. should be mentioned here. It is unlikely that EW, on balance, would remove any Co2 from the atmosphere. Also: The amount of water required to react with the rock material – in times of climate change-induced water scarcity in many regions. [Linda Schneider, Germany]	Rejected - the role of WGI is to assess the technical potential of CDR measures and their biogeochemical and biophysical side-effects.
35414	79	17	79	29	It should be made clear that as soon as Ocean Alkalinisation is implemented as a CDR measure, this is different from restoring the original ocean pH, and will result in large surface pH changes (Mengis et al., 2019). 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 - a sentence has been added in section 5.6.6.2.3 reflecting this point

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37748	79	21			See comment 11 on the entire document. Here is another place where ka is used to denote a thousand years rather than a thousand years BP. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	I could not find "comment 11", but since this sentence is about a residence time, this is definitely a place where ka means a thousand years, not thousand years before present.
16234	79	31	79	40	this paragraph mentions the high energy penalty of DAC as well as of CCS – what can reliably said about DACCS capacity to actually remove CO2 from the atmosphere then? It is important that there are clear messages in this report whether technologies are effective or not, otherwise policy makers will rely on them if they figure in the models – it is the IPCCs role to adequately reflect their potential and call them out as ineffective if needed. [Linda Schneider, Germany]	Rejected - the role of WGI is to assess the technical potential of CDR measures and their biogeochemical and biophysical side-effects.
9376	79	31	79	40	It is suggested to differentiate more between storage in pressurised gas form and mineral form as the risks identified in table 5.9 for DACCS are relevant only for storage in pressurized gas form. [Klaus Radunsky Radunsky, Austria]	Accepted - text has been revised
17776	79	34	79	34	Define DACCS [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - defined in the text.
49124	79	36	79	37	This would apply to all geological storage (the S bit of CCS). Note the wording suggests that CO2 gas is compressed in geological formations. That isn't the mechanism. There is no reference - maybe a contributing author could help. [Jim Skea, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text has been revised
27814	79	49	79	49	attend the placeholders [Poot Delgado Carlos Antonio, Mexico]	Taken into account - the figure has been updated
17778	80	8	80	16	Change to Pre-Industrial x7 [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - The same spelling as in the IPCC SR1.5 has been applied
23252	80	10	80	11	Decide on the term whether to call it "ocean mean-pH" (line 10) or "mean ocean pH" (line 11), but do not change! Check for entire paragraph! [Gwenaëlle GREMION, Canada]	Noted.
13540	80	13	80	14	For how long or for what duration is the CDR of 25 PgC applied in this simulation? [Govindasamy Bala, India]	Noted.
27816	80	21	80	21	attend the placeholders [Poot Delgado Carlos Antonio, Mexico]	Noted - this section has been moved to 5.3.3.4
46872	80	24	80	25	Why focussing only on SRM with respect to preindustrial ? There is lot of literature that have focussed on transient simulations. They need to be assessed here. Besides, This section is poorly related to the other sections of this chapter= for instance no discussion about the fertilization effect of CO2 (and it quantification with Beta) and so on. I understand the scope of assessing the response under a CO2-only forcing and the compensation a declingin solar constant However the climate response of SAI differs from that of declining solar constant... Besides, there will be several interestingCIP6 simulations that needs to be taken in to account in your assessment. They are based on transient multi-gas and aerosols. Focussing your assessment on CO2-only simulation and using a preindustrial reference seems to be very limited to me given the body of literature on the SRM [Roland Séférian, France]	Agreed. We reorganize this section and add discussions about SRM effect from transient simulations. For example, the recent publication using GeoMIP simulations "Plazzotta, M., R. Seéférian, and H. Douville (2019), Impact of Solar Radiation Modification on allowable CO2 emissions: what can we learn from multi-model simulations?, Earth's Future, in press, doi:10.1029/2019EF001165."

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55846	80	24	81	55	It would be appropriate to refer to and cite research papers coming out of the Geoengineering Large Ensemble (GLENS) effort described by Tilmes et al. (BAMS), particularly because they are based on a high existing scenario (RCP8.5). Fasullo et al.'s recent paper on ocean effects is particularly relevant. Yang, Hoffman et al. (in prep) will describe terrestrial feedbacks more explicitly from these GLENS simulations. More information will be available once modeling centers complete and analyze CMIP6's GeoMIP simulations. As far as I know, there have been no emissions-forced fully coupled simulations that take into account land and ocean BGC feedbacks completed yet. [Forrest Hoffman, United States of America]	Agreed. Relevant GLENS papers discussed.
35416	80	24	82	7	Rather than discussing the Biogeochemical responses to Radiation Management measures by reference state (i.e., relative to pre-industrial, relative to unmitigated climate change), I would propose to restructure this section to discuss individual methods and their assessed impacts on the carbon cycle, land and ocean. This way, the ambiguity of using SRM as an umbrella term for multiple proposed methods is avoided and it becomes clearer which methods impact on the carbon cycle has yet to be assessed. [Nadine Mengis, Canada]	Agreed. Text rewritten.
12436	80	24	82	9	Several studies investigated the potential of terrestrial radiation management by thinning of cirrus clouds e.g. Gasparini et al. (2016, 2017), Gruber et al. (2019), Storelvmo et al. (2014).  Gasparini, B. and Lohmann, U.: Why cirrus cloud seeding cannot substantially cool the planet, J. Geophys. Res. Atmos., 121, 4877–4893, doi:10.1002/2015JD024666, 2016. Gasparini, B., Münch, S., Poncet, L., Feldmann, M., and Lohmann, U.: Is increasing ice crystal sedimentation velocity in geoengineering simulations a good proxy for cirrus cloud seeding?, Atmos. Chem. Phys., 17, 4871-4885, doi:10.5194/acp-17-4871-2017, 2017. Gruber, S., Blahak, U., Haenel, F., Kottmeier, C., Leisner, T., Muskatel, H., Storelvmo, T., and Vogel, B.: A process study on thinning of Arctic winter cirrus clouds with high-resolution ICONART simulation, J. Geophys. Res. Atmos., accepted, doi:10.1029/2018JD029815, 2019. Storelvmo, T. and Herger, N.: Cirrus cloud susceptibility to the injection of ice nuclei in the upper troposphere, J. Geophys. Res. Atmos., 119, 2375–2389, doi:10.1002/2013JD020816, 2014. [David Neubauer, Switzerland]	Noted. We are aware of these studies, but none of them discuss the impact on the biogeochemical cycles.
57282	80	26	26	40	List of options hinted at should include sea surface methods since this might actually be tried in Australia during the preparation of AR6 [Oliver Geden, Germany]	Agreed.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
41104	80	32	80	32	Change "volcanoes" to "volcanic eruptions." Volcanoes do not affect climate by themselves. It is the eruptions that do. [Alan Robock, United States of America]	Agreed.
42496	80	33	80	33	Other aerosol types have also been suggested (e.g. TiO2) [Peter Croot, Ireland]	Noted
16236	80	33	80	40	There is an invalid causal relationship established here. There are manifold reasons why SRM is still being discussed, not least because authoritarian governments, climate change denialists and the fossil fuel industry take an interest in a „quick fix“ for the climate crisis. Also, the term „cost“ is applied in a very narrow sense here and is only measured against investment costs of rapid decarbonisation. Such a portrayal is politically dangerous and misleading. [Linda Schneider, Germany]	Noted.
47508	80	35	80	37	Express the cost of rapid decarbonization as % of GNI / GDP compared to the cost of SRM as % of GNI / GDP to be able to evaluate the cost / benefit of each. This is certainly policy relevant and key information required for decision makers [Birgit van Munster, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - beyond the scope of this section.
32776	80	36	80	37	It makes no sense to be discussing the potential impacts of SRM without comparing them to the benefits of undertaking them, given that SRM is only being proposed to offset the increasingly severe and already apparent impact of climate change without SRM. So, with respect to the statements here, would the regional changes from slowly built up SRM be worse or not compared to the consequences of global warming without SRM. Similarly, for the issue of ending SRM--given mitigation costs much more than SRM, why would SRM be given up, allowing climate change to greatly worsen--this is just a rather implausible hypothetical, even though it has gotten undue attention in the literature. [Michael MacCracken, United States of America]	Agreed. Text rewritten.
32778	80	37	80	40	How is it that the effects on the biosphere and biogeochemical cycling of the climatic cooling induced by SRM is not the first and most important aspect to be assessed in this section? The cooling itself will lead to reduced impacts on the biosphere, affecting plant growth, etc., and the cooling will lead to reduced demand for electricity for air-conditioning, so some reductions in fossil fuel emissions if such power is used for the air-conditioning. There will be effects on the hydrologic cycle and more and I just do not see how this aspect is not the most important to be discussed here. [Michael MacCracken, United States of America]	Noted. This section focusses on the impact of SRM on biogeochemical cycles. The SRM effect on other aspects of the climate system, such as the hydrological cycle, is discussed in section 4.6.3. The broad implication of SRM, such as reduced demand for fossil fuel, is beyond the scope of this section.
41106	80	39	80	39	Change "GEOMIP" to "GeoMIP" [Alan Robock, United States of America]	Accepted - text revised
41108	80	40	80	40	The correct reference for the GeoMIP project is: Kravitz, Ben, Alan Robock, Olivier Boucher, Hauke Schmidt, Karl Taylor, Georgiy Stenchikov, and Michael Schulz, 2011: The Geoengineering Model Intercomparison Project (GeoMIP). Atmos. Sci. Lett., 12, 162-167, doi:10.1002/asl.316. [Alan Robock, United States of America]	Agreed - Kravitz et al. (2011) reference corrected.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
9378	80	45	80	45	This chapter should be rewritten because on page 80, lines 33/34 it is correctly stated that "SRM is not part of the standard RCP and SSP scenarios considered throughout this report". However, in line 45 on the same page the following assumption is made: "As atmospheric CO2 continues to increase under SRM". This assumption is highly policy prescriptive. It seems much more appropriate to differentiate between several assumptions described in the literature. The assessment should furthermore differentiate among the various phases of the use of various approaches to address climate change risks by a combination of mitigation, CDR and SRM. See e.g. chapter 4.6.3. [Klaus Radunsky Radunsky, Austria]	Agreed. Text rewritten.
23256	80	45	80	53	Please provide sources for the informations you give in this pargraph. [Gwenaelle GREMION, Canada]	Agreed. References added.
16238	80	45	81	17	how does the reduction in incident solar radiation (that reduces photosynthesis) impact on GPP discussed in the previos paragraph? [Linda Schneider, Germany]	Agreed. Text rewritten.
23254	80	45			Indicate abbreviation ESM for Earth System Models for further use in line 48 [Gwenaelle GREMION, Canada]	Noted.
32780	81	5	81	6	It really needs to be said that the amount of sunlight reduction being talked about is quite small, 2% being equivalent to totally reversing the forcing associated with a CO2 doubling, which is actually a good bit greater that would be needed presuming that reasonable mitigation measures are being taken and all SRM proposals make clear that SRM is not a substitute for mitigation. It might also be noted that the additional fraction of solar radiation that becomes diffuse is about an order of magnitude larger than the percentage reflected to space. [Michael MacCracken, United States of America]	Agreed. Text rewritten.
13588	81	5	81	17	The changes in direct and diffuse components of solar radiation could also depend on the height of the aerosol layer. This effect was neatly isolated in the recent study in ESD which may be cited: <a href="https://www.earth-syst-dynam-discuss.net/esd-2019-21/#discussion">https://www.earth-syst-dynam-discuss.net/esd-2019-21/#discussion</a> [Govindasamy Bala, India]	Agreed. Reference added.
35418	81	5	81	17	Several studies that assessed the impact of Radiation management on crop yields are missing: Xia, L., Robock, A., Cole, J., Curry, C. L., Ji, D., Jones, A., ... & Singh, B. (2014). Solar radiation management impacts on agriculture in China: A case study in the Geoengineering Model Intercomparison Project (GeoMIP). Journal of Geophysical Research: Atmospheres, 119(14), 8695-8711. Pongratz, J., Lobell, D. B., Cao, L., & Caldeira, K. (2012). Crop yields in a geoengineered climate. Nature Climate Change, 2(2), 101. [Nadine Mengis, Canada]	Agreed. Reference added.
47778	81	13	81	13	"Seems likely" Please check the use of this IPCC uncertainty language term. Are you able to provide a traceable account to assigning this uncertainty statement? Note that likelihood statements are quantified terms - phrases like likely and very likely have quantifiable probabilities associated with them. Please check it has been used correctly here. Please refer to the IPCC guidance note on uncertainty: <a href="https://wg1.ipcc.ch/SR/documents/ar5_uncertainty-guidance-note.pdf">https://wg1.ipcc.ch/SR/documents/ar5_uncertainty-guidance-note.pdf</a> [WGI TSU, France]	Agreed.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
32782	81	14	81	17	The key question is, however, how these effects would compare to the impacts on agriculture of not having the SRM induced climate offset. So, global warming will cause serious threats/risks to agriculture due to droughts and excess heat and evaporation--so how will the hypothesized and uncertain SRM induced impacts compare. It is simply not helpful or impartial scientific assessment to make a statement such as this without providing context of what would happen without SRM. [Michael MacCracken, United States of America]	Agreed. Discussion added about impact on crop yields.
55544	81	15	81	16	The Proctor et al 2018 paper does not indicate that yields could decrease, only that SRM might not increase them, "that projected mid-twentyfirst century damages due to scattering sunlight caused by solar radiation management are roughly equal in magnitude to benefits from cooling. This suggests that solar radiation management— if deployed using stratospheric sulfate aerosols similar to those emitted by the volcanic eruptions it seeks to mimic—would, on net, attenuate little of the global agricultural damage from climate change." [Matthisa Honegger, Germany]	Agreed. Text rewritten.
55546	81	20	81	30	We are unsure of the utility of this paragraph. It compares a world of elevated atmospheric greenhouse gases and SRM, with pre-industrial conditions. However, this comparison conflates two changing (largely) independent variables. This risks creating confusion by implying humanity is facing a choice between a pre-industrial climate and a climate-change-with-SRM climate. We would suggest comparing the climate states of 'SRM + elevated GHG' with 'elevated GHG', as that both isolates a single salient variable and more closely resembles the policy choices at hand. [Matthisa Honegger, Germany]	Agreed. Text rewritten.
47404	81	20			I'm not convinced this is a useful comparison - you appear to compare a future with (CO2 increase, climate change and SRM) against a pre-industrial one, but without attributing the changes to each forcing. Surely what you need here (as per the section title of 5.6.3.3) is a comparison of two scenarios WITH and WITHOUT SRM and then you can see the "net" effect of SRM [chris jones, United Kingdom (of Great Britain and Northern Ireland)]	Noted.
35420	81	22	81	24	This statement is in line with the findings from Kravitz et al , 2013, which looked at the multi-model response of an idealized Geoengineering scenario in which global insolation was reduced. Otherwise, there is a paper from Yu et al., 2015, who assessed the regional responses to the two different sulphate aerosol experiments, but does not find the same robust patterns, as for the solar insolation experiments. Kravitz, B., Caldeira, K., Boucher, O., Robock, A., Rasch, P. J., Alterskjær, K., ... & Irvine, P. J. (2013). Climate model response from the geoengineering model intercomparison project (GeoMIP). Journal of Geophysical Research: Atmospheres, 118(15), 8320-8332. Yu, X., J. C. Moore, X. Cui, A. Rinke, D. Ji, B. Kravitz, and J.-H. Yoon (2015), Impacts, effectiveness and regional inequalities of the GeoMIP G1 to G4 solar radiation management scenarios, Global and Planetary Change, 129, 10-22, doi:10.1016/j.gloplacha.2015.02.010. [Nadine Mengis, Canada]	Agreed. Text rewritten.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
55548	81	22	81	24	Models indicate that these conditions would arise if SRM were used to fully compensate the change in GSAT from climate change via a uniform change in radiative forcing. Yet SRM could be used at e.g. a lesser intensity (thus not reducing global mean precipitation) and it could be spatially (and seasonally) nonuniform. [Matthisa Honegger, Germany]	Agreed. Text rewritten.
32784	81	22	81	27	But this is not the comparison to make--the situation that we face is GHG-induced warming and the comparison that is appropriate is GHG-induced warming with or without SRM. That the match might not be perfect to preindustrial CO2 is perhaps interesting, but is really not the situation to be considered by policymakers. [Michael MacCracken, United States of America]	Agreed. In the revised text, we emphasize the impact of SRM relative to high CO2 world.
16242	81	22	81	46	why only compare SRM to pre-industrial and unmitigated climate change? Why not compare to mitigated climate change? [Linda Schneider, Germany]	Agreed. In the revised text, we emphasize the impact of SRM relative to high CO2 world.
17780	81	24	81	24	Reference missing [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Agreed. Reference added.
13514	81	24	81	24	The 2008 paper in PNAS by Bala et al. is the first study that pointed that the global mean precipitation would be reduced under SRM. [Govindasamy Bala, India]	Noted.
47780	81	26	81	51	Please check the use of this IPCC uncertainty language term. Are you able to provide a traceable account to assigning this uncertainty statement? Note that likelihood statements are quantified terms - phrases like likely and very likely have quantifiable probabilities associated with them. Please check it has been used correctly here. Please refer to the IPCC guidance note on uncertainty: <a href="https://wg1.ipcc.ch/SR/documents/ar5_uncertainty-guidance-note.pdf">https://wg1.ipcc.ch/SR/documents/ar5_uncertainty-guidance-note.pdf</a> [WGI TSU, France]	Agreed. it is from the Global Carbon Project (GCP). Now spelled-out in the figure caption.
32786	81	27	81	30	This is a very meek statement of the general influence of SRM (it would likely make for less serious consequences for agriculture, reduce biodiversity loss, reduce deoxygenation of the ocean, etc., etc.--please make the statement of likely benefits better. And then, since when is not preventing an increase in plant productivity and carbon stocks a negative impact, as implied by use of the word "but" especially given that this is just what proposals for CDR want to achieve and are considered helpful in offsetting the CO2 increase. This whole paragraph needs revision, making a comparison to the impacts associated with GHG induced climate change without SRM. While I see this is done in the following paragraph, what is done in this paragraph really needs reconsideration and revision, indicating it represents a purely hypothetical situation. [Michael MacCracken, United States of America]	Agreed. Text rewritten.
41110	81	33	81	33	You should include the impacts of SRM on surface ozone, which will influence natural and anthropogenic vegetation, as shown in: Xia, Lili, Peer J. Nowack, Alan Robock, and Simone Tilmes, 2017: Impacts of stratospheric sulfate geoengineering on tropospheric ozone. Atmos. Chem. Phys., 17, 11,913-11,928, doi:10.5194/acp-17-11913-2017. [Alan Robock, United States of America]	Agreed. Text rewritten.



Comment ID	From Page	From Line	To Page	To Line	Comment	Response
47406	81	33			as for section 5.6.3.3 I'm not convinced this is a useful comparison - you appear to compare scenarios with multiple forcing changing, but without attributing the changes to each forcing. Surely what you need here (as per the section title of 5.6.3.4) is a comparison of two scenarios WITH and WITHOUT SRM and then you can see the "net" effect of SRM [chris jones, United Kingdom (of Great Britain and Northern Ireland)]	Noted.
16240	81	35	81	46	this paragraph is misleadingly called „net impacts of SRM compared to unmitigated climate change“, however, it only compares the carbon cycle responses and leaves aside the manifold profound risks and adverse side effects of SRM. This is not an accurate description of the „net impacts of SRM“! Also, the Trisos et al. 2018 paper clearly finds that the termination effect would imply such rapid climate change that human communities and other species could impossibly adapt – it therefore seems an understatement to refer to „potentially dangerous consequences“, and „potentially disastrous consequences“ not just for biodiversity, but also for humanity would be more appropriate. [Linda Schneider, Germany]	Agreed. Text rewritten.
36450	81	35		36	This wasn't clear to me from Fig 5.35. It looks like GPP is generally lower in G1 than in the abrupt4xCO2 experiment. [Nathan Gillett, Canada]	Agreed. Text rewritten.
17782	81	39	81	39	Delete , [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
17784	81	40	81	41	Don't split numbers and units across a line [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. Please note that the final report will undergo a professional copy-edit before publication. This sort of issue will be fixed then.
32788	81	42	81	46	This statement needs context. If one has ongoing emissions with the CO2 concentration climbing strongly throughout as in RCP8.5, the question then is how would the sudden warming compare to going through the RCP8.5 without SRM at all--and it would be appropriate to have an assessment of the likelihood of nations making such a dumb decision. On the other hand, and important to mention, is the temperature shaving scenario discussed by Shepherd and others in which SRM is used to limit warming as CO2 emissions are reduced and SRM used to pull the CO2 concentration back down. In this situation, SRM could be phased out and there would have been a significant net benefit of SRM---CDR being the exit strategy for SRM. This much more likely potential scenario needs to be mentioned (and there are references regarding this). [Michael MacCracken, United States of America]	Noted, but discussions like 'assessment of the likelihood of nations making such as dumb decision' is beyond the scope of this section.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
55550	81	43	81	46	Although termination of SRM could cause "a rapid increase in global temperature and precipitation", it would also need to be sudden and sustained termination, as well as occurring 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 simply 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." <i>Earth's Future</i> 6, no. 3 (2018): 456-467; Rabitz, Florian. "Governing the termination problem in solar radiation management." <i>Environmental Politics</i> 28, no. 3 (2019): 502-522. [Matthisa Honegger, Germany]	Agreed. Text rewritten.
28006	81	49	81	51	Page 5-81, line 49-51: "relative to both the pre-industrial climate (because of CO2 fertilisation of photosynthesis), and also relative to an elevated CO2 world without SRM (because of reduced plant and soil respiration at the lower temperatures)." For readability I would suggest: ""relative to both the pre-industrial climate (because of CO2 fertilisation of photosynthesis), and to an elevated CO2 world without SRM (because of reduced plant and soil respiration at the lower temperatures). [roderik van de wal, Netherlands]	Agreed. Text rewritten.
47408	81	51	81	52	specific example of why this comparison is mis-leading. You say "CO2 + SRM is detrimental due to ocean acidification". While this is true, the reason is because te CO2 increases. SRM neither causes nor reduces ocean acidification. So this sentence in a "net effect of SRM" section is very misleading! [chris jones, United Kingdom (of Great Britain and Northern Ireland)]	Agreed. Text rewritten.
55552	81	51	81	53	This sentence regarding the effects of ocean acidification does not concern "Net impacts of SRM compared to unmitigated climate change" (i.e. the subheading), but instead that of the previous one. However, as we noted there, that conflates two variables. Conflating those two variables creates confusion, highlighted by this very sentence, which actually is about the impacts of elevated atmospheric CO2 concentrations, not of SRM. [Matthisa Honegger, Germany]	Agreed. Text rewritten.
32790	81	51	81	53	There is no reason here to attribute the ocean acidification problem to SRM--the impacts happening are due to the CO2 concentration itself, and SRM likely even slightly moderates the situation by keeping the ocean cooler and by inducing reductions in emissions associated with air-conditioning, etc. Sure, if SRM is used instead of mitigation, one might say this but there are no advocates of SRM who suggest reducing the mitigation effort--and indeed SRM can contribute to some reductions in emissions (less emissions from dying ecosystems, permafrost thawing, and so on). This statment simply has to be revised, even though I'll agree there are those out there who mistakenly make it. [Michael MacCracken, United States of America]	Agreed. Text rewritten.
27818	82	3	82	3	attend the placeholders [Poot Delgado Carlos Antonio, Mexico]	Noted.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
47410	82	10			some comments on knowledge gaps. I fully agree magnitude and trend of land co2 sink is crucial. Land use is clearly a part. You could add that response to CO2 (i.e. fertilisation) is still crucially under-constrained, especially for the long-term response of carbo storage (even if we know the short-term response of productivity). Remote sensing products of biomass are therefore crucial (for bth natural sinks and land-use changes) [chris jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Text modified.
23440	82	10			There are no references in this section. Even if no articles are directly referenced, I would find it helpful to get a reference to the section in the report which the statements are based on. [Gwenaelle GREMION, Canada]	Accepted: the links have been made to where the knowledge gap will improve the confidence of the assessment
51962	82	10			This section is considerably longer than the similar sections in other chapters which have tended to be 1-2 pages of briefer bullets than is the case here. Consideration vis-à-vis inter-chapter consistency in approach may be required froma. Whole-of-report perspective. [Peter Thorne, Ireland]	Noted
23304	82	14	82	14	"Two key developments require further development"... some rewording needed. I suggest "Two key aspects". [Gwenaelle GREMION, Canada]	Accepted - text revised
46876	82	14	82	21	The representation of plant ecophysiology (mortality processes) is creudly represented in most lan surface model. A few of second generation starts to better represent these processes but are not included in ESMs. [Roland Séférian, France]	Noted, text modified.
13528	82	14	82	21	I would place finer-resolution observations of land sinks and sources of carbon at the top of the knowledge gap. Maybe remote sensing would help in the future. Do we really think the flux from land to ocean is that important. Isn't it (mean) only about 1 PgC per year? The interannual fluctuations should be too small. [Govindasamy Bala, India]	Noted. On the land-ocean flux the uncertainties are large ( $\pm X$ ) and we know little about the variability and trends (Resplandy et al., 2018)
23306	82	16	82	16	change 'such' to 'such as' [Gwenaelle GREMION, Canada]	Accepted - text revised

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
23308	82	17	82	17	should not this corrspnd to the first one to start with verb.? E.g. second, to represent lateral flows of ... [Gwenaelle GREMION, Canada]	Accepted: Text changed - Two key aspects of assessing the magnitude and trends of the land CO2 sink through terrestrial models require further development to improve the assessment confidence. Firstly, to further constrain the flux from the land use, land use change and forestry. Models have inadequate resolution or lack of representation of land management, such as forestry, grazing and cropland management, which covers three quarters of the ice-free land surface. Secondly, to better represent the variability and trends in the movement of carbon through the land to ocean continuum has implications for the strength of the land CO2 sink. Land surface modelling is only at the beginning of representing lateral flows of carbon resulting in carbon accumulation (sinks) in freshwater reservoirs and carbon releases to the atmosphere through the whole continuum of freshwater bodies, rivers to coastal zones
17786	82	23	82	23	Change 'Data' to 'data [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
47412	82	23			BGC-Argo floats are trying to fill this crucial gap nd offer real advances in coming years in the same way Aro had massive imapct on knwoedlge of phsyical ocean state [chris jones, United Kingdom (of Great Britain and Northern Ireland)]	Noted: This clarification is added as an example of the initial steps towards addressing this gap. However it is almost certain that the 0 day sampling period from the BGC-floats is not sufficient to reduce the biases and RMSEs to the required levels. Additional high resolution platforms integrated with machine learning are still a gap.
23310	82	26	82	26	change'This applies particularly for...' to 'This applies particularly to..' [Gwenaelle GREMION, Canada]	Accepted - text revised

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
23312	82	27	82	27	I suggest adding here that “Emerging observational networks like biogeochemical argo floats, as well as remote sensing observations of salinity from space, can help constrain carbonate system parameters. These observations can be ingested by operational models or used for model evaluation, ultimately improving the performance of biogeochemical models”. Specific examples are biogeochemical argo floats equipped with pH sensors, or recent initiatives of the European Space to exploit remotely sensed salinity and sea surface temperature by means of empirical methods to monitor surface-ocean carbonate chemistry at synoptic scale (Pathfinders Ocean Acidification, OceanSODA). References: Williams, N. L., Juraneck, L. W., Feely, R. A., Johnson, K. S., Sarmiento, J. L., Talley, L. D., ... & Riser, S. C. (2017). Calculating surface ocean pCO <sub>2</sub> from biogeochemical Argo floats equipped with pH: An uncertainty analysis. <i>Global Biogeochemical Cycles</i> , 31(3), 591-604.) <a href="https://eo4society.esa.int/projects/satellite-oceanographic-datasets-for-acidification-oceansoda/">https://eo4society.esa.int/projects/satellite-oceanographic-datasets-for-acidification-oceansoda/</a> [Gwenaelle GREMION, Canada]	Accepted: Emerging observational networks like biogeochemical Argo floats, remote sensing observations as well as new machine learning platforms are beginning to contribute to addressing these data product limitations.
32792	82	29	82	31	Very helpful here, even if general terms, to explain why it is important to resolve this issue and how it might affect decisions to be made. This type of explanation is needed in the first paragraph, so lines 16 to 21, and also in the second paragraph, so lines 23-27. Yes, these advances are important for better scientific understanding, but why should policymakers care about reducing these uncertainties--would doing this at all affect what measures need to be taken and how soon? To justify more funds for better understanding, there needs to be an explanation of how this would affect overall options and understanding for policy makers, not just scientists. {and this comment applies to this whole section!!} [Michael MacCracken, United States of America]	Accepted: the following sentence was added at the beginning: In order to improve the confidence of a future assessment of the trends and attribution for GHGs the following knowledge gaps have been identified for in the land, ocean and atmosphere.
37750	82	29			"Full" at the beginning of the line can be removed, as the word "fully" appears at the end of the line. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
23314	82	30	82	30	remove 'a' [Gwenaelle GREMION, Canada]	Accepted - text revised
17788	82	31	82	31	Change 'remian to partition' to 'remain in partitioning' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
23316	82	33	82	33	CO should be CO <sub>2</sub> . [Gwenaelle GREMION, Canada]	Accepted - text revised
17790	82	33	82	33	I suspect this should be CO <sub>2</sub> [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Reject: correct as is - CH4
23318	82	35	82	35	I would mention permafrost thaw feedbacks at the end of the paragraph. They are very uncertain but potentially very important, as acknowledged in other parts of the chapter and in the FAQs. [Gwenaelle GREMION, Canada]	Accepted: This may also apply to the perma-frost stocks of methane

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37752	82	43			See comment 234. Referring to "all three major greenhouse gases" as including N2O is at odds with ozone's larger RF due to changes since the pre-industrial baeline period. Moreover, the text to which comment 234 applies referred to greenhouse gases that dominate the human perturbation of the radiation budget. But here the reference is to human perturbation is missing, allowing another greenhouse gas, water vapour, to enter the frame. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. The mandate of the chapter is to cover co2, ch4 and N2O. Other important GHGs are covered in other chapters including ch7
17792	82	45	82	45	Insert , after 'Presently' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
17794	82	46	82	46	Change 'The space-borne' to 'Space-borne' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
17796	82	47	82	47	Change 'The chemistry-transport' to 'Chemistry-transport. [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
8330	82	52	84	54	I'm assuming this will get rewritten; it's entirely vague and free of citations or assessment. [Sarah Cooley, United States of America]	Accepted. Text rewritten
46874	83	1	83	17	Two mention ofthe concentration-driven simulations instead of emission driven simulation: in that context, it is worth mentionning that the carbon cycle in most ESMs is not bounded (lake, riverine, blue carbon, and so on). The role of the redfields versus non-redifields assumption in future projections for OD and OA needs to be acknowledged here. [Roland Séférian, France]	Accepted: Although model projections of changes in ocean carbon fluxes and storage agree much better than for land, projections of future global ocean primary production do not even agree on the direction of change to 2100. In order to strengthen future assessment confidence an improved understanding of the processes affecting the efficiency and climate sensitivity of the biological carbon pump, such as for example dynamic Redfield ratios, additional observations are necessary to constrain large-scale models as well as the projection of contemporary biases.
57238	83	7	83	17	Similarly to previous sections, knowledge gaps section could benefit from more detailed discussion of the role of natural variability in driving oxygen changes in the tropical Oceans, and gaps in understanding of how anthropogneic wamring is linked to the expansion of the OMZs (role of changes in transport, wamring, productivity, et.c).. Oschlies et al 2018 I believe provides a through examination of these gaps. [Yassir Eddebbar, United States of America]	Accepted: The primary uncertainty on future ocean de-oxygenation in the subsurface tropical ocean relates to a consistent compensation in the trends and variability between oxygen saturation due to warming and decreasing apparent oxygen utilisation as a result of increased ventilation.
28008	83	13	82	14	Page 5-83, line 13-14: "Nitrous oxide (N2O) is produced hypoxic areas, mediated by bacteria. It is still unclear at which threshold of oxygen concentration does N2O production turn into consumption, and how it would affect ocean emissions." The sentence structure here is not correct. I would suggest: "Nitrous oxide (N2O) is produced in hypoxic areas, mediated by bacteria. It is still unclear at which threshold of oxygen concentration N2O production turns into consumption, and how it would affect ocean emissions." [roderik van de wal, Netherlands]	Accepted: Nitrous oxide (N2O) is produced hypoxic areas, mediated by bacteria. It is still unclear at which threshold of oxygen concentration does N2O production turn into consumption, and how it would affect ocean emissions.

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23320	83	13	83	13	a 'in' is missing between 'produced' and 'hypoxic' [Gwenaelle GREMION, Canada]	Accepted - text revised
23324	83	16	83	16	It is not clear here how future model projections use atmospheric CO2 concentrations or emissions, such as input or output? Or in other ways? Better to clarify this sentence. [Gwenaelle GREMION, Canada]	Accept: The impact of differences between prescribed concentrations and emissions driven models on the variability, trends and feedbacks on OA needs to be better understood in order to strengthen the confidence in future projections
23322	83	16	83	17	I guess these lines can be removed as the same information is given a few lines above (“atmospheric CO2 concentrations rather than emissions often drive models”). [Gwenaelle GREMION, Canada]	Accepted - text revised
46056	83	16	83	17	What this part means is unclear. C4MIP has emission driven experiments; is that not enough? [Kaoru Tachiiri, Japan]	Accept: The impact of differences between prescribed concentrations and emissions driven models on the variability, trends and feedbacks on OA needs to be better understood in order to strengthen the confidence in future projections
36452	83	16		17	Why would prescribed concentrations instead of emissions lead to an underestimate of ocean acidification? This is not true for a prescribed concentration scenario, which is the framework used in most of the report. [Nathan Gillett, Canada]	Accepted - this sentence needs clarification: The impact of differences between prescribed concentrations and emissions driven models on the variability, trends and feedbacks on on OA needs to be better understood in order to strengthen the confidence in future projections
9584	83	19	83	24	Also, a clear way to diagnose LUC in CMIP5 models would be helpful, and currently presents an area of challenge and opportunity. [Katarzyna (Kasia) Tokarska, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Included suggestion.
23326	83	22	83	22	change 'treatment' to 'treatments' [Gwenaelle GREMION, Canada]	Accepted - text revised
23328	83	24	83	24	change 'focus' to 'focus on' [Gwenaelle GREMION, Canada]	Accepted - text revised

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
32794	83	33	83	34	This is a statement that could be taken way out of context unless there is an indication of how the supposed bias might influence actions that policymakers should consider and change. So far, I've not seen any indication that any of these uncertainties would in any way affect the grand scale policy aspects relating to mitigation, warming, impacts, etc. On questions of exactly how big the impacts might be and what might need to be done, all of these statements seem to me to be referring to second or even third-order aspects--not the main findings of the assessment--and this needs to be explicitly said or we'll get calls for dealy due uncertainties, and this is not, I'm quite sure, the intent of the statement. So, when it is said in the next sentence that the aspects would be "more confidently project[ed]"--by how much and would it make a significant difference in any of the proposed actions that need to be taken? I am all for better science, but policy makers will want to know how this might affect their decisions, will the outcomes be worth what must be invested, etc.--and there is no indication of these types of considerations. [Michael MacCracken, United States of America]	Noted. Made it clear.
36724	83	33	83	42	Remaining uncertainties in soil dynamics, which should have a considerable feedback effect, should be discussed around here. Although several model intercomparison studies revealed huge uncertainties in model-simulated soil carbon stock (e.g., Nishina et al., 2014; Tian et al. 2015), it has not shrunked yet. Several global maps of soil carbon are now available, allowing us to conduct benchmarking studies. Nishina, K., Ito, A., Beerling, D.J., Cadule, P., Ciais, P., Clark, D.B., Falloon, P., Friend, A.D., Kahana, R., Kato, E., Keribin, R., Lucht, W., Lomas, M., Rademacher, T.T., Pavlick, R., Schaphoff, S., Vuichard, N., Warszwaski, L., Yokohata, T., 2014. Quantifying uncertainties in soil carbon responses to changes in global mean temperature and precipitation. Earth System Dynamics 5, 197–209. Tian, H., Lu, C., Yang, J., Banger, K., Huntzinger, D.N., Schwalm, C.R., Schwalm, C.R., Michalak, A.M., Cook, R., Ciais, P., Hayes, D., Huang, M., Ito, A., Jain, A., Lei, H., Mao, J., Pan, S., Post, W.M., Peng, S., Poulter, B., Ren, W., Ricciuto, D., Schaefer, K., Shi, X., Tao, B., Wang, W., Wei, Y., Yang, Q., Zhang, B., Zeng, N., 2015. Global patterns and controls of soil organic carbon dynamics as simulated by multiple terrestrial biosphere models: current status and future directions. Global Biogeochem. Cycles 29, 10.1002/2014GB005021. [Akihiko Ito, Japan]	Accepted. Added.
36454	83	33		42	Should permafrost be included here, as a process which is missing in most ESMs, and which could contribute to biogeochemical feedbacks? [Nathan Gillett, Canada]	Accepted: included in the text
17798	83	34	83	35	Move 'more confidently' to after 'feedbacks' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
23330	83	39	83	39	change 'model' to 'modeling' [Gwenaelle GREMION, Canada]	Accepted - text revised



Comment ID	From Page	From Line	To Page	To Line	Comment	Response
32796	83	42	83	42	Please provide context for the assertion that "uncertainty remains a major problem"--do any of these uncertainties affect the decisions that the international community of nations needs to make to address the Paris goals, or more appropriately, the UNFCCC objective. I think not and again, it seems to me there is a very significant risk that this statement could be taken out of context and used as an excuse not to take any action at all now when this is not the intent at all. Please revise. [Michael MacCracken, United States of America]	Accepted: sentence removed
32798	83	44	83	45	Again, context is missing--would this at all affect any of the decisions expected over most of this century? [Michael MacCracken, United States of America]	Accepted: revised the sentence to strengthen the context
23442	83	44	83	48	There are also very little observations of the oceanic carbon storage, especially on a global scale. There are studies that investigate the decadal changes in anthropogenic DIC (Clement and Gruber, 2018; doi: 10.1002/2017GB005819). But as far as I am aware no such global observational product of DIC exists to date that shows the changes of DIC over time in the water column. [Gwenaelle GREMION, Canada]	Noted: GLODAP provides the best product in respect of this need
23332	83	46	83	48	I would add "and carbon storage" at the end of the sentence. Prediction of ocean primary production is clearly less uncertain than prediction of carbon export and sequestration driven by the large ensemble of processes that form the biological carbon pump. [Gwenaelle GREMION, Canada]	Accepted: revised sentence: In order to strengthen the confidence for future assessments, an improved understanding of the processes (for example dynamic Redfield ratios), affecting the efficiency, climate sensitivity and emerging feedbacks in the ocean carbon cycle are necessary.
23334	83	47	83	47	change 'large-scales' to 'large-scale' [Gwenaelle GREMION, Canada]	Accepted - text revised
32800	83	50	83	51	Indeed, coming out of the last glacial maximum, sea level rose at an average rate of roughly 1-2 meters per century for 120-60 centuries while the global average temperature was going up 1-2 C per 2000 years, so a rate far faster than for the present situation. And yet, IPCC 1.5 suggested that sea level rise by 2100 would be less than a meter. This seems absurd--or at least an absurd presentation of the risks being faced. [Michael MacCracken, United States of America]	Accepted. Text modified.
23336	84	2	84	2	change 'ocean' to 'oceanic' [Gwenaelle GREMION, Canada]	Accepted - text revised
23338	84	8	84	8	this sentence is incomplete, remain what? [Gwenaelle GREMION, Canada]	Accept: sentence edited: Important areas remain for advancing the more precise estimation of carbon budgets
17800	84	10	84	10	Change 'carbon dioxide' to 'CO2' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
55840	84	14	84	17	This and other projections related to permafrost emissions should be revised based on the SROCC and anticipated published work prior to literature deadline, as the scale is significant in impacts on mitigation levels. [Pam Pearson, Sweden]	Noted

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23340	84	14	84	17	It would be fair to list “currently not represented Earth system feedbacks”. What potentially important feedbacks are we missing? Current ESMs include only a small amount of feedback processes involving mainly C cycling. We may be missing important feedbacks just because we deemed current mechanistic knowledge insufficient to include them in models, or worse, because their inclusion would make models computationally too costly. For example, feedbacks arising from biogenic aerosol precursors (eg marine dimethylsulfide, marine and terrestrial VOCs, primary organic aerosols, etc.) are not even mentioned in this chapter (I have not read other chapters). Although they are probably mentioned in the chapter dealing with short-lived climate forcers, they are linked through the dynamics of marine and terrestrial ecosystems to the carbon cycle and long-lived gases dealt with in this chapter. Following with the same example, biogenic aerosol-related processes could obviously interfere with hypothetical SRM approaches mentioned in this chapter. In summary, I suggest mentioning here additional feedbacks explicitly, which will ultimately stimulate inclusion of more interactive processes in future models and MIPs and perhaps make our projections more robust. Otherwise, we may keep overlooking potentially important processes. [Gwenaelle GREMION, Canada]	Noted: Aerosols are dealt with in detail in Chapter 6 - short lived GHGs
17802	84	16	84	16	Change 'system' to 'System' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
23342	84	19	84	21	This sentence is hard to read and to understand. Some commas or periods needed, as well as less cryptic language. [Gwenaelle GREMION, Canada]	Accepted - text revised
32802	84	48	84	52	A significant lack is studies that might be plausibly practical, so a slow increase in SRM to offset future warming and a bit more rather than model simulations that envision very large amounts of SRM--so we need studies of SRM as a peak temperature shaving approach and how this might be coupled with CDR, and mitigation rather than used alone. We need approaches to doing comparative effect and impact and risk analyses of GHG warming with and without SRM. We need studies of perhaps starting sRM with regional interventions, learning as the effort goes on [see MacCracken, M. C., 2016: The rationale for accelerating regionally focused climate intervention research, Earth's Future 4, 649-657, doi:10.1002/2016EF000450.and 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. as references on this suggestion]. [Michael MacCracken, United States of America]	Accepted. Text modified.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
22350	85	1	90	47	Another key Chpt 5 FAQ that was not addressed here is "What are tipping points and what is their role in shaping future climate trajectories?" A figure could include some of the same components as Figure 5.30 but with ocean processes, clathrates, ice-shelf dynamics etc included. This section could tie together and place into context some of the large scale biological, physical, terrestrial and ocean based tipping points that grab people's attention in climate reporting. [Gwenaelle GREMION, Canada]	Noted - As the number of FAQs is limited, this suggestion has not been included.
40340	85	1	90	47	Include FAQ: Are humanity's emissions into the atmosphere slowing down, and are negative emissions at the scale required likely to be achieved. [Michael Wadleigh, United States of America]	Rejected - While indeed FAQs, the answer to the first is a simple "no" based on the assessment in this Chapter, and the answer to the second would lie outside the remit of Working Group 1 of the IPCC.
47110	85	1			FAQ: Maybe you can add CO2eqv as a FAQ or cross-box. [Sophie von Fromm, Germany]	taken into account - Section 5.2.4 deals with comparison of CO2, CH4 and N2O
22352	85	3	85	3	Is this the best way to phrase this question? Is it a bit confusing. I suggest "Is the rate at which nature removes carbon from the atmosphere slowing down?" [Gwenaelle GREMION, Canada]	noted
19222	85	3	85	64	it would be nice to mention long-term neutralization via weathering, which takes tens of thousands of years. In geologic time, this is the pathway how CO2 was ultimately removed from the atmosphere, but because of its long duration, the elevated anthropogenic CO2 levels will be effectively permanent on human time scales. It would be a lost opportunity to not make this point. [Baerbel Hoenisch, United States of America]	noted - however, this is beyond the scope of the FAQ
13520	85	3	86	9	Does the term "size of the sink" refer to potential additional storage in land and ocean? It could also mean the total reservoir size. Please clarify this in this FAQ. [Govindasamy Bala, India]	taken into account
22354	85	8	85	9	Page 6, lines 50-51 indicates that ocean sink capacity is slowing, which contradicts the line 8-9 on page i-85. [Gwenaelle GREMION, Canada]	Accepted - text revised
19236	85	11	85	11	Mauna Loa measurements started in 1958 [Baerbel Hoenisch, United States of America]	Accepted - text revised
25686	85	11	85	11	The important part of the "since" clause is CO2 concentrations, not scientists. Better: "Since CO2 concentrations in the atmosphere have been measured, commencing in 1959," or the like [Stephen E Schwartz, United States of America]	Accepted - text revised
37754	85	11	85	14	The language in this paragraph has the same problem as discussed in comments 231, 238 and 244. Comment 232 also applies here. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	noted
23444	85	16	85	29	L18 ("Longer growing seasons in cold places due to global warming") seems to be in contrast with L.23 ("higher temperatures and droughts reduce the land sink"). It should be clarified that these feedbacks can both occur and one might outweigh the other. [Gwenaelle GREMION, Canada]	accepted - language added to make clear that the responses occur at regional not global level

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13516	85	19	85	20	I believe "deforestation" is a major component of the disturbances. This may be stated here. [Govindasamy Bala, India]	taken into account - revised to "natural disturbances". Anthropogenic disturbances are discussed in the following paragraph
17804	85	24	85	24	Change Nino to Niño [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
23344	85	25	85	25	remove 'the' before 'Earth' [Gwenaelle GREMION, Canada]	Accepted - text revised
23346	85	25	85	25	El Nino is defined as the anomalously higher SST, is it suitable to say that during El Nino years Earth surface has higher temperature? Better to make it more precise. [Gwenaelle GREMION, Canada]	accepted - added to specify that this only occurs regionally
37756	85	25			It would be better to write "when parts of the Earth's surface have well-above average temperatures". [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	editorial
29134	85	27			FAQ5.1 Here it should be clarified that nitrogen deposition may increase carbon storage in some but not all ecosystems. There is evidence that many ecosystems including tropical forests and northern peatlands are phosphorus limited or phosphorus-nitrogen co-limited. In these ecosystems N deposition may increase net carbon loss or does not affect C sink (e.g., Bragazza et al 2006 PNAS, Larmola et al. 2013 Global Change Biology 19: 3729-3739. This is not trivial as the latter store 30-50% global soil carbon, Gorham 1991 Ecol Appl 1:182-195, Tarnocai et al. 2009 Global Biogeochem Cycles 23: 10.1029/2008GB003327). Moreover the effect of N deposition on carbon sink in N limited ecosystems (such as intensively studied temperate forests) is likely to be transient as other constraints (P-limitation etc) emerge (e.g., Penuelas et al. 2017 Nature Ecology and Evolution 1:1438-1445). [Tuula Larmola, Finland]	taken into account - the effect of N deposition is clarified, but without adding the suggested detail, because this is too much detail for an FAQ
22356	85	28	85	29	Referencing ups and downs in the land sink panel of FAQ Fig 5.1 here would strengthen this statement. [Gwenaelle GREMION, Canada]	accepted
23446	85	31	85	37	It could be emphasized that the buffering capacity is partially linked to temperature and is hence expected to change in a changing climate. [Gwenaelle GREMION, Canada]	accepted - see response to comment 16340
17806	85	32	85	32	Insert space between number and units (50 m) [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
17808	85	35	85	35	Replace 'like' with 'such as' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
22358	85	36	85	37	Referencing the smooth nature of the ocean sink panel of FAQ Fig 5.1 here would strengthen this statement. [Gwenaelle GREMION, Canada]	accepted
16340	85	39	85	54	This FAQ could be a bit confusing - from lines 39-44 it states that there is no evidence that land or ocean sinks are slowing down, and that both the land and ocean sinks have been growing largely proportional to the increase in CO2 emissions, but from lines 49-53 it suggests that the ocean and land sinks will become smaller due to increasing atmospheric CO2. [Renee van Diemen, United Kingdom (of Great Britain and Northern Ireland)]	accepted - text explaining why the ocean sink will decline in the future has been added, and the cause for future land sink declines linked better to existing text

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
22360	85	40	85	40	This statement relies heavily on the reader understanding that the airborne fraction refers to the proportion of CO2 emissions rather than total amount of CO2 emissions. I do not think that the average lay-person would understand this nuance. The airborne fraction needs to be defined in more simple language. [Gwenaelle GREMION, Canada]	taken into account
22362	85	42	85	42	There is no evidence these sinks have slowed down already but there is a clear reason to suspect they may in the future (as reference in line 47). This statement therefore seems a bit misleading. [Gwenaelle GREMION, Canada]	accepted - brief explanation is given
22364	85	42	85	44	Again, this is in contradiction to page 6 line 50. [Gwenaelle GREMION, Canada]	accepted - text revised
22366	85	44	85	44	Pointing out the fact the fact that the atmospheric pool is increasing in size through time in figure and that this process inherits a lot of noise from the land sink would be key here. [Gwenaelle GREMION, Canada]	Noted
17810	85	44	85	44	Figure number missing [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account
25688	85	46	85	46	The text makes the decrease in sink rate sound bad; it would simply be a consequence of proportionality. I suggest strike the sentence. [Stephen E Schwartz, United States of America]	accepted - text revised to avoid negative connotation. However, we believe it is important to draw out the effect of the proportionality on future carbon sinks.
13518	85	46	85	54	The statement "Also, if emissions are not reduced that strongly, the ocean sink is expected to become smaller" is not consistent with line 47-47 "The fact that both the land and ocean sink respond to excess anthropogenic CO2 in the atmosphere, suggests that the absolute sink strength of land and ocean will vary in proportion to future anthropogenic emissions." [Govindasamy Bala, India]	accepted - revised text explains better why the future ocean sink will decline
36456	85	46		47	The second part of the sentence doesn't follow from the first. [Nathan Gillett, Canada]	accepted - text revised to make intention clearer
26524	85	49	85	50	"Also, if emissions are not reduced that strongly, the ocean sink is expected to become smaller". This sentence seems to contradict several of the sentences that have been used before, so it needs further explaining. Maybe one could add that the buffering capacity will decrease due to the amount of CO2 that has already been taken up and that warming decrease the air-sea difference as well as the transport from the surface to the deep. [Nadine Goris, Norway]	accepted - revised text explains better why the future ocean sink will decline
36458	85	53		54	Understanding and quantifying land and ocean sink changes is important for designing mitigation pathways to meet particular temperature targets, but I'm not sure that it is fundamental in general. [Nathan Gillett, Canada]	text revised - added for a particular temperature change target

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
22368	86	1	86	11	I assume that this figure will be based on LeQuere 2018a's figure 4 but could not find a draft of the figure in my materials currently. If this figure is used, I think it would be helpful to have a consistent sign convention (+ is emissions to atmosphere, - is sink to land or ocean). It would also be good to have the land sink, ocean sink, FF emissions, and land use change panels together and with the same y axis (in the publication the y axis varies making it hard to judge their relative magnitude at first glance). These four panels could perhaps be smaller and in a 2x2 square with a larger panel being devoted to the net atmospheric and budget imbalance graphs so that it is clear that these two are calculated based on the differences between the previous four fluxes. [Gwenaelle GREMION, Canada]	noted - new figure is provided in SOD
22370	87	1	87	1	This question needs to be reworded, right now it sounds like the mechanisms being discussed are permafrost thaw and ocean warming. Suggestion: Can thawing permafrost on land and under the ocean substantially increase global temperatures? [Gwenaelle GREMION, Canada]	Accepted: Text Revised
55842	87	1	87	54	Excellent and clear FAQ. [Pam Pearson, Sweden]	Noted
49490	87	1			Discussion of the uncertainty but potential large and overlooked contribution of non-growing season carbon fluxes from warming permafrost is notably missing in this section and should be included. Susan Natali presented on this at AGU in 2018 ( <a href="http://adsabs.harvard.edu/abs/2018AGUFM.B23A..01N">http://adsabs.harvard.edu/abs/2018AGUFM.B23A..01N</a> ). She also has papers describing the potential importance of winter emissions: <a href="https://doi.org/10.1890/13-0602.1">https://doi.org/10.1890/13-0602.1</a> [Seth Spawn, United States of America]	Rejected: More detail than possible in a FAQ
44880	87	3	87	3	N2O needs to be considered when evaluation climate feedbacks from the Arctic, given that there is increasing evidence that emissions of this strong GHG are not negligible from the Arctic and may even increase with warming/permafrost thawing (see comments and references above). In addition, i advocate the term permafrost climate feedback rather than permafrost carbon feedback, to include non-carbon feedbacks and dynamics including N2O. Thus is strongly suggest to modify that sentence to make it read: "Carbon released as carbon dioxide (CO2) or methane (CH4) and nitrogen as nitrous oxide (N2O) as a result of increased rates of decomposition in thawing permafrost soils may add an additional amount of warming, that is significant enough that it should be considered when evaluation premafrost-climate feedbacks, but does not appear ....." [Christina Biasi, Finland]	Rejected: More detail than possible in a FAQ
25478	87	6	87	7	Are you referring to gas hydrates here? [Sharon Smith, Canada]	Rejected: More detail than possible in a FAQ
17812	87	9	87	9	Change 'arctic' to 'Arctic' and 'amount' to 'amounts' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
25480	87	9	87	10	How deep is deep. Few m or 100s of m. If you are considering C in soils, peatlands then a few m whereas gas hydrates in Arctic would be at depths of a few 100 m and require much thicker permafrost [Sharon Smith, Canada]	Rejected: More detail than possible in a FAQ

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
25482	87	16	87	16	Isn't drainage an important factor determining whether CO2 or CH4 emitted? [Sharon Smith, Canada]	Rejected: More detail than possible in a FAQ
25484	87	17	87	17	By "near-surface" what depth is being referred to (important for timing of thaw). Also there should be more said about where this high potential for carbon release from thawing permafrost exists as it will not be the same throughout Arctic. There are extensive areas where very little soil and organic material so lower potential for carbon release (areas of bedrock etc.) [Sharon Smith, Canada]	Accepted: Text Revised
23348	87	20	87	20	change 'began' to 'begun' [Gwenaëlle GREMION, Canada]	Accepted - text revised
25486	87	20	87	20	Shouldn't you say warming and thawing of permafrost has already been observed throughout the Arctic (refer to Ch 2 section 2.3.2.5) and in some areas this has already resulted in C emissions? [Sharon Smith, Canada]	Accepted: Text Revised
22372	87	20	87	21	Unclear what process is happening twice as fast, permafrost thaw or warming. It is warming (Serreze et al 2011 Global and Planetary Change). Suggestion: "... Arctic, where temperatures are increasing twice as fast..." [Gwenaëlle GREMION, Canada]	Accepted: Text Revised
17814	87	24	87	24	Change 'arctic' to 'Arctic' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
22374	87	26	87	26	The relevance of nutrient is key, these soils are typically N and P limited (Shaver et al 1992 Biosciences). Suggestion: replace "decomposing" with "previously infertile" [Gwenaëlle GREMION, Canada]	Accepted: Text Revised
32244	87	26	87	26	I think it is worthwhile here to also mention that wildfires, which are becoming more frequent, can cause further acceleration of permafrost thaw. There are citations both for the increased fire regimes and its impacts on permafrost stability, but I see that these questions are not with referenced answers. [David Olefeldt, Canada]	Accepted: Text Revised
25488	87	26	87	27	Are you saying that that Carbon will not be released everywhere from permafrost thaw? [Sharon Smith, Canada]	Accepted: Text Revised
23350	87	32	87	36	This paragraph is hard to follow. [Gwenaëlle GREMION, Canada]	Taken into account
22376	87	32	87	36	Some context for the size of this flux would be helpful here, right now the language is vague. Schuur and Mack 2019 (Annual Review of Ecology, Evolution, and Systematics) places it at 1/3 of the flux associated with land use change. [Gwenaëlle GREMION, Canada]	Accepted: have added an estimate of the magnitude of this feedback here.
32246	87	34	87	35	I think it could be good to give a rough estimate of the central estimate of the permafrost carbon feedback - e.g. from Schuur et al., 2015, Nature. I think it could be stated roughly as "average GHG emissions from thawing permafrost soils throughout the 21st century are expected to represent 3 to 15 % of current CO2 emissions from use of fossil fuels". I think this gives a good idea of the expected strength of the permafrost carbon feedback. [David Olefeldt, Canada]	Accepted: have added an estimate of the magnitude of this feedback here.
36460	87	34		35	Saying only that additional warming due to emissions from permafrost thaw will be less than the fossil-fuel induced warming is a weak statement, because the latter is so high. This would be consistent with a statement that emissions from permafrost could increase fossil-fuel induced warming by a factor of up to two. Can the authors put a tighter constraint on this? [Nathan Gillett, Canada]	Accepted: have added an estimate of the magnitude of this feedback here.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
22378	87	35	87	35	Unclear what is meant by "itself at any level of warming" [Gwenaelle GREMION, Canada]	Accepted: Text Revised
44882	87	37	87	37	I suggest to add the following paragraph: "In addition to carbon, also nitrogen has to be considered when evaluating the climate feedbacks from the Arctic. That is because next to carbon, Arctic soils store also huge amounts of nitrogen. And increased mineralization and nitrification rates with warming may stimulate the release of the potent greenhouse gas N2O. N2O hotspots have been identified on peatlands and uplands, around bird colonies and on grazed areas, and significant regional N2O emissions from the Arctic have been recently observed. There is increasing evidence that emissions of N2O can rise when permafrost thaws and the climate warms. However, the magnitude of these non-carbon feedbacks remain highly unclear. It remains also uncertain to which extent higher nutrient availability will lead to increased plant growth and thus carbon sequestration or nitrogen loss in form of e.g. N2O." [Christina Biasi, Finland]	Rejected: More detail than possible in a FAQ
25490	87	38	87	43	Need to say something more about depth of occurrence. As mentioned in earlier comment for permafrost areas this depth will be a few 100 m and permafrost >200m thick is usually required for hydrate occurrence so only have right conditions in a portion of the permafrost region. Also due to their depth - longer time frame required for their dissociation. The right environment is also required in permafrost regions - sedimentary basins where hydrocarbons form so you will not find hydrates everywhere. Is "frozen" the right term to use when referring to deep ocean hydrate as they don't require permafrost. Is it ice or an ice-like structure? What happens when sea level increases does this stabilize deep ocean hydrates? [Sharon Smith, Canada]	Rejected: More detail than possible in a FAQ
17816	87	43	87	43	Probably worth cross-referencing the PETM event here as a 'historical' precedent [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Rejected: More detail than possible in a FAQ
23352	87	50	87	50	I find a bit confusing that CH4 emission is given here in TgCH4 whereas CO2 emissions are generally given in TgC in this paragraph. Is there a clear criterion in this report? I suggest using TgC everywhere. See also comment below (for page 90). [Gwenaelle GREMION, Canada]	Rejected, keeping units for consistency
45684	87	51			I agree with this general discussion. [Euan Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Noted
27820	87	53	87	53	attend the placeholders [Poot Delgado Carlos Antonio, Mexico]	Accepted: Text Revised
45686	87	53			Effect" - i.e. make happen - or 'affect' - i.e. influence?? [Euan Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Accepted: Text Revised
22382	88	1	88	43	This FAQ should be listed first since it covers some of the basic concepts needed to understand the other chapter 5 FAQ; This FAQ represent an opportunity to clear define and discuss not only the carbon budget but also what is meant by a feedback (and that there are potential +/- feedbacks) [Gwenaelle GREMION, Canada]	Reordered. [[Side note: Refers to FAQ 5.4]]



Comment ID	From Page	From Line	To Page	To Line	Comment	Response
16170	88	1	88	52	FAQ 5.3: The response to the question on "negative emissions" does not adequately reflect the risks of relying on so-called "negative emissions". A) none of the proposed technologies exist, especially at scales envisaged in some of the models. B) for most technologies, it is essentially unclear if they would ever work at all and if they would even be able to effectively remove CO2 from the atmosphere due to the lifecycle emissions of such large-scale, industrial CDR measures. C) removing CO2 from the atmosphere is in all cases of proposed technologies fraught with deep uncertainty as to the permanence of storage. The response should be very clear that fossil GHG/CO2 emissions remain in the atmosphere far more permanently than any CDR technology could remove it. D) the response does not adequately discriminate between technological, industrial CDR technologies, which come with large scale risks and negative impacts for ecosystems, biodiversity, food security, livelihoods, land tenure rights etc (see SR1.5), which include approaches such as BECCS and large-scale afforestation with invasive monoculture trees. However, there is ample and increasing evidence of the potential of CO2 drawdown through ecological restoration of ecosystems - forests, above all, but also other land-based and coastal ecosystems, that come with co-benefits for biodiversity and other internationally agreed goals such as the SDGs (see SPM of SR1.5 for an acknowledgment of these ecosystem-based approaches). See Climate Land Ambition Rights Allianca (CLARA)'s report "Missing Pathways to 1.5°C. The role of the land sector in ambitious climate action" - <a href="https://www.climatelandambitionrightsalliance.org/report">https://www.climatelandambitionrightsalliance.org/report</a> . E) The underlying Chapter 5 makes it very clear that the carbon cycle response to CDR is far less linear than is implicitly assumed in the models relying on them for reaching 1.5°C/2°C, which do not necessarily account for the	Rejected. The proposed discussion is beyond the scope of this FAQ, which attempts to explain the relationship between negative emissions, atmospheric CO2 and climate without reference to specific CDR methods.
22380	88	1	89	3	Overall this FAQ seems off-base, it seems to use more basic language than the rest of the report which is generally good but seems out of line with the general tone of the FAQ, summary, and chapter. The text dwells too much on defining negative emmissions rather than describing the nuances associated with carbon removal processes. [Gwenaelle GREMION, Canada]	Rejected. FAQs intentionally use a simpler language than the rest of the report.
23448	88	1			I understand the word "reversing" as to go back to a pre-industrial climate. However, the debate is rather on using negative emissions with the aim of not exceeding the 1.5 or 2.0 degrees limits. I would therefore use a different word (e.g. "halting" or "limiting"). If reversal was really meant, then that should be clearer in the text. [Gwenaelle GREMION, Canada]	Taken into account. We have replaced "reversal" with "attenuation" to clarify that we mean a change in direction, not a return to pre-industrial conditions.
22384	88	3	88	9	This statement does not seem wholly consistent with the chapter 5 text. "Remaining carbon budget" is used in this way but usually the general term carbon budget or balance is used to refer to what is described here as the human perturbation/contemporary carbon budget. The historic (natural, non-anthropogenically forced) carbon budget is referenced in the paleoclimate section 5.1.3 but they do not use this language. [Gwenaelle GREMION, Canada]	Taken into account - We have rewritten text to make clearer the different types of budgets that exist and that are used in this chapter and WGI. [[Side note: Refers to FAQ 5.4]]

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
23450	88	3	88	17	Some sentences in the second paragraph (L13-17) are a direct repeat from the introductory paragraph (L.3-11). The two paragraphs could be combined into one. [Gwenaëlle GREMION, Canada]	Taken into account. Repetition between the summary paragraph and the rest of the FAQ has been reduced.
22386	88	4	88	5	"If CO2 removal from the atmosphere is greater than CO2 release [for a given process, natural or anthropogenic], emissions are said to be net negative." [Gwenaëlle GREMION, Canada]	Taken into account. We have clarified that by release we refer to anthropogenic release.
32804	88	9	88	9	"will not" would seem to make this statement a "fact" for which science is more confident than virtually every other finding in this chapter. I just do not think that this statement can be made with such confidence (e.g., with greater certainty that human activities are raising the GHG concentrations, which was only "virtually certain"). Revision is needed--though by including "immediate" I guess it is justified, but rather unfairly. The issue is if CO2 removal would be generally beneficial as compared to not doing this, and the answer would "very likely" (?) seem to be yes. Please provide a more nuanced response here (lines 10-11 being a start). [Michael MacCracken, United States of America]	Accepted. "Will" has been changed to "would".
22388	88	11	88	11	Define "quite rapidly" [Gwenaëlle GREMION, Canada]	Accepted. "Rapidly" has been replaced with "within a few years".
16342	88	13	88	17	The paragraph could be strengthened by an additional sentence explaining how negative emissions are achieved (for example, through what technologies) [Renee van Diemen, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. An additional sentence explaining how negative emissions could be achieved has been included.
27822	88	13	88	41	highlight subtitles [Poot Delgado Carlos Antonio, Mexico]	Not applicable. Subtitles are for internal use only and will not be published.
22390	88	17	88	27	The paragraph starting at line 19 seems repetitive and could be replaced by the statement "Net negative emissions would result in a lower amount of CO2 in the atmosphere." [Gwenaëlle GREMION, Canada]	Taken into account. Repetition with the summary statement has been reduced.
22392	88	18	88	18	The word "harmless" seems loaded and a bit subjective [Gwenaëlle GREMION, Canada]	
17818	88	19	88	19	I was unfamiliar with the term 'carbon bathtub' and had to look it up. I think a clearer description/definition of the term would help readers. The context of a 'bathtub' does not come out clearly in the supporting text [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. Headers are for internal use only and will not be published.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
23354	88	19	88	27	The paragraph can be shortened because, in my opinion, there are some unnecessary repetitions. I understand the point of the paragraph, which is explaining “negative emissions” in the context of natural carbon budgets. But the current writing is confusing. My proposed writing: “The CO2 concentration in the atmosphere results from a balance between CO2 release and removal. If CO2 release exceeds removal, the CO2 concentration in the atmosphere will increase; if CO2 release equals removal the, atmospheric CO2 concentration will stabilise; and if CO2 removal exceeds release, the CO2 concentration will decline. Both CO2 release and removal processes may have natural and anthropogenic components. When anthropogenic CO2 removal (the so-called “negative emissions”) exceeds anthropogenic emissions, human activity becomes a net CO2 sink.” [Gwenaelle GREMION, Canada]	Rejected. We think it is important to make the distinction between the CO2 response to emissions and net emissions.
22394	88	22	88	23	In FAQ 5.1 the airborne fraction is defined as roughly half rather than 40%. Consistent language needed throughout. [Gwenaelle GREMION, Canada]	Taken into account. Language has been made consistent with rest of report. [[Side note: Refers to FAQ 5.4]]
22396	88	29	88	39	This fails to mention the potential reversal of the ocean sink, as mentioned in 5.6.21 and the executive summary. [Gwenaelle GREMION, Canada]	Taken into account. Mention of the slow reversal of ocean carbon storage has been included.
22398	88	31	88	34	These three sentences are too vague and the language is not precise. If the goal of the FAQ is to roughly outline the carbon budget than this would be better addressed with a figure and associated discussion. [Gwenaelle GREMION, Canada]	Noted. [[Side note: Refers to FAQ 5.4]]
25690	88	32	88	33	Reference reqd [Stephen E Schwartz, United States of America]	Rejected. FAQs do not include references.
22400	88	34	88	34	Unclear what is meant by "net zero levels" [Gwenaelle GREMION, Canada]	Net zero CO2 emissions. [[Side note: Refers to FAQ 5.4]]
22402	88	41	88	43	I have a hard time grasping what this figure would look like and how it would convey the concepts addressed in this FAQ [Gwenaelle GREMION, Canada]	Noted - an actual figure is produced for the SOD. [[Side note: Refers to FAQ 5.4]]
22404	88	44	88	44	Potential figure idea: two panels, one with historic carbon budget portrayed as CO2 fluxes through time (+source, - sink) so that their combined effect on the historic budget is clear. Fluxes in this panel could be differentiated by whether they are directly controlled by human activities (ie, land use change, FF emissions, are directly controlled). Second panel could contain pie charts for different warming scenarios with the pie size reflecting the remaining carbon budget and the slices of the pie representing fluxes contributing to future emmissions. Fluxes outside of our control (ocean sink, some portions of the land sink) would be the same in all scenarios which would cause the "allowable" contribution of anthropogenic emissions to shrink. [Gwenaelle GREMION, Canada]	Noted - the final figures are developed with expert designers. [[Side note: Refers to FAQ 5.4]]

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9586	88	45	88	48	Perhaps it would be good to indicate that some components (e.g. global mean temperature) are reversible soon after the overshoot but others are not (e.g. sea level rise, state of carbon reservoirs, ocean heat uptake) may not be as easily reversible, and depends on the size of the overshoot (the higher the overshoot, the longer it takes to restore the previous climate state (and sea level is practically irreversible on century time-scales). [Katarzyna (Kasia) Tokarska, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. A short statement about the overshoot dependence has been included.
23356	88	52	88	52	I guess the rapidity of surface air temperature evolution will depend on the proximity to the sea, and on the amount of heat stored in the deep sea being released at (and influencing the weather and climate of) a particular location. [Gwenaelle GREMION, Canada]	Rejected. Global mean quantities are considered here.
23452	88	53	88	54	I think it is important to not just understand the direction of change, but also the magnitude. [Gwenaelle GREMION, Canada]	Noted. A more quantitative discussion is provided in the relevant sections of the report.
22406	88	54	88	55	This figure does not sound appropriate for the question at hand, it seems like it would be better suited to FAQ 5.1 or FAQ 5.4 [Gwenaelle GREMION, Canada]	Not applicable. Unclear why figure is not deemed appropriate.
37758	88				Should "net" or "global-average" be inserted before "atmospheric" towards the end of this line? If emissions rise in one location, but sinks rise by the same amount in another location, it is the net CO2 that will not change. The geographical distribution of CO2 would change. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. CO2 mixes quickly in the atmosphere and its concentration changes little from one location to the other on annual time scales.
36462	90	1		43	For the purposes of a FAQ, I think it is confusing to discuss two different meanings of 'carbon budget'. I recommend just focussing on the remaining carbon budget. [Nathan Gillett, Canada]	Rejected - Precisely because it is confusing, we would like to provide some clarity in this FAQ. We will hence try our best once more to limit confusion.
56644	90	1			Given the confusion about the double use of "carbon budget", can AR6 consider implementing a gentle evolutionary step of the terminology? Specifically, call the first carbon budget the "carbon cycle budget". The second budget can either be still called "remaining carbon budget" or "carbon budget". The introduction of "cycle" into the former would strongly point to the focus of that concept to understand pools and fluxes.... [Malte Meinshausen, Australia]	Taken into account - a consistent diversified terminology has been implemented across the chapter.
37760	90	3	90	5	Sorry to be so repetitive but comments 231,238,244 and 248 apply here also. A wording such as "It can refer to how carbon dioxide is redistributed in the Earth System as a result of emissions from human activities: to what extent is the increase in carbon dioxide in the atmosphere limited by uptake by the ocean and land biosphere." avoids having to refer to emissions (as opposed to the well mixed GHG) being taken up by the oceanic and land sinks. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Noted - FAQ 5.4 has been entirely rewritten.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
35422	90	3	90	9	The term 'carbon budget' further can mean the amount of carbon dioxide emissions we can emit while still having a likely chance of limiting global temperature rise to for example 2 degrees Celsius above pre-industrial levels, so the 'total carbon budget'. This should be included, and discussed how the total budget should be equal to the historical and future/remaining budget. [Nadine Mengis, Canada]	Rejected - The historical carbon budget is not simply the cumulative historical CO2 emissions, but refers to the full budget (all sinks and sources) of CO2 in the planetary carbon cycle. This has been further clarified in the SOD.
35424	90	3	90	39	Be more specific about the kind of tempertaure change you are refering to here. This is CO2-induced temperature change only. Especially on page 90, line 29 this should be made explicit. Otherwise this metric can be misunderstood. [Nadine Mengis, Canada]	Taken into account - This has been clarified.
17820	90	4	90	4	Change 'system' to 'System' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. Please note that the final report will undergo a professional copy-edit before publication. This sort of issue will be fixed then.
23454	90	11	90	12	This sentence is a repeat from the introductory paragraph (L.3-9) and can be removed. [Gwenaelle GREMION, Canada]	Accepted - text revised
17822	90	14	90	14	Change 'system' to 'System' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. Please note that the final report will undergo a professional copy-edit before publication. This sort of issue will be fixed then.
23358	90	18	90	19	This senctence seems not relevant to the carbon budget [Gwenaelle GREMION, Canada]	Noted - FAQ 5.4 has been entirely rewritten.
23360	90	20	90	20	I suggest expressing cumulative CO2 emission since industrial era in more widely used units of Pg C (660) rather GT of CO2 (2440). [Gwenaelle GREMION, Canada]	Rejected - The remaining carbon budget is still expressed in GtCO2 as this is how it is being used by stakeholders, although this is a decision that can easily be reverted to PgC if needed for internal consistency.
13522	90	31	90	32	Is the linear proportionality between warming and cumulative emitted carbon due to the long lifetime of CO2 in the atmosphere? Also, I believe the proportionality is only approximate: the proportionality can be derived by assuming that the radiative forcing of linearly proportional to CO2 concentration. Hence, it should work only small emissions. [Govindasamy Bala, India]	Noted - Please see Section 5.5 in main text.
17824	90	32	90	32	Change 'system' to 'System' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. Please note that the final report will undergo a professional copy-edit before publication. This sort of issue will be fixed then.
13524	90	34	90	34	Delete "to stay" [Govindasamy Bala, India]	Accepted - text revised
35426	90	35	90	38	suggest changing the sentence to: "The exact size of the remaining carbon budget depends on the global warming level that we set as a limit, the probability with which we want to ensure that warming is held below that limit, and how successful we are in limiting emissions of other non-CO2 forcing agents that affect the climate, like methane or nitrous oxide." [Nadine Mengis, Canada]	Accepted - text revised [also incorporating comment 17826]
13526	90	35	90	39	It also depends on "climate sensitivity": amount of warming per doubling of CO2 [Govindasamy Bala, India]	Accepted - this is correct.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
17826	90	38	90	38	Change 'like' to 'such as' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
47856	90	41	90	43	FAQ: Schematic explaining what is a carbon budget and what factors add to its uncertainty range could be a useful communication figure. [WGI TSU, France]	Agreed.
29136	90				FAQ5.4 Definition of carbon budget. It should be clarified why the other major radiatively important carbon gas, methane is not part of carbon budget by this convention. Now methane is only mentioned as other greenhouse gas on L37-38. This can be unclear to the wide readership of FAQs. It would also be useful to briefly explain the difference of the two concepts: carbon budget and CO2 equivalent here, as Methane is included in the budgets expressed as CO2 equivalents. [Tuula Larmola, Finland]	Noted - the carbon budgets here speak to budgets of CO2 which take into account emissions from methane. It does not speak to budgets of CO2 equivalents.
36732	91	3	137	26	Use lower case for 2 in CO2. [Akihiko Ito, Japan]	Taken into account
36730	92	16	92	16	Remode (80-) after Science. [Akihiko Ito, Japan]	Noted. We are unsure why the Mendeley Word Plugin adds (80-) for Science Publications (thought this number isn't in the Mendeley Desktop). We can remove it manually from the bibliography, but at every "refresh" it will reappear. Point raised with TSU.
36726	93	26	93	26	Do not use capitales for author names, except for the first character: i.e., Beerling, Lucht, and Schaphoff. [Akihiko Ito, Japan]	Taken into account
36728	93	33	93	33	Add page number to BGR (2017): 184 p. [Akihiko Ito, Japan]	Noted - Page number are onto Mendeley but are not displayed by the bibliography. Point raised with TSU.
36734	95	2	95	2	Tags remained between CO and 2. [Akihiko Ito, Japan]	Taken into account
36736	95	26	95	27	Reference of C, De Klein et al. (2006) seems incomplete or incorrect. Please check. [Akihiko Ito, Japan]	Noted. We added the chapter name to the reference ( <a href="https://www.ipcc-nggip.iges.or.jp/public/2006gl/pdf/4_Volume4/V4_11_Ch11_N2O&amp;CO2.pdf">https://www.ipcc-nggip.iges.or.jp/public/2006gl/pdf/4_Volume4/V4_11_Ch11_N2O&amp;CO2.pdf</a> )
23362	95	30	95	33	These two references are repeated [Gwenaelle GREMION, Canada]	Taken into account
36720	96	34	96	35	Correct refernce should be: Ciais, P., Tan, J., Wang, X., Roedenbeck, C., Chevallier, F., Piao, S.-L., Moriarty, R., Broquet, G., Le Quéré, C., Canadell, J.G., Peng, S., Poulter, B., Liu, Z., Tans, P., 2019. Five decades of northern land carbon uptake revealed by the interhemispheric CO2 gradient. Nature 568, 221–225. doi:10.1038/s41586-019-1078-6 [Akihiko Ito, Japan]	Accepted
36738	97	37	97	37	Do not use capitales for words in the paper title except for the first one. [Akihiko Ito, Japan]	Taken into account
36740	98	16	98	17	Do not use capitales for words in the paper title except for the first one. [Akihiko Ito, Japan]	Taken into account
36742	98	22	98	23	Do not use capitales for words in the paper title except for the first one. [Akihiko Ito, Japan]	Taken into account
36744	99	4	99	5	Add URL or report information. [Akihiko Ito, Japan]	Taken into account
23364	100	6	100	9	These two references are repeated [Gwenaelle GREMION, Canada]	Taken into account
36746	100	8	100	9	This Erb et al. (2018b) is duplicate of the above one. [Akihiko Ito, Japan]	Taken into account

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
36748	100	27	100	28	Do not use capitales for words in the paper title except for the first one. [Akihiko Ito, Japan]	Taken into account
23366	104	20	104	20	use 2019a instead [Gwenaelle GREMION, Canada]	Taken into account
36752	105	3	105	3	Replace incomplete page numbers by GB4023. [Akihiko Ito, Japan]	Taken in to account
36754	107	44	107	44	Add report information to IEA (2017). [Akihiko Ito, Japan]	Taken into account
36756	108	5	108	6	Correct author or editor names: e.g., D. Q. G. -K. [Akihiko Ito, Japan]	Taken into account
36758	108	15	108	18	Why Index was separated from the report body (above one)? [Akihiko Ito, Japan]	Noted - the glossary is now cited.
36760	109	1	109	6	The first one (Janssens-maenhout et al., 2017) may not be necessary. [Akihiko Ito, Japan]	Taken in to account
36762	110	1	110	4	Do not use capitales for author names and title. [Akihiko Ito, Japan]	Taken into account
36764	110	11	110	12	Incomplete record: add journal name, volume, pages. [Akihiko Ito, Japan]	Noted - The citation is saved as a report (while institution is in Mendeley it is not displayed), but we added the URL now displayed.
36766	110	24	110	29	The first one (Keller et al., 2017, discussion paper of the below one) may not be necessary. [Akihiko Ito, Japan]	Noted
36768	113	13	113	16	Replace the order. 2017 paper should appear before 2018 one. [Akihiko Ito, Japan]	Noted - this display aspect should be sorted by TSU (Frontier Journals, recommended to be used by TSU) doesn't provide the final "correct" display of references.
23368	114	9	114	12	These two references are repeated [Gwenaelle GREMION, Canada]	Taken into account - duplicate solved
36770	114	9	114	12	This Li and Ilyina. (2018b) is duplicate of the above one. [Akihiko Ito, Japan]	Taken into account - duplicate solved
36772	114	25	114	25	Correct journal name: J. Soil Sci. Plant Nutr. [Akihiko Ito, Japan]	Noted - The journal is entered as "Journal of soil science and plant nutrition" in Mendeley (as suggested by Mendeley). The display of the journal by Mendeley Word Plugging is beyond us. Maybe TSU can help with this.
36774	116	14	114	22	Three Martizez-Botti et al. (2015a,b,c) are duplicate (same doi but different titles and pages). [Akihiko Ito, Japan]	Taken into account - duplicate solved
22408	116	17	116	21	Literature list: Martinez-Boti et al., 2015b and 2015c are the same, please delete one. [Gwenaelle GREMION, Canada]	Taken into account - duplicate solved
23370	116	17	116	22	These two references are repeated [Gwenaelle GREMION, Canada]	Taken into account - duplicate solved
43756	116	17	116	22	Same reference listed twice [Carles Pelejero, Spain]	Taken into account - duplicate solved
23372	118	35	118	38	there are some un-recognized sybmols [Gwenaelle GREMION, Canada]	Taken into account - corrected
36776	119	5	119	5	Incomplete record: page numbers. [Akihiko Ito, Japan]	Taken into account - now complete
36778	119	36	119	36	Incomplete record: page numbers seems incorrect and "doi:" is duplicate. [Akihiko Ito, Japan]	Taken into account - now complete
36780	120	7	120	7	Name of "O'Neill" has some error. [Akihiko Ito, Japan]	Taken into account - corrected
36782	120	21	120	22	Oschlies (2010) needs doi. [Akihiko Ito, Japan]	Taken into account - now complete
36784	120	29	120	32	P. Ciais et al. (2019) should be Ciais P. (2019) and moved to C area. [Akihiko Ito, Japan]	Taken into account - corrected
36786	120	40	120	42	Incomplete record: add page numbers, 91–113. [Akihiko Ito, Japan]	Taken into account - corrected
29512	122	1	122	6	Piao et al. 2018a and Piao et al. 2018b are the same reference [Rona Thompson, Norway]	Taken into account - duplicate solved

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
23374	122	1	122	6	These two references are repeated [Gwenaelle GREMION, Canada]	Taken into account - duplicate solved
36788	122	1	122	6	The Piao et al. (2018a,b) are duplicate. [Akihiko Ito, Japan]	Taken into account - duplicate solved
23376	122	19	122	24	These two references are repeated [Gwenaelle GREMION, Canada]	Taken into account - duplicate solved
36790	122	19	122	24	The Pongratz et al. (2018a,b) are duplicate. [Akihiko Ito, Japan]	Taken into account - duplicate solved
16310	122	19	122	25	same reference Pongratz2018a and Pongratz 2018b [Wolfgang Obermeier, Germany]	Taken into account - duplicate solved
16312	123	10	123	13	update references for Quéré (both) [Wolfgang Obermeier, Germany]	Taken into account -
36792	123	10	123	13	Quéré et al. (2018) can be removed, because they appeared as Le Quéré et al. (2018) [Akihiko Ito, Japan]	Taken into account
36794	123	39	123	39	Do not use capitales for author names. [Akihiko Ito, Japan]	Taken into account - corrected
36796	124	37	124	37	Incomplete record: no title, journal name, pages. [Akihiko Ito, Japan]	Rejected - no longer applicable
36798	125	3	125	7	The IPCC 1.5°C report has been published (not inpress). [Akihiko Ito, Japan]	Taken into account - corrected
36800	128	4	128	4	Correct journal name: J. Soil Sci. Plant Nutr. [Akihiko Ito, Japan]	Noted - The journal is entered as "Journal of soil science and plant nutrition" in Mendeley (as suggested by Mendeley). The display of the journal by Mendeley Word Plugging is beyond us. Maybe TSU can help with this.
36802	129	22	129	23	Incomplete record: no journal name, pages, doi. [Akihiko Ito, Japan]	Rejected - The reference is a conference proceedings. This option in Mendeley does not provide page, DOI, journal name option.
36804	129	27	129	28	Reference of FAOSTAT may be better as: FAO (2018) FAOSTAT, Statistical Division, FAO, Rome. URL. [Akihiko Ito, Japan]	Taken into account
36806	130	6	130	7	Correct author or editor names: e.g., D. Q. G. -K. [Akihiko Ito, Japan]	Taken into account - now corrected
36808	131	3	131	3	Incomplete record: no page numbers. [Akihiko Ito, Japan]	Taken into account - now complete
36810	131	31	131	32	Incomplete record: no year, page numbers, doi. [Akihiko Ito, Japan]	Taken into account - it was a newly submitted paper, which is better indicated now
36812	131	35	131	35	Page numbers, 11,411-499,508, look incorrect. [Akihiko Ito, Japan]	Taken into account - now corrected
23378	131	36	131	39	there are some un-recognized sybmols [Gwenaelle GREMION, Canada]	Taken into account - corrected
36814	131	37	131	38	"N2O" contains tags that should not appear. [Akihiko Ito, Japan]	Taken into account - corrected
23102	132	3	132	4	The DOI for this reference is wrong and should read 10.1038/nature16946 [Gwenaelle GREMION, Canada]	Accepted
36816	132	28	132	28	Remove this line. [Akihiko Ito, Japan]	Taken into account - reference updated
28436	132	29	132	30	A complete citation/reference is needed for this article. [Claude-Michel Nzotungicimpaye, Canada]	Taken into account
23104	132	29	132	30	This reference needs re-formatting (title is truncated and year, page and issue numbers are missing). [Gwenaelle GREMION, Canada]	Taken into account
36818	132	29	132	30	Incomplete record: no year, page numbers, doi. [Akihiko Ito, Japan]	Taken into account
36820	135	6	135	7	Incomplete record: no journal name, pages, doi. [Akihiko Ito, Japan]	Rejected - citation no longer cited.
36822	136	43	136	43	Page numbers, 20130096-20130096, look incorrect. [Akihiko Ito, Japan]	Taken into account - now corrected
51558	138	1	138	1	I do not understand why the holocene until 0 is not important in that figure 5.1? [Christian Beer, Germany]	Noted. But don't understand if a change is being requested.
22410	138	1	138	1	Figure 5.1 uses BC/AD, however, the description of Figure 5.1 uses BCE/CE. Be consistent to avoid confusion. [Gwenaelle GREMION, Canada]	Accepted. Changed to BCE/CE



Comment ID	From Page	From Line	To Page	To Line	Comment	Response
47112	138	1	138	17	Figure 5.1: The units of the x-axis (BC, AD) are not the same as in the figure description (BCE, CE) [Sophie von Fromm, Germany]	Accepted. Changed to BCE/CE
51964	138	1	139	1	There are direct equivalent figures to these in chapter 2. They seem consistent so main point is to wonder whether instead of having redundant images chapter 5 may refer to these figures and thus save space for presenting other material. Does the report as a whole need these twice? [Peter Thorne, Ireland]	Rejected. Figure 2 is not in ch2, and chapter 2, and here we need Fig. 1 as it is complementary to Fig.2. They are such important figures there is no harm in repeat them.
6714	138	4	138	4	Change 'BC' to 'BCE' and 'AD' to 'CE' [Andrew MacDougall, Canada]	Accepted. Done.
23264	138	4	138	5	Use only one legend on the side for all four observatories, instead of including/repeating them in each gas concentration. Furthermore, they are sometimes not even visible, i.e.: SPO in CO2 has the same colour, while MLO is almost covered up; CGO and SPO in are not visible in CH4 and N2O; ALT and MLO are barely visible in N2O [Gwenaelle GREMION, Canada]	Accepted. Suggestion adopted.
23258	138	4			Figure 5.1: In my version, due to really bad quality of figure, I can barely read the rates written in the figure (growth rate, rise rate, fall rate). [Gwenaelle GREMION, Canada]	Noted. New figure high resolution submitted.
23262	138	4			Figure 5.1: The observatory data line are barely noticeable. Use different colours than the actual concentration rates or different line types/shapes (line with connected dots, triangles etc). [Gwenaelle GREMION, Canada]	Accepted. A higher resolution with more distinctive colours provided.
27824	138	6	138	10	verify the format of the chemical formulas [Poot Delgado Carlos Antonio, Mexico]	Accepted, included subscripts.
56622	138	6			I think the current figure would deserve a number of additional datasets. Also, if wanted, I would be happy to provide a modified version of Figure 6b in the CMIP6 GHG historical concentration manuscript ( <a href="https://www.geosci-model-dev.net/10/2057/2017/">https://www.geosci-model-dev.net/10/2057/2017/</a> ) which combines both the timeseries that are run by the CMIP6 ESMs and multiple additional datasets/stations. The middle and right panels could be presented in that style - complemented by the 800k data. [Malte Meinshausen, Australia]	Accepted. New figure includes all datasets agreed in WGI for such a figure in different chapters.
13660	138	14	138	14	Add reference "Bereiter et al., (2015)" to the source of CO2 data from ice cores, AND revise CO2 over the last 800,000 years plotted in Figure 5.1-top-row-left with the new data of that paper (changing CO2 between 600,000 and 800,000 BCE). Also: Cross-check throughout AR6, if the last 800,000 yr of CO2 are somewhere else plotted, that do not include this revision in the CO2 data provided by Bereiter et al., (2015), (so far I found additionally Figure 1.2a to be corrupted in that sense), revise citation and replot data. Bereiter, B., S. Eggleston, J. Schmitt, C. Nehrbass-Ahles, T. F. Stocker, H. Fischer, S. Kipfstuhl, and J. Chappellaz (2015), Revision of the EPICA Dome C CO2 record from 800 to 600 kyr before present, Geophys. Res. Lett., 42, 542–549, doi:10.1002/2014GL061957. [Peter Köhler, Germany]	Accepted. New data used and reference included.
23260	138	16	138	17	Figure 5.1: The indicated observatory CGO is not mentioned/explained/defined. [Gwenaelle GREMION, Canada]	Accepted. Defined.
22412	138				Improve resolution or increase font size of the given rates in Figure 5.1 as they are difficult to read. [Gwenaelle GREMION, Canada]	Accepted, done.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
31962	139	0	139	0	Unit is missing for the CO2 growth rate in the future [Marie-France Loutre, Switzerland]	Accepted. Added.
51560	139	1	139	1	why only 60M years in that fig 5.2 when foster et al provides 400 M years? Differnt growth rates units are difficult to understand. [Christian Beer, Germany]	Noted. We have make the axis clearer to help the reader not to be confused. We stopped at 60M because it is the period which is most similar to current and possible worse case scenario over this coming century, and therefore most policy relevant.
6716	139	1	139	8	There are many things wrong with this figure. 1) There is no year 0, 1 BCE is directly followed by 1 CE. 2) The period from 1 CE to present is represented with a logarithmic scale where the other periods are represented with linear scales. 3) There is no scientific rational given for separating the Common Era from the rest of the Holocene. I would recommend the third column being the CO2 concentration for the Holocene. 4) CE usually stands for 'Common Era' or 'Christian Era' not 'Current Era'. 5) The first column really needs error bars or error shading. The CO2 proxy record is poorly constrained before the ice-core records. [Andrew MacDougall, Canada]	Accepted. We have added a zero, and redone the time frameworks consistent with suggestion.
56648	139	2			Great figure, but would be good to be consistent that changes in x axis scale is shown either by vertical line or background stripes etc... specifically the 1920 transition point might cause confusion to the hasty reader.... You could get the consolidated data used in CMIP6 from year 0 to year 2500 (future for SSP scenarios) from ESGF servers or greenhousegases.science.unimelb.edu.au. [Malte Meinshausen, Australia]	Accepted, showing better the changes in the x axis scale. We'll stop to 2100 thought as the most policy relevant timeframe.
56650	139	2			To make rates of change visually comparable between current and former periods, it is a bit confusing having the y axis scale change between the different lower panels. Not sure it can b solved, given that lack of yearly or 100-yearly resolution in the panels on the left... But the lower panel arrangement would ideally clarify that there is a factor of 100 between the left and middle and then again between the middle and righter-most panel. Happy to assist with a sketch. [Malte Meinshausen, Australia]	Noted. We have tried to improve the readability of changing scales in each of the panels to help the reader.
22414	139	3	139	4	The abbreviations BCE and CE are explained in the description of Figure 5.2, but already used in Figure 5.1. [Gwenaelle GREMION, Canada]	Accepted. Move the acronym description to Figure 5.1
28062	140	1	140	1	I assume that a legenda, axis titles and values, and further titles and subtitles of the subfigures still have to be added to clarify what is shown in the figures? Right now it is not clear what is shown in this figure. [roderik van de wal, Netherlands]	Reject: The caption should explain this
28064	140	1	140	1	Maybe indicate for clarity in figure b itself that the respectively 100, 500, 100 PgC are pulse removals, now it can also be interpreted as pulse additions. [roderik van de wal, Netherlands]	Reject: not for Figure 5.3

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
28066	140	1	140	1	It is very hard to precisely read the time scale and relative sequestration potentials from the figure, especially the horizontal bars that indicate uncertainty ranges in the time scale of C storage are very hard to interpret as the ranges on the x-axis (such as centuries-millenia) are not very exact. Furthermore on the y-axis the space between 3.0 and 30.0 Pg C per year is too small, which makes it hard to read the values. Finally, in the text there is no reference to this figure. I would suggest to incorporate this figure in the section. [roderik van de wal, Netherlands]	Reject: not for Figure 5.3
28068	140	1	140	1	Maybe elaborate more in the caption on the differences between the three experiments (abrupt4xCO2, G1, piControl), since that is not very clear now. Are the variables Ra and GPP averaged over land as well? Furthermore, why is it important to show the mean excluding GISS-E2-R? And finally, maybe indicate what the variable Ra represents. This variable was not discussed in the chapter. [roderik van de wal, Netherlands]	Reject: not for Figure 5.3
32216	140	1	140	1	In the figure, I would argue that thawing permafrost should include CH4 - as thaw can cause land subsidence into wetlands and lakes where the thawed soil will decompose anaerobically and release CH4 [David Olefeldt, Canada]	Accept: change to be made in final figure
32218	140	1	140	1	For wetland processes, I would suggest that you should change it to "Wetland and Freshwater processes", or have freshwater processes as a separate line. Ponds, lakes, streams, and rivers have significant CH4 emissions which also are expected to increase. [David Olefeldt, Canada]	Accept: change to be made in final figure
32220	140	1	140	1	In the box "Land" there are Disturbances indicated - perhaps you could be more specific, as this is vague. I'm assuming wildfire is a major disturbance considered. Wildfire could even be considered to have its own line in my opinion. [David Olefeldt, Canada]	Agriculture and Disturbances have been removed as forcing factors rather than feedback mechanisms
47114	140	1	140	19	Figure 5.3.: Why does Ocean acidification has as positive feedback on the total CO2. To my understanding the ocean acidification is caused by a higher concentration of CO2 in the oceans due to higher uptake from the atmosphere. [Sophie von Fromm, Germany]	Accept: the + feedback is largely driven by the changing buffering capacity, OA is also part of the same overall change in carbonate chemistry driven by increasing DIC that drives the feedback.
23266	140	1			Figure oval "Warming, Changes in rainfalls and climate extremes" improve from general to specific, i.e. "Climate extremes events, warming and changes in precipitation" [Gwenaelle GREMION, Canada]	Accepted: change made
23268	140	1			Adjust the black arrow's sizes leading from anthropogenic CO2 emissions to "Land, Atmosphere, and Ocean" according to their contribution [Gwenaelle GREMION, Canada]	Accepted: scaled them approximately 1:2:1
13666	140	3	140	17	The caption to Fig 5.3 contains the partitioning of where the anthropogenic CO2 is going (ending in 44% airborne fraction). Please add for which time window this partitioning has been calculated, maybe by using and refering to Table 5.1 (page 26). [Peter Köhler, Germany]	Accept: 2008 - 2017 added refence to Table 5.1

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
9588	140	3	140	18	Since the figure shows climate-carbon feedbacks, the interface between climate and carbon effects (warming and carbon uptake) should be clearer. -i.e. how does warming affect carbon storage? Perhaps it would be useful to include a second panel of this figure, which would consist of the climate effects (from higher/lower CO2) as they act together with the carbon effects. [Katarzyna (Kasia) Tokarska, United Kingdom (of Great Britain and Northern Ireland)]	Accept: effect on carbon storage will be added
23270	140	7		8	Can you show in the figure, how the ocean actually dominates the heat feedback?? Maybe also adjust the purple, brown, and magenta arrow's sizes accordingly and make them larger than coming in from the land-box. [Gwenaelle GREMION, Canada]	Accepted
23274	140	8		10	The percentages in the small circles are supposed to show what exactly? It seems to be the percentage of contribution of the anthropogenic emissions which will be stored/present in land, atmosphere and ocean. Maybe the percentages need to be mentioned in the boxes (land, ocean) and not put on the arrows, which usually show a flow. Here it looks, like there is a flow of 29% of anthropogenic emissions into the land, a flow of 22% into the oceans and a flow of 44% into the atmosphere. [Gwenaelle GREMION, Canada]	Noted: The % show the partitioning of the total anthropogenic emissions into the atmosphere, ocean and land reservoirs. The figure has been improved to clarify this issue
23272	140	9			We read from left to right and it would be easier to follow the text, if you mention terrestrial (29%) before ocean (22%) for the negative feedback. [Gwenaelle GREMION, Canada]	Accepted: Caption has been edited
23278	140	10			Indication of "ocean and terrestrial domains" shown in magenta does not seem right. [Gwenaelle GREMION, Canada]	Accept: Have clarified that we mean the processes that drive the negative feedbacks
23280	140	11			There seems to be a lot of valuable information and also opinion about the contribution of all processes in this paragraph (line 11-15), that you should rather mention in the regular text, instead of elongating this text explaining the figure. The long text makes it hard to grasp the information in the figure quickly. Maybe change line 11-15 to: Positive carbon and climate forcing feedback processes (purple), as well as negative feedback processes (magenta) and uncertain biosphere processes (brown) contribute to the total CO2 airborne fraction. [Gwenaelle GREMION, Canada]	Accept: a slightly modified version of the suggestion: Positive carbon and climate forcing feedback processes (purple), negative feedback processes (magenta) and uncertain biosphere processes (brown) collectively influence the variability and rates of change of CO2 airborne fraction
23276	140	12			purple not capital. Check colours in entire paragraph, to not be capital. [Gwenaelle GREMION, Canada]	Accept
23282	140	15		17	Keep it compact, i.e.: The focus on the dominant GHG CO2 does not intend to exclude the processes' influence on the CH4 and N2O fluxes, which are accounted for in the total radiative forcing. [Gwenaelle GREMION, Canada]	Accept: change made
47414	140				fig 5.3. I would not include "agriculture and disturbance" as a feedback in this figure - they are external forcings to the system [chris jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted: Have removed it and changed soil respiration to soil respiration and biogeochemistry
22416	140				In Figure 5.3, text box on "Land": "Wetland processes" instead of "Wetland processes" [Gwenaelle GREMION, Canada]	Accepted.
47116	141	1	141	8	Figure 5.4.: It would be better if you reverse the y-axis of the last graph in order to make the decrease O2/N2 better visible. [Sophie von Fromm, Germany]	Taken into account. Improved Fig 5.4

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
42814	141	1			At figure 5.4, we should add global mean CO2 concentrations calculated by NOAA or WDCGG(JMA) for further understanding. [Takashi Maki, Japan]	Accepted. Global mean CO2 values from NOAA are now included.
42816	141	1			At figure 5.4, we should add $d(\text{CO}_2)/dt$ such as figure 5.12 for methane. In CO2 case, we can obtain detailed growth rate (latitudinal zone) from NOAA or WDCGG(JMA). [Takashi Maki, Japan]	Taken into account. This should also be helpful for explaining Fig. 5.10 - the CO2 flux IAVs
27826	141	2	141	6	verify the format of the chemical formulas [Poot Delgado Carlos Antonio, Mexico]	Noted. All looks fine
56332	141	2	141	8	There seems to be something wrong with the difference curve between MLO and SPO in Fig. 5.4. According to the orange and black curves, although CO2 values at MLO and SPO clearly differ by a bias, they also coincide once or twice a year. So the green-colored difference curve, labeled MLO-SPO, should reach zero once or twice a year. But it doesn't. How is that possible? Are the curves filtered differently? Seems like some explanation in the caption would be warranted. [Steven Neshyba, United States of America]	Taken into account. Plot data modified.
32806	141	2	141	8	What are the green curves and why is Wellington as it is? And plotting the MLO-SPO so positive values are down seems very odd. [Michael MacCracken, United States of America]	Taken into account. Improved Fig 5.4
42752	141		141		Fig 5.4 - Difficult to differentiate which y-axes belong to which time series in panel b, consider using more text and/or color-coding (wouldn't hurt panel a either) [Stephanie Courtney, United States of America]	Taken into account. Improved Fig 5.4
47118	142	1	142	8	Figure 5.5a: Is it possible to define others? [Sophie von Fromm, Germany]	Taken in to account
47120	142	1	142	8	Figure 5.5b: What are the green lines? Better description of this graph needed. [Sophie von Fromm, Germany]	Taken into account. Text revised to "The net land use change CO2 flux (Pg yr-1) as estimated by two bookkeeping models and 16 dynamic global vegetation models (DGVM) for the global annual carbon budget 2018 (Le Quéré et al., 2018a). The two bookkeeping models are from Hansis et al., 2015 and Houghton and Nassikas, 2017 both updated to 2018; their average is used as to determine the net land use change flux in the annual global carbon budget. The DGVM estimates are the result of differencing a simulation with and one without land use changes run under observed historical climate and CO2, following the TRENDY v7 protocol; they are used to provide an uncertainty range to the bookkeeping estimates. "

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
47510	142	1	142	8	Add a graph with global anthropogenic CO2 emissions 1870 - 2017 by nations grouped by Human Development level (UNDP HDI) including information of cumulative emissions per capita. Important context for the Sustainable Development Goals. [Birgit van Munster, United Kingdom (of Great Britain and Northern Ireland)]	Noted. We think that details are not needed for understanding the science of carbon cycle, but certainly of interests to the other working groups of IPCC.
41790	143	1	143	14	Colors are confusing. Please use more cointerasted colors for the different curves. [Marc Aubinet, Belgium]	Taken into account - Figure 5.6 has been redrawn in response to other comments
47122	143	1	143	25	Figure 5.6.: Difficult to distinguish between models and observation-based products - especially in the dark blue box. Might be helpful to use in addition to colors dashed and solid lines. [Sophie von Fromm, Germany]	Taken into account - Figure 5.6 has been redrawn in response to other comments
42818	143	1			At figure 5.6, we should add top-down estimated sea-air CO2 fluxes such as TransCom or RECCAP model inter-comparison results. [Takashi Maki, Japan]	Noted
32808	143	16	143	25	Why plot the sea-air flux and have negative numbers than plot the air-sea flux and have positive numbers? In Figure 5.9, the convention seems to be that flux into the land reservoir is positive. [Michael MacCracken, United States of America]	Rejected: It is a convention in the ocean community that the flux from the atmosphere to the ocean is negative because the pCO2 gradient is expressed as (pCO2ocean - pCO2atm)
22638	144	1	144	1	Should read 2000-2016 in Fig 5.7b? Not immediately clear why this goes to 2016 when Fig5.7a goes to 2015, should be 2016? Missing explanation of other panels in this figure [Gwenaelle GREMION, Canada]	Accept: Fig 5.7 is being modified to include an atmospheric inversion and the updated version will take this comment into account
47124	144	1	144	9	Figure 5.7.: Acronyms in the panels are not well explained. [Sophie von Fromm, Germany]	Accept: Fig 5.7 is being modified to include an atmospheric inversion and the updated version will take this comment into account
47416	144				fig 5.7. The "placeholder" text suggest this figure may change - I actually found it very helpful, so hopefully you can keep it. The caption, though, should descibe what is in the figure and not get into analysis - explanation of the implications of the figure should go in the main text and not the caption [chris jones, United Kingdom (of Great Britain and Northern Ireland)]	Noted: Fig 5.7 is being modified to include an atmospheric inversion and the updated version will take this comment into account. It will essentially be the same but we will not name the specific models - and include a mean of all the models
22640	145	3	145	4	Please mark whether data available from Antarctica (Figure 5.8) [Gwenaelle GREMION, Canada]	Accepted-figure revised.
32810	145	5	145	5	This needs to say it refers to column loading in the ocean in the first sentence. [Michael MacCracken, United States of America]	Taken into account - text revised. The term "storage rate" was added in order to explain the "column inventory change".
26526	145	6	145	6	Please correct this sentence to "It shows that the change in the regional ocean inventories", otherwise the description is wrong. [Nadine Goris, Norway]	Accepted - text revised. The text indicated was removed, because it is explained in the main text.
44492	145	7	145	7	replace "letter" with "later" [VIJAY SONI, India]	Not Applicable - text revised and this is no longer included in the Figure caption.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
13328	146	7	146	13	The legend of the top-right y axis is not correct: the NBP is not inverted, but the transport model is. [Frederic Chevallier, France]	Taken into account. The "Inversed NBP" is changed to "Land CO2 flux" for better clarity, as also suggested by Comment ID 29472.
36464	146	34		36	Clarify whether or not this includes LUC emissions. [Nathan Gillett, Canada]	Accepted. Figure legends have been checked for consistency in distinguishing land CO2 sink and net land CO2 sink.
47126	147	1	147	17	Figure 5.10: Use a different color code for sources and sinks (y-axis) that are not already used for other paramters in the graph. Makes it easier to read the graph. [Sophie von Fromm, Germany]	Accepted. Figure 5.10 is revised accordingly.
27228	147	2	147	17	O. Humlum et al, 2013, Global & Planetary Change 100, 51, updated in www.climate4you.com/ show a systematic lag of CO2 fluctuations that FOLLOW temperature fluctuations. This expert reviewer recommends a discussion of this important point which is not apparent in Fig. 5.10 due to the choice of the time scale. [François GERVAIS, France]	Taken into account. We clarify in the figure legend that the temperature data used here is land surface air temperature over the tropics (widely used to correlate with CO2 growth rate, e.g. Keppel-Aleks et al. (2014), Anderegg et al. (2015), Fang et al. (2017) and Humphrey et al. (2018)), which do not have lag with CO2 growth rate, as similar results were reported by Wang W et al. (2013) and Wang X et al. (2014). The lag between CO2 fluctuations and global surface temperature fluctuations reported by Humlum et al. (2013) should not be directly compared here because they used surface air temperature over global land and ocean.
47418	147				fig 5.10. you need a consistent definition of "tropics". In geography it would be +-23 degrees based on the sun. Climate modellers often use +-30 to split the world into equal area sections. +-20 here I strange - why use that? [chris jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Figure 5.10 is revised with the definition of the tropics as 30 degree, following the norm of climate modellers.
32222	148	1	140	1	Estimate of the permafrost carbon may be reconsidered. The most recent comprehensive estimate I know of is 1,300 Pg from Hugelius 2014 Biogeosciences, and is valid for the top 3 m of ground. However, 500 Pg of this carbon stock is actually not in permafrost, but found above the permafrost in the active layer where the soil freezes and thaws every year. "Only" ~800 Pg C is thus actually estimated to be found in current permafrost. Should thw 500 Pg in the non-permafrost soils be included in the soils estimate instead in the figure? Is there a double accounting of carbon in Arctic soils and that included in permafrost? [David Olefeldt, Canada]	Accepted. Updated.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
41802	148	1	148	1	Figure 5.11 not fully clear : what is the meaning of the numbers in dashed circle for the atmosphere? : Is 275 the atmospheric increase (Table 5.1, first column) ? If yes, why isn't it in pink ? What's the meaning of the number 589 ? Is it the preindustrial value ? Why isn't it in a white circle ? [Marc Aubinet, Belgium]	Accepted. Made clear what the dash circle.
41804	148	1	148	1	Fig 5.11: net land use budget : a + must replace the comma (0.75+3.2) [Marc Aubinet, Belgium]	Accepted. Changed.
36704	148	1	148	1	In Figure 5.11, it is ambiguous whether 'Net land flux' includes land-use change or not. [Akihiko Ito, Japan]	Accepted. Made it clear.
51562	148	1	148	1	Clarify that permafrost stands for all soils in the permafrost zone including the active layer, and soils all soils outside that zone. [Christian Beer, Germany]	Accepted. Clarified in new sentence.
47128	148	1	148	33	Figure 5.11: Where does the change in anthropogenic stock of vegetation and soils (20 Pg) comes from? There is nor flux that is high enough to explain this change in the graph. [Sophie von Fromm, Germany]	Accepted. Made it clear.
47130	148	1	148	33	Figure 5.11: Is there already a change in the stock of permafrost soils due to climate change visible? If so, such change should be included in the graph. [Sophie von Fromm, Germany]	Rejected. Section 5.4 provides an assessment of current fluxes due to climate change and shows there are not discernible measured fluxes coming from the thawing of permafrost
47132	148	1	148	33	Figure 5.11: Use for (natural) and anthropogenic stocks same color code as for the fluxes in addition to the dashed and solid circles. [Sophie von Fromm, Germany]	Accepted. Changed.
36706	148	13	148	13	Based on multi-data and multi-model simulations in ISIMIP2a, Ito et al. (2017) indicates that model-estimated GPP spans from 98.4 to 141.2 Pg C /yr in 1981-2000. Ito, A., Nishina, K., Reyer, C.P.O., François, L., Henrot, A.-J., Munhoven, G., Jacquemin, I., Tian, H., Yang, J., Pan, S., Morfopoulos, C., Betts, R., Hickler, T., Steinkamp, J., Ostberg, S., Schaphoff, S., Ciais, P., Chang, J., Rafique, R., Zeng, F., Zhao, F., 2017. Photosynthetic productivity and its efficiencies in ISIMIP2a biome models: benchmarking for impact assessment studies. Environmental Research Letters 12, 085001. doi:10.1088/1748-9326/aa7a19. [Akihiko Ito, Japan]	Noted. Taken into account.
36702	148	25	148	25	Do not use lowercase for (Archer et al. 1998). [Akihiko Ito, Japan]	Accepted. Changed.
49022	148	27	148	28	Fig. 5.11 presents 1400 PgC for permafrost, which is different from the number suggested by AR5 (1700 in Fig 6.1 in AR5). Would that imply that the newer studies referred in FOD has solved previous problems in AR5? Such as the suggested possible overlap between the carbon estimates for wetland and permafrost. Also, it would be more comprehensive if the description regarding the new advances of AR6 to assess permafrost carbon is available. [Minchao Wu, Sweden]	Noted. Yes. New publications diminish overlaps.
36466	148	27			Explain the definition of 'reserves' briefly here. The difference between reserves and resources is a constant source of confusion to non-specialists. [Nathan Gillett, Canada]	Accepted. Added.



Comment ID	From Page	From Line	To Page	To Line	Comment	Response
49024	148	31	148	33	For the carbon export to the ocean, Fig. 5.11 only includes the estimates for "Natural", missing the part from "Anthropogenic perturbation". According to Regnier et al., 2013. the anthropogenic export from soil to river (0.8 PgC/yr) is almost as large as the natural one, missing this may be misleading. [Minchao Wu, Sweden]	Accepted. Added.
13782	148	31	148	33	same as comment 4 above. [Pierre Regnier, Belgium]	Rejected - We don't know where comment 4 is
47420	148				fig .11. pink and blue circles for the atmos store are wrong way round? [chris jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Corrected.
36708	149	1	149	1	In Figure 5.12 (middle), what does the 'MEI' represent? Multi-variate ENSO index? Please clarify. [Akihiko Ito, Japan]	Taken into account. Added in the figure caption and plot revised
26850	149	1	149	2	Define MEI used in the middle panel. [Ragnhild Bieltvedt Skeie, Norway]	Taken into account. Added in the figure caption and plot revised
47134	149	1	149	12	Figure 5.12: Acronym (MEI) within the graph not explained. [Sophie von Fromm, Germany]	Taken into account. Added in the figure caption and plot revised
22642	149	1	149	12	MEI (visible in second panel) is not explained (Multivariate ENSO Index) [Gwenaelle GREMION, Canada]	Taken into account. Added in the figure caption and plot revised
22644	149	1	149	12	Why is data for CGO and THD shown separately, when they are both included in AGAGE? At the same time it is stated that "data from many other measurement networks are not included here". It could be good to explain why this choice was made. Do CGO and THD show the outer boundaries of the available data? Would one of the missing datasets be of interest? [Gwenaelle GREMION, Canada]	Noted. This was a challenging task to increase the time series of CH4 to the 1970s. We have tried to harmonise some of the available data. Some mean and site-specific data are shown to make sure there is no inconsistency in the time series merging, e.g. that for the PDX data and AGAGE THD data as the data were not gathered in the same manner.
56646	149	3			Figure 5.12 - It would be illustrative to include the CMIP6 global-mean, NH and SH average data in panel a. Data available on ESGF servers or greenhousegases.science.unimelb.edu.au. Advantage: Providing readers with a sense of what concentrations the ESMs are run with .... [Malte Meinshausen, Australia]	Taken into account. Followed the cross-chapter discussions at LAM3 in the revised plots
36468	149	3			Explain what delta_13C shows - what is the physical interpretation/driver of variations in this quantity? [Nathan Gillett, Canada]	Noted. The information is available in the text, section 5.2.2.1 and later
28438	150	0	150	0	While most numbers in Figure 5.13 are consistent with the information in Table 5.2 for 2010-2017, some sectorial emissions are different between the table and the figure. For instance, the figure shows that CH4 emissions from fossil fuels for 2010-2017 were 85-105 Tg yr-1 whereas the table shows 77-126 Tg yr-1 (actually 85-105 Tg yr-1 reported for 1990s in the table). For termites, the figure reports 2-22 Tg yr-1 for 2010-2017 whereas the table indicates 3-15 Tg yr-1 (actually 2-22 Tg yr-1 reported for 1990s in the table). In brief, there is need for consistency in the information provided in Figure 5.13 and Table 5.2 for 2010-2017. [Claude-Michel Nzotungicimpaye, Canada]	Taken into account. We have made the numbers consistent with those in Table 5.2
22646	150	1	150	4	Please mention the methane pools ("GAS RESERVE" and "PERMAFROST HYDRATES") [Gwenaelle GREMION, Canada]	Taken into account. Explained in the figure caption

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
22648	150	1	150	4	Is there an available range for PERMAFROST HYDRATES? If so, please show it. [Gwenaelle GREMION, Canada]	Noted.
47136	150	1	150	5	Figure 5.13: There are no stocks for water bodies, soils and vegetation. At the moment most of the fluxes have no corresponding stock. [Sophie von Fromm, Germany]	Noted.
47138	150	1	150	5	Figure 5.13: Keep the order of the legend same as in Figure 5.11. Legend and units for stocks are missing. [Sophie von Fromm, Germany]	Accepted. Added.
41810	150	3	150	3	What is the meaning of arrow colors? Why not use the same rules as for Fig 5.11? [Marc Aubinet, Belgium]	Noted and accepted. The meaning is described in the figure bottom; we'll change colours.
41812	150	3	150	3	specify the units of methane stocks [Marc Aubinet, Belgium]	Accepted. Added.
42754	150		155		Schematics are excellent, though would help 5.13 and 5.16 to have the same representation and key as 5.11, mainly a preindustrial atmospheric stock separate from anthropogenic increase, and different color scheme than fluxes like in 5.11. Also more text for understanding, such as spelling out atmospheric interactions (OH, Cl) and naming flux processes rather than sources (wetlands, freshwaters, etc.). I think each of these will help differentiate stocks and flows which non-experts have trouble with, and the design of the schematics implies they are intended for use with non-experts. [Stephanie Courtney, United States of America]	Accepted. Changed.
47140	151	1	151	9	Cross-chapter box: Figure 1: Better to use same color code for anthropogenic and natural fluxes as in Figure 5.13 [Sophie von Fromm, Germany]	Accepted. Changed.
41814	152	3	152	6	difference between red and black lines should be better explained in legend (prognostic vs inversion models?) [Marc Aubinet, Belgium]	accepted - Figure revised
47142	153	1	153	8	Figure 5.14: Might be helpful to explain the difference between 15N and 15N-alpha. [Sophie von Fromm, Germany]	accepted, see also comment 22584
47144	153	1	153	8	Figure 5.14: Small letter (a-c) are not shown in the panels but only in the figure description. [Sophie von Fromm, Germany]	accepted - Figure revised
56652	153	2			Figure 5.14. To show the reader the composite timeseries that was fed to CMIP6 ESM models, it would be nice to show in panel a that global-mean N2O dataset as a faint line in the background... Available from ESGF servers or greenhousegases.unimelb.edu.au [Malte Meinshausen, Australia]	Noted
47146	154	1	154	10	Figure 5.15: It is confusing that for the graphs and the map a very similar color code is used, even though they show different parameters. [Sophie von Fromm, Germany]	accepted - line code now follows IPCC style guides
36470	154				Figure 5.15. If I understand correctly, changes in N2O driven by changes in atmospheric CO2, climate change, and land cover change are labelled 'natural'. This is misleading - I suggest using another term, such as 'baseline' or similar. Also I suggest using common scales on all panels, since it allows easy intercomparison of regions. [Nathan Gillett, Canada]	accepted - figure changed
22650	155	1	155	4	To which arrow do inland-water sources belong to? [Gwenaelle GREMION, Canada]	accepted - Figure revised

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
47148	155	1	155	5	Figure 5.16: It is possible to add stocks for e.g. land and oceans? If not it should be explained in the figure description why those are not shown/calculated. [Sophie von Fromm, Germany]	accepted - caption revised
47150	156	1	156	7	Figure 5.17: Use same color code for the three gases as in the previous graphs. [Sophie von Fromm, Germany]	Taken into account.
25692	156	2	156	2	Strike "Change in" [Stephen E Schwartz, United States of America]	Accepted
56654	156	2			The figure caption says that the CO2, CH4, and N2O timeseries are taken from Figure 5.1. However, in Figure 5.1, there do not seem to be continuous global-mean timeseries shown for those three gases. [Malte Meinshausen, Australia]	Not Applicable. We have taken these numbers from Chapter 2 now
36472	156				The authors should cross-reference Chapter 7 here, and ensure consistency between the radiative forcings presented here and those in Chapter 7. [Nathan Gillett, Canada]	Noted
47152	157	1	157	7	Figure 5.18: Why is CO2 not differentiated by GWP and GTP? [Sophie von Fromm, Germany]	Not applicable. CO2 is the reference or the unit of GWP and GTP calculation
36712	157	8	157	8	Year is lacking for 'Thompson et al.'. [Akihiko Ito, Japan]	Accepted. Year added as the paper is now published
47154	158	1	158	4	Figure 5.19: Explain the difference/calculation between pH and seasonally-detrended pH [Sophie von Fromm, Germany]	Rejected - caption revised. Legends inserted into the figure. The method of seasonally-detrending has been described in each literature cited in the caption, or otherwise the same as Bates et al., 2014.
47156	158	1	158	4	Figure 5.19: Lines might be easier to read than dots in the individual graphs. [Sophie von Fromm, Germany]	Accepted - figure revised
47158	158	1	158	4	Figure 5.19: Where does the data comes from for this graph? [Sophie von Fromm, Germany]	Accepted - references added to the caption.
23106	158	1	158	4	If this figure is kept, I suggest to add trend lines and/or significance of the correlation to each of the time-series. [Gwenaelle GREMION, Canada]	Taken into account - Trend of ocean acidification appears not always linear. Annual mean values of deseasonalized pH are connected with line.
28440	159	0	159	0	Based on the figure caption, it is not clear to me what the top panel (of the three panels) represents. Also, the figure caption does not refer to the (b) and (c) from the two lower panels. Overall, the clarity and quality of the figure should be revised. [Claude-Michel Nzotungicimpaye, Canada]	Not applicable - the figure removed.
47160	159	1	159	6	Figure 5.20: Impossible to read graph at this status: numbers to blurry, units missing... [Sophie von Fromm, Germany]	Not applicable - the figure removed.
23108	159	3	159	5	This figure has unreadable labels. As for the second and third panel, the focus seems to be on polar regions, polar-projection maps would be desirable. The color scale could also be reversed since generally red implies more urgency, which is now is used for a later onset of undersaturation if I read the labels right (?). [Gwenaelle GREMION, Canada]	Not applicable - the figure removed.
26528	159	3	159	5	This figure caption needs to be revised, it is not clear what is shown in which figure panel. [Nadine Goris, Norway]	Not applicable - the figure removed.
36474	159				The units are missing in this figure. [Nathan Gillett, Canada]	Not applicable - the figure removed.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
26530	160	0	160	0	Make a clear line in this figure to divide between CMIP5 and CMIP6 models. As it stands of now, it is not clear where the line of CMIP5 models ends and where the line of CMIP6 models starts. [Nadine Goris, Norway]	Noted.
38174	160	1	160	1	What grey color mean? [Hiroaki Kondo, Japan]	Accepted. Figure revised.
47162	160	1	160	7	Figure 5-21: Which are the CMIP5 and which are the CMIP6 models? Use different color or seperate them by a black vertical line. [Sophie von Fromm, Germany]	Accepted. Figure revised.
51564	160	1	161	1	Fig 5.22 and 5.23: I suggest delete panels a, b is more clear. Can be combined then. [Christian Beer, Germany]	Agreed. Figures reworked.
51966	160	1			I would consider a more intuitive way of seperating CMIP5 from CMIP6 here. Perhaps putting a brief piece of vertical white space between the twoi sets of models would aid reader interpretation along with highlighting via e.g. bold column outlines the two multimodel mean sets of values? [Peter Thorne, Ireland]	Accepted. Figure revised.
47422	160				fig 5.21 - can you more cleary demarcate CMIP5 vs CMIP6 (a blank column perhaps to separate left and right sides of the figure) [chris jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Figure revised.
47424	160				fig 5.21. suggest you only use one variant of each model unless there's clear reason not to (e.g. here the two CESM2 models are the same except fr chemistry which doesn't affect the land eval). Same would be true of HadGEM2 variants from CMIP5 (why/how do you only select some but not all CMIP5 models?) [chris jones, United Kingdom (of Great Britain and Northern Ireland)]	Noted.
36476	160				Please add references, and a description of the periods and diagnostics used to evaluate simulations of these variables. [Nathan Gillett, Canada]	Noted.
26532	161	0	161	0	Please add the time of reference to the change in Ocean carbon ("change in ocean carbon relative to the year xx"). I am assuming that it is 1860, but it would be better to mention it. Also, The picture shows simulations from 1860 to 2005, while the caption says 1850 to 2005. [Nadine Goris, Norway]	Accepted. Figure caption revised to :Figure 5.22: "Modelled ocean carbon sink for 1860 to 2005 in historical ESM simulations, compared to observation-based estimates (from GCP); panel (a): uptake rate (PgC yr-1), panel (b): change in carbon store from 1860 (PgC)".
25694	161	0	162	0	I am pleased that rates AND cumulative rates are shown. Please make sure that data tables with the values of these quantities are provided with the final report [Stephen E Schwartz, United States of America]	Noted.
44868	161	1	161	1	The color of legend overlaps, needs to be changed. [Kaoru Kubota, Japan]	Accepted. Colour of lines in Figures 5.22 and 5.23 revised to make them easier to distinguish.
36478	161				Add an estimate of observational uncertainty. [Nathan Gillett, Canada]	Noted.
26534	162	0	162	0	Please add the time of reference to the change in land carbon ("change in land carbon relative to the year xx"). I am assuming that it is 1850, but it would be better to mention it. [Nadine Goris, Norway]	Accepted. Figure caption revised to :Figure 5.23: "Modelled land carbon sink for 1860 to 2005 in historical ESM simulations, compared to observation-based estimates (from GCP); panel (a): uptake rate (PgC yr-1), panel (b): change in carbon store from 1860 (PgC)".

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
38176	162	1	162	1	As the lines are so crowded, the variation of each model is not clear in the left panel. [Hiroaki Kondo, Japan]	Accepted. Figure revised.
44866	162	1	162	1	The color of legend overlaps, needs to be changed. [Kaoru Kubota, Japan]	Accepted. Figure revised.
6836	162	1	162	1	The orange lines are difficult to distinguish (i.e., CANESM and NORESM). [Nicholas Smith, United States of America]	Accepted. Figure revised.
9590	162	3	162	6	It is unclear what is the net land carbon uptake referring to (is it the net atmosphere-land carbon flux, the 'nbp' CMIP5 variable?), and how does it account for land use change (which is present in the historical simulations)? [Katarzyna (Kasia) Tokarska, United Kingdom (of Great Britain and Northern Ireland)]	Agreed - clarified in the SOD
9592	162	3	162	6	It is difficult to distinguish the different colours of the models. Perhaps use some colour coding, to easily identify models with the negative net change in land carbon (in panel b) in shades of one colour, and models with the positive change in shades of another colour. [Katarzyna (Kasia) Tokarska, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Figure revised.
36480	162				Add an estimate of observational uncertainty. [Nathan Gillett, Canada]	Noted.
26536	163	0	163	0	Please write "2000-2009" above the figures, as "2000-09" is confusing (it might as well be interpreted as September 2000) [Nadine Goris, Norway]	Accepted. "2000-09" changed in "2000-2009".
51566	163	1	163	1	Source of inversion missing. Would be good to see all model results in addition. [Christian Beer, Germany]	Agreed - references for inversion(s) now included.
36482	163				Rather than showing an observational estimate with no uncertainties, and a model ensemble range, it would be better to show the observations with associated uncertainties, and consider showing individual models. This would allow a better assessment of where models are or are not consistent with observations. [Nathan Gillett, Canada]	Noted.
25696	164	0	164	0	Show also for RCP 2.6, especially as this RCP is used for other quantities subsequently, Fig 5.26. Also, please show derivative (yearly atmos growth) and also please show emissions and cumulative emissions for these RCPs. And please provide numerical data with final report. All figs should show indiv model results color coded and labeled. [Stephen E Schwartz, United States of America]	Noted, but we do not have space to include similar plots for other RCPs and SSPs.
38178	164	1	164	1	What is meant by red and black broken lines and chain line in panel (b)? [Hiroaki Kondo, Japan]	Accepted. Figure caption extended to include: "Vertical black dashed line shows the best estimate of global mean atmospheric CO2 concentration in 2014. Horizontal dashed red line shows best estimate of global mean atmospheric CO2 concentration by 2060 under the RCP8.5 emissions scenario, using this emergent constraint (see Section 5.4.6)"

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
23110	164	3	164	7	Figure 5.25b): What are the red and black dashed lines? [Gwenaelle GREMION, Canada]	Accepted. Figure caption extended to include: "Vertical black dashed line shows the best estimate of global mean atmospheric CO2 concentration in 2014. Horizontal dashed read line shows best estimate of global mean atmospheric CO2 concentration by 2060 under the RCP8.5 emissions scenario, using this emergent constraint (see Section 5.4.6)".
56656	164	3			Figure 5.25. The units should likely by dry air mole fractions and not ppmv. Apart from that... great figure and curious how the full CMIP6 ensemble will feature on the scatter plot... [Malte Meinshausen, Australia]	Noted, but we prefer to use ppmv as this is more understood by non-experts.
25698	165	0	165	0	please show emissions and cumulative emissions for these RCPs. All figs should show indiv model results color coded and labeled. And please provide numerical data with final report. [Stephen E Schwartz, United States of America]	Noted, but we do not have space to include similar plots for other RCPs and SSPs.
25700	165	0	165	0	The figure needs a citation. One should be able to have access to the primary data, which would include the atmospheric CO2 mixing ratio as a function of time and the ocean uptake rate as a function of time on a model by model basis. The ocean uptake rate should be more or less proportional to the excess CO2 above preindustrial, so it would seem valuable to examine that, by showing the ratio of stock in the atmosphere to flux into the ocean, an overall transfer coefficient, again on a model by model basis. Simply showing the fluxes for the two scenarios is of little value. Or alternatively, a plot of flux into ocean against excess atmospheric CO2 (mixing ratio, or better, stock) to examine the linearity of that uptake or the departure therefrom, the slope of which would be the transfer coefficient, an intensive variable, a much sharper tool for comparing models. . [Stephen E Schwartz, United States of America]	Noted.
26538	165	0	165	0	Please add the time of reference to the change in Ocean carbon ("change in ocean carbon relative to the year xx"). I am assuming that it is 2005, but it would be better to mention it. [Nadine Goris, Norway]	Accepted - Figure caption changed to : "Projected evolution of the ocean carbon sink for 2005 to 2090 in concentration-driven RCP2.6 (blue) and RCP8.5 (red) scenarios. Panel (a): net uptake rate (PgC yr-1); panel (b): change in carbon store from 2005 (PgC)".
25702	166	0	166	0	please show emissions and cumulative emissions for these RCPs. All figs should show indiv model results color coded and labeled. And please provide numerical data with final report. Better two separate panels than having the two RCP's in the same panel. [Stephen E Schwartz, United States of America]	Noted, but we do not have space to include similar plots for other RCPs and SSPs.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
25704	166	0	166	0	The figure needs a citation. One should be able to have access to the primary data, which would include the atmospheric CO2 mixing ratio as a function of time and the land uptake rate as a function of time on a model by model basis. The land uptake rate should be more or less proportional to the excess CO2 above preindustrial, so it would seem valuable to examine that, by showing the ratio of stock in the atmosphere to flux into the land (as a function of time), an overall transfer coefficient, again on a model by model basis. Simply showing the fluxes for the two scenarios is of little value. Or alternatively, a plot of flux into land against excess atmospheric CO2 (mixing ratio, or better, stock) to examine the linearity of that uptake or the departure therefrom, the slope of which would be the transfer coefficient, an intensive variable, a much sharper tool for comparing models. [Stephen E Schwartz, United States of America]	Noted, but rejected. Emergent constraints use the ensemble of models to relate contemporary observations to future projections. They are a good example of the whole being more than the sum of the parts with regard to multi-model ensembles.
26540	166	0	166	0	Please add the time of reference to the change in land carbon ("change in land carbon relative to the year xx"). I am assuming that it is 2005, but it would be better to mention it. [Nadine Goris, Norway]	Accepted - Figure caption changed to : "Projected evolution of the land carbon sink for 2005 to 2090 in concentration-driven RCP2.6 (blue) and RCP8.5 (red) scenarios. Panel (a): net uptake rate (PgC yr-1); panel (b): change in carbon store from 2005 (PgC)".
36484	166	6		7	Better to show the 5-95% range across the ensemble. [Nathan Gillett, Canada]	Noted.
25706	167	0	167	0	Fig 5.28a. If there is an error in the model, throw it out; don't try to infer emergent constraints when models can be rejected. There might be some value in examining the covariance between quantities when neither of them can be constrained by observation, such as forcing and climate sensitivity. [Stephen E Schwartz, United States of America]	Noted, but rejected. Emergent constraints use the ensemble of models to relate contemporary observations to future projections. They are a good example of the whole being more than the sum of the parts with regard to multi-model ensembles.
38180	167	1	167	1	What is meant by lines, dots and ellipses? [Hiroaki Kondo, Japan]	Agreed. Figure caption made more descriptive.
47164	167	1	167	11	Figure 5.28.: No legend/explanation for the red dots. [Sophie von Fromm, Germany]	Agreed. Figure caption made more descriptive.
47166	167	1	167	11	Figure 5.28.: In the figure description it says 2100, whereas on the y-axis it says 2060. [Sophie von Fromm, Germany]	Agreed. Figure caption corrected.
9594	167	2	167	12	It would be helpful to show the emergent constraint for two different observational datasets, to show its robustness regardless of the observational product used. [Katarzyna (Kasia) Tokarska, United Kingdom (of Great Britain and Northern Ireland)]	Noted.
26542	167	6	167	6	The figure caption says "against the error in simulation", but the x-axis does not show the error but the CO2 concentration. When leaving out "the error in simulation of", the caption is correct. [Nadine Goris, Norway]	Agreed - caption corrected.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
28442	168	0	168	0	It would be good to indicate the difference between the feedbacks in the top panel versus those in the bottom panel in the first two sentences of the caption. For example, instead of: "A synthesis of the magnitude of biogeochemical feedbacks on climate expressing non-climate feedbacks in common units (W m-2 K-1) with physical feedbacks, following (Arneth et al., 2010; Gregory et al., 2009) and revised radiative forcing calculations (Etmiman et al., 2016). Black dots represent single estimates, and coloured bars denote the simple mean of the dots with no weighting or assessment being made to likelihood of any single estimate. These feedback metrics have, where possible, been assessed for the RCP8.5 scenario in year 2100. They may be state or scenario dependent and therefore cannot always be compared like-for-like. Note the different x-axis scale for the lower portion of the Figure. There is low confidence in the magnitude of the feedbacks in the lower panel of the figure. The role of nitrogen limitation..." Rephrase the caption as follows (check the second sentence): "A synthesis of the magnitude of biogeochemical feedbacks on climate expressing non-climate feedbacks in common units (W m-2 K-1) with physical feedbacks, following (Arneth et al., 2010; Gregory et al., 2009) and revised radiative forcing calculations (Etmiman et al., 2016). There is low confidence in the magnitude of the feedbacks in the lower panel of the figure. Black dots represent single estimates, and coloured bars denote the simple mean of the dots with no weighting or assessment being made to likelihood of any single estimate..." [Claude-Michel Nzotungicimpaye, Canada]	Accepted
47168	168	1	168	19	Figure 5.29: Explain briefly what the negative and positive values mean? Will make it easier to fully understand the graph [Sophie von Fromm, Germany]	Accepted
36486	168				Figure 5.29. This is a great summary figure. Somewhere I suggest clearly indicating that the scale on the lower panel is different than that on the upper panel, perhaps with lines from the lower to the upper panel showing the range covered in the lower panel on the upper scale. It took me a long time to realise the scales were different. [Nathan Gillett, Canada]	Rejected - this statement is already in the Figure caption.
51968	169	1			This relative projection is the wrong way around I think. The map should be small (also in robinson projection) and the time seres plots should be much larger as it is the data that are key here. [Peter Thorne, Ireland]	Not applicable – the figure is excluded
36488	169	2			Insert 'variables which have been suggested to exhibit' after 'Projections of some'. [Nathan Gillett, Canada]	Not applicable – the figure is excluded
38182	170	1	170	1	It might be better to draw such that the amount of storage in the atmosphere will reduce in panel (d). [Hiroaki Kondo, Japan]	Noted.
35430	171	0	171	0	Figure 5.31 misses labels for axes and legend. [Nadine Mengis, Canada]	Accepted. Original figure had labels etc. but was not correctly reproduced in pdf.



Comment ID	From Page	From Line	To Page	To Line	Comment	Response
25708	171	0	171	0	Fig 5.3; this is evidently a placeholder but it is quite non-informative; axes not labeled and the like. Studies not identified. Cases not identified. [Stephen E Schwartz, United States of America]	Agreed - Figure 5.31 reworked.
38184	171	1	171	1	Add the numerals, labels of axis and meaning of symbols. [Hiroaki Kondo, Japan]	Accepted. Original figure had labels etc. but was not correctly reproduced in pdf.
47170	171	1	171	12	Figure 5.31: Impossible to comment on this graph without units and further explanations. [Sophie von Fromm, Germany]	Accepted. Original figure had labels etc. but was not correctly reproduced in pdf.
26974	171	1	171	12	Figure 5.31: Please add a legend to each axis. It is completely unclear what is shown here. [Joachim Rock, Germany]	Agreed - Figure 5.31 reworked.
23284	171	1			Figure 5.31: Legend missing [Gwenaelle GREMION, Canada]	Agreed -corrected.
23286	171	1			Figure 5.31: Axe titles missing [Gwenaelle GREMION, Canada]	Agreed -corrected.
38186	172	1	172	1	What is meant by 'NETs'? [Hiroaki Kondo, Japan]	Taken into account. Definition of NETs (Negative Emission Technologies) was included in figure caption.
23288	172	3		6	Figure 5.32: Is there supposed to be two texts explaining this figure, where one says "Figure 4" and one says "figure 5.32"?! Combine both informations, but erase the original text that comes with the figure. [Gwenaelle GREMION, Canada]	Agreed - text corrected.
23290	172		172		FIG 5.32 : It needs more explanations to be clear even considering the paragraph preceding the figure : what is "NET"? Carbon removed thanks to CDR? Or removed by CDR + other mitigation effects? [Gwenaelle GREMION, Canada]	Taken into account. Definition of NETs (Negative Emission Technologies) was included in figure caption.
38188	173	1	173	1	What does the diamond symbol mean? [Hiroaki Kondo, Japan]	Taken into account. It has been clarified that symbols refer to different scenarios.
23292	173	1			Figure 5.33: Missing explanation for colour-code [Gwenaelle GREMION, Canada]	Noted.
23294	173	1			Figure 5.33: Top figure, missing explanation for diamond shape --> missing legend? Unclear, if same legend as from bottom figure applies. [Gwenaelle GREMION, Canada]	Noted.
36492	173				The figure shows measures of the effectiveness of CDR in terms of airborne fraction only. But a full assessment of the effectiveness should also consider the effect on GSAT, which is likely to be less state-dependent than the PAF, because of the compensating effect of the logarithmic dependence of radiative forcing on the CO2 concentration perturbation. [Nathan Gillett, Canada]	Noted
23296	174	1			Figure 5.34: Text in figure and legend too small to read. [Gwenaelle GREMION, Canada]	Noted.
23298	174	1			Figure 5.34: red and green colour code for "other environmental impacts" not readable for colour-blind people. [Gwenaelle GREMION, Canada]	Noted - we will keep in mind when redesigning these figures.
23302	174	1			Figure 5.34: Last vertical bar has no impact whatsoever indicated. What does this mean? --> There is uncertainty about this entire method, so that would mean, there is not point really in mentioning it here at all. [Gwenaelle GREMION, Canada]	Noted.
23300	174	4		5	Figure 5.34: What are the horizontal bars you mention? I can see only vertical bars. [Gwenaelle GREMION, Canada]	Noted.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
36494	174				Figure 5.34 For best compability of approaches, consider using a continuous scale on the y-axis, perhaps a log-scale. [Nathan Gillett, Canada]	Noted.
27828	175	3	175	3	attend the placeholders [Poot Delgado Carlos Antonio, Mexico]	Noted.
36496	175				Figure 5.35. I found this figure hard to interpret. If I understand correctly $GPP - R_a = NPP$ . I think this relationship could be better represented by stacking the grey bars on top of the coloured bars. Then readers could easily read off NPP and GPP from the y-axis. The grey $R_a$ bar could be overplotted with a downward arrow to show that this makes a negative contribution to NPP (either in every case, or perhaps in a key showing how to interpret at the side). [Nathan Gillett, Canada]	Agreed. Figure updated.
25710	176	0	176	0	the legends for the line colors are not clear. Not at all clear what is meant by a land or ocean sink efficiency: Transfer coefficient? Suggest not put different quantities in the same figure with left and right axes. Plot as separate figures so as to avoid confusion. [Stephen E Schwartz, United States of America]	accepted - new figure is provided in SOD
25712	176	0	176	0	Rather than the figure shown, it would be much more valuable to have the figure support the statements in the text to the effect that the land and ocean sinks have increased in proportion to atmospheric CO <sub>2</sub> , so rather than (or in addition to) the plots shown, plot these fluxes versus atmospheric CO <sub>2</sub> . [Stephen E Schwartz, United States of America]	accepted - new figure is provided in SOD
36498	176				FAQ 5.1, Fig 1: I found this figure hard to interpret and certainly too technical for a FAQ. I suggesting simplifying it. [Nathan Gillett, Canada]	accepted - new figure is provided in SOD
33282	FAQ 5,3				Negative emissions might be better as absorption [Michael Schwabe, Uruguay]	Not applicable. Unclear which sentence this comment refers to.
33284	Figure 5.11				Possibly this figure tries to achieve too much and hence seems a little busy. Maybe simply the anthropogenic change only should be emphasised for a more powerful effect. [Michael Schwabe, Uruguay]	Noted. It has been simplified but maintain the natural versus anthropogenic components.
29442					Fig. 5.1: Add a comment about the north-south gradient of atmospheric CH <sub>4</sub> in the panel for 1900-2020 as it this could be interpreted as a disagreement of the direct atmospheric record and the ice core data [Rona Thompson, Norway]	Accepted. Comment added.
29448					Fig.5.4: The right y-axis in figure (a) is inverted, I suggest changing the direction of the axis so that it is more apparent that the MLO-SPO gradient is actually increasing. Similarly for figure (b) for d13C-CO <sub>2</sub> and figure (c) for O <sub>2</sub> /N <sub>2</sub> . This would be more intuitive. [Rona Thompson, Norway]	Taken in to account. Figure revised
29468					Fig. 5.6: I find the colours of the thick lines difficult to distinguish [Rona Thompson, Norway]	Taken into account - Figure 5.6 has been redrawn in response to other comments
29470					Fig. 5.7: inconsistency between the time given in the caption and heading for panel (b), is it the difference 2016-2000 or 2000-2016? Also, more explanation of the panel text (c) to (h) is needed, e.g. what are HL and ST? [Rona Thompson, Norway]	Accept: Fig 5.7 is being modified to include an atmospheric inversion and the updated version will take this comment into account

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
29472					Fig. 5.9: second panel from the top, the y-axis label should be "Inversion NBP". Also, I think a reference is needed for the satellite-based (i.e. MODIS and AVHRR) NPP estimates, since this is a highly derived product. [Rona Thompson, Norway]	Accepted. Figure 5.9 and its legend are revised accordingly.
29476					Table 5.1: missing units [Rona Thompson, Norway]	Accepted. Added.
46656					Little coverage of lakes in the chapter or lake acidification (e.g. <a href="https://www.cell.com/current-biology/pdfExtended/S0960-9822(17)31655-X">https://www.cell.com/current-biology/pdfExtended/S0960-9822(17)31655-X</a> ) [WGI TSU, France]	Rejected on lake acidification. It is not an impact chapter albeit we do cover ocean acidification. Best for WGII.
56906					Figure general comments Chapter 5: units have to be in ( ) and not in [ ] and font is Arial. For more information about Visual guidelines, please refer to the IPCC visual style guide ( <a href="https://www.ipcc.ch/site/assets/uploads/2019/04/IPCC-visual-style-guide.pdf">https://www.ipcc.ch/site/assets/uploads/2019/04/IPCC-visual-style-guide.pdf</a> ) [WGI TSU, France]	Noted, all new figures are following guidelines
56908					Figure 5.2: the colors for RCP lines should be the ones mentioned for RCP in the Visual Style Guide (see page 8) [WGI TSU, France]	Accepted, changed.
56910					Figure 5.3: the schematic could benefit from a more engaging design. For more guidelines, contact the TSU's graphic officer [WGI TSU, France]	Accepted. Schematic improved.
56912					Figure 5.4: green and yellow are not easily distinguishable with colorblind vision. Better to replace yellow with blue [WGI TSU, France]	Accepted
56914					Figure 5.5: This figure would benefit from a title next to a. (e.g. anthropogenic CO2 emission from 1870 to 2016) and b. (e.g. modelled net land use change flux ) [WGI TSU, France]	Accepted. Changed
56916					Figure 5.6: This figure would benefit from a title right on top of the panel (e.g. Temporal evolution of sea-air CO2 flux) [WGI TSU, France]	Taken into account - Figure 5.6 has been redrawn in response to other comments
56918					Figure 5.7: it is not clear why the lower panel are labelled ( c), (d) etc. // colors are not suitable for colorblind vision (more guidance in the visual style guide or with the TSU graphic officer) // font should be Arial [WGI TSU, France]	Accept: Fig 5.7 is being modified to include an atmospheric inversion and the updated version will take this comment into account
56920					Figure 5.7: it would be quite powerful to display in some way the three explanatory points from the caption into the figure directly. Consult with the graphics officer for more guidance. [WGI TSU, France]	Accept: Fig 5.7 is being modified to include an atmospheric inversion and the updated version will take this comment into account.
56922					Figure 5.8: "column inventory change between 1994 to 2007" could be used as title above the panel (and the white annotation removed) // it is unclear why there are two different scales and units in the color bar // "no data" can stay but most of the figures in the report display the regions where data are missing in white. since hatching tend to be used for visualizing model agreement in the report, it is recommended to change the "no data" area in white. // It is unclear what B stands for [WGI TSU, France]	Accepted - figure revised
56924					Figure 5.10: colors are not suitable for colorblind vision - green can be replaced with turquoise // explanations on what the grey shaded area are should appear in the figure for a better understanding // the figure holds a lot of valuable information which would be clearer if the figure would be a little bit less cluttered (e.g. remove the grid) . Contact the TSU graphics officer for guidance. [WGI TSU, France]	Accepted. Figure 5.10 is revised accordingly.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
56926					Figure 5.10: colors are not suitable for colorblind vision - green can be replaced with turquoise [WGI TSU, France]	Accepted. Figure 5.10 is revised accordingly.
56928					Figure 5.10: green/orangecolors are not suitable for colorblind vision - green can be replaced with turquoise [WGI TSU, France]	Accepted. Figure 5.10 is revised accordingly.
56930					Figure 5.12: This figure would benefit from a general title right on top of the panel, if any comes to mind (e.g. Methane measurements in the atmosphere, or something in that line) // labelling the panells with a short annotation would help navigating the figure without refering directly to the caption (e.g. (a) mole fraction, (b) growth rate...) [WGI TSU, France]	Taken into account. Labels added.
56932					Figure 5.11 and 5.13: it would improve readability to bring the legend right below the main title (which requires probably to increase the height of the sky) // ideally, legend should be the same in both figures (anthropogenic=pink) // if enough space, "natural" could be added to "stocks" (fig 5.11) [WGI TSU, France]	Accepted. Added.
56934					Figure CC box 5.1 fig 1: you could add the meaning behind the black dots in the legend as well. [WGI TSU, France]	Accepted. Legends added for the dots and line
36200					Overall this chapter is in very good shape. It is complete and well-written, and is much improved from the ZOD. The structure works well, without too many overlaps, and the chapter has appropriate cross-references to other chapters in the report. [Nathan Gillett, Canada]	Thank you.
56936					Figure CC box 5.1 fig 2: the grey colors in the map could be misleading: the reader expects some data behind the different shades. Unless it is the case, it is suggested to just outline the regional limits with black or one single color [WGI TSU, France]	Accepted. We have shown region division as lines and emissions as map
36202					The chapter has lots of thorough literature review on the main topics covered, and comprehensive discussion of relevant work, including much discussion of biogeophysical processes. Some sections however, are missing assessment conclusions, so it is not always clear what the main assessments arising from each section are. The main point of the IPCC assessment is of course to inform policymakers, mainly via the messages in the SPM which in turn are drawn from the ESs of each chapter. The material in the sections should therefore support and underlie assessment conclusions taken forward the ES - for some of the sections this link was not clear in the present draft. I would recommend that for the SOD, the authors aim to finish each section with a short statement of the main assessment conclusion for the section, with calibrated uncertainty language, and with justification for the confidence/likelihood level attached to the statement, based on the evidence assessed in the section. [Nathan Gillett, Canada]	Accepted. We have improve the level of assessment and conclusions in the many sections.
56938					Figure 5.14: This figure would benefit from a general title right on top of the panel, if any comes to mind (e.g. N2O measurements in the atmosphere, or something in that line) // labelling the panels with a short annotation would help navigating the figure without refering directly to the caption (e.g. (a) concentration, (b) growth rate...) [WGI TSU, France]	accepted - figure has been updated to be consist with comparable figures in the chapter

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
36204					In terms of how it fits into the overall WGI assessment, perhaps the most important role of this chapter is an assessment of biogeochemical feedbacks on climate change - how will carbon-climate and other biogeochemical feedbacks amplify or dampen future climate change driven by human emissions? New results since AR5 presented in this chapter suggest that these feedbacks can be closely constrained by observations - Figure 5.28 shows that the CO2 concentration in 2100 under RCP8.5 emissions can be constrained to a range much narrower than the spread across CMIP5 models using an emergent constraint. But this figure is only very briefly mentioned in the text (pg 55, ln 38-39), and there is no overall assessment of this result, nor is it mentioned in the ES. This result needs much more discussion and real assessment in the chapter. If the authors assess that it is robust, then it should be highlighted in the ES and is a key result of the chapter - carbon cycle uncertainties can be constrained to a much narrower range than in the AR5. If the authors don't assess that it is robust, then the authors should report the result, but explain why it is not assessed to be robust. The ES statement on pg 8 ln 18-20 about unrepresented feedbacks suggests that the authors think this isn't robust - if so, this should also come out in the discussion of Fig 5.28. Also - are uncertainties associated with LUC emissions adequately accounted for in this analysis? [Nathan Gillett, Canada]	Noted. Section 5.4 has been expanded, and RCP8.5 is no longer presented without other RCPs for comparison.
56940					Figure 5.16: it would improve readability to bring the legend right below the main title (which requires probably to increase the height of the sky) // same as fig 11 and 13: natural fluxes in green and same legend for stocks (dotted circle, white circle) [WGI TSU, France]	Accepted. Changed.
36206					Several places in the chapter the authors just report results based on RCP 8.5. See, for example, pg 38 ln 51-52 'The degree of acidification is similar to the 0.4 pH unit decrease predicted for the end of the twenty-first century (RCP8.5) due to anthropogenic carbon emissions'. The implicit implication is that this is the most plausible future emissions scenario. But RCP 8.5 is a very fossil fuel intensive emissions scenario. As the authors develop the SOD with ScenarioMIP simulations available, they should aim to report results from a range of scenarios, not just the highest emissions scenario. Or where only one scenario has been studied in the literature, the authors should at least comment on to what extent similar conclusions would hold for other scenarios. [Nathan Gillett, Canada]	Accepted. We no longer show RCP8.5 alone.
56942					Figure 5.18: figure could be more independent from caption: spelling out acronyms (GWP and WTP) would improve readability of figure // the grey colors in the map could be misleading: the reader expects some data behind the different shades and will expect north section of Africa to be linked to temp. south america. Unless it is what is expected from the reader, it is suggested to just outline the regional limits with black. [WGI TSU, France]	Taken in to account. Region divisions are shown as lines
56944					Figure 5.19: ideally the legend for the two colors should be indicated in the figure as well (e.g. right above the map) [WGI TSU, France]	Accepted - figure revised

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
56946					Figure 5.20: label ordered are wrong (c/b) It would be better to use (a) and (b) to reference back to the plots in the caption. // rainbow color bar should be avoided. Guidance on aragonite color palette will be communicated by WGI TSU. // the upper map has regions in dark grey and others in white. does that represent data? if not, it should be uniformed [WGI TSU, France]	Not applicable - the figure removed.
56948					Figure 5.21: the light green shade and light blue shade should be labelled [WGI TSU, France]	Noted.
56950					Figure 5.25: "observations" in (a)'s legend should be moved to (b) and the dashed red and black lines pointing to the regression line should be explained as well [WGI TSU, France]	Accepted. Figure caption extended to include: "Vertical black dashed line shows the best estimate of global mean atmospheric CO2 concentration in 2014. Horizontal dashed read line shows best estimate of global mean atmospheric CO2 concentration by 2060 under the RCP8.5 emissions scenario, using this emergent constraint (see Section 5.4.6)"
56952					Figure 5.26: the RCP colors should be the ones mentioned in the Visual Style Guide p.8 [WGI TSU, France]	Noted.
56954					Figure 5.27: the RCP colors should be the ones mentioned in the Visual Style Guide p.8 [WGI TSU, France]	Noted.
56956					Figure 5.29: This figure would benefit from a general title right on top of the panel, if any comes to mind (e.g. magnitude of biogeochemical feedbacks on climate, or something in that line) // a legend could be included in the figure (e.g. top right corner) stating that blue=negative feedback, red=positive feedback, hatching = ..., symbols =single estimates [WGI TSU, France]	Accepted - the figure is clarified
56958					Figure 5.30: what do the different colors indicate? [WGI TSU, France]	Not applicable – the figure is excluded
56960					Figure 5.31: this figure needs: axis numbers, axis titles/units, legend [WGI TSU, France]	Accepted. Original figure had labels etc. but was not correctly reproduced in pdf.
56962					Figure 5.33: what are the different color representing? If there is a meaning, it should be shown in the figure with a legend. [WGI TSU, France]	Agreed. Figure updated.
56964					Figure 5.34: This figure would benefit from a general title right on top of the panel, if any comes to mind // CDR should be spelled out in caption [WGI TSU, France]	Agreed. Figure updated.
56966					FAQ 5.1 figure: the figure as presented here is not suitable for a lay audience: line plots are fine but axis units etc are too complicated. Quantitative information should be mixed with qualitative information: The graphs should be combined with a schematic showing ways how nature remove carbon from atmosphere [WGI TSU, France]	accepted - new figure is provided for SOD

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
36490					As defined in this chapter (pg 58, line 1), 'tipping points' correspond to abrupt changes in the biosphere/climate system. Figure 5.30 shows projections of variables which have been suggested to exhibit such 'tipping points' in projections from ESMs, some including dynamic vegetation. None of the variables shown exhibit abrupt changes - rather all variables shown exhibit smooth variations. This point does not appear to be taken up in the assessment in 5.4.8.5, or elsewhere in the chapter. [Nathan Gillett, Canada]	Noted