Comment ID	From Page	From Line	To Page	To Line	Comment	Response
11163	0	0	0	0	the format of ordinal numeral should be consistent. For example, some of '21st' are superscript while others not. [Teng Li, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Made consistent.
86713	0	0	0	0	We have not found much information about peatlands and mire, and their role in the climate system, especially for the carbon budget. Please consider adding more information regarding this in Chapter 5. [Oyvind Christophersen, Norway]	Accepted. Added in emissions section.
86715	0	0	0	0	The role of forests, and how they are described and used in the models is not easy to understand. We have struggled to find that kind of information, and also how different types of forests e.g. tropical, boral and temperate act differently in the climate system. Please consider to add more information about the specificities of the different forests, and an explanation about how these important ecosystems are modelled in ESMs. We would expect to find such information in Chapter 5. [Oyvind Christophersen, Norway]	Partially accepted. We have produce new regions in the global maps which show clearly the role of boreal, versus temperature versus tropical forest. More specifics will be provided in WGIII.
17381	0		211		In this chapter forest and forestry, as well as mitigation is well documented. AFOLU report should be used with more details. [Mostafa Jafari, Iran]	Rejected. The suggested assessment is core to WGIII, not much for WGI.
39963	0				Assessment conclusions should be provided in all sub/sections 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 that must include IPCC confidence language. Some sections currently read more as a review of the literature rather than an assessment of our current understanding of the literature. [TSU WGI, France]	Accepted. Structure adopted in many subsections.
40481	0				It's often not clear what the previous findings (AR5, SRs) were and therefore what the big improvements of this report are and when previous reports are referred to, it is more often AR5 than SRCCL, which is problematic, given that the latter is the most recent one. [TSU WGI, France]	Accepted. Structure improved.
21795	0				Chapter has a tendancy to marry deterministic percentages with probabilistic (uncertain) underlying quantitative numbers. The percentages cannot be more precise than the underlying numbers. Thus all percentages should also be quoted as ranges throughout. I started by calling out individual occurences but the issue appears widespread so I am raising as a chapter-wide comment. Unless the percentage really is deterministic no percentage should be reported without a commensurate very likely range being attached to it. [Peter Thorne, Ireland]	Accepted. % ranges more widely used.
21799	0				There are several places where the term confidence is used but not in a manner consistent with the language guidance on uncertainty. These occurences should be replaced with alternative wording. [Peter Thorne, Ireland]	Accepted. Language improved.
84779	0				The chapter has substantially improved compared to previous versions. [Martin Heimann, Germany]	Thank you.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
84781	0				Who reads this chapter? A carbon cycle scientist might use it to find some latest references and perhaps new assessments of some particular carbon cycle science aspects. For this the chapter is fine. On the other hand the IPCC report should provide a clear description of the fundamental processes underlying the observed increase of atmospheric greenhouse gases. This should be readable and understandable to non-experts, policy makers on a "Scientific American" level. For this readership, the chapter is difficult. In previous assessment reports we had at least in the introduction an overview of the key processes needed to understand the evolution of atmospheric greenhouse gases given a prescribed emission trajectory. It is not obvious to even moderately informed laypersons, that this question essentially boils down to quantifying the redistribution of the emitted carbon between atmosphere, ocean and land. The carbon cycle science relevant here is simply to understand these redistribution processes and their drivers on time scales of up to a few hundred years. This should be described in simple terms in the introduction at the beginning. The paleo perspective in the introduction is nice, but what does it tell us? It tells us that natural Earth System processes of the slow components (sedimentation, weathering, volcanism) are slow and can not be responsible for the fast changes that we see today. The separation of the fast and slow carbon cycle must be mentioned in the introduction. [Martin Heimann, Germany]	Accepted. We have included now text on the slow and fast carbon cycle and referred to previous assessment to gain insights on the fundamentals on the carbon cycle which remain equally valid today, and therefore no need to repeat it here.
84783	0				In the context of the IPCC process it is also very important to stress that the scientific understanding of the fundamental essentials of the fast carbon cycle dynamics, i.e. the redistribution of carbon between atmosphere ocean and land is not new, but that this science has not really changed since at least the 1970's (or even earlier). E.g. it is long known that stabilisation or even reducing atmospheric CO2 requires drastic emission reductions, which is a key message of this chapter. True, many modern and very recent studies with fancy models and new data streams and sophisticated methods allow us to quantify the redistribution quantitatively much more detailed than previously. But the fundamentals remain the same. It would be worthwhile to stress this; it provides an argument that the key findings of AR6 carbon cycle science are robust and not just obtained during the last years (as an uninformed reader might think if he looks at the cited literature). [Martin Heimann, Germany]	Accepted. See below.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
84785	0				I am missing in the chapter addressing the issue of land management. Right now humans occupy and manage more than 75% of the ice-free land surfaces (see e.g. Ellis et al,. 2010). This clearly must have an impact on the terrestrial carbon budget and its dynamics. Reading the chapter one gets the impression that the terrestrial carbon sink in the 21st century is controlled primarily by atmospheric CO2 and by the climate (beta, gamma). I would think that human management of land biomes through land use and land management will be as important for the net land carbon budget in this century. Present models use some scenario of land use change for agriculture, but human impacts will also change the dynamics of the biomes (e.g. carbon transit times) and also the vulnerability of crops to climate (agricultural practices, genetic engineering, irrigation etc.). These processes are not reflected in the beta and gamma formalism. They may be included in future land use emission scenarios, but these are at present very rudimentary and are not discussed in the chapter. E.g. Tilman et al., 2011 show convincingly how agricultural practices and N-fertilisation determine the future land use needs in order to feed the global human population. Obviously, whatever scenario the world will follow will have large impacts on the net land carbon budget. In the chapter the issue of land management is mentioned here and there but it requires a more prominent mentioning, both in the overview and, since global assessments/scenarios are currently still scarce, it requires highlighting in the knowledge gap section. [Martin Heimann, Germany]	Accepted. Made it clear in the introduction.
21815	0				In general this chapter is very well written. However, there are then segments that are very hard to parse and where the language would benefit from considerable improvement. More stringent efforts to attain homogeneity of the presentational style across all sections would help make the chapter as a whole much more accessible. I have called out some egregrious examples explicitly but this is also a general issue hence the overarching comment. [Peter Thorne, Ireland]	Thank you, writing style improved.
21827	0				Overall I really liked section 5.2 for the way it went through and considered using the same structure each of the big three WMGHGs followed by a summary. My feeling is that this made the piece accessible. [Peter Thorne, Ireland]	Thank you.
41027	0				The way ch5 deals with knowledge gaps is not consistent: There is one subsection about this (5.4.8.6) as well as an entire section (5.7) [TSU WGI, France]	Accepted. Consolidated in new 5.7.
40005	0				Ch5 presents permafrost as a potential large source of GHG with high uncertainty but ch9 doesn't make it seem as large a 'risk'. Could you ensure consistent treatment across chapters? [TSU WGI, France]	Accepted. Consistent.
21837	0				The general paucity of assessment summaries per section reduces the traceability of the underlying text to the ES. It would be nice if substantive assessment findings summaries could be added at the end of the sections to help ensure better traceability of the chapter key findings to the underlying assessment. Such additions may also help to sharpen the text within the sections so it more directly supports to final assessment findings made by the section. [Peter Thorne, Ireland]	Accepted. More concluding paragraphs have been added.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
41037	0				There are many parts of ch5 where the flow is hard to follow. It would greatly help to introduce the structure better, to guide the reader through the chapters and the sections [TSU WGI, France]	Accepted. Intro paragraphs included.
67415	0				I did not review the FOD, mainly because I was too busy with my own group's CMIP6 contributions. I find the current text very good for the most part. [James Christian, Canada]	Thank you.
41047	0				There are some issues left with the executive summary. Some important conclusions seem to be missing (e.g. the fact nutrient availability will limit CO2 fertilisation (Sec 5.4.1) and the conclusion about deoxygenation) and in addition to that there is redundancy. [TSU WGI, France]	Accepted. Fixed.
67417	0				I am suggesting some changes to the wording of several items in the Executive Summary. I am well aware that scientific reviewers are asked not to suggest changes that are purely editorial, and I hope that the authors will not dismiss my comments as such. What I am suggesting is substantive improvement in the readability of the text. Although it does not greatly affect the content, it is more than purely editorial, and simply can not be done by copy-editors. Consider, for, example, Barkmeyer et al 2015 (10.1038/nclimate2824). This paper is concerned primarily with the Summaries for Policymakers and the public communication of Assessment Reports, so perhaps it is not strictly relevant. But even within the main body of the report, and particularly in a chapter's Executive Summary, we should strive to make the text as clear as possible. The review format does not really lend itself to this kind of suggestion, as it is not possible to track changes in the existing text. Nonetheless I hope that the authors will give my suggestions careful consideration (there aren't many, but in a few places the current text really needs work). [James Christian, Canada]	Thank you . Accepted. Interesting papers. We have improved ES.
40537	0				Most of the sections lack a sort of conclusion/wrap up that summarises the key points/findings of the sections. [TSU WGI, France]	Accepted. Many sections now with concluding paragraphs.
67419	0				In several cases the same reference is repeated twice as Author YYYYa and YYYYb (e.g., Tokarska and Gillett 2018, Taylor et al 2015). I did not go through the list systematically; there could be more. [James Christian, Canada]	Accepted. Revised and fixed.
78455	0				Whole chapter – the chapter claims to cover CO2, CH4 and N2O, but the latter are only discussed in the context of past/present and lack any assessment of future changes. The future projections covers entirely CO2. There is literature on future feedbacks on CH4 emissions and concentrations – including new models with emissions-driven CH4 in ESMs (Folberth et al – submitted – copy will be provided). But other studies with simple models also include projections. Folberth et al demonstrate in a complex ESM changes in CH4 cycling are irreversible on century timescale even if CH4 concentration is reduced to near pre-industrial [Chris Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Included new references and text.
78457	0				Whole chapter – wherever possible need to use results updated to SSPs – in many places (e.g. ocean acidification section 5.3.3.3.3) you draw on literature that discusses RCPs, but can be updated with CMIP6 results. [Chris Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
21883	0				With some additional attention the vast majority of figures could be made a lot more intuitive. There is great value in use of IPCC figures for outreach and education. But this requires figures to be self describing. A general effort to increase the ability of figures to stand without explanation would be hugely beneficial. Simple things like titles for figures as a whole and panels, axis labels etc. can make figures much more accessible. I have only called out in specific comments a subset of the more obvious issues but this comment applies to most figures [Peter Thorne, Ireland]	Accepted. Figures improved.
3453	0				I don't understand why the silicon and phosphorus cycles are excluded in this chapter. Cycling of these elements also highly influences carbon cycling and sequestration of atmospheric CO2. [Georgi Laukert, Germany]	Thanks. The scope of the chapter was CO2, CH4 and N2O. We mentioned phosphorus in the context of the land CO2 sink.
88965	0				The word "biological carbon pump" is used through the chaper, The word is usually used as "biological pump". I think it is not necessary to confine the process to carbon. This process is important also for transporting nutrients into deep waters. [AKIHIKO MURATA, Japan]	Accepted. Changed.
40327	0				General comment on units in chapter 5. It is confusing to have most (but not all) quantities expressed in Pg of Carbon and not Gt of carbon as it will be the case in the SPM and the TS [TSU WGI, France]	Accepted. Changed.
41103	0				To ensure consistency across sections, should sections like the one on sea ice (9.3) follow the same structure as the following ones, splitting specifically on recent observation, model evaluation, future projection? [TSU WGI, France]	misplaced comment. We canote identify what it refers to.
103069	0				language could still be streamlined: the combination of well-defined IPCC value statements (high confidence, low confidence; is also unequivocal one of these defined words) with value loden statements does not always work out. A statement like "do not provide strong support" combined with a qualifier "low confidence" sounds odd. Does that mean, authors are not sure whether there is strong or weak support? [Philippe Tulkens, Belgium]	Accepted. Language streamlined.
111023	0				mix of various global carbon budgets, maybe consider to update numbers (were possible) to 2019? [Julia Nabel, Germany]	Accepted. Updated
40637	0				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: https://wg1.ipcc.ch/SR/documents/ar5_uncertainty-guidance-note.pdf or the presentation from the pre-LAM activities [TSU WGI, France]	Accepted. Improved traceability throughout the chapter.
32205	0				This Chapter is overall well written. In the sections addressing GHG metrics (e.g. section 5.2.4, 7.6), we recommand to recall that the GWP100 metric is currently in use under the Kyoto Protocol and the Paris Agreement. Alongside, a note should be mentioning that under the Paris Agreement, Parties may in addition report supplemental information using other metrics. [Eric Brun, France]	Accepted. And made clear in the section.
40923	0				The fact that negative emissions are not equal and opposite to positive emissions and have a lot of side effects, which is to me an important conclusion of chapter 5, seems to be absent from the SPM. [TSU WGI, France]	Accepted. The effect of negative emissions is now considered in the SPM (D1.5), the potential side effects in SPM D1.4

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110835	0				AR5 stated the shortcomings of models without N in the model evaluation section in 6.3. In AR6, several models contain aspects of the N cycle. It would be helpful to see a diagram or discussion of which elements of the N cycle were included in the various models. It would also be helpful to see mentioned how the inclusion of these aspects of the N cycle affected model results, perhaps with a table like how N limitation and model estimates for CO2 land sinks were shown in AR5 table 6.7 'Estimates of the land CO2 sink from process-based terrestrial ecosystem models driven by rising CO2 and by changes in climate' or a reference to where this is discussed if it is in a later chapter. [Claudia Steadman, United Kingdom (of Great Britain and Northern Ireland)]	Partially accepted. We don't have any more room to add new tables but we provide links to relevant literature where it show which models have and have not N limitations.
110837	0				The topic of N2O is covered well in this chapter. It would be useful to see more information about the nitrogen cycle as a whole, as it is an important biogeochemical cycle with many interactions with the carbon cycle. The N cycle was given substantially more coverage in AR5. For example, it would be useful to see an updated version of AR5 WG1 Box 6.2 Nitrogen Cycle and Climate-Carbon Cycle Feedbacks, and an updated schematic of the global nitrogen cycle (see AR5 WG1 Figure 6.4). [Claudia Steadman, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. The approved scope of the paper only covered N2O in the N cycle.
87711	1	1	1	1	I mean to say, though, that I find the chapter quite nicely overall makes a point to be readable for a more general audience (at least from my perspective) [Ivy Frenger, Germany]	Thank you.
42821	1	1	5	13	The structure needs improvement and a clearer diviision.This is to avoid redundancy and repeats, and will make it easier to find the information. Now same information is given at several places for example anthropogenic CO2 uptake and ocean acidification. All parts would be impprved by a better structure. [Melissa Chierici, Norway]	Accepted. Ocean acidification discussion all consolidated in section 5.3.
42823	1	1	5	13	Require more information from several regions, now it is biased against tropical, Pacific, Southern Ocean and based mainly on model results. Include more regional information and more references mirroring the actual knowledge for example on the ocean CO2 uptake and how it is changing in different regions. Same references repeated and mainly models, include more observational evidence. [Melissa Chierici, Norway]	Accepted. Ocean section more focus on data products as part of the flux assessment. Limited space to address all regions.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
72889	1	1	111	51	My major concern is the citation of material 'submitted'. Normally, journals will not accept such citations, although those accepted may be cited as such ('accepted' or 'in press'). Presumably there will be a check on whether or not these papers have been accepted. I have not flagged these instances in the text. There are several other persistent editorial issues. 'Century' should be capitalised when it is used as a proper noun (e.g. '20th Century'). This is done inconsistently: I have not flagged all the instances of this in the text. Throughout the Chapter, by and large, British spellings are used. The exception to this is the use of 'paleo' as a single word or a suffix. This is incongruous in the context of other spellings, and ideally should be changed to 'palaeo'. Again, I have not flagged these instances, but a global search/replace could be applied. Please check table formats. There are several instances where tables have ben split across pages. Ideally, this should be avoided, but if essential please ensure that the column headings are imported into the second page of the table as well so we can more easily see what is being presented. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. All submitted papers. either accepted or removed in the final version. Other editorial issues addressed.
32013	1	1	163	1	GENERAL COMMENT ON CHAPTER 5 WRITTEN TEXT Overall this is a thorough and careful review in so far as methane is concerned, a great deal of hard work that fairly covers what we know and what we don't know. My most significant concerns are with the paragraph on page 42, and also the comment on page 102, which both seem to weaken the more detailed discussion earlier. Nevertheless, this is an excellent broad review, well done and comprehensive. [Euan G. Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Thank you. Gaps section very much modified as per new instructions. Comment. Pg 42. Provider further links to other chapters to ensure information is found.
77245	1	1	211	1	This is a very important chapter which provides key information for policy. It is largely accessible and clear. Some additional framing and information would be useful and could link it to other chapters. Also some material may be better addressed in other chapters e.g. on common metrics. [Emer Griffin, Ireland]	Thank you. Added links to other chapters.
44653	1	1	211	7	Please make sure throughout the entire chapter that it is clearly stated whether a statement refers to the contemporary/total, anthropogenic, or anthropogenically perturbed (S_OCEAN in GCB) ocean carbon sink.Ideally, decide on one definition to discuss throughout the chapter. [Judith Hauck, Germany]	Accepted. Revised and implemented throughout the chapter.
77271	1	1	211	21	The Special Report on Land provided a table on emissions of key GHGs from their sources. This included the percentage of methane from fossil and biogenic sources. Can an update of this table along with ERF values be included in this chapter? Such a table could be included in the SPM [Emer Griffin, Ireland]	No. ERF belongs to another chapter in the way WGI is structured.
77273	1	1	211	21	Has there been a revision to the fossil/biogenic methane ratio provided in SR land 2019 based on Robert W. Howarth, Biogeosciences, 16, 3033–3046, 2019 ? [Emer Griffin, Ireland]	Yes, methane section and cross chapter box discuss it in great detail.
33029	1	1	211	70	in this chapter forest and forestry, as well as, mitigation is well documented .AFOUL report should be used with more details. [Sahar Tajbakhsh Mosalman, Iran]	WGIII will cover topic in great detail
32699	1	1	211	70	in this chapter forest and forestry, as well as, mitigation is well documented .AFOUL report should be used with more details. [sadegh zeyaeyan, Iran]	WGIII will cover topic in great detail

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69785	1		167		Overall - Very well done. I look forward to the final chapter! Thank you. Very well done CDR assessment too. [Gyami Shrestha, United States of America]	Thank you.
68865	1				Paleoclimate information has been successfully distributed across the WG1 report, as envisaged by the scoping documents. The Paleo BOG has now developed key messages to consolidate and convey the most policy-relevant paleoclimate content, and to advance it to the summary documents (TS & SPM). The Paleo BOG looks to CH5 to include critical information needed to address its key messages from paleoclimate and to include the outcome of the assessment in its Executive Summary, namely: Paleo key message I. (model veracity) How well do Earth-system models – including carbon-cycle models -- with paleoclimate forcings simulate large-scale Earth system changes? Paleo key message II. (multi-centennial climate) What are the long-term effects of sustained warming across the Earth system, including the carbon cycle? To address these two key messages, please provide an assessment carbon-cycle models based on comparisons with proxy evidence for paleo reference periods, and an assessment of how long-term reservoirs of carbon (ocean, soils, permafrost) have transferred carbon during past global changes, including the take-up and release of greenhouse gases. [Darrell Kaufman, United States of America]	Rejected. This discussion, while certainly valid is beyond the scope of the present discussion and unfortunately has been removed for the sake of brievity.
52285	2	11	2	11	The structure could be better; now there are several sections about ocean CO2, ocean acidification and carbon uptake that are similar with the same references presented several times. [Agneta Fransson, Norway]	Accepted. Reduced reference overlap
52287	2	11	2	11	suggestion: merge into one or two sub-sections, such as global and regional variability. much repetition and same references in many sections. [Agneta Fransson, Norway]	Accepted. Merged.
52315	2	11	3	37	Comments on structure: Ocean acidification could be described once. Now, it is presented several times, ocean-atmosphere CO2 fluxes and CO2 sink. Drivers for CO2 fluxes could be described in one section. [Agneta Fransson, Norway]	Rejected - OA is treated separately to fluxes
52317	2	11	3	37	Suggest that the structure could be more consistent between areas, now there are presented most results from certain oceans (such as tropical ocean, Pacific Ocean), which are repeatedly shown, while polar regions, such as the Arctic Ocean are not well represented, although there are existing data and models. I have suggested a few references and new information, see other comments. [Agneta Fransson, Norway]	Noted - the regional balance has been addressed subjected to available published work
52289	2	16	2	18	In each section, divide into regions tropical, temporate, sub-polar and polar, to be consistent and present all regions, temporal/spatial/coastal etc. [Agneta Fransson, Norway]	Noted - have integrated the regional spatial and temporal more closely. The purpose of the regional assessment is to point to regions that influence the global mean rather than characterize the regions

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52291	3	7	3	7	"Ocean interior" means "Deep ocean"? Perhaps change this [Agneta Fransson, Norway]	Not accepted - the expression "ocean interior" relates to the water column below the mixed layer and has been used elsewhere in this chapter and previously in the SROCC. This may appear in the AR6 Glossary, as a definition.
52293	3	9	3	9	perhaps move de-oxygenation section to after ocean acidification sections for consistency, easier to follw [Agneta Fransson, Norway]	Taken into account - The text has been rewritten, so now coastal drivers of ocean acidification is followed by de-oxygenation, and the same applies to the Spatial characteristics section.
52295	3	13	3	13	Drivers could be added for all regions [Agneta Fransson, Norway]	Taken into account - The text has been rewritten so now the text on drivers for acidification and de-oxygenation is more comprehensive. However, as this is an assessment and not a review, the text should point to where trends are detectable from observation/confirmed by models. The word count allowed should also be respected.
58535	3	24	3	24	The range for the LGM (21-19 ka) seems a little on the recent side, especially since PMIP/CMIP LGM definition is 21 ka, and maximum ice sheet extent is likely before 21 ka. There is of course no commonly-accepted age definition of the LGM, but the PMIP experimental value (21ka) would seem the most consistent. For consistency, I suggest widening the stated range to 23-19 ka. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. The LGM has been defined as the interval spanning 19-26.5 kyr in AR6. Text modified accordingly.
69771	3				excellent note on sink rate [Gyami Shrestha, United States of America]	Thank you
37753	4	5	4	23	I agree that TCRE is a recent topic on carbon cycle, but I feel that the SOD spent too much pages for this issue in comparison with other important topics such as land-use change. [Akihiko Ito, Japan]	Taken into account - The assignment of space within the Chapter 5 assessment is based on the approved outline for the IPCC AR6 sixth assessment report, in which TCRE and remaining carbon budgets feature explicitly
37755	4	24	5	3	I agree that geoenginnering (CDR and SRM) is a recent topic on carbon cycle, but I feel that the SOD spent too much pages for this issue in comparison with other important topics such as land-use change. [Akihiko Ito, Japan]	Taken into account. The chapter follows the approved WGI report outline, which asks for a detailed assessment of the carbon-cycle effects of CDR, SRM. We agree that land-use change is an important topic but limited our assessment to LUC emissions due to space constraints.
797	5	32			Add citation to SROCC. These items were analysed in this report as well [Baruch Rinkevich, Israel]	Not applicable - this comment is wrongly assigned to the table of contents. The chapter has been revised to refer to SROCC where relevant.
23679	5	41	5	41	TgN to Tg N to match the style of the other GHGs values [Massimo Lupascu, Singapore]	Accepted. Change made.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
70427	5	54			I wouldn't characterise the non-GHG impact of land use change as a 'major anthropogenic driver' of climate. [Gillett Nathan, Canada]	Page misalignment. Corrected in Emissions section.
109677	6	1	9	38	There are many references in the Executive Summary (beginning with the first paragraph) to "direct emission of GHGs from human activities." What exactly constitutes direct vs. indirect anthropogenically enhanced GHG emissions is not stated. Do "direct emissions" include net GHG increases from clearing the tropical rainforests, or industrial cattle production, for example? This may seem obvious to the authors, but if I saw ambiguity in the way this is phrased, it can be pretty much guaranteed others will too. [Sean Fleming, United States of America]	Accepted. Removed.
114851	6	1	9	38	A very clear and informative Executive Summary bringing forward many issues of cross-cutting relevance for the entire Report. Also, it articulates messages important for Global Stocktake (2023) under the Paris Agreement. [Roxana Bojariu, Romania]	Thank you.
86717	6	1	9	38	Policymakers are significantly more acustomed with GtCO2 and GtCO2eq than PgC, TgCH4 and TgN. Please try, as far as possible, to use CO2 and CO2eq in at least the executive summary. And when doing so use GWP(100) since this is what is used under the convention. [Oyvind Christophersen, Norway]	partially accepted. We have added the Gt of CO2 as opposed to PgC only, however, the use of Tg is important because smaller unit than Pg, but we add Mt.
77247	6	1	9	38	The messages are clear but some additional information could further inform policy, including how to frame removals in the context of the balance text in the Paris Agreement. [Emer Griffin, Ireland]	Rejected. This information is not assessed in detail in the chapter.
17871	6	1	9	38	Can you provide more information on factors and processes that affect future carbon sinks? Right now, the ES only discusses model uncertainty as a whole and emissions scenario uncertainty. What would lead to more or less uptake? Are nutrient limits important? Is fire or other disturbance a big driver of uncertainty? [Katherine Calvin, United States of America]	Accepted. Text added.
21761	6	3	6	4	This seems outside of the chapter scope and is already covered between chapters 1-3. The ES should rather concentrate upon what is in-scope for the chapter. So I would drop this opening sentence. [Peter Thorne, Ireland]	Accepted. removed.
127637	6	3	6	6	Delete the first two sentences which cover already known facts: "Increasing accumulation of greenhouse gases, … GHGs from human activities. However,.." [Trigg Talley, United States of America]	Accepted. Removed.
77249	6	3	6	12	Some text linking the change in radiative forcing to the earth energy budget could act to link this material to other key chapters/ I.e create a narrative around impacts of the human disruption (enhancement) of the natural carbon cycle as the key driver of the changes to the Earth's energy balance. [Emer Griffin, Ireland]	No longer valid. Sentence removed as per above comments.
114675	6	3	6	12	useful intro [Jan Fuglestvedt, Norway]	Thanks.
109329	6	6	6	7	"…determined by the balance of human GHG emissions and biogeochemical source-sink dynamics." Would not "combination" or "interaction" be better than "balance" here? Alternatively, "by both human GHG emissions and biogeochem source-sink dynamics"? [Paul Edwards, United States of America]	Accepted. Changed.
103071	6	7			"human activities triggering GHG release" would be more precise [Philippe Tulkens, Belgium]	Accepted - change made.
116387	6	10	6	12	Rather than "acceleration, slowdown or abrupt change", what about : which can affect future rates of… [Valerie Masson-Delmotte, France]	Accepted. Change made.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
21763	6	11	6	11	I assume the budgets are all to limit warming / keep warming below some set thresholds? In which case this sentence should be rewritten accordingly for clarity. [Peter Thorne, Ireland]	Accepted. Made clearer..
9285	6	11	6	11	Suggest replacing "The chapter" with "This chapter [Christine Weldrick, Australia]	Accepted - change was made.
81559	6	11	6	11	suggest to change "the remaining carbon budget forfor mitigating global warming" to "the remaining carbon budget for limiting the increase in surface air temperature" to be more accurate [Fortunat Joos, Switzerland]	Accepted, rephrased.
127639	6	11	6	11	"the remaining carbon budget for mitigating global warming". The "remaining carbon budget" is not made clear in the Executive Summary. [Trigg Talley, United States of America]	Accepted, rephrased.
86719	6	11	6	11	Please include an explanation of what the carbon budget refers to. [Oyvind Christophersen, Norway]	Accepted, rephrased.
18131	6	11	6	11	The phrase 'assesses the remaining carbon budget for mitigating global warming' may be unclear to readers who haven't read the explanation of what the 'remaining' carbon budget is later in the document. I would provide some further explanation here. [Chelsey Baker, United Kingdom (of Great Britain and Northern Ireland)]	Accepted, rephrased.
64739	6	11	6	11	In the definiton of « carbon budget » in the Glossary, the expression « global surface temperature » is unclear since there is two distinct definition of this temperature in the report with the GMST and GSAT (well defined in the Glossary). It should be precised that this is the GSAT that is used by convention in this report to calculate the carbon budget as stated in the SPM (Box SPM.1), the TS (Box TS.1) and Chapter 2 (cross-chaper Box 2.3). [Serge PLANTON, France]	Partially rejected. Full explanation in section 5.5. Rephrased for clarity.
5653	6	11	6	12	Please check: "the remaining carbon budget for mitigating" does not make sense. Mitigation is a reduction of emissions or a removal, whereas 'the budget' is a measure of what is still available despite mitigation. [Joachim Rock, Germany]	Accepted. Rephrased.
77251	6	11	6	12	Consider other wording for reference to the carbon budget. Is it the carbon budget for Paris Agreement Temperature goals? [Emer Griffin, Ireland]	Accepted, rephrased.
77253	6	11	6	12	The material on negative emissions could be increased as this is important information for policy. [Emer Griffin, Ireland]	Accepted. It has a paragraph summary later on, as other major conclusions of the chapter.
77255	6	11	6	12	The material on negative emissions should not be linked in one sentence to SRM. [Emer Griffin, Ireland]	Accepted. It has a paragraph summary later on, as other major conclusions of the chapter.
127643	6	14	8	12	[PROGRESS] There is almost nothing in the Executive Summary about improvement in representation of ocean and land biogeochemical processes since AR5. There has been considerable improvement in the process understanding of both ocean biogeochemical processes through the intercomparison of global models that occurred largely after the AR5 report including the role of changes to the various carbon pumps, the importance of the Southern Ocean as a carbon sink, importance of river and coastal ocean processes in completing the carbon budget, and time scales of emergence of biogeochemical change, just to name a few. [Trigg Talley, United States of America]	Rejected. The Exe. Summ. Largely focuses on new results from AR5 and model improvement are well address in the corresponding sections.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
127641	6	14		14	"the carbon and biogeochemical cycles". Many authors consider CH4 a subcycle of the Carbon Cycle. It's not clear from the introduction what biogeochemical cycles this chapter is about -- carbon and nitrogen? It should be stated up front. [Trigg Talley, United States of America]	Accepted, added
103073	6	16	6	17	Please report measurement data for 2020: 2020 is a "marker year" in comparison to models, and thus much more useful than 2018 data. 2020 measuremtns are already available (at least Jan 2020) [Philippe Tulkens, Belgium]	Rejected. Unfortunately, global atmospheric concentrations won't be available for the entire 2020 year by the time this chapter stops making changes.
81017	6	16	6	19	Perhaps the 2018 data for GHG emissions can be updated prior to publication with the latest information available. [Jeffrey Philip OBBARD, Singapore]	Accepted. Updated.
15499	6	16	6	19	Re: 407 ppm of CO2 and 1859 ppb for CH4. According to the WMO Greenhouse Gas Bulletin 2019 (https://library.wmo.int/doc_num.php?explnum_id=10100), the atmospheric carbon dioxide concentration in 2018 was 407.8 ppm. It would become 408 ppm if rounding is considered. Also, according to the Bulletin, the atmospheric methane concentration in 2018 was 1869 ppb. Please check and revise as appropriate. [SAI MING LEE, China]	Accepted.
16503	6	16	6	24	This is very simlar to the chapter 2 third ES point. It would be good for ch 2 and 5 to confer and decide what goes in which chapter. [William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Rejected, as long s it is consistent, the info is important for the overall narrative of the Exe. Summ.
127645	6	16	6	24	Suggest adding "at least" to the time frames, and perhaps also providing (at least for CO2) the opposite framing, which would be "there is high confidence that current CO2 concentrations were exceeded XX million years ago". [Trigg Talley, United States of America]	Accepted. added.
99027	6	17	6	17	Can this precision really be justified? Three significant figures? [Michael MacCracken, United States of America]	Rejected, numbers based on published date and IPCC reference year.
31843	6	17	6	19	It is now possible to update to 2019 numbers. CH4 annual mean for 2019 was 1867 ppb. Note that this number tends to change slightly as the regression spline settles, so needs to be looked at again, but if the draft's edit is in June/July 2020, it should have settled. Check with Ed Dlugokenkcy. Same comments apply to CO2 and N2O. This will also affect the % increases in line 17. [Euan G. Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Accepted-updated.
18645	6	18	6	19	Hope these numbers will be updated with 2019 values. [Govindasamy Bala, India]	accepted - updated
90057	6	19	6	19	This statement uploaded by Ted Schuur on behalf of a graduate class that reviewed the chapter (this was communicated with IPCC TSU in advance): Regarding present-day abundance of N2O, text (p 37, line 52) says "robust evidence, high agreement" not "very high confidence". [Edward Schuur, United States of America]	rejected. We don't understand the comment.
99029	6	19	6	20	I'd suggest making "concentrations" singular. [Michael MacCracken, United States of America]	Accepted - change was made.
41581	6	19	6	24	Since this is one of the paragraphs most readers (and policy makers) will read, I am wondering if "unprecendented" (ll. 19 and 23) could be replaced by "unique" to make it easier to understand [Katharina Meurer, Sweden]	rejected - the use of unprecedented is not a technical word

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
86721	6	20	6	23	Please consider to delete "much" or replace it with "significantly" or something similar. When using "much" it opens up for asking the question - how much and how important is this? Please also consider to insert "ancient" before "history", since this makes it easier for the reader to understand that this is a very long time ago. [Oyvind Christophersen, Norway]	Accepted - replaced
112293	6	21	6	23	This sentence should be reworded or removed because most of the paleorecords for the deep past don't have resolution of 300 years or less. [Jinho Ahn, Republic of Korea]	rejected - there are multi lines of evidence showing that emissions in the past have been higher than present
71657	6	21	6	23	This is based on Zeebe et al 2016 which is a very impressive study but it also noted that large changes occurring over periods of less than 4000 years cannot be excluded. [Martin Manning, New Zealand]	accepted - added text in section 5.1
18647	6	23	6	23	The reason for the rapid rate of CO2 increase 66 million years back may be briefly mentioned here. [Govindasamy Bala, India]	rejected - we appreciate the comment but there is not space to provide much detail in ex. Summ.
21765	6	23	6	24	This final sentence could be seen as editorialising and doesn't really add anything scientifically. I would therefore suggest removal. I think the statement is stronger and more robust without. [Peter Thorne, Ireland]	Accepted - removed
39899	6	26	6	26	“do not provide strong support” seems not IPCC confidence language. [TSU WGI, France]	Accepted - paragraph removed.
45419	6	26	6	27	Having a whole statement, written in bold font, as an introduction for the paragraph, knowing that this statement is associated with a "low confidence", is a questionable choice. I would rather suggest something like: "There is no evidence that global carbon cycle changes have been associated with less than 2 degC global warming episodes in the preindustrial times". [Olivier Sulpis, Netherlands]	Accepted - paragraph removed.
15501	6	26	6	27	The statement with low confidence should not be treated as a headliner. A highlighted headliner with low confidence could mislead the readers. Please consider revision. [SAI MING LEE, China]	Accepted - paragraph removed.
58519	6	26	6	27	It is not crystal clear to me what is being communicated here. I would suggest to the authors that this statement could be rephrased to be less wordy, without changing the meaning. I suggest: "For past global warming of no more than 2°C, paleo records provide weak support for abrupt changes in the carbon cycle (low confidence)," or clearest: "For past global warming of no more than 2°C, paleo records show low-magnitude changes in the carbon cycle (low confidence)." [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - paragraph removed.
18079	6	26	6	27	please double check the result "low confidence" [Zhang Zhihua, China]	Accepted - paragraph removed.
58873	6	26	6	27	It seems odd to say "of no more than 2*C." Instead, "less than 2*C" might be more intuitive here. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - paragraph removed.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
2765	6	26	6	31	I'm uncertain how there can be low confidence about a lack of data/support. If there is some evidence, then this would provide at least some confidence. If there is no evidence from current paleo records, then this can be stated with higher confidence. Does low confidence here mean that there is evidence for the opposing position, but the evidence is unsound, or does low confidence mean that there is no evidence, but evidence might be found in the future...? I think the confidence in this statement should be raised. An alternative approach would be to remove the confidence statement from line 27 and add a confidence statement after "over centuries" on line 29 and another after anthropogenic emissions on line 31. This would make it clear where the uncertainty is. [Stephen Wilkinson, United Arab Emirates]	Accepted - paragraph removed.
34627	6	26	6	32	I don't think this material has a sufficient evidence or confidence base to rise to the level of a key finding. [Russell Vose, United States of America]	Accepted - paragraph removed.
45417	6	26	6	32	Some of the sentences in this paragraph are poorly constructed which makes it somewhat difficult to understand (e.g., "for global warming of no more than 2degC"). In general, it should be clearly stated what times period the term "paleo records" refers to. [Olivier Sulpis, Netherlands]	Accepted - paragraph removed.
127647	6	26	6	32	[RISK] The key finding is not clear and ambiguous. "For global warming of no more than 2°C", warming since when? How are "abrupt changes in the carbon cycle" defined? The second part of the finding is a repetition of the previous finding that "the annual CO2 emissions rates from any of these paleo changes are one order of magnitude slower than the contemporary perturbation from anthropogenic emissions". Perhaps delete this message. [Trigg Talley, United States of America]	Accepted - paragraph removed.
127649	6	26	6	32	[RISK] This highlight is difficult to understand. What is the connection between the 2°C warming and paleo support/non-support for changes in the carbon cycle? [Trigg Talley, United States of America]	Accepted - paragraph removed.
90059	6	26	6	32	This statement uploaded by Ted Schuur on behalf of a graduate class that reviewed the chapter (this was communicated with IPCC TSU in advance): This ES statement has unexpectedly few confidence statements. [Edward Schuur, United States of America]	Accepted - paragraph removed.
115305	6	26		27	Unclear. Can you phrase in a more definitive way. 'abrupt change in the carbon cycle is not expected (low confidence)'? Or something similar. [Gillett Nathan, Canada]	Accepted - paragraph removed.
31845	6	26			I'm a bit puzzled by this. Yes, true if the paleo record is the past few million years. But the PETM (see page 12 l15 for example), which was 56Ma ago, likely had a change of this magnitude and it seems to have been very rapid. Might be better to edit to say paleorecord in the past X million years, or else limit the remark to the 800,000yr long ice core record in Antarctica, and then say high confidence. [Euan G. Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - paragraph removed.
58521	6	27	6	27	Since the magnitudes of (palaeo) atmospheric CO2 change are mentioned (i.e. 100 ppm, 10 ppm), the level of (palaeo)climate forcing needs to also be described here, for comparison to anthropogenic forcing (e.g. ~2xPI? ~1.5xPI?) [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - paragraph removed.
19901	6	27	6	27	What does "in response to climate forcing" mean? [philippe waldteufel, France]	Accepted - paragraph removed.
40495	6	27	6	32	lack of confidence level [TSU WGI, France]	Accepted - paragraph removed.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
67421	6	28	6	28	delete "of a magnitude" [James Christian, Canada]	Accepted - paragraph removed.
4369	6	29	6	32	"ten times slower" might be more understandeable by the broader community than "one order of magnitude" [Ana Bastos, Germany]	Accepted, change
81561	6	29	6	32	I don't understand the argument presented. What is meant with "annual emissions from paleo changes" -volcanic emissions, air-sea or air-land fluxes which represent the carbon climate-feedback? Why is caution required because "annual emissions rates are (s)lower. than contemporary fossil emissions? The previous sentence refers to "climate forcing" and not to emission forcing. Probably the sentence here should rather say that cimatic conditions and warmng rates projected under BaU emissions in the 21st century and beyond represent a non-analog situation when considering the climate variations of the last million year. This suggests caution when using the paleo record as an analogue for future carbon-climate feedbacks [Fortunat Joos, Switzerland]	Accepted - paragraph removed.
112295	6	29	6	32	This sentence may be misleading because the paleorecords have not a resolution of a year. A calcuation of an annual emission with very low-resolution data (e.g., 100-1000 years) does not make sense. [Jinho Ahn, Republic of Korea]	Accepted - paragraph removed.
67423	6	30	6	30	change "one" to "an" [James Christian, Canada]	Accepted - change was made.
68867	6	31	6	32	I could not find in section 5.1.3.3 “suggesting caution when using the paleo record as an analogue for contemporary and future carbon-climate feedbacks”. No paleo period should be considered an “analog” without a clear explanation of the context. There are no perfect analogs; there are, however, useful comparisons, depending on the purpose. Either omit this statement or clarify the point. Also, note that this statement challenges the CH7 assessment of ESC as determine by paleo climate states. [Darrell Kaufman, United States of America]	Accepted - paragraph removed.
66661	6	36	6	36	"Unequivocal" seems a strange choice of word here. Maybe "clear" or "obvious" or "beyond reasonable scientific doubt". [Dave Frame, New Zealand]	Rejected - this terminology is being used across multiple chapters, eg also in Chapter 2
40497	6	36	6	37	lack of confidence level [TSU WGI, France]	accepted, added
58613	6	36	6	39	Line 36 : "unequivocal" / Line 39 : "high confidence" --> An other adjective might be better regarding the confidence langage that is used here, or the confidence level should be revised [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	accepted. We added the equivalent IPCC language "very high confidence.
34891	6	36	6	42	Detailed Comments by SOD Chapter – Chapter 5: The SOD notes that the rate of CO2 build-up in the Industrial Era has been 10 times faster than in the last 66m years. Is this comment of any consequence, as CO2 levels are still well below most periods in the paleoclimate? [Jim O'Brien, Ireland]	Thanks for comment. We think it is incredible that the current perturbation has been unprecedented for so long.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
127651	6	36	6	42	[PROGRESS] The key finding is not a new message since AR5 or SRCCL, and needs to be restated in terms of new literature assessed. The sentence "It is unequivocal that the increase of CO2, CH4, and N2O in the atmosphere over the Industrial Era is the result of human activities" needs to be revised to convey a new message. Do numbers reported here include both fossil fuel AND land use emissions? It could be useful to report fossil fuel emissions separately from agriculture, forestry, and other land use (AFOLU) and build upon the SRCCL SPM reporting -- that is, expand on the SRCCL Table SPM.1 and update it to 2009-2018 numbers for CO2, CH4, and N20. [Trigg Talley, United States of America]	Thanks for comment. We report land use change and fossil fuel emissions further down in the ex. Summ. Yes, it is not new, but this fundamental statement from IPCC hasn't changed and we think it is not a justification for not including it again.
41003	6	39	6	39	The specific time period that "the last measured decade" refers to should be clarified. [TSU WGI, France]	rejected. The decades are specified further in the same sentence.
58875	6	39	6	39	"During the last measured decade" - which decade is this? To promote recency, years (e.g. "During the period 2009-2018") might be more compelling. I realize those years are provided later, but placing them here seems appropriate. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	rejected, decadal numbers shown in the same sentence a few words further.
58705	6	39	6	40	For clarity it may be helpful to include the world 'global' - "During the last measured decade, average global annual anthropogenic emissions…" [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	accepted. added.
109635	6	39	6	42	Why are CO2 and N2O emissions expressed in Pg C yr-1 and Tg N, yr-1 respectively, but CH4 emissions are not expressed in Tg C yr-1? [Carolyn-Monika Görres, Germany]	we have no explanation other than the methane community has always expressed the methane as CH4 not as 4, and given we are reporting what has been published, we keep CH4.
38317	6	39	6	47	The data in this connection is inconsistent in this chapter. For example: in lines 40-41, the average annual anthropogenic emission of CO2 (2009-2018) is 11.8±0.8 PgC yr-1, while in line 45 11.0 PgC yr-1; in Table 5.1 on page 30 it is 11.0±0.8 PgC yr-1; in line 46, the average annual emission of terrestrial ecosystems is 3.5±0.7 PgC yr-1, while in line 50 on page 28 and in Table 5.1 it is 3.2±0.6 PgC yr-1. It is suggested to verify and modify this. [Yaming LIU, China]	Accepted - changed and updated.
58707	6	40	6	41	The annual anthropogenic CO2 emissions number (11.8 ± 0.8 PgC/yr) for 2009 - 2018 does not match the numbers given in section 5.2.1.2 (Land Use Change: 1.5 ± 0.7 PgC/yr for 2008 - 2017 plus Fossil: 9.5 ± 0.5 PgC/yr for 2009 - 2018) or Table 5.1 (11.0 ± 0.8 PgC/yr for 2009-2008). So I assume this is an error and should read "11.0 ± 0.8" instead. However, this does not match the number used in section 5.2.1.5 (page 28, line 48) (10.9 ± 1.1 PgC/yr) for the exact same decade. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted, changed.
15503	6	40	6	41	The figures 11.8 ± 0.8 PgC yr-1 and 348-392 TgCH4 yr-1 cannot be found in the main text. Please check. [SAI MING LEE, China]	Accepted, changed.
18141	6	40	6	42	Is it possible to report the average annual anthropogenic emissions of the three GHGs in the same format? i.e xxx +/- xxx [Chelsey Baker, United Kingdom (of Great Britain and Northern Ireland)]	rejected. The papers assessed provide the ranges and plus/minus and we reproduce what has been published.
103075	6	40			please check data: here value is 11.8; p.6, line 45 it is 11.0; p. 28, line 48 (probably the only place in the core chapter) it is 10.9 [Philippe Tulkens, Belgium]	Accepted, changed.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
26137	6	41	6	41	swap year 2017 and 2018 for methane emission estimate [Mingkai Jiang, Australia]	Accepted. Changed
82977	6	41	6	41	The reported time period to indicate the highest level of the average annual anthropogenic emissions of CH4 seems strange to me. [Susanna Strada, Italy]	accepted. Changed.
58709	6	41	6	41	Should the range of CH4 annual emissions (currently 348-392 Tg CH4/yr) be expanded to include the top-down estimates? (This would change it to 334-392 Tg CH4/yr.) [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Changed
15505	6	41	6	41	Re: (2018-2017). Likely a typo error. Should it be (2008-2017)? Please check and revise. [SAI MING LEE, China]	Accepted. Changed
51097	6	41	6	41	Typo: 2018-2017 should read instead 2008-2017 (?). [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Changed
8865	6	41	6	41	change 2018-2017 to 2008-2017 [Vaishali Naik, United States of America]	Accepted. Changed
98225	6	41	6	41	WRONG DATES: 348–392 Tg CH4 yr-1 (2018–2017) [Gregory Cutter, United States of America]	Accepted. Changed
74173	6	41	6	41	the years 2018-2017 for the CH4 concentration should probably be 2008-2017 [Christoph Völker, Germany]	Accepted. Changed
18649	6	41	6	41	The period for methane emissions considered here is mentioned as 2018-2017? May be 2008-2017? [Govindasamy Bala, India]	Accepted, changed.
77257	6	41	6	42	Are the mass units for the molecule or the element? Molecule is assumed but please clarify. [Emer Griffin, Ireland]	they are as indicated.
103077	6	41			CH4: (2008-2017) [not: 2018] [Philippe Tulkens, Belgium]	Accepted. Changed
18093	6	41			There is a mistake in the starting or ending year for the amount of methane anthropogenic emissions. It cannot be 2018-2017 [Vlad Macovei, Germany]	Accepted. Changed
26881	6	42	6	42	Please change figure 5.4 by Figure 5.1 [Eric Brun, France]	We don't understand the comment, whether it is a swap or delete figure 5.4. We kept both.
130507	6	44	4	44	"fate"? Should be "rate"? [Panmao Zhai, China]	Rejected, we mean fate, but rephrase for more clarity.
114671	6	44	6	44	Woudl be good if you specify whether this included CH4 [Jan Fuglestvedt, Norway]	accepted. Specified.
58711	6	44	6	45	The decadal average for CO2 emissions from 2009-2018 here (11.0 PgC/yr) does not match that stated in the previous paragraph (11.8 ± 0.8 PgC/yr) or the value stated in section 5.2.1.5 (page 28, line 48) (10.9 ± 1.1 PgC/yr). Yet these are all for the same decade range of 2009-2018. The 11.0 PgC/yr seems to align with the numbers provided in section 5.2.1.2 for Land Use Change and Fossil emissions (Land Use Change: 1.5 ± 0.7 PgC/yr for 2008 - 2017; and Fossil: 9.5 ± 0.5 PgC/yr for 2009 - 2018) but the decade range does not match exactly. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Changed.
58751	6	44	6	45	Sentence structure is confusing and obscures meaning. Suggest rearranging to improve clarity: "During the decade of 2009-2018, an average of 11.0 Pg C yr-1 was emitted from human activities. These emissions had three fates: 44% accumulated in atmosphere…[etc]" [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Changed.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
21767	6	44	6	46	The uncertainty ranges in absolute values naturally should be reflected in uncertainty ranges in the quoted percentages. The percentage change cannot be more certain than the underlying quantitative estimates so the currently deterministic percentages should instead be given as ranges. If given as ranges it is questionable whether the absolute numbers breakdown is still required. [Peter Thorne, Ireland]	Accepted.
127653	6	44	6	46	The list of fates here doesn't specify a fraction that was taken up by the atmosphere (e.g., OH sink for methane). Is this only talking about CO2-C? [Trigg Talley, United States of America]	Yes, only CO2.
86723	6	44	6	46	It is highly relevant for policymakers knowing how large proportion of the anthropogenic CO2 emissions are taken up by terrestrial ecosystems and by the ocean. Thus, please consider including this information in the SPM under section B.1. [Oyvind Christophersen, Norway]	Accepted. Information has been included in the next SPM draft.
4371	6	44	6	47	The value 3.5 for the terrestrial sink is incorrect, following Friedlingstein et al. 2019, for 2009-2018, 4.9PgC/yr in the atmosphere, 2.5 removed by the ocean and 3.2 by terrestrial ecosystems [Ana Bastos, Germany]	Accepted, changes and updated.
2767	6	44	6	48	4.9+2.5+3.5=10.9 this is only 0.1 PgCyr-1 underestimate not 0.4PgCyr-1 and 0.1 PgCyr-1 is well within the error bars for the quantities given. Is there an error bar on the decadal average of 11.0? Given this sum, either one of the numbers is incorrect, or the sentence "The sum.....of both" should be removed. [Stephen Wilkinson, United Arab Emirates]	Accepted, corrected.
52781	6	44	6	50	The sum of carbon sinks is 4.9+2.5+3.5 = 10.9 PgC/yr, compared to 11.0 PgC/yr emissions. Why the imbalance is said to be 0.4 PgC? Why the imbalance is measured in PgC, not in PgC/yr? Since the listed unceratinties of ocean and land uptake are +-0.6 and +-0.7 PgC/yr respectively, does it even make sense to speak about imbalance that is smaller than uncertainties? Unless the estimates of ocean and land sinks are highly negatively correlated (in which case a separate discussion is required), it appears that there is no imbalance, given the uncertainty of the sink components. [Sergey Malyshev, United States of America]	Accepted. Changed.
86613	6	44	6	50	You should provide the estimate of emissions from fossil fuel and from land use, not just the sum. [Pierre Friedlingstein, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. added.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
127655	6	44	6	50	[CONFIDENCE] There are two major problems with the assessment of carbon budget in this chapter: (1) The report is expected to assess ALL literature and all methods to evaluate components of the global carbon cycle, including land and ocean fluxes, not just those reported in the GCP 19 budget, and (2) the Executive Summary key message is from the GCP 2019 budget, not based on the assessment of a broader set of models or literature, and, as a result, underestimates uncertainty in the net land flux. The carbon budget section (5.2.1.5) presents land flux using a different approach from those used in the AR4 and AR5. The GCP 2019 C budgeting approach is different from the prior IPCC assessments in terms of reporting the net land C flux, previously reported as residual sink from global budget. Prior to 2017, GCP reported land flux as a residual as well. GCP 2017 and 2018 budget reported land sinks based on residual approach and from DGVMs. GCP DGVMs estimate the land sink with only one reconstruction of climate forcing (either the merged monthly CRU and 6-hourly JRA-55 data set or monthly CRU). DGVMs have a number of limitations in capturing many anthropogenic and natural processes. The Executive Summary estimate should report land flux and its uncertainty based on all approaches, including DGVMs and residual approach (e.g., other methods summarized in Section 5.2.1.4.1 and Figure 5.10). Surprisingly, Section 5.1 does not include any estimates or a discussion of land fluxes from historical ESM experiments, while the same ESMs are discussed extensively in other sections of this chapter. [Trigg Talley, United States of America]	For the last decade we find no difference between calculating the land sink as residual or as independent estimate with DGVMs which we value as a more robust approach. That is why this assessment has also settle to do the budget using independent measurements for the land sink. In addition, among the new estimates of this budget compared to the AR5 include: a) the addition of four dynamic vegetation models to estimate the land sink, now a total of 14 models. b) improvements in the estimates of emissions associated with cement production, including the emissions associated with clinker production (Andrews 2019) and the sink associated with cement carbonation c) improved and new emission estimates from agriculture, forestry and other land use d) the use of ocean observed sink estimates based on repeated observations and a revised river flux partition between Northern Hemisphere, tropics and Southern Hemisphere e) the expansion of constraints from atmospheric inversions, both based on
127657	6	44	6	50	The sum of carbon sinks is 4.9+2.5+3.5 = 10.9 PgC/yr, compared to 11.0 PgC/yr emissions. Why is the imbalance said to be 0.4 PgC? Why is the imbalance measured in PgC, not in PgC/yr? Since the listed uncertainties of ocean and land uptake are ±0.6 and ±0.7 PgC/yr, respectively, does it even make sense to speak about imbalance that is smaller than uncertainties? Unless the estimates of ocean and land sinks are highly negatively correlated (in which case a separate discussion is required), it appears that there is no imbalance, given the uncertainty of the sink components. [Trigg Talley, United States of America]	Accepted. Numbers have been corrected and updated. You are correct, there is no imbalance now.
127659	6	44	6	50	Section 5.1 suggests that estimates based on stand-alone models of land and ocean carbon fluxes and anthropogenic emissions cannot close the carbon budget. The only currently available method for closing the carbon budget is emission-driven ESMs, but their results are not assessed in terms of historical land an ocean fluxes. [Trigg Talley, United States of America]	Accepted. The historical ESMs are presented.
111019	6	45	6	45	CO2 emissions should be 11.0 not 11.8 [Julia Nabel, Germany]	Accepted. Changed.
130509	6	45	6	46	Please note that "23% was taken up by the ocean (2.5 ± 0.6 PgC yr-1)" here is not same as "ocean continuing to take up 23 ± 6% of the global anthropogenic CO2 emissions in line 20, page 7. [Panmao Zhai, China]	Accepted. Changed.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
51099	6	45	7	13	carbon accumulated in the atmosphere is only presented in PgC while CH4 and N2O are presented in ppb. It would be useful to add the same type of metric for the 3 gases (ppm for CO2 or Gg for CH4 and N2O to ease comparison for the reader). [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. The literature we have reviewed comes with this difference.
103079	6	45			please check data: here value is 11.0; p.6, line 41 it is 11.8; p. 28, line 48 (probably the only place in the core chapter) it is 10.9 [Philippe Tulkens, Belgium]	Accepted. Changed.
69181	6	46	6	46	The amount of terrestrial ecosystem sink is referred as 3.5 ± 0.7 PgC yr-1 here, while referred as 3.2 ± 0.6 PgC yr-1 in table 5.1. Please check the consistency. As the sum of each element: 4.9+2.5+3.5+0.4 = 11.3, does not match to the emissions amount of 11.0, 3.5 seems a mistake. [Kaoru Magosaki, Japan]	Accepted. Changed.
58713	6	46	6	46	The amount of carbon removed by terrestrial ecosystems stated in this sentence (3.5 ± 0.7 PgC/yr) does not match the values in Table 5.1, or the values in line 50 on page 28, or the values stated in Friedlingstein et al., 2019. Table 5.1 says the terrestrial sink is 3.2 ± 0.6 PgC/yr which matches Friedlingstein et al., 2019 and corresponds correctly with the 29%. I believe this sentence should read "...and 29% was removed by terrestrial ecosystems (3.2 ± 0.6 PgC yr-1)..." [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Changed.
63605	6	46	6	46	3.5 +- 0.7 PgC/yr is incorrect, as it is the value for land for just 2018 in Friedlingstein et al. 2019; for 2009-2018, it is 3.2+-0.6 [Galen McKinley, United States of America]	Accepted. Changed.
15507	6	46	6	46	The figure 3.5 ± 0.7 PgC yr-1 does not tally with 3.2 ± 0.6 PgC yr-1 shown in the main text (P.28, line 50 and Table 5.1). Please check and revise as appropriate. [SAI MING LEE, China]	Accepted. Changed.
111021	6	46	6	46	terrestrial sink should be 3.2±0.6 not 3.5 ± 0.7 [Julia Nabel, Germany]	Accepted. Changed.
86725	6	46	6	46	Please consider a more specific phrasing than the "removal" of CO2 in terrestrial ecosystems (f.ex. by referring to "carbon storage" instead), so that the reader does not get the impression that the carbon is actually removed. [Oyvind Christophersen, Norway]	Accepted. Changed.
78459	6	47	6	47	“imbalance of 0.4”? the numbers quoted have an imbalance of 0.1. Need to make sure in the exec summary these agree without need to delve into the details in the text [Chris Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Changed.
115307	6	47		48	The budget is closed to within the stated uncertainties. You don't need to explain the small residual. You do not expect the budget to close exactly given the stated unceratinties. [Gillett Nathan, Canada]	Accepted. Changed
4373	6	48	6	48	"or combination of both" -> "or A combination of both" [Ana Bastos, Germany]	Accepted - change was made.
86727	6	48	6	50	This is vital information for policymakers, but check the numbers, since in the SPM 81-91% is associated with both fossil fuel combustion and cement production. Please correct and make the information in the SPM consistent with the Executive Summary. If possible, it would be useful to have the amount from each of these activities separately at least in either the SPM or exe.sum. [Oyvind Christophersen, Norway]	Accepted. Corrected.
18095	6	48			Can we add which source of error is most likely? [Vlad Macovei, Germany]	We don't know.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
67425	6	49	6	49	change "remaining" to "remainder" [James Christian, Canada]	Accepted. Changed.
39773	6	49	6	50	"land use" or "land use change" or "land use and land use change"? [TSU WGI, France]	Accepted. Now changed to AFOLU for consistency with other WGs
58753	6	49	6	50	The final sentence in this paragraph ("Of the total anthropogenic CO2 emissions…") does not follow logically from the previous two sentences. Suggest rearranging to improve logical flow. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected. We don't see the problem.
18143	6	49	6	50	land use is repeated twice [Chelsey Baker, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Changed.
93421	6	49	6	50	perhaps better '... land use and land use change' [Carles Pelejero, Spain]	Accepted. Changed.
115309	6	49			If you give a range you do not need 'about'. [Gillett Nathan, Canada]	Accepted. Changed.
4375	6	50	6	50	the word "management" should be included, since these estimates include harvest and shifting cultivation [Ana Bastos, Germany]	Accepted. Changed.
99033	6	52	6	52	I like how you are saying "It is unequivocal that …" here and above rather than what earlier chapters have done saying "It is virtually certain that .." on findings that are completely clear. Please help them follow your lead. [Michael MacCracken, United States of America]	Thank you.
34629	6	52	6	53	Has uptake in the biosphere continued to grow as well? There should probably be some mention of this (e.g., it has grown, or it hasn't grown, or we don't know). [Russell Vose, United States of America]	Yes, it is stated in the same sentence.
131505	6	52	6	53	Reference to chapter 5.4 is missing? [Hans Poertner and WGII TSU, Germany]	Accepted. Added.
131507	6	52	6	53	Why? You mention some reasons in section 5.4, yet a few examples would be useful here. [Hans Poertner and WGII TSU, Germany]	Rejected. The statements are across the combined sink as measured in the global budget, not from individual fluxes.
86615	6	52	6	54	The high confidence statement on sink rate decline is not cosnsitent with section 5.2.1 which states medium confidence. Given the absence of trend in the airborne fraction, medium confidence seems more appppropriate. Also Section 5.2.1.4.1 says it's not conclusive (page 26, lines 20-22). [Pierre Friedlingstein, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Made consistent.
58755	6	52	6	55	Suggest rearranging order of concepts to improve logical flow, such as: "As anthropogenic CO2 emissions have increased over the past six decades, ocean and land uptake of CO2 has continued to increase but there has been a decrease in the total rate of CO2 removal per unit excess anthropogenic CO2 in the atmosphere." [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. We have amended the sentences.
77259	6	52	6	55	Consider a simpler statement e.g. the relative rate of uptake has declined. Is there a quantification of this? [Emer Griffin, Ireland]	Rejected. The sink rate is an establish term and we report it as in the papers we assess.
84787	6	52	7	2	why is here stated “high confidence” while in section 5.2.1.1 it is “medium confidence”. In 5.2.1.4.1 there is also a lot of talk on terrestrial sink acceleration. This is not consistent with the statement here in the executive summary [Martin Heimann, Germany]	Accepted. Made consistent.
115311	6	52		53	Is this really unequivocal? For example, given the uncertainties in LUC emissions and observational estimate of the land sink, can we absolutely certain that the land sink has increased over this period? [Gillett Nathan, Canada]	Yes. Many lines of evidence.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
109583	6	53	6	53	“High confidence” sounds accurate, though no confidence interval is given on the AF trend in Figure 5.5. Without quantification in this summary statement of how much the trend has declined, it could easily be mistaken that the trend has changed to a high degree. Instead of high confidence, in what appears to be a relatively small trend. [Anthony Walker, United States of America]	Accepted. Added.
58715	6	53	6	53	The confidence level that the combined rates of CO2 removal by oceans and land per unit of excess anthropogenic CO2 in the atmosphere has declined is stated as 'high confidence' here. However, similar statements made in section 5.2.1.1 (page 19, lines 34-36; and page 19, lines 41-45) are classified as 'medium confidence.' Judging by the conclusions of the referenced papers, the papers referenced therein, and a search for additional papers, it seems most appropriate to classify these statements both as only 'medium confidence.' There are few papers that address the change in sink rate specifically. Additionally, there is mixed evidence regarding a change in the atmospheric fraction, which should increase if the sink rate were decreasing. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Changed.
18651	6	54	6	54	Maybe better to say that the uptake by land and oceans per unit of anthropogenic emissions have declined. [Govindasamy Bala, India]	Accepted, we have amended the sentence to make it more readable.
104849	6	54	6	55	This is not true for the ocean. There is no noticeable trend in ratio of carbon accumulated in the ocean to carbon accumulated in the atmosphere. [Timothy DeVries, United States of America]	Accepted. Added text in section and ex. Summ.
19903	6	55	7	2	Sentence needs corrections [philippe waldteufel, France]	noted
16501	6		9		The ES points are very clear, succinct and make specific points backed up by evidence. This style could be adopted by other chapters. [William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Thanks.
109331	7	1	7	1	delete "to be sensitive" in this line, or at least delete "to be" [Paul Edwards, United States of America]	Accepted. Changed.
51113	7	1	7	1	"Interannual and decadal variability of the ocean and land sinks indicate that these sinks are sensitive to climate conditions and therefore to be sensitive to climate change (high confidence)"- suggest this is reworded for clarity, do you mean, and therefore are senstive to climate change? [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Changed.
54973	7	1	7	1	Replace "to be" with "are." [Nancy Hamzawi, Canada]	Accepted. Changed.
18149	7	1	7	1	The line reads 'sensitive to climate conditions and therefore to be sensitive to climate change' and doesn't flow well. Change to read 'sensitive to climate conditions and therefore to climate change'. [Chelsey Baker, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Changed.
103081	7	1			… therefore also to climate change … [Philippe Tulkens, Belgium]	Accepted. Changed
98227	7	4	7	4	Atmospheric CH4 resumed its long-term growth trend in 2007: "resumed" implies it had stopped or even decreased, so given this is part of the Executive Summary, it needs a little explanation, e.g., after a ten year hiatus it resumed… [Gregory Cutter, United States of America]	Accepted. Rephrased.
34631	7	4	7	6	A longer-term context would be helpful here -- i.e., some brief mention that methane levels resumed a steady upward trend after a period of quasi-stability from the mid-1990s to the mid-2000s. [Russell Vose, United States of America]	Accepted. rephrased.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
58757	7	4	7	6	Sentence structure is confusing and obscures meaning. Suggest simplifying to improve clarity. "Atmospheric CH4 grew at an average rate of 7.1 +/- 2.7 ppb yr-1 over the last decade (2009-2018), but the growth rate was higher than average over the last measured 5-year period (2014-2018)." [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Changed.
31847	7	4	7	10	Again, could be updated to 2019. Would need to check with Ed Dlugokencky to see if the curve has settled enough by June, but I would think this is now stable enough to give growth over the 2014-2019 period. [Euan G. Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Updated.
77261	7	4	7	10	Publications since the Special Report on Land suggest that fossil fuel extraction is the major source of this increase, Has this been considered in this assessment? [Emer Griffin, Ireland]	Partially accepted. We have rephrased but new data shows both are equally important. Yes, it has been considered in this assessment.
77263	7	4	7	10	Publications since the SRLand suggest that fossil fuel extraction is the major source of this increase, Has this been considered in this assessment? [Emer Griffin, Ireland]	Partially accepted. We have rephrased but new data shows both are equally important. Yes, it has been considered in this assessment.
18157	7	5	7	6	The sentences finishes 'a growth rate that increased over the last measured 5-year period (2014–2018)' which disrupts the flow. Perhaps it could be amended to ', whilst the growth rate has increased over the last measured 5-year period (2014–2018) [Chelsey Baker, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. rephrased.
8867	7	6	7	8	The assessment of the reasons for the growth in atmospheric CH4 post-2006 is quite fragmented in section 5.2.2. While this statement may be accurate it needs a better line of sight. [Vaishali Naik, United States of America]	Accepted. Assessment in 5.2.2. improved with a full cross chapter box dedicated to it.
115313	7	6		7	Does 'the multi-decadal growth trend' refer to the growth in concentration or the growth in emissions? If it's the growth in concentration, then this is inconsistent with pg 6, ln 36-37 which states that it is unequivocal the increase in CH4 is due to human activities. Here this is only assessed ad 'very likely'. [Gillett Nathan, Canada]	Accepted. Made clear. The full assessment of anthropogenic attributing is provided in section 5.2.
31849	7	8	7	8	This line can be challenged. 1. the isotopic shift implies a declining % share for fossil fuel emissions. Fossil fuel emissions might indeed be growing, but are a minor part of the change. 2. Climate warming feedback from tropical wetlands is very likely occurring and is geographically and isotopically indistinguishable from ruminant emissions. 3. OH may be changing in the tropical mis-troposphere (see discussion elsewhere) . I would rephrase the comment and say more cautiously "The causes of growth are not fully understood but there is medium confidence that agricultural emissions are growing." [Euan G. Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Partially accepted. Rephrased. Full assessment of the issue in cross chapter box on methane.
40499	7	8	7	10	lack of confidence level [TSU WGI, France]	Accepted. Added.
19905	7	8	7	10	Careful reading of section 5.2.2 does not provide strong evidence for ENSO cycles driving the variability of atmospheric CH4 growth rate; neither does it give clues for the suggested link between ENSO cycles and biomass burning. [philippe waldteufel, France]	Rejected.. See clearer statements in the section and box showing this is the case.
58623	7	8	7	10	A confidence level for this last sentence would be good. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Added.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
31851	7	9			ENSO -note there is also strong evidence for the expansion and intensification of the meteorological tropics. Staten, P. W., Lu, J., Grise, K. M., Davis, S. M., & Birner, T. (2018). Re-examining tropical expansion. Nature Climate Change, 8(9), 768-775. That's why line 8 seems insecure - it seems to deny the hypothesis that there is a feedback going on, warming feeding warming, as the warmer wetter tropicsl swamps emit more methane. Maybe rephrase to say the inter-annual variability is primarily driven by the ENSO cycle, but long-term meteorological change may also be a factor. [Euan G. Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Partial rephrased.
58717	7	10	7	10	The Cross-Chapter Box 5.1 should be included in the referenced sections. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	accepted, added
9677	7	12	7	12	by or to ? [Olivier Boucher, France]	Accepted. Changed.
58759	7	12	7	14	Sentence structure disjointed. Suggest rewording: "This section assesses the trends and variability in atmospheric accumulation of the three main GHGs (CO2, CH4, and N2O), including their ocean and terrestrial sources and sinks as well as their budget during the historical period (1750-2018)." [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Rephrased.
127661	7	12	7	16	[PROGRESS] The message about increases in AFOLU agricultural emissions does not contain much novel information. Check the SRCCL SPM bullet A3.5 and identify what is new in this chapter. [Trigg Talley, United States of America]	Agreed, yet still important.
18159	7	12	7	16	Percentage increases are referred to as 'about 70%' - instead of 'about' can this be quantified i.e by 70 +/- ??% [Chelsey Baker, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Changed.
103083	7	12			"The concentration of atmospheric N2O ..." The term "growth rate " is wrong here [Philippe Tulkens, Belgium]	Accepted. Changed
58719	7	14	7	15	This statement says that the agricultural N2O emissions have increased by about 80% since the early 1900s. The reference for this value is provided in lines 40-42 on page 38 (Davidson, 2009). However, in reading the 2009 Davidson paper, I do not find where this 80% came from. In calculations derived from the data in the supplementary info of this paper, it appears that the value should be much higher than an 80% increase if comparing to the early 1900's. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Addressed both in section and here.
10997	7	14	7	15	There is a discrepancy here between this ES statement and the text. These lines state that agricultural N2O emissions have increased by 30% since the 1980s. However, the text, page 5-38 lines 41-42, states that agricultural N2O emissions have increased by more than 45% since the 1980s. Please check yor cited paper (Davidson, 2009, doi:10.1038/ngeo608) [Emmy Wrobleski, United States of America]	Accepted. Addressed in both section and here.
15509	7	15	7	15	Re: by 30% since the 1980s. The figure does not tally with the main text: "by more than 45% since the 1980s" (P.38, lines 41-42). Please check and revise as appropriate. [SAI MING LEE, China]	Accepted. Addressed in both section and here.
41583	7	15	7	16	Other contributors (and the missing 30 %) should be mentioned here as well [Katharina Meurer, Sweden]	Accepted. Added.
103085	7	16			"(high confidence)" is used twice in this paragraph. Needed? [Philippe Tulkens, Belgium]	Yes. Different statements.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
18047	7	18	7	18	This section is titled acidification and deoxygenation but there is only a paragraph about acidification. Where is a statement on deoxygenation? [Lisa Levin, United States of America]	Accepted. Added highlight on deoxygenation
81567	7	18	7	18	Missing bullet on deoxygenation [Fortunat Joos, Switzerland]	Accepted. Added highlight on deoxygenation
41585	7	18	7	27	Ocean de-Oxydenation is not mentioned in the text [Katharina Meurer, Sweden]	Accepted. Added highlight on deoxygenation
51103	7	18	7	27	The caption refers to "de-oxygenation" but in fact "de-oxygenation" isn't referred to in this paragraph or anywhere else in the Exec Summary. Suggest addition of a sentence about de-oxygenation eg "The open ocean is losing oxygen and this is expected to continue, but the rate cannot be predicted as models under-predict the observed rate of loss." (drawing on material on p.48 lines 38-41) The reference saying the rate will accelerate is from 2013, is there a more up-to-date refererence you could cite? [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Added highlight on deoxygenation
51105	7	18	7	27	The impossibility of reversing ocean acidification is described (slightly unclearly - what is meant by the "reversal of the CO2 increase"?) on p. 50, lines 25-32. Suggest that this important conclusion is elevated to the Exec. Summary. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Added.
51111	7	18	7	27	Despite the subtitle "Ocean Acidification and Ocean de-Oxygenation", nothing is mentioned about ocean deoxygenation. Suggest content is amended to include deoxygenation information or subtitle is changed. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Added highlight on deoxygenation
54975	7	18	7	27	This section of text contains "Ocean de-Oxygenation" in its heading. Yet, only ocean acidification is discussed in this block of text. Either delete "and Ocean de-Oxygenation" from the heading, or add a new paragraph that deals specifically with ocean deoxygenation. [Nancy Hamzawi, Canada]	Accepted. Added highlight on deoxygenation
90061	7	18	7	27	This statement uploaded by Ted Schuur on behalf of a graduate class that reviewed the chapter (this was communicated with IPCC TSU in advance): There was only one ES which related to Ocean Acidification and Deoxygenation. ES mentioned skeletons and shells of marine mammals, this was not discussed in section. Otherwise I think the ES captures the ideas well. [Edward Schuur, United States of America]	Accepted. Made consistent between Ex. Summ and section
77277	7	18	7	27	Executive summary: Heading includes dexoygenation but there is no mention of deoygenation in text. It might be useful to have a conclusion on deoxygenation for consistency through to the SPM as there deoxygenation is mentioned in the SPM [Emer Griffin, Ireland]	Accepted. Added highlight on deoxygenation
72891	7	20	7	20	No capital 'A' for 'acidification' [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change was made.
18163	7	20	7	20	'Ocean Acidification is strengthening' - I find the use of the word strengthening vague. I would suggest rephrasing or reordering the sentence. [Chelsey Baker, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Changed.
34633	7	20	7	21	I realize the term "ocean acidification" is used with some regularity, but it is technically inaccurate. The average pH of the ocean is about 8.1, wheres a pH less than 7 is acidic, so ocean water is slightly basic. A more precise choice of words would be, "... increase in the relative acidity of the global ocean..." [Russell Vose, United States of America]	Thank you. It is said in the section. Term widely used and kept.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
54969	7	20	7	21	The verb tense of this sentence ("continuing to take up") makes it unclear over what time period the uptake by the ocean of 23% of anthropogenic CO2 is valid. Recommend revising to clarify the time period(s) over which this result is true. [Nancy Hamzawi, Canada]	Accepted. Delete the %
36359	7	20			Because rates of ocean uptake vary considerably depending of the time period of interest, the time period the 23% estimate is associated with should be included in this statement. The value of 23% only refers to the decade of 2009-2018. [Adrienne Sutton, United States of America]	Accepted. Delete it.
18161	7	21	7	21	Sentence starts with 'This uptake' which is vague. Change to 'This uptake of anthropogenic CO2…' [Chelsey Baker, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Changed.
40501	7	21	7	23	lack of confidence level [TSU WGI, France]	It already had "high confidence"
45421	7	21	7	23	"reductions in the level of calcium carbonate minerals" is quite vague. I suggest rewriting this sentence to: "This CO2 uptake is driving changes in seawater chemistry that result in a pH decrease, driving the calcium carbonate minerals that form the skeletons and shells of a variety of marine organisms toward dissolution, and inhibiting their precipitation." [Olivier Sulpis, Netherlands]	Accepted. Changed.
67427	7	22	7	22	change "level" to "saturation state" [James Christian, Canada]	Rejected. Saturation state is too technical for Exe. Summ.
52235	7	22	7	23	suggestion: after "reductions in the level of calcium carbonate" add "saturation and carbonate ion concentrations" "that form calcium carbonate skeleton and shells…" remove "minerals" [Agneta Fransson, Norway]	Accepted. Sentence rewritten
74175	7	22	7	23	“in the level of calcium carbonate minerals”: maybe add “saturation” before level [Christoph Völker, Germany]	Rephrased using "concentration"
69829	7	22	7	23	The word "the level of carlicum carbonate minerals" is very wired. [Kaoru Kubota, Japan]	Accepted. Changed.
18097	7	22			"level" of minerals doesn't sound good. Better "concentration"? [Vlad Macovei, Germany]	Accepted. Changed.
10999	7	23	7	25	The ES statement on ocean acidification should incorporate more on past changes in ocean acidification, especially because this topic is discussed in the text (section 5.3.1.1, pages 5-44 to 5-45, and section 5.3.1.2 page 5-45). Consider addressing pH in paleo records, particularly the PETM and the last deglaciation (18-11 kyr). Points made in the text that could be considered for the ES include that during the PETM an increase in atmospheric CO2 concentrations (800 ppm ->2000 ppm; Gutjahr et al., 2017, doi:10.1038/nature23646) is attributed to a negative surface ocean pH excursion ranging from 0.15 to 0.30 units over several thousand years (Gutjahr et al., 2017, doi:10.1038/nature23646; Penman et al., 2014, doi:10.1002/2014PA002621; Babila et al., 2018, doi:10.1098/rsta.2017.0072), which is an order of magnitude slower than the current rate of ocean acidification (Zeebe et al., 2016, doi:10.1038/ngeo2681). The last deglaciation (18-11 kyr ago) saw a 0.15-0.05 unit decrease in surface ocean pH across the deglacial transition. These events highlight that high rates of CO2 emissions have adverse consequences on ocean systems and their efficiency as CO2 sinks. [Emmy Wrobleski, United States of America]	Rejected. There is limited space and key policy relevant message is present and future.

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98229	7	25	7	26	into the ocean interior over time, having already reached depths surpassing 2000 meters. To put this in context of the ocean interior, need average depth of ocean, so add, "…surpassing 2000 meters of the ocean's average depth of 3700 m. [Gregory Cutter, United States of America]	Accepted. Included.
72893	7	26	7	26	For parity elsewhere, change ;metres' to 'm' [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Changed.
34635	7	31	7	32	I don't think "will continue to grow" and "weaken those sinks" are a particularly good choice of words. It seems more accurate to say ocean and terrestrial carbon sinks are projected to absorb more carbon through mid-century, but thereafter emerging feedbacks may eliminate the ocean sink entirely and drastically reduce the importance of the land as a sink. [Russell Vose, United States of America]	Accepted. We rephrase partially, but stay with the specific comment given that with most scenarios sinks continue to grow.
114853	7	31	7	32	In the same phrase the sinks "will continue to grow" and "will weaken". Perhaps a better way to explain the issue would be to introduce the term "oceanic and terrestrial carbon uptake". [Roxana Bojariu, Romania]	Accepted. Rephrased.
18653	7	31	7	32	Maybe better to highlight that the fraction of the anthropogenic emissions that is taken up by land and oceans would decline in the future. I think that is better messaging than the current one. [Govindasamy Bala, India]	Accepted, partial. Sentence rephrased but main current message. Add message of fraction.
64591	7	31	7	34	Suggest: "Oceanic and terrestrial carbon sinks will continue to grow in absolute magnitude over the 21st century due to increased atmospheric concentrations of CO2 but emerging feedbacks will weaken those sinks in relative terms." The sentence which follows is potentially misleading and also needs to be clarified, viz., "It is very likely that the global ocean sink will stop taking up more CO2 from the second part of the century under any emission scenario, at a level varying from about 4 to 6 PgC yr-1." Specifically, the phrase "stop taking up more CO2" could be confused as meaning "stop taking up any CO2." Clearly the statement should be something more like: "It is very likely that the ocean sink will saturate at a level somewhere between 4 to 6 PgC yr-1 after about 2080, and slowly decrease thereafter." [Charles Curry, Canada]	Accepted. WE have rephrased but not exactly in the way suggested.
79383	7	31	7	38	I don't find this paragraph vey clear. I think the message is that ocean/land will take more carbon in the future but that the rates of uptake will decrease due to some feedback processes. However the sentence "It is very likely that the global ocean sink will stop taking up more CO2 from the second part of the century under any emission scenario" seems to say something different. Need to be clear when you talk about totals amounts of carbon and rates of removal per unit of excess CO2. [Alejandro Di Luca, Australia]	Accepted. This is what we way in the top sentence. Then we provide more specific model results.
54971	7	31	7	38	Second sentence in this paragraph is unclear. Saying "the global ocean sink will stop taking up more CO2" could be misunderstood to mean that it will no longer function as a sink. We presume you mean the ocean sink will no longer grow in tandem with rising atmospheric CO2 concentrations suggesting the airborne fraction will increase as a result. Somewhere in this paragraph it would be useful to state clearly (if true) that the consequence of weakening sinks is a growing airborne fraction of CO2 emitted from human activities. [Nancy Hamzawi, Canada]	Accepted. Rephrased and removed stop.

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52783	7	32	7	34	The meaning of this phrase is very unclear. The first par of the phrase sounds like the oceans will stop taking up CO2 (i.e. sink becomes zero). Is this interpretation correct, or will the sink magnitude stop increasing and remain more or less constant in the second part of the century? [Sergey Malyshev, United States of America]	Accepted. Rephrased.
41587	7	32	7	34	The content of the sentence is confusing: I understand that the global ocean sink will stop taking up more CO2, but what is meant with "at a level varying from about 4 to 6 PgC yr-1"? Is that the current rate or the level at which the ocean will stop taking up more CO2? [Katharina Meurer, Sweden]	Accepted. Rephrased.
21131	7	32	7	34	I do not understand this sentence. If the ocean will stop taking up CO2 why is an uptake rate given? [Steven Lade, Sweden]	Accepted. Rephrased.
81563	7	32	7	34	"any emission scenario" seems to general - may be true for the SSP. (In general, air-to-sea flux is expected to peak around the time where the second derivative of the atm. CO2 growth rate becomes zero.). [Fortunat Joos, Switzerland]	Accepted. Rephrased.
127663	7	32	7	34	The meaning of this phrase is very unclear. The first part of the phrase sounds like the oceans will stop taking up CO2 (i.e., sink becomes zero). Is this interpretation correct, or will the sink magnitude stop increasing and remain more or less constant in the second part of the century? [Trigg Talley, United States of America]	Accepted. Rephrased.
17863	7	32	7	34	This sentence is very confusingly phrased. Does "ocean sink will stop taking up more" mean that the sink saturates or that the ocean becomes a source of carbon? [Katherine Calvin, United States of America]	Accepted. Rephrased.
86729	7	32	7	36	It is highly relevant for policymakers knowing the risk of the land and the oceans switching from being a sink to a source. Thus, please include this information to the SPM, under B.1.2. [Oyvind Christophersen, Norway]	Comment passed on to TSU and feedback to SPM
115315	7	32		34	Replace 'the global ocean sink will stop taking up more CO2' with 'the rate at which the global ocean sink takes up CO2 will stop increasing' [Gillett Nathan, Canada]	Accepted. Rephrased.
67429	7	33	7	33	"stop taking up more CO2"??? I think they mean that the magnitude of the ocean sink will decline. But I am not sure even that is an accurate statement: under a high emissions scenario, ocean uptake will continue to grow even if it declines in relative terms (increasing airborne fraction). [James Christian, Canada]	Accepted. Rephrased.
104851	7	33	7	33	This is a confusing statement. It sounds like the global ocean will stop taking up anthropogenic CO2, but that can't be right. The ocean will of course keep taking up CO2. Do you mean the ocean CO2 sink will decline due to climate feedbacks? [Timothy DeVries, United States of America]	Accepted. Rephrased.
74177	7	33	7	33	“the global ocean sink will stop taking up more CO2” the sink does not take up CO2 (the ocean does), but may stop becoming larger [Christoph Völker, Germany]	Accepted. Rephrased.
18167	7	33	7	33	CO2 from the second part of the century' shorten to 'CO2 from 2050' or 'CO2 from the second half of the century' [Chelsey Baker, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Changed.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
7421	7	33	7	33	This sentence could be misintepreted : « t is very likely that the global ocean sink will stop taking up more CO 2 ». It would more comprehensive to indicate if (i) the ocean uptake will plateauing at a given rate, or (ii) if the ocean uptake could stop (CO2 flux from atmosphere to ocean go to 0). The second part of the sentence seems to say the first (i) option. But I suggest to rephrase it to avoid confusing. [Geremy PANTHOU, France]	Accepted. Rephrased.
15511	7	33	7	34	Re: the global ocean sink will stop taking up more CO2 from the second part of the century under any emission scenario, at a level varying from about 4 to 6 PgC yr-1. The statement does not seem to tally with Figure 5.25(a) which shows that under SSP1, RCP2.6, the ocean uptake will start to decline soon after 2020, becoming close to zero by 2100 which is outside the range of "4 to 6 PgC yr-1"; while under SSP5, RCP8.5, the ocean uptake will start to decline near the end of this century. Furthermore, only two scenarios are shown in Figure 5.25(a). Please check and revise as appropriate. [SAI MING LEE, China]	Accepted. Taken into account.
51101	7	33	7	34	"It is very likely that the global ocean sink will stop taking up more CO2 from the second part of the century under any emission scenario". Figure 5.25 show that the stock continue to increase. Can it be made clearer that this statement does not mean there will not be any ocean sink but that the sink will not increase any longer? Suggest rephrasing this as: "It is very likely that the global ocean sink with stop continuing to take up CO2 from the second part of the century" [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Rephrased.
69183	7	34	7	36	Please make it clear that the description of "It is very likely that the land carbon sink will decline from mid-century onwards under high-emissions scenarios, but there is low confidence that the land will switch from being a sink to a source." is talking about global land. (for example put "global" before "land") [Kaoru Magosaki, Japan]	Accepted. Added..
15513	7	34	7	36	Re: the land carbon sink will decline from mid-century onwards under high-emissions scenarios. The statement does not seem to tally with Figure 5.25(b) which shows that under SSP5, RCP8.5, the land uptake does not have a decline for the 2nd half of the century. Please check and revise as appropriate. [SAI MING LEE, China]	Accepted. Statement made consistent between ex. Summ and section
131509	7	34	7	36	Why? You mention some reasons in section 5.4, yet a few examples would be useful here. [Hans Poertner and WGII TSU, Germany]	Rejected. We are constrained by space.
26139	7	35	7	36	suggest to replace with "but it is likely, with low confidence, that the land carbon sink will switch from being a sink to a source," [Mingkai Jiang, Australia]	Partially accepted, rephrased.
51109	7	35	7	36	the expression "there is low confidence that the land will switch from being a sink to a source." Is it possible to be precise about whether this statement is about the net land flux excluding or including the net land use change flux and/or to refer to a higher confidence statement about terrestrial carbon feedback? [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Clarified.
103087	7	35			misleading statement. Better: " ..., but the land may remain a sink rather than switch to a source (low confidence)" [Philippe Tulkens, Belgium]	Accepted. Rephrased.

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19907	7	36	7	38	While section 5.4 does analyse the evolution of the ocean sink as a function of atmospheric CO2, it does not provide arguments concerning the relative sizes of uncertainties on emissions and carbon feedback respectively. [philippe waldteufel, France]	Thank you, addressed in section.
17865	7	36	7	38	Can you provide quantitative information? What is the range in ppm due to model uncertainty and what is the range due to scenario uncertainty? Also, this is missing a confidence statement. [Katherine Calvin, United States of America]	Accepted. We didn't have this information in the main section, we have now discussed further but the ranges are too dependent on scenarios used and cannot be simplified in short statement in ES.
109517	7	36	7	38	Although uncertainty is dominated by emissions scenario there is presumably large uncertainty within any given scenario caused by our knowledge of land and ocean sink responses to increasing CO2 into the future. This makes planning a specific scenario to meet targets difficult. This summary would benefit from a (quantitative) statement on how much the process-understanding uncertainty contributes to projected rates of CO2 or climate change within a given (target) scenario. [Anthony Walker, United States of America]	Accepted. Changed.
81565	7	38	7	38	suggest to modify to "rather than uncertainties in modelling carbon sources and sinks". It is not just the feedback with climate that is important, but also how carbon uptake is modelled in response to raising CO2. [Fortunat Joos, Switzerland]	Rejected. The uncertainties of the carbon-climate feedbacks are in fact the uncertainties of modelling (well or not) the processes involved.
10253	7	40	7	40	Replace 7th word "from" with "by". [Chris Vivian, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Changed.
51107	7	40	7	40	the relative contribution of the physical ocean sink compared to the one mediated by living organisms has not been discussed before, which would make this statement more informative. Is it possible to incorporate this information? [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We have rewritten the paragraph to make clearer what we know and what we don't know. Otherwise, please refer to main text.
34637	7	40	7	41	The following sentence in section 5.4.4.2 is a better choice of words: "Paleoceanographic observations thus suggest a strong sensitivity of the biological carbon pump to climate, with a weaker efficiency in warm climate intervals." As an aside, it's a bit surprising to see this paleo conclusion in a section about projected changes. [Russell Vose, United States of America]	Accepted. Changed.
103089	7	40	7	44	maybe useful to add: C uptake by organisms contributes … Pg C [Philippe Tulkens, Belgium]	Accepted. The biological pump is not contributing to the net carbon update by oceans, but it will be negative effective in the future.. We have rewritten the paragraph to make it clear

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
127665	7	40	7	44	[RISK] "Paleo records indicate that carbon uptake from living organisms in the ocean is sensitive to climate and directly decreases the CO2 ocean sink in periods of increased warming (medium confidence). Thus, future warming is expected to reduce carbon uptake by living organisms, with an overall decline in the ocean carbon uptake. The processes driving future changes in the magnitude and efficiency of this process remains uncertain. {5.4.4.2}" What is the reader to take from this? If the magnitude is uncertain, is there really any confidence that the information is robust and relevant to the future? [Trigg Talley, United States of America]	Accepted. The paragraph has been rewritten for clarity and show the potential relative importance of the biological pump in the future.
18655	7	40	7	44	It is worth pointing here the approximate ratio of net ocean uptake by living organisms and the inorganic processes. The ratio would give an idea to the readers whether this is too serious a problem for the overall weakening uptake of carbon. [Govindasamy Bala, India]	Accepted. The biological pump is not contributing to the net carbon update by oceans, but it will be negative effective in the future.. We have rewritten the paragraph to make it clear
11001	7	40	7	44	Consider revising and restructuring this ES statement. It resides under 'Future Projections of Carbon Feedbacks on Climate Change', but the first sentence introduces paleo records. Restructure to start with the second sentence, "...future warming is expected to reduce carbon uptake…." Then proceed with a statement highlighting that paleo records support that during periods of increased warming and atmospheric CO2, carbon uptake from living organisms in the ocean is compromised, directly decreasing the CO2 ocean sink. [Emmy Wrobleski, United States of America]	Accepted. Changed.
18169	7	40	7	44	I find this paragraph quite vague and the second sentence basically repeats the bold sentence. How does climate change reduce the carbon uptake of living organisms? What type of organisms, animals or plants or both? I think some further detail should be added and the repetition throughout the paragraph should be removed. [Chelsey Baker, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We have rewritten the sentence for clarity. Please refer to 5.4.4.2 for further details.
40503	7	42	7	43	lack of confidence level [TSU WGI, France]	Accepted. Added.
39875	7	43	7	44	"uncertain" is not IPCC confidence language [TSU WGI, France]	Accepted. Changed.
11003	7	43	7	44	The sentence, "The processes driving future changes in the magnitude and efficiency of this process remains uncertain." is unclear. Consider omitting or embelishing. What about the processes driving future changes in the magnitude and efficiency of carbon uptake by living oceanic organisms remain uncertain? Do the processes themselves remain uncertain? [Emmy Wrobleski, United States of America]	Accepted. Change.
37747	7	46	7	49	This paragraph needs more explanatory sentences. For example: Both CH4 and N2O have climatic feedback loops through microbial responses to temperature and moisture conditions, but only a few studies have been conducted to quantify the effect using Earth system models. [Akihiko Ito, Japan]	Accepted. WE have decrease the confidence level.
103091	7	46	7	49	not too surprising, given the general domiance of CO2. Possibly it makes sense to qualiy this in respeoct to non-CO2 on one hand and total GHG on the other. As such it is not helpful and might as well be totally removed [Philippe Tulkens, Belgium]	Rejected. Non co2 can be more important than thought given the low confidence in understanding.

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127667	7	46	7	49	"medium to high confidence" seems inconsistent with the uncertainties captured in Section 5.4.7, but in the context of anthropogenic emissions is correct. The statement is a bit confusing because it is in the context of anthropogenic emissions. [Trigg Talley, United States of America]	Accepted. WE have decrease the confidence level.
86731	7	46	7	49	please include an explanation/example of the climate-CH4 and climate-N2O feedbacks e.g. for CH4, the response of CH4 emissions from wetlands due to increased temperatures. [Oyvind Christophersen, Norway]	Accepted. Included examples.
31853	7	46			I'm not sure we can quantify this as we don't know the tropical climate CH4 feedback, which is very complex. It is not just wetlands getting warmer and having a strong Arrhenius-type response as they warm (and indeed get wetter in many areas also, e.g. N Zambia, Bolivian Amazon). It is also the subtle CH4 impact of good rains on the inner tropics making more cattle feed. I agree though that Arctic hydrates are probably not going to be a big problem. [Euan G. Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. WE have decrease the confidence level.
127669	7	48	7	48	The overall change is small, so "large uncertainty" should be "large relative uncertainty". [Trigg Talley, United States of America]	Accepted. Change to the use of IPCC confidence level language.
41187	7	48	7	48	Why "medium to high confidence"? Are the confidence level different between different points that stated in this sentence? [TSU WGI, France]	Rejected. All non CO2 climate feedbacks are medium to low confidence as a whole as shown here. Individual processes which we don't show have indeed different levels of confidence.
89463	7	51	7	55	We spent a lot of time in SROCC to update the state of knowledge on permafrost carbon. While there is a range of estimates out there, the chapter authors and other contributors thought that it was important to highlight what was known much better than AR5. I suggest that the authors review both the ES statements of Chapter 3 and the SPM of SROCC to look at the reporting for SROCC. This is specifically in regards to the last sentence that gives low confidence to magnitude, timing, and form of carbon release. For SROCC we found a way to highlight what was known such that confidence levels were higher; some of that has to do with the wording of the statement. We realize that SROCC has been consulted to some degree but think that the message about our knowledge of permafrost carbon could be stronger here, epecially seeing how permafrost carbon feedback figures prominently through many sections of this chapter. This is in part due to the rapid increase in knowledge on this topic since AR5. [Edward Schuur, United States of America]	Accepted. Revised.
41589	7	51	8	4	Research has shown that permafrost thaw also releases high amount of N2O. This has been shown in laboratory experiments (e.g. Voigt et al. 2017 PNAS 114(24): 6238 - 6243, doi: 10.1073/pnas.1702902114) and should not be neglected in this report. However, measurements of N2O from permafrost soils are still scarce and further research is needed to allow for a sound budgeting [Katharina Meurer, Sweden]	Accepted. We have reduce the level of uncertainty.
71167	7	51	8	4	A comment about the sink potential for changes in permafrost, increase in active layer thickness and associated changes in vegetation would be valuable. I understand that there is currently very little research and a very low confidence in the extent, but it is important to mention that this effect also occurs. [Lukas Arenson, Canada]	Accepted. Dealt in the section. No room in the ES.

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26883	7	53	7	53	Consistency must be ensured between the units used throughout the report: SPM, page 25, line 49, says : "74+/- 48 GtCO2 " which corresponds to 74PgCO2 and chapter 5, page 7, line 53 says : "20 +/- 13 PgC". [Eric Brun, France]	Accepted.
4377	8	2	8	4	The phrasing and grammar of this sentence are confusing. [Ana Bastos, Germany]	Accepted. Sentence rephrased.
17867	8	2	8	4	Can you provide quantitative information on the fraction of CH4? [Katherine Calvin, United States of America]	Accepted.
71659	8	2	8	4	Given the wide range of estimates for the net effect of methanogenic and methanotrophic processes shown in recent studies “is” would be better as “may be” [Martin Manning, New Zealand]	Accepted, but we used instead confidence level.
18171	8	2	8	4	'Because of water-saturated soils and a lack of oxygen in thawing permafrost regions, part of the carbon is released as CH4, which leads to the combined radiative forcing being larger than from if there were CO2 emissions only'. Rephrase so that the sentence does not start with because. I suggest ' Water-saturated soils and a lack of oxygen in thawing permafrost regions mean that some of the carbon is released as CH4 ...' [Chelsey Baker, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Rephrased.
109333	8	3	8	3	delete "from" in this line [Paul Edwards, United States of America]	Accepted. Removed.
67431	8	3	8	3	"leads to the combined radiative forcing being larger than from if there were CO2 emissions only" leads to a combined radiative forcing larger than from CO2 emissions only [James Christian, Canada]	Accepted. Rephrased.
19909	8	3	8	3	Spurious "from" [philippe waldteufel, France]	Accepted. Removed.
78461	8	5	8	5	this conclusion seems to contradict p.6 line 26 which abrupt changes are not supported by the evidence [Chris Jones, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. Here is regional, above is global.
58795	8	5	8	5	The use of 'human-forcing' is odd terminology and not clear. This not using anywhere else in the chapter. Change to 'anthropogenic perturbation' or similar. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Changed.
58305	8	5	8	40	I feel you should cite your sources here, i.e. for for all those emission numbers [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Sources at the end of paragraph.
4379	8	6	8	8	"no one" -> "none". The second part of the sentence also seems incomplete. [Ana Bastos, Germany]	Taken into account. Text revised to 'not one'.
40505	8	6	8	8	lack of confidence level [TSU WGI, France]	Accepted. Added.
58333	8	6	8	8	It has been hypothesised that there could be such a global threshold (Steffen et al., 2018, doi:10.1073/pnas.1810141115), but work testing this hypothesis is only just emerging and you're right to say that such a threshold isn't apparent in current ESM projections. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Thank you, it is a hypothesis only.
58797	8	7	8	7	It is not clear why only forest dieback was used as the example of regional threshold. Was it simply the first in the list 5.4.8? Suggest removing or making it a longer list with 3-4 examples. Additionally forest dieback is occuring in many regions across the planet, need to clarify this possibly by saying Forest dieback in XX region e.g. boreal forest dieback [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Added fire.

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58799	8	7	8	8	Start a new sentence after climate change This sentence could be taken out of context very easily and I think it is necessary to change it. The phrase 'no one particular threshold' in particular should be changed.By saying there is no one threshold, makes it sounds like these thresholds don't exist or there is no consensus on them. It also contradicts the 2 degree narrative which has been central to recent IPCC special reports. Quantify that the biogeochemical feedbacks in the models occurs over e.g. X-X degrees and or XXX -XXXppm CO2. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Partially accepted. We have rephrased but the thresholds are very different depending on where in the world we are, and therefore no possible to generalize in one sentence.
31855	8	7			e.g. fire as well as forest dieback. Fire in drought is arguably a much bigger cause of sudden/catastrophic irretrievable change (at least in human lifespans) [Euan G. Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Added.
41591	8	8	8	10	The term "runaway changes" is not clear [Katharina Meurer, Sweden]	Accepted. Removed.
17869	8	8	8	10	Does the confidence in the absense of runaway changes account for the missing feedbacks? [Katherine Calvin, United States of America]	Not applicable. Removed term.
58335	8	8	8	10	I'd suggest that this statement could be borderline high confidence, as ESMs have pretty consistently shown that carbon-climate feedbacks are projected to have some discernible impact on GHGs (even if only modest) but that none show large or runaway changes. Presumably the medium confidence is based on medium evidence & high agreement? [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Add high confidence level
115317	8	8		10	This text seems to say that there is only medium confidence that carbon climate feedbacks 'do not lead to runaway changes over the next 100 years'. Based on my reading of the chapter there is no evidence of runaway feedbacks in the next 100 years, and there is no assessment suggesting that there might be. I would either delete this phrase, or if kept, then add assessment to the chapter on runaway feedbacks and add a separate confidence qualifier on the statement that there will not be runaway carbon climate feedbacks. [Gillett Nathan, Canada]	Accepted. Rephrase for clarity.
67433	8	10	8	10	change "do not" to "will not" [James Christian, Canada]	Accepted. Changed.
67435	8	10	8	12	"Large uncertainties remain on the possibility of additional feedbacks not represented in many current models" Not sure this conforms to IPCC guidelines for describing uncertainties. [James Christian, Canada]	Accepted. Changed.
127671	8	11	8	11	The reference to "fires" is excessively broad and non-specific. Some kinds of wildland fire, such as fires in natural grasslands, savannahs, and prescribed fires, generally emit little carbon and actually can stabilize existing terrestrial carbon pools. [Trigg Talley, United States of America]	Accepted. We have added other processes.
58337	8	11	8	11	A key additional feedback with large potential impacts are fine-scale permafrost processes, which could potentially double existing permafrost thaw GHG release estimates (Turetsky et al., 2019; 2020) and so boosts uncertainty - perhaps consider adding alongside fires as a key example [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted, added.
127673	8	14	9	3	Key messages are too long and too detailed. [Trigg Talley, United States of America]	Accepted. Some shortening done.
127675	8	16	8	16	"Medium evidence with high agreement ". Is medium confidence based on limited studies with high agreement? [Trigg Talley, United States of America]	Taken into account - The medium confidence is indeed the result of several studies being in high agreement. This has been updated.

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19911	8	16	8	16	Rather than "Medium evidence with high agreement", one should probably read "evidence with medium confidence, high agreement"? [philippe waldteufel, France]	Taken into account - this message has been reworded. Confidence language has been made consistent with the formal IPCC uncertainty guidance.
71661	8	16	8	16	What is "Medium evidence"? [Martin Manning, New Zealand]	Noted - Medium evidence is part of the IPCC calibrated uncertainty language vocabulary. The evidence that is considered in informing this statement is presented in the underlying sections that are referenced at the end of the paragraph. The confidence assessment and paragraph has been reworded entirely
18173	8	16	8	16	'Medium evidence' is unclear phrasing. I assume it relates to the 'medium confidence' assigned to the findings? I would suggest rephrasing. [Chelsey Baker, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - the statement has been reworded.
58801	8	16	8	18	The title sentence is bloated and the sentence structure is in the wrong order. For example, this could be changed to 'the near linear relationship between cumulative CO2 emissions and maximum global mean temperature increase caused by CO2 (for the range of temperatures included in the Paris Agreement) during the 21st century is underpinned by high agreemenet for the (medium level) or (moderate) evidence. The way 'Medium evidence' is used on the first line is not grammatically correct , without saying levels/amounts/volume etc. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected. We don't understand the proposed new sentence.
54977	8	16	8	18	Since the AR5, a key message to policy-makers has been that the relationship between cumulative CO2 emissions and maximum global mean temperature is approixmately linear and that this relationship is robust and well-established for a large range of cumulative emissions. Here a similar statement is given only medium confidencealthough the medium confidence seems to be tied to the validity of the linear relationship over specific temperature ranges. To avoid misunderstanding, we would suggest these separate messages be provided in separate conclusions: 1. the high confidence general conclusion about the near linear relationship between global warming and cumulative CO2 emissions (consistent with assessment statement on page 5-7 lines 53-55), and 2. a medium confidence statement specific to low global warming levels (consistent with conclusions on page 5-78 lines 29-31). Also, reference to the Paris Agreement requires more specificity; is this intended to refer to the range of global temperatures in article 2 (the long term global temperature goal of keeping warming to well below 2C?). [Nancy Hamzawi, Canada]	Taken into account - these two aspects have been separated. The reference to the Paris Agreement indeed refers to everything well below 2°C.
86733	8	16	8	18	The sentence is long and a bit difficult to understand, especially with the long term "maximum global mean temperature increase". Please consider rephrasing this sentence, making it easier to understand. [Oyvind Christophersen, Norway]	Taken into account - the statement has been reworded.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
58339	8	16	8	18	I'd argue that this statement too is borderline high evidence/confidence, given how many models this result emerges from and our theoretical Earth system understanding. There is a possibility that feedbacks may weaken the relationship (as noted l.24-25), but this is much less likely below the 2C Paris Target mentioned in the hedline statement. Furthermore, the subsequent paragraph (l.29-40) ranks using this relationship as the basis for quantifying a carbon budget as high confidence, which seems contradictory if the relationship itself only has medium confidence. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - the confidence levels for the respective changes have been reconsidered. Note that the subsequent paragraph can be higher confidence, because the assessment of the remaining carbon budget takes into account several aspects that would reduce the linear relationship of the TCRE (e.g. non-CO2 warming, ZEC, earth system feedbacks).
99035	8	16	8	20	This presentation suggests that the fate of CO2 is all that matters and seems to ignore the influences of other GHGs and of aerosols, and this is just not correct. Basically, the emissions of all GHGs need to get to zero. One could go to CO2e, but I don't think this would be helpful. And it needs to be said that this applies for both fossil fuel CO2 and CO2 released due to net deforestation, etc. I am also not convinced that there will not be further warming after this occurs given potential changes in the natural carbon cycle that could be triggered (e.g., the Amazon no longer pulling CO2 out of the atmosphere. I just think this statement needs to be made more carefully. [Michael MacCracken, United States of America]	Taken into account - the statement has to be seen in combination with the two subsequent statements, which explicitly highlight the issues indicated by the reviewer. Net zero CO2e would result in other climate outcomes than the ones described here, as discussed in Section 7.6 of Chapter 7.
79385	8	16	8	27	Instead of saying linear relationship maybe better to say that the global mean temperature change is proportional to the cumulative... What is the "maximum global mean temperature"? Should be global mean maximum temperature? Is the cumulative CO2 emissions refering only to CO2 emissions or to "equivalent CO2 emissions" (i.e., includes CH4)? [Alejandro Di Luca, Australia]	Partially accepted. Some rephrasing.
77265	8	16	8	27	This is very important information. As above it is assumed that the mass units refer to the molecule but can this be confirmed? [Emer Griffin, Ireland]	Taken into account - the units refer to the mass of C in the total emissions of CO2, as per standard scientific use. In the ES, units of PgC are used, consistent with the underlying chapter. In the TS and SPM, alternative units of GtCO2 can be used to link more directly to policy discussions.
34639	8	17	8	17	Should probably say "global mean surface air temperature" in this sentence. [Russell Vose, United States of America]	Taken into account - we can, although the relationship applies to both GMST and GSAT
96531	8	18	8	20	Two sentences are combined in way that it is not clear, what is meant: "...emissions to become zero, and no significant warming occurs..." Was it meant to say: "...emissions to become zero, leading to no significant warming occurs ..."? [Nicole Wilke, Germany]	Taken into account - This was unclear. The second part pointed toward the existence of ZEC, currently assessed to be zero in magnitude with a range of about +-0.2°C. This has been edited for clarity.
58877	8	18	8	20	"This relationship implies... and no significant warming ocurs afterwards." This statement claims that halting global warming requires zero net emissions and no significant warming afterwards. Does it mean "halting global warming requires... emissions to become zero, which will lead to no significant warming."? [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account - This was unclear. The second part pointed toward the existence of ZEC, currently assessed to be zero in magnitude with a range of about +-0.2°C. This has been edited for clarity.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
18175	8	18	8	20	The sentence ends 'and no significant warming occurs afterwards' - does this mean that if 'halting global warming requires global net anthropogenic CO2 emissions to become zero' then there will be no significant warming afterwards or does it mean that it assumes that no significant warming occurs afterwards? If the latter change to 'warming will occur afterwards'. [Chelsey Baker, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - This was unclear. The second part pointed toward the existence of ZEC, currently assessed to be zero in magnitude with a range of about +-0.2°C. This has been edited for clarity.
58285	8	19	8	19	Two instances of word "global" in the sentence reads awkwardly; consider using "climate" warming [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected - we intend to keep the terms "global warming" as "climate warming" is not a very often-heard term.
58341	8	19	8	19	I'd say little rather than no significant warming after net-zero - a lagged warming of ~0.1C or so seems likely after net-zero CO2 (excluding non-CO2 GHGs), which although small is arguably not insignificant. The possibility of further feedbacks resulting in additional gradual warming (l.23-27) also make "no significant warming" seem overly strong - I suggest "little significant warming" instead. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected - The AR6 assessment of ZEC suggests a value of zero, but with an uncertainty range. Highlight one side of this range over the other is not preferable.
34641	8	19	8	20	I don't think it's accurate to say "no significant warming occurs thereafter." [Russell Vose, United States of America]	Taken into account - This was unclear. The second part pointed toward the existence of ZEC, currently assessed to be zero in magnitude with a range of about +-0.2°C. This has been clarified.
103093	8	19	8	24	What is the difference between this section and the one before? Clarify, or delete [Philippe Tulkens, Belgium]	Taken into account - This section intends to provide the quantitative assessment. This has been clarified as the entire message was reworded.
67437	8	20	8	21	"The ratio between cumulative CO2 emissions and the global surface air temperature increase they cause is assessed to be in the likely in the 1.0–2.2°C per 1000 PgC range" The ratio of cumulative CO2 emissions to the global surface air temperature increase they cause is assessed *likely* to fall in the 1.0–2.2°C per 1000 PgC range [James Christian, Canada]	Taken into account - the entire paragraph was reworded in response to reviewer comments.
58803	8	20	8	21	Sentence grammar- change to 'The ratio between cumulative CO2 emissions and the resultant increase in global surface air temperature is assessed to be 1.0–2.2°C per 1000 PgC'. No need to say range as that is implicit from the the dash. 'in the likely in the' does not make sense, there is also '(high confidence)' at the end of the sentence so you don't also need 'likely' here. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account - the entire paragraph was reworded in response to reviewer comments.
2325	8	20		21	Please correct "The ratio between cumulative CO2 emissions and the caused global surface air temperature increase is assessed to be..." instead of "The ratio between cumulative CO2 emissions and the global surface air temperature increase they cause is assessed to be..." [Abed El Rahman Hassoun, Lebanon]	Taken into account - the entire paragraph was reworded in response to reviewer comments.
2181	8	20		21	Please correct: "The ratio between cumulative CO2 emissions and the caused global surface air temperature increase is assessed to be..." instead of "The ratio between cumulative CO2 emissions and the global surface air temperature increase they cause is assessed to be..." [Abed El Rahman Hassoun, Lebanon]	Taken into account - the entire paragraph was reworded in response to reviewer comments.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
18177	8	21	8	21	Remove one instance of 'in the' [Chelsey Baker, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change was made.
9287	8	21	8	21	Change "…they cause is assessed to be in the likely in the…" to "…they cause is assessed to be likely in the…" [Christine Weldrick, Australia]	Accepted - change was made.
19301	8	21	8	21	" … assessed to be in the likely in the …." should be "… assessed to be likely in the …" [Benjamin Lamptey, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change was made.
127677	8	21	8	21	Change "they" to "the". [Trigg Talley, United States of America]	Taken into account - this statement was revisited
19913	8	21	8	21	Typo in the middle of the line [philippe waldteufel, France]	Accepted - change was made.
112591	8	21	8	22	Seems odd not to give a central estimate when one is given for both ECS and TCR. Check internal consistency of TCRE, TCR and AGWP_CO2 estimates (see Allen et al, 2018, for the formulae). I think they are OK. [Myles Allen, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - The TCRE assessment is fully consistent with the ECS and TCR assessments, but also depends on the assessment of AF. Statements in the ES are made consistent with the available information on central estimates in the underlying chapter.
103095	8	21			"is assessed to be likely …" [Philippe Tulkens, Belgium]	Accepted - change was made.
58805	8	22	8	22	Sentence grammar - change to 'This is a slightly smaller range than the 0.8–2.5°C per 1000 PgC assessment made in AR5' [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account - the entire paragraph was reworded in response to reviewer comments.
65709	8	23	8	25	For clarity, suggest changing to: "Additional Earth system feedbacks that operate on century timescales, such as permafrost thawing, have the potential to alter the linearity of the cumulative carbon- climate relationship." since the original wording may be read as meaning the feedbacks would weaken the relationship itself. [Kushla Munro, Australia]	Taken into account - the entire paragraph was reworded in response to reviewer comments. It is indeed correct that in the TCRE framework, the long-term response of permafrost would be taken up by ZEC, while the TCRE remains broadly linear.
99037	8	23	8	27	Given this statement, which seems plausible, how is it justified to then be saying on lines 19-20 that "no significant warming occurs afterwards"--why include this given the risk indicated below. [Michael MacCracken, United States of America]	Taken into account - the central assessed value of the ZEC is zero. This has been clarified
2183	8	23			Could you please mention few different lines of evidence between parentheses? [Abed El Rahman Hassoun, Lebanon]	Taken into account - The sentence was reworded with indications of the better integration of evidence from various chapters.
9289	8	24	8	24	Add the word "the" to the following "…to weaken the liearity of [the] cumulative carbon…" [Christine Weldrick, Australia]	Accepted - The entire paragraph was reworded in response to reviewer comments.
58287	8	24	8	24	Permafrost "thaw" rather than "thawing" [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
58807	8	25	8	27	It is not clear what section this sentence is attempting to summarise.Is the writer trying to say 'This could result in additional warming after net zero CO2 emissions are reached' ? Is it a follow up sentenceto the previous one referring specifically to the permafrost feedbacks in 5.4.8.2 or is it more general to all feedbacks? Is it talking about the strenthening of the feedback in models on century timescales 5.4.9? Is it trying to discuss ths concept of overshoot in 5.5.1.2.3? These are already part of the summary on page 7. The reference for this paragraph is 5.1, in that section it says the permafrost feedback is not in many of the models and 'terrestrial carbon cycle feedbacks (such as the permafrost carbon feedback) have the potential to break both the linearity and pathway independence of TCRE'. Again this is already part of the summary on page 7. 'Path dependency' is also mentioned here but this concept is not directly introduced or explained elsewhere in the chapter, can terminology in common with the rest of the chapter be used instead? [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - references to section 5.4 have been included. Section 5.1 is the introduction, which provides an overview of the other aspects that are assessed in the chapter. Section 5.5.1, which provides the assessment underlying this statement cross-references to Section 5.4
21769	8	29	8	40	It is unclear whether these have used the updated estimates of GSAT changes to date arising from chapter 2 as the starting point. Regardless, given the major role that this updated estimate will have on this metric I would expect its role in the difference from AR5 to be mentioned in this bullet. [Peter Thorne, Ireland]	Taken into account - Estimates are fully consistent with the Chapter 2 and Chapter 3 estimates of GSAT and human-induced warming. A dedicated box (Box 5.1) in the chapter discusses the impact of methodological improvements and updates since AR5.
34643	8	29	8	40	Since the CO2-global temperature relationship is near-linear anyway, the 1.7C statistics could be dropped from this key message to simplify things a bit. [Russell Vose, United States of America]	Noted - The choice of temperature levels is a function of space as well as not being implicitly policy prescriptive by only providing data for 1.5°C and 2°C.
86617	8	29	8	40	These remaining budgets are for GSAT. You might want to provide GSAT and GMST budgets for full transparency. [Pierre Friedlingstein, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - GMST and GSAT estimates of historical warming are now identical bar a difference in surrounding uncertainties (Box 2.3). For TCRE estimates no solid GMST estimates have been published in the literature and as the Chapter 2 assessment of GMST and GSAT did not conclude on a translation factor between both, the assessed budgets would be similar.
86619	8	29	8	40	This remaining budget will look totally inconsistent with observations of historical GMST and C budget. Such as : historical emissions 2363 GtC, historical warming 1°C. Remaining budget for an additional 0.5°C would be expected to be about 2363/ = 1180 GtCO2. That is much much larger than the numbers reported here 310 (66%), 390 (50%) GtCO2. You need to explain why you believe this is right ! [Pierre Friedlingstein, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - The Chapter 2 assessment of GMST and GSAT highlights that the difference is not as clear cut

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
77267	8	29	8	40	This is very important information. Can some statement be included about the scale of negative emissions removals that may be required e.g. relative to current emissions or current uptake by terrestrial or ocean systems? [Emer Griffin, Ireland]	Rejected - the amount of negative emissions required depends on the level to which society initially fails to stay within the budget. This is a value judgment and also requires the WG3 assessment of societal trends and drivers. This cannot be taken up in the WG1 Chapter 5 ES.
103097	8	29	8	53	Please do not switch units: previously, all CO2 was presented in PgC. Here this changes into GtCO2. it will be ok to have GtCO2 in addition, in parenthesis, but there needs to be one standard unit [Philippe Tulkens, Belgium]	Accepted. Greater care has been taken to use consistent units where possible and to be clearer on why different units are used where necessary.
115319	8	29		31	This sentence does not need a confidence qualifier. [Gillett Nathan, Canada]	Accepted
18189	8	31	8	31	Should pre-industrial time refer to 1750 rather than 1850? Throughout the chapter the different sections seems to flit between 1750 and 1850. I think more precise definitions may be required to clarify the intended meaning of the different years. [Chelsey Baker, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - a harmonized definition and use of the term pre-industrial is applied throughout the report. In this case, the preindustrial proxy period of 1850-1900 is taken.
114673	8	31	8	31	Based on LAM3 discussions, I think you rather coudl write 1850 and not labelling it pre-industrial [Jan Fuglestvedt, Norway]	Accepted
15403	8	31	8	32	Historical CO2 emissions are shown for different periods and confusing. The periods are 1850-2019 in Executive Summary, 1750-2018 in 5.1 and 5.2.1.5 with Table 5.1, and 1875-2019 in Table 5.8 Footnote (2). These should be revised in a consistent way. [Junichi Tsutsui, Japan]	Taken into account - the ES has been streamlined to minimize confusion. However, the use of different historical periods is unavoidable.
15515	8	31	8	32	Re: Since pre-industrial times (1850), a total of 2,363 Gt (645 ±65 PgC) of anthropogenic CO2 has been emitted. However, according to Table 5.8, note (2): Historical CO2 emissions since the middle of the 1850-1900 reference period (mid-1875) until and including 2019 are estimated at 2120 GtCO2. The figure 2363 Gt cannot be located in the main text. Please consider harmonizing the use of reference period and making corresponding adjustment to the historical CO2 emissions. [SAI MING LEE, China]	Accepted - all numbers have been double-checked with values in the underlying chapter.
52661	8	31	8	32	Please clarify for which period exactly is this amount of cumulative CO2 emissions reported (i.e. until which year), and does it include fossil fuel and land use change emissions? [Katarzyna Tokarska, Switzerland]	Taken into account - the budgets do not come with an end year, as this depends on the rate at which emissions are emitted. It has been clarified that this applies to all CO2 emissions.
51115	8	31	8	33	If the 'remaining carbon budgets from 2020 for limiting warming to 1.5C' can't be considered without consideration of the upper range of ZEC for low emissions scenarios, it would be worth flagging it in this summary. Otherwise this statement does not seem to be consistent with "SPM Page 23 lines 12-21". [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - in no instance does the text suggest that the upper range of ZEC should be considered exclusively for low emissions scenarios. The assessed value of ZEC is zero, with a likely range of +- 0.18°C. SPM page 23 lines 12-21 provides estimated warming for the SSP1-1.9 scenario, with a range of 1.2-1.8 °C of warming and hence a central estimate of 1.5°C. Cumulative CO2 emissions in SSP1-1.9 until net zero are in line with the budgets highlighted here, see SPM Box 2

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
112585	8	31	8	34	Need to clarify that these budgets are for an additional warming above an assessed 2020 level, because percentiles do not include uncertainty in cumulative emissions to date. The assessed warming level in 2020 is 1.25°C (1.1°C plus 5 years of warming at 0.25°C per decade -- note the current rate of human-induced warming is a highly policy-relevant quantity that, as far as I can tell, is missing from the assessment). [Myles Allen, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - this has been clarified
99039	8	31	8	40	On line 31, is four-figure precision really justified here--why is the uncertainty only in the parentheses? Overall, this all seems far too precisely stated given the ranges in climate sensitivity, understanding of carbon fluxes with global warming, wild-fire probability, climate variabillity, etc. I would also note that the investment and infrastructure communities want estimates of the worst plausible outcome for the due-diligence studies that they undertake, and giving the mean and one sigma only results is not the information needed for proper studies. As Greta Thunberg makes clear, she does not want to livei in 50% or even 66% chance world--she wants the assurance of being able to withstand extreme weather will continue to be at the once in a hundred years as has been used to build the stable environment that we now enjoy. NOTE: REading the next finding makes clear that there really are uncertainties, etc.--it seems to me it would hlep in this finding to indicate that these numbers are central estimates and that, as explained in the next finding, there are significant uncertainties. Basically, the two findings need to be better linked. [Michael MacCracken, United States of America]	Taken into account - while we cannot link probabilities in global climate outcomes to return rates of extreme events, the ES message now better connects to the surrounding uncertainties.
52663	8	32	8	33	Please clarify that these estimates are subject to additional uncertainties (e.g. the earth system feedbacks and other sources of uncertainity listed in the table 5.8 are not included in the central estimates). [Katarzyna Tokarska, Switzerland]	Accepted - the uncertainties are now better linked to the remaining carbon budget estimates
52665	8	32	8	33	It would be good to indicate an approximate level of accuracy in estimating remaining carbon budgets cannot be higher than +/- 50 GtCO2 or similar (see Tokarska et al. in review for more details). (Reference: Tokarska et al., Uncertainty in carbon budget estimates due to internal climate variability (in review at ERL)). [Katarzyna Tokarska, Switzerland]	Rejected - while this is included in the chapter discussion, it goes beyond the high-level information that can be provided in the ES. The limited accuracy in determining TCRE due to internal variability is taken into account in the assessment of TCRE.
127679	8	32	8	35	This highlight should try to clarify CO2 or CO2eq earlier on. It's not clear until the following highlight that this is referencing CO2, not CO2eq. Throughout Chapter 5, the authors have worked hard to caveat the remaining carbon budget concept with uncertainties from climate-carbon feedbacks and CO2 vs CO2eq, but there are opportunities to try to make this clearer. [Trigg Talley, United States of America]	Taken into account - the ES always explicitly states the gas to which the assessment or values apply.
127681	8	33	8	33	[PRECISION] Why is a threshold of 1.7°C added? Other chapters are only focusing on 1.5 and 2°C thresholds. [Trigg Talley, United States of America]	To not present 1.5°C or 2°C as a binary choice.
4381	8	33	8	35	"for the same temperature targets" may be misinterpreted, "for these temperature targets" might be clearer [Ana Bastos, Germany]	Rejected - we consider the original wording to be sufficiently clear
41593	8	33	8	38	In ll. 33 - 35, it is not clear what is meant by "a probability of at least 50 %", but it is well explained in ll. 37 - 38. This should be changed. [Katharina Meurer, Sweden]	Taken into account - the text was revised accordingly

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
103099	8	33			possibly misleading, as SRM is not _meant_ to reduce CO2 concentrations. Maybe clarify: "Biochemical implications of Solar Radiation Modification (SRM) are likely to …" [Philippe Tulkens, Belgium]	Accepted. Made it clear, rephrased. However, it is important the reader understand it is a different type of mitigation not affecting much the GHG trends.
58809	8	35	8	36	This sentence is vague, why not briefly list the methodological improvements? [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account - The ES for the entire chapter has to fit on no more than 2 IPCC pages and space is thus extremely limited. This statement has reconsidered all information that is provided and includes now clarifications where possible.
81019	8	36	8	36	Perhaps the 2018 data for CO2 emissions can be updated priro to publication with the latest informationavailable, particulalrly given the likely reduction in annual emissions due to COVID-19. [Jeffrey Philip OBBARD, Singapore]	Taken into account - the data was updated to the latest historical year available.
52667	8	36	8	36	Please clarify that 40 GtCO2 is the annual emissions rate (I think the units should be GtCO2 per year) [Katarzyna Tokarska, Switzerland]	Accepted
67439	8	36	8	38	"Starting from 2018 global CO2 emissions of 42 GtCO2 (11.5 ±0.9 PgC) and following a linear downward trajectory from today onwards, the values for a 50% probability of limiting warming to 1.5°C, 1.7°C or 2°C correspond to reaching net zero in about 20, 35, and 55 years, respectively. If a specific remaining carbon budget is exceeded, carbon dioxide removal will be required to return warming to a certain temperature level." Starting from 2018, for global CO2 emissions of 42 GtCO2 (11.5 ±0.9 PgC) and assuming a linear downward trajectory to net zero, net zero would need to be reached in about 20, 35, and 55 years for a 50% probability of limiting warming to 1.5°C, 1.7°C or 2°C, respectively. If the estimated remaining carbon budget is exceeded, carbon dioxide removal will be required to limit warming to the specific target temperature. [James Christian, Canada]	Taken into account - the statement was revised for clarity.
15517	8	36	8	38	Re: The statement "Starting from 2018 global CO2 emissions …… reaching net zero in 20, 35, 55 years". The conclusion cannot be found in the main text. Please consider revising the main text or the Excecutive Summary. [SAI MING LEE, China]	Accepted - The underlying text has been added to the chapter text and the ES has been made fully internally consistent.
74179	8	36	8	38	This sentence is formulated confusingly: the “values” of what? Maybe replace with “rate of emission decrease” [Christoph Völker, Germany]	Taken into account - the statement was revised for clarity.
65711	8	36	8	40	The Paris Agreement states two warming objectives and for simplicity the IPCC should be consistent. The report discusses three temperature thresholds. [Kushla Munro, Australia]	Rejected - The Paris Agreement includes one long-term temperature goal referring to both a "well below 2°C" and a "1.5°C" level. In order not to presuppose what "well below 2°C" means, this chapter provides information for three warming levels in that range.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
65713	8	36	8	40	For clarity, suggest changing to: "Starting from 2018 global CO2 emissions of 42 GtCO2 and following a linear downward trajectory from today onwards, the values for a 50% probability of limiting warming to 1.5°C or 2°C correspond to reaching net zero in about 20 or 55 years, respectively. If a specific remaining carbon budget is exceeded, carbon dioxide removal would be required to return warming to a certain temperature level." [Kushla Munro, Australia]	Taken into account - the statement was revised for clarity.
18179	8	37	8	37	'from today onwards' is vague. From 2018 or 2020? Be specific. [Chelsey Baker, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - the statement was revised for clarity.
81021	8	37	8	37	Perhaps the reference tyo a 'linear downward trajectory' of GHG emissions needs to be quantified. What is the year-on-year reduction required to reach the probabilities referred to? [Jeffrey Philip OBBARD, Singapore]	Rejected - the year-to-year reduction equals the start emissions divided by the number of years until net zero.
127683	8	37	8	37	How is the chapter defining "today"? [Trigg Talley, United States of America]	Taken into account - the statement was revised for clarity.
86735	8	38	8	38	Please specify what is meant by net zero e.g., in a parenthesis such as "(total uptake equal total emissions)" [Oyvind Christophersen, Norway]	Rejected - unclear what this comment refers to.
58343	8	38	8	38	20 years to net-zero for 1.5C will certainly be noticed as a significantly shorter time than in SR1.5, which gave ~30y (2018-2050) to reach net-zero to keep to 1.5C. I believe a key difference here is that this is a simple linear trajectory drawn to zero based on carbon budgets and their associated probabilities, whereas SR1.5's net-zero date is based on more complex scenario modelling, but as the overall carbon budgets have now been revised higher the now shorter time appears to be somewhat surprising Another incomparability is that the SR1.5 net-zero time for 1.5C didn't specify if it was for 50 or 66% probability of success (whereas the 2C net-zero time of ~50y was stated as 66%, and so is similar to the ~55y for 50% chance given here), making it unclear quite how to compare these differing estimates. Of course the likely answer is that they're not entirely comparable and shouldn't be used in the same way, but that almost certainly won't stop readers comparing them anyway and taking the difference as a headline result from this report. To avoid this, I suggest that if these time calculations are included here it's also worth briefly spelling out how comparable or not these numbers really are to SR1.5 and what the key differences are between them (especially as this time calculation doesn't seem to appear in the main text itself). [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account - information was added in the main text. Note that the SR1.5 timing is for scenarios with limited or no overshoot (of up to 0.1°C) and also does not consider a linear trajectory.
96533	8	38	8	40	Please delete sentence, because it is policy-prescriptive. It also implies that the carbon budget can be precisely specified and that feedbacks are perfectly understood - this is not the case however. [Nicole Wilke, Germany]	Rejected - the sentence was instead edited for it not to prescribe a single policy implication.
52671	8	39	8	39	Please note that carbon dioxide removal may also offset positive emissions towards the net-zero target. I would suggest re-phrasing it. [Katarzyna Tokarska, Switzerland]	Accepted - the word "net" was included before CDR
111849	8	39	8	39	CDR will be needed anyway (to reach net zero), therefore better to write "net negative emissions will be required" (or "net negative emissions through CDR…") [Oliver Geden, Germany]	Accepted
58625	8	40			It could be wise to refer to the TCRE Figure 5.31 [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account - generally only sections are cited in the line of sight of ES statements. If acceptable, also figure and table references can be included.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
78463	8	42	8	42	what does “medium confidence” refer to here. It is surely very certain that all these processes will “affect the precise value” of carbon budgets. The sentence has two halves – be clear which the confidence assessment refers to [Chris Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - this has been revised
18181	8	42	8	45	A very informative and clear statement. [Chelsey Baker, United Kingdom (of Great Britain and Northern Ireland)]	Accepted
109585	8	42	8	45	Suggest adding CO2-response of the land carbon sink to this list of modifiers. It’s a substantial uncertainty that will alter “allowable” emissions. [Anthony Walker, United States of America]	Rejected - while not untrue, we can only highlight what is assessed in the chapter.
54979	8	42	8	45	Is it the authors' intent that the medium confidence here apply to both parts of the sentence? As written, it seems to and yet the second part of this sentence, the "need for global CO2 emissions to decline to net zero to halt global warming" is expressed elsewhere in the Ch. 5 executive summary (lines 18-20) as a statement of fact. [Nancy Hamzawi, Canada]	Taken into account - this has been revised
19915	8	42	8	45	As it is, the "medium confidence" appreciation applies to the whole sentence [philippe waldteufel, France]	Taken into account - this has been revised
34893	8	42	8	54	The SOD indicates that CO2 emissions must go to net zero to halt global warming. Any serious consideration of net zero will show that it is technically unachievable, economically unaffordable and socially undesirable. The SOD statement assumes the models are correct, which they are not, even by its own admission. Please see comment #14 above. [Jim O'Brien, Ireland]	Rejected - This chapter and this IPCC Working Group assesses the physical science basis and hence makes no statement about the technical achievability of emissions reductions. The statements in this ES speak to the geophysical requirement of reaching net zero CO2 emissions. This assessment assumes models are useful tools and one of the lines of evidence we can draw on to understand the reality that surrounds us.
67441	8	42	8	54	Each occurrence of "non-CO2" in this paragraph could be changed to "non-CO2 greenhouse gas" (see also 9/28) [James Christian, Canada]	Rejected - The term non-CO2 emissions denotes more than only non-CO2 greenhouse gases. It also refers to aerosols and aerosol precursors.
103101	8	42	8	54	It is quite unclear what authors wish to say here. Maybe: Further factors have been investigated and may also contribute to the carbon budget, but currently no conclusive quantification is possible. OR is this rather on uncertainties? [Philippe Tulkens, Belgium]	Taken into account - this has been revised

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
67443	8	42	9	3	At a minimum I would change "variations" to "uncertainty" and delete "median" and "of this overall range". But this is one case where I have actually overhauled the whole paragraph. "There is medium confidence that several factors affect the precise value of carbon budgets, including estimates of historical warming, future emissions from thawing permafrost, and uncertainty in projected non-CO2 GHG emissions. These uncertainties do not change the basic conclusion that global CO2 emissions must decline to net zero to halt global warming. Geophysical uncertainties related to the climate response to non-CO2 GHG emissions and the transient temperature response to cumulative CO2 emissions result in an uncertainty of at least ±300 GtCO2 in the estimates of remaining cumulative emissions. Uncertainties in the level of historical warming result in a ±450 GtCO2 uncertainty, and estimates may vary by ±250 GtCO2 depending on the amount of warming caused by past and future non-CO2 GHG emissions. The combined effect of all additional Earth system feedbacks – included in the estimates of the remaining carbon budget – is assessed to result in a reduction of the remaining carbon budget of about 135 GtCO2 per °C of additional warming relative to the recent past (2010–2019), with a 1-sigma range of ±135 GtCO2. Release of CO2 from thawing permafrost alone is estimated to be responsible for about 75 GtCO2 (±50 GtCO2, 1-sigma range) per degree of additional warming. There is very low confidence in the estimates of the size of these contributions. Despite the large uncertainties surrounding the understanding and quantification of the impact of these processes, they represent identified additional risk factors that scale with additional warming and mostly increase the challenge of limiting warming to specific temperature thresholds." [James Christian, Canada]	Taken into account - The suggested improvements were considered in the revised ES message.
18659	8	42	9	3	This paragraph is policy relevant but I find it too hard to understand. Simple and clear messaging would be useful. [Govindasamy Bala, India]	Accepted - The text has been edited for clarity
15519	8	46	8	54	Re: the assessment of the size of these contributions has very low confidence. Please re-consider whether the figures with very low confidence should be presented in the Executive Summary. [SAI MING LEE, China]	Taken into account - information on the impact of permafrost is a recurring demand and providing a statement, even if with low confidence, is thus deemed appropriate to inform the state of knowledge on this issue.
18657	8	47	8	47	The 300 PgCO2: Does this correspond to limiting warming to 1.5 or 2 deg C? Or per degree of warming? Please specify here. [Govindasamy Bala, India]	Accepted - this has been clarified
72895	8	48	8	48	Text does not make sense: 'on median the amount of warming'. I suspect it should be 'on the median amount of warming'. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - this has been edited and clarified
19917	8	48	8	48	Spurious "median" apparently [philippe waldteufel, France]	Accepted - this has been edited and clarified

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
86623	8	49	8	50	As mentioned in the SPM, I would strongly suggest NOT to include ESM feedbacks in the numbers provided. They operate on very long time scales and hence make the simple estimate of remaining years (assuming current emission) not valid anymore. I would mention them as additional source of carbon, that might continue to operate on lmonger time sacles, even after emissions cease. [Pierre Friedlingstein, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - ESM feedbacks are an integral part of how the Earth system responds to anthropogenic disturbances and therefore an integral part of any assessment of the remaining carbon budget. Currently, these additional Earth system feedbacks are already considered in the way suggested here, i.e. as an additional source of carbon that is reduced from the available remaining carbon budget.
86625	8	51	8	52	There is no clear explanation on how this 135GtCO2 is coming from. Text in section 5.5.2 is opaque. 75GtCO2 from permafrost (no reference given), 35GtCO2 from CH4-lifetime (process not described, why is this in the ESM feedbacks and not in the non-CO2 forcing, why is ity positive although it's negative in figure 5.28, no reference given…), Then another 100GtCO2 comes from figure 5.28 (which does not give any estimate in GtCO2). How does all of these gives 135 GtCO2? It seems made up sorry. I would strongly advise taking this numbers OFF the remaining C estimate, we just don't know enough on what this number is to include it in such a policy relevant quantity. [Pierre Friedlingstein, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - the chapter text now further elaborates how these values are derived from section 5.4's assessment of Earth system feedbacks.
89465	8	51	8	54	This is another statement about use of uncertainty language. The 1 sigma range is presented here, and so it would be possible to assign 'likely' to your range of estimates shown here. Assigning a very low confidence statement seems to reduce the impact of this ES statement. When you present sigma ranges for some model estimates, it would seem that you have some confidence in your assessment, beyond the absolutely lowest level. Please consider that these statements are boiled down for the SPM and so communicating that a lot of work has been done to estimate the reduction in potential carbon emissions from human sources to account for natural feedbacks. In my view, this ES should be strengthened; the medium confidence statement at the top does help but I'm not sure it needs to be weakened by the very low confidence statement at the end. [Edward Schuur, United States of America]	Accepted. We have changed to low confidence.
72897	9	5	9	5	Capital 'I' for 'implications' [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change was made.
111851	9	5	9	33	Quite often, it is not easy to detect if these paragraphs do talk about net negative emissions or gross CDR. Would be better to distinguish clearly, since volumes of CDR simply to reach net zero are quite substantial, so it's definitely not the same (as described in 5.6) [Oliver Geden, Germany]	Accepted. Paragraph rewritten for clarity and gross versus net made clearer
96535	9	7	9	24	It seems like the two paragraphs partly double the same statement. It might be possible two combine these two paragraphs. [Nicole Wilke, Germany]	Accepted. Still leave two paragraphs but eliminate repetition.
67445	9	10	9	10	change "degassing" to "outgassing" [James Christian, Canada]	Accepted. Changed.
34645	9	12	9	12	This sentence would be clearer if you drop the phrase "is largely independent of the magnitude and rate of CDR but." [Russell Vose, United States of America]	Accepted. Paragraph rewritten, sentence dropped.
69763	9	12	9	16	replace 'of' by 'on' in 'is largely independent of the magnitude and rate' [Gyami Shrestha, United States of America]	Accepted - change was made.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
86737	9	14	9	17	Quotation:"Due to asymmetries in the climate-carbon cycle response, CO2 emissions are more effective at raising atmospheric CO2 than CO2 removals are at lowering atmospheric CO2, particularly for large emissions/removals (>100 PgC). This asymmetry implies that an extra amount of CDR is required to offset a positive emission of a given magnitude (low confidence). {5.6.2.1, Figures 5.32, 5.34, 5.36}". Comment: This could easily be misinterpreted, and might feel counter-intuitive for some policymakers. Firstly, CO2 emissions will partly be absorbed by land -and ocean sinks, and only the residual amount of CO2 will reach the atmosphere. CDR removals from the atmosphere will be in absolute numbers? Please consider to be more specific when describing CO2 emissions and concentrations in the start of your statement. Secondly, please also consider to describe the similar situation, but then under the assumption that CO2 concentration has stabilized and that it is net-zero emissions due to a balance between emissions and removals, as in the Paris Agreements long term global goal. In such a case we assume that the extra amount mentioned in the sentence is not relevant? This is also connected to the statement from SR1.5 which said that historical emissions up until today alone are unlikely to cause global warming of 1.5°C (see SR1.5 Section A.2). Please consider to be more nuanced and you might need to distinguish between a situation when net zero is reached, and a situation where CDR is used to compensate for earlier emissions (e.g. in overshoot scenarios). [Oyvind Christophersen, Norway]	Accepted. We have rewritten the entire paragraph to be more clear and address your concerns.
67447	9	15	9	17	The last two sentences basically repeat the same point; could be streamlined/condensed. [James Christian, Canada]	Accepted. One removed.
78465	9	16	9	16	can you quantify this asymmetry here? E.g. is it 1%, 10%, 50% etc? [Chris Jones, United Kingdom (of Great Britain and Northern Ireland)]	We would need to add too much detail to quantify the results, but we have added additional models and text in the main section 5.6.
69765	9	16	9	16	define or specify how much 'extra' is needed in 'is largely independent of the magnitude and rate' [Gyami Shrestha, United States of America]	We would need to add too much detail to quantify the results, but we have added additional models and text in the main section 5.6.
2769	9	16	9	17	I assume ocean-based CDR is removal of dissolved CO2 from the ocean. I believe that removal of CO2 from the ocean will mean that it remains a CO2 sink. Hence the line "(e.g., degassing from the ocean)" does not apply to ocean-based CDR. If it were atmospheric CO2 removal, then the ocean would convert from a sink to a source or atmospheric CO2 so land based CDR does have this issue. It would be useful to provide an example of Ocean redistribution (e.g. release of CO2 from sediments). [Stephen Wilkinson, United Arab Emirates]	Accepted. Degassing removed.
34647	9	19	9	24	This key messages seem somewhat redundant with the key message on page 9 lines 7-17. [Russell Vose, United States of America]	Accepted. Removed.
78467	9	19	9	24	this paragraph just seems to duplicate the one above re asymmetry? Suggest you delete the end of previous paragraph (lines 13-17) and keep this as a distinct point [Chris Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We have consolidated the information, still two paragraphs with no repetitions.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
127685	9	19	9	24	[RISK] This paragraph seems to contradict itself. First it says that the nonlinearity is only seen in pulse emissions/removal greater than 100 PgC, then it says that "The asymmetry originates largely from state-dependencies and nonlinearities in the ocean and will require proportionally larger removal efforts as the reliance on CDR methods increases" ignoring the fact that CDR cannot be implemented as a single, huge pulse. [Trigg Talley, United States of America]	Accepted. Paragraph rewritten.
2771	9	19	9	24	The key thing here is "pulse" unless there is evidence of damage to the system the equilibrium location should not change. I'm assuming that if we were to remove all CO2 released from the atmosphere since preindustrial times across 150 years that we would reach pretty much the same atmospheric CO2 levels, but if we tried to do it in a year then we are likely to cause the release of previously stored CO2. It this is the key point of this statement it would be useful to get an idea of the timescales in the model... i.e. how long is a 100PgC pulse removal/emission. How long until a second "pulse" would be symmetric. It would be good to state in this section what a pulse is. [Stephen Wilkinson, United Arab Emirates]	Thank you. We have now included more models doing pulse to understand better dynamics. Paragraphs rewritten.
18661	9	19	9	24	The "asymmetry" discussion in the previous paragraph could be merged here. It looks odd when I read now- the same concept is discussed in 2 places. [Govindasamy Bala, India]	Accepted. We have rewritten paragraphs and eliminated repetition.
131511	9	21	9	21	Define "pulse emissions"; if the term is used across different chapters, consider adding it to the Glossary. [Hans Poertner and WGII TSU, Germany]	Accepted. Submitted to Glossary.
58627	9	32			It could be wise to refer to the Figure 5.36 [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Added.
8869	9	33	9	34	Need a line of sight for this liklihood statement. I am not sure if a liklihood can be ascribed to this statement...perhaps it should be in terms of confidence level. On page 101, the statement is "Compared to a scenario of unmitigated anthropogenic emissions, SRM reduces the burden of atmospheric CO2 by enhancing global land and ocean sinks." but does not have a confidence level attached to it. [Vaishali Naik, United States of America]	Accepted. Confidence statement provided.
81569	9	33	9	36	It is necessary to indicate the order of magnitude of the reduction and to qualify this statement. SRM will not stopp CO2 to increase in the atmosphere under continued anthropogenic CO2 emissions. [Fortunat Joos, Switzerland]	Accepted. Included your sentence.
99041	9	33	9	36	SRM would also very likely reduce CO2 emissions be reducing the demand for air conditioning, both to cool the temperatures being experienced and to dehumidify the air, this latter action requiring well more than an order of magnitude more energy that simpl cooling dry air. SRM would also likley place ecosystems under less stress and so counteract increasing emissions from the biosphere, permafrost, etc. [Michael MacCracken, United States of America]	We agree. Chapter doesn't cover human energy demand issues.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
67449	9	33	9	38	This last item is very confusing and needs to be substantially rewritten. It is not clear what is meant by "reduces the negative impacts of warming on ocean carbon uptake". To first order, SRM will increase ocean uptake by cooling the surface ocean, although it is possible (or even likely) that the major driver of ocean-atmosphere flux will be changes in atmosphere concentration caused by effects on the terrestrial biosphere. "return of positive and negative effects on carbon sinks" is equally vague. Effects of diffuse/direct sunlight on terrestrial carbon balance should possibly be mentioned (e.g., Cheng et al 2015 Agricultural and Forest Meteorology 201: 98–110). [James Christian, Canada]	Accepted. Rephrase both problematic sentences.
68277	9	33	9	38	The emphasis should be on the limited benefit of SRM on ocean acidification. See excerpt from the Climate Science Special Report, Fourth National Climate Assessment (NCA4), Volume I (2017) ("14.3. … An important limitation of SRM is that it would not address damage to ocean ecosystems from increasing ocean acidification due to continued CO2 uptake…."). [Durwood Zaelke, United States of America]	Partially accepted. Some rephrasing, but the scope of the chapter is not on impacts on biological systems, that is WGII
41985	9	33	104	6	Chapters 5 and 8 use the term 'SRM' without distinguishing the large differences between sulphur injected into the stratosphere and sea salt injected into the troposphere. Most of the negative points relate to stratospheric sulphur. Marine cloud brightening in the troposphere does not affect ozone and does not affect the solar input to crops on land even though sea surface temperatures have a strong effect on temperatures over land. [Stephen Salter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Section 5.6.3 now differentiates between sulphur and sea salt injection where relevant.
89435	9	33			Underlying evidence base is very weak -based on a single study with questionable assumptions. Outcomes are not generalisable. See comment on 5.6.3.3 [Carl-Friedrich Schleussner, Germany]	Accepted. More models added.
19515	9	34	9	34	after climate change add " impacts" [Hamideh Dalaei, Iran]	Rejected. The mandate of the chapter is impacts on climate only. WGII covers to ecosystems
5795	9	36	9	36	To be more accurate and to be consistent with Chapter 5 (page 101) and Chapter 4 (page 87), I recommend the "sudden and sustained termination" of SRM [Jesse Reynolds, United States of America]	Accepted. Changed.
8871	9	36	9	36	There is high confidence that SRM will not counteract ocean acidification based on text on page 10. [Vaishali Naik, United States of America]	Accepted. Added.
51117	9	36	9	37	"The rapid termination of SRM, if required" - suggest deleting "if required". SRM might be terminated for a number of reasons which may not be deliberate, or related to "requirements" [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Changed. Increase level of confidence
74181	9	36	9	37	Sentence could be clearer and avoid having "rapid" twice: "Any sudden termination of SRM, for whatever reason, would cause rapid increase.." [Christoph Völker, Germany]	Accepted. Rephrased.
109587	9	36	9	38	It's not clear why the confidence in the positive effect of SRM are of medium confidence while the negative effects associated with rapid removal of SRM are low confidence. It seems logical to assume equal confidence to each side of what is effectively the same process. [Anthony Walker, United States of America]	Accepted. Changed. Increase level of confidence
96537	9	36	9	38	Is the "rapid increase in global warming" here really low confidence? [Nicole Wilke, Germany]	Accepted. Changed. Increase level of confidence

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
132197	9	36	9	38	"The rapid termination of SRM, if required, would cause rapid increase in global warming with return of positive and negative effects on carbon sinks (low confidence)": Why write "if required". It may not be required but it may happen nonetheless, because e.g. 1) the country potentially doing SRM becomes disfunctional, or 2) the SRM infrastructure is damaged for some reason. Maybe replace with "If SRM were to be implemented and then suddenly terminated for any reason, this would cause rapid ..." [Sonia Seneviratne, Switzerland]	Accepted, rephrased.
132199	9	36	9	38	"The rapid termination of SRM, if required, would cause rapid increase in global warming with return of positive and negative effects on carbon sinks (low confidence)". Why set this sentence at "low confidence". A rapid increase in global warming would be "extremely likely" given the short atmospheric lifetime of sulphate aerosols. The rest of the sentence is so vague ("with return of positive and negative effects on carbon sinks") that it could also be set at "extremely likely" or even a statement of fact. [Sonia Seneviratne, Switzerland]	Accepted. Change level of confidence to very high confidence.
52669	9	37	8	38	Please note that thevalues (for years until net-zero emissions) are not explicitly mentioned anywhere else in this chapter, and it may be confusing how they were estimated. Please clarify. [Katarzyna Tokarska, Switzerland]	misplaced comment, incorrect numbering. We cannot identify what it refers to.
131515	10	1	10	1	Rename 5.1 Introduction and Paleo Context to 5.0 "Introduction". Then proceed with 5.1 "Paleo Context" [Hans Poertner and WGII TSU, Germany]	Accepted - paleo section has its own section
131513	10	1	11	23	The introduction of section 5.1 explains not only the main objectives and contents of section 5.1, but it also introduces the contents of sections 5.2 - 5.7. This additional information is unexpected and confusing at this point. As an introduction to section 5.1, the reader would rather expect an explanation that covers section 5.1 only. I would therefore recommend to give the cross-sectorial introduction at the beginning of chapter 5. [Hans Poertner and WGII TSU, Germany]	Accepted: The structure of Section 5.1 has been edited to clarify this comment
83975	10	1			As Chapter 2 uses the terminology "Well-mixed Greenhouse gases" (WMGHGs), maybe Chapter 5 should use it too. [Marco Tulio Cabral, Brazil]	Accepted - defined
78469	10	1			the intro section from pages 10 through 12 is good and nicely written, but seems long winded for background. Unless it covers new knowledge, could be shortened [Chris Jones, United Kingdom (of Great Britain and Northern Ireland)]	Noted: This text ties in with Figs 5.1 and 5.2 which provides a time and process context to the human perturbation of the carbon cycle - has been edited to focus on new knowledge
115321	10	4		5	The chapter 3 assessment is that 'it is extremely likely that human influence is the main driver of the observed warming'. [Gillett Nathan, Canada]	Noted. Chapter 5 uses th term 'the dominant cause of the human induced climate change'.
72899	10	5	10	5	Change 'GHG' to 'GhGs'. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - use of GHG is standard in the literature
4383	10	6	10	9	A better word could be used instead of "quantities", for example "mass" [Ana Bastos, Germany]	Accepted changed to mass
18081	10	10	10	10	the amount of Carbon emissions is extreme important, it is not enough to use only one research [Zhang Zhihua, China]	Rejected: Friedlingstein 2019 reflects the most up to date GCB - further references provided in the context of the sinks
40683	10	10	18	7	Section 5.1 is not following the IPCC guidelines when it refers to years 50 Ma = 50 million years ago, and 50Myr = 50 million years. [TSU WGI, France]	Accepted - text has been carefully edited

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90063	10	11	10	11	This statement uploaded by Ted Schuur on behalf of a graduate class that reviewed the chapter (this was communicated with IPCC TSU in advance): Change wording from "underscores" to "highlights" [Edward Schuur, United States of America]	Rejected - underscores = emphasizes which is not equivalent to highlights
4385	10	13	10	13	Remove ; following the last reference of this line. [Ana Bastos, Germany]	Accepted - change was made.
74183	10	13	10	13	semicolon after Gruber citation unneeded [Christoph Völker, Germany]	Accepted - change was made.
19919	10	13	11	30	For the present reader, the major weakness of this WG1 report is its enormous, disheartening size. This passage is no exception in including many partial repetitions of material present in previous chapter (or expected to be presented in later chapters), as well as comments with little relevance or information. Examples: Page 10 lines 3-7 (we know already all this), line 43 (off topic), page 11 lines 14-17 (we know already all this), lines 21-23 (no information), figure 5.0 (repeat the summary). In some cases, of course, chapter 5 is the right place where to provide information, while previous chapters are unduly talkative. It may be that authors of the WG1 report disagree about the negative impact of the size of this document. In any case, knowing that such an opinion exists should be of interest to them. [philippe waldteufel, France]	Rejected - this is a scene setting section that provides important AR6 and WG1 context which helps clarify the boundaries for WG1 report. The text is brief and informative - however, the Chapter as whole has been re-edited for succinctness.
86739	10	18	10	18	Please consider to elaborate more around the term "climate stabilization" versus "GHG stabilisation". [Oyvind Christophersen, Norway]	Noted - Section 5.1 has been significantly re-edited for clarity and succinctness. This comment has been addressed.
74185	10	19	10	19	should mitigations be mitigation? [Christoph Völker, Germany]	Accepted edit done
69767	10	22	10	23	could you at least acknowledge black carbon (from fossil fuel and biomass combustion) [Gyami Shrestha, United States of America]	Noted - covered in Chapter 7
58563	10	25	20	26	The meaning of the word "unique" is unclear in this sentence. Phrasing could be modified to state the characteristics (for instance, the rate of GHG increase) that render current and projected GHG scenarios as unprecedented or atypical in Earth's history. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - texted was edited to unprecedented and unique - existing text refers to GHGs
8707	10	31	10	32	"as well as more recent time periods" could be replaced with the specific time period being assessed [Vaishali Naik, United States of America]	Accepted - - time periods used
41595	10	33	10	33	One page 8 l. 31, the pre-industrial times are said to start or be around 1850, while here there Industrail area is since c.f. 1750. Is that on purpose? [Katharina Meurer, Sweden]	Accepted - inconsistency addressed
4387	10	33	10	34	Not only are the measurements high-resolution, but also direct ([CO2] and delta pCO2 in the ocean). I suggest adding "direct" [Ana Bastos, Germany]	Accepted - edit done
74187	10	52	10	52	database OF the Coupled Model.. [Christoph Völker, Germany]	Accepted - edited
19921	10	52	10	52	Sentence needs correction [philippe waldteufel, France]	Accepted - edited for clarity
4389	11	1	11	1	"brought about non-linear" --> "brought about BY non-linear" [Ana Bastos, Germany]	Accepted - change was made.
96539	11	2			Maybe you could mention the aspect of 'tipping point(s)' here (like on p. 5-18 or 5-69). [Nicole Wilke, Germany]	Accepted - included - Tipping Points

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
86741	11	5	11	6	Quotation: " Section 5.5 covers the development of the total and remaining carbon budgets to climate stabilisation targets and the associated transient climate response to cumulative CO2 emissions". Comment: Isn't it GHG stabilisation levels (article 2 of the Climate Convention" and temperature goal like in the Paris agreement that are the "target" here? [Oyvind Christophersen, Norway]	Rejected - The Paris Agreement does not speak to GHG stabilisation, but instead sets as its goal to "Holding the increase in the global average temperature to well below 2°C above pre-industrial levels and pursuing efforts to limit the temperature increase to 1.5°C above pre-industrial levels". The literature of remaining carbon speaks to how that target of the Paris Agreement can be achieved. Also in the IPCC Glossary, remaining carbon budgets (or total carbon budgets) are defined in terms of the amount of emissions that can still be emitted for global warming to stay below a certain level. The GHG stabilisation target of the UNFCCC convention text is unspecific (it only says that GHG concentrations have to be stabilized at a level that prevents dangerous anthropogenic interference with the climate system). The literature assessed in Section 5.5 does not speak to that issue.
16547	11	12	11	16	There are also lots of Shindell papers on co-benefits of SLCF mitigation. [William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Noted
72901	11	14	11	15	Move 'directly' to after 'emissions'. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - edit done
58523	11	15	11	17	This is a nice discusson of the C-cycle aspects of the PETM. The PETM carbon pertubation (duration, rates, magnitude) is reasonably well understood, but I am slightly concerned that the CO2 upper bound of 2200 ppm is subject to more uncertainy than is implied here, and the same goes for the Palaeogene CO2 baseline. If this number is based off the Foster (2017) compilation or Felcher et al (2008) for liverworts, or Cui & Schubert (2018), for C3 land plants, or an average of any of these studies, then this should probably be stated. Note that Gehler (2016) PNAS found that PETM pCO2 ~ 1500 ppm for the D17O proxy under a high GPP scenario, although I would agree that most proxies give pCO2 ~ 2000 ppm for peak PETM. All proxies subject to large error bounds, well reviewed in Hollis et al. (2019) GMD - this also perhaps needs to be stated. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted -There are a lot of uncertainties in the CO2 reconstructions (and many assumptions underlying the use of d11B to reconstruct ocean pH). Recent estimates tend to cluster towards higher concentrations (i.e. 2000 ppm e.g. Gutjahr et al., 2017, Nature). Hollis reference is mentioned in the re-revised text.
18191	11	17	11	17	Change delivery to deliver [Chelsey Baker, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change was made.
96541	11	17	11	18	What is meant here: "delivery reductions" or "to deliver reductions"? [Nicole Wilke, Germany]	Accepted - change was made.
115323	11	22		23	I believe that we are not allowed to make research recommendations in IPCC reports. [Gillett Nathan, Canada]	Accepted - deleted sentence

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
58539	11	22			Issue with the sentence ; suggested rewording: "[…] strengthened the assessment reported in this chapter if those gaps did not exist." [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - sentence changed
98231	11	28	11	28	If figure 5.0 changes to 5.1, all the subsequent figures need to be incremented [Gregory Cutter, United States of America]	Taken in account. The figure numbers have been re-sequenced in the FGD
131517	11	28	11	28	The reader expects an overview figure of chapter 5 at the start of chapter 5. Yet instead, the figure is located in the more topic-specific section 5.1. This is confusing to read. Relocate Chapter overview figure from Section 5.1 to an overview Chapter 5.0. [Hans Poertner and WGII TSU, Germany]	Accepted - Section re-organized
9843	11	33	12	53	I urge the inclusion of more assessments of the paleo data [Robert Kopp, United States of America]	Noted. Unfortunately we are constrained by word limit.
8709	11	35	11	46	There seems to be some overlap here with Chapter 2. It is not clear if the assessment of proxy data here is separate from that in chapter 2. If these are relying on the same dataset, cross-referencing is needed. [Vaishali Naik, United States of America]	Accepted. Cross references made.
26885	11	37	11	37	Please consider adding Ruddiman et al. hypothesis on human impact as Neolithic period. Impact is less visible on CO2 than for industrial era but it exists [Eric Brun, France]	Aceepted. Reference has been taken into consideration
72903	11	37	11	37	Move 'rapidly' to after 'increase'. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change was made.
90065	11	37	11	37	This statement uploaded by Ted Schuur on behalf of a graduate class that reviewed the chapter (this was communicated with IPCC TSU in advance): The rapid rise around 1750 is not evident in Fig. 5.1 [Edward Schuur, United States of America]	Noted - this statement is supported by the growth rates on the RHS of Fig 5.1 but in any event Section 5.1 was re-edited for clarity and succincness and this specific point was removed from the text.
9291	11	38	11	38	Subscripts needed for CO2, CH4, and N2O [Christine Weldrick, Australia]	Accepted - change was made.
72905	11	38	11	38	Format the subscripts for the 2s in the molecular formulae [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change was made.
19923	11	38	11	38	"including CO2, CH4, and N2O," is not necessary [philippe waldteufel, France]	Accepted
31857	11	38			? Update to 2019? Or maybe there's an exec decision to stop at 2018, which is understandable as it is usually mid year before the past year's numbers settle down. [Euan G. Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Accepted
8711	11	41	11	42	Chapter 2 reports values for 2018 while 2017 values are reported here. Is there a specific reason why 2017 is chosen? If not, it may be better to be consistent. Further, with the delay in the report, it maybe expected to report 2019 values. Close coordination with Chapter 2 will be needed. [Vaishali Naik, United States of America]	Accepted and noted
18083	11	41	11	42	Since 2018 data have been used before, please use 2018 data to replace 2017 data here [Zhang Zhihua, China]	Accepted
21773	11	41	11	43	These numbers should be updated through 2019 in the FGD and checked for consistency with chapter 2. [Peter Thorne, Ireland]	Accepted
31859	11	41			update 2017 numbers for all three gases - either to 2018 as in rest of text, or to end 2019 as should be feasible during the final edit. [Euan G. Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Accepted

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
103103	11	41			values presented differ from Exec.Summ (p.5, lines 18-19) [Philippe Tulkens, Belgium]	Noted - this has been addressed in the FGD where the numbers are coherent
45423	11	42	11	42	Here I think it would be good to define what "preindustrial" means. Although the CO2 concentration in the atmosphere is higher than at the end of the preindustrial era, it is lower than at earlier stages of Earth's history. [Olivier Sulpis, Netherlands]	Accepted - link to Ch 1made
39623	11	42	11	42	Why not update GHG concentrations with 2020 levels? [Xavier Faïn, France]	Noted - used 2019
19925	11	42	11	42	"increases" rather than "increase" [philippe waldteufel, France]	Accepted - change was made.
31861	11	42			update % numbers and make compatible with page 6 line 17 to 19. [Euan G. Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Accepted
103105	11	42			use of 2020 values would be much more useful [Philippe Tulkens, Belgium]	Noted - ODIAC is a gridded data set based on CDIAC, so not really relevant for the text here. https://www.odiac.org/index.html
35097	11	43	11	44	At the firn-ice transition and below, because of the combined effects of air mixing by diffusion through the firn column and of the large number of bubbles that close-off at various times, the air in a piece of ice is not composed of molecules with a single age, but rather with a range of ages, constrained by the time it takes the bubbles to close. As a consequence, the record of air measured along an ice core is smoothed with time. Instead of "it can be established with high confidence that current concentrations of the three GHGs are the highest in the last 800,000 years", it will be more accurate and safer to state"Current atmospheric concentrations of the three GHCs are higher than at any point in the last 800,000 year- ice core record and it can be established with confidence that current concentrations of the three GHGs are the highest in the last 800,000 years." [Dominique Raynaud, France]	Accepted. Sentence has been rephrased
13421	11	45	11	46	Check parenthesis. The last one is not necessary. [Maria Amparo Martinez Arroyo, Mexico]	Accepted - change was made.
58525	11	45	11	46	I would agree that this is now known to high confidence. Martinez-Botí et al. is mentioned several times in the Chapter (see also pg. 12, lines 39-42), so the authors might consider adding a very nice recent paper by Da et al. (2019) to either/both sections. Da et al. (2019) Low CO2 levels for the entire Pleistocene epoch. Nature Communications, 10:4342 (and reflect this citation in Annex II). [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted
93453	11	46	11	46	The proper ref here should be Martínez-Botí et al., 2018a (Plio-Pleistocene) and not 2018b (last deglaciation). In fact, Martnez-Botí et al., 2018b should not be in the list of this chapter, since the only cite is this wrong one. [Carles Pelejero, Spain]	Accepted
68869	11	46			Please double check that all of the data sources for Fig. 5.1 and 5.2 are included in Annex II. Thank you. [Darrell Kaufman, United States of America]	Taken into account - this check has been done.
45425	11	49	11	56	Figure 1: the bar plots on the right are unclear. 1) There are three grey bars in each plot for the period 800k-0k: the bar on the right is for the fall rate, the bar in the middle is for the rise rate, what is the bar on the left for? 2) For the period 1900-2018: is it a rise rate, fall rate? [Olivier Sulpis, Netherlands]	Accepted

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
39625	11	51	11	56	About Fig 5.1 and the CO2 dataset shown: why not using the recent compilation by Beireter et al., 2015 (https://www.ncdc.noaa.gov/paleo-search/study/17975) ? It is not fully exact when Fig. legend mentions only EPICA dome C for BCE : actually Vostok data are also plotted. CE CO2 : instead of MacFarling Meure data, it could be plotted the updated Law Dome dataset as publisehd by Rubino et al., 2019 (https://doi.org/10.25919/5bfe29ff807fb). [Xavier Faïn, France]	Accepted -
39627	11	51	11	56	About Fig 5.1 : the growth rate panel was not easy to read. I did not understand what were the left, smaller, bars. Would be interesting to higlight difference in growth rate unit (ppm/Kyrs, vs ppm/yr). [Xavier Faïn, France]	Accepted - changes made to improve readability
35099	11	51	12	7	It seems to me that the Dionysian BC and AD system of time periods is more current and appropriate here for the reader than BCE and CE [Dominique Raynaud, France]	Rejected - BCE and CE are current
58541	11				Figure 5.1: I suggest to change the maximum of the N2O concentration scale from 380 to 350 ppb. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - figure edited
58543	11				Figure 5.1: I suggest to change the maximum of the N2O growth rate scale from 4 to 2.2 ppb/(k)yr. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Reject - 4 includes the variance
58545	11				Figure 5.1: I suggest to insist on the different units of the growth rates ; "(ppb/yr)" could be in the same colour than the corresponding colorbar (grey and pink). [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - modifications made to Fig 5.1
58547	11				Figure 5.1: the uncertainty of the growth rates for the period 1900-2018 is not represented. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - Fig 5.1 edited further
58549	11				Figure 5.1: I suggest either to include the growth rates for the periode 0-1900 CE or to delete its mention in the legend. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - included in Fig 5.1
42987	12	4	12	4	The Law Dome records of CO2, CH4 and N2O published in MacFarling Meure et al. (2006) have recently been revised and should be replaced by Rubino et al. (2019) - Revised records of atmospheric trace gases CO2, CH4, N2O, and δ13C-CO2 over the last 2000 years from Law Dome, Antarctica (https://www.earth-syst-sci-data.net/11/473/2019/) [Mauro Rubino, Italy]	Accepted
58551	12	14	12	15	I suggest to add the value of temperature change experienced during the PETM (as mentionned in page 5-44 line 17-18): possibly exceeding 4-8°C [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted
100661	12	15	12	15	Note: These values are based on a model, not a proxy. Some qualifying language might be appropriate, perhaps: "…are inferred to have increased…" Or, "Models indicate that…" [Matthew Kohn, United States of America]	Accepted

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
83477	12	15	12	53	There needs to be coordination with the authors of Chapter 2 regarding the use of ka/Ma and kyr/Myr! Following IUGS guidelines ka/Ma should be used when refering to a date (fixed point in time) and kyr/Myr when talking about duration and ratios. So correctly, it should 55.8 Ma ga in line 15, 3 -3.3 Ma in line 40 and 2.7 Ma in line 42. However, I believe the switch between the use of ka/Ma and kyr/Myr is confusing for non-expert readers. I left a comment for Chapter 2 that this needs to be addressed by using either one type "a" or "yr", even if not fully correct, or by including an explanation as a cross-chapter box, in Annex II and/or the glossary. Chapter 7 authors write out as million years or use Ma. [Antje H. L. Voelker, Portugal]	Accepted - inconsistencies addressed
35101	12	16	12	16	Would be nice to have a few words describing the type of pulse release [Dominique Raynaud, France]	Rejected. The nature of the CO2 is described below in this section.
83479	12	17	12	17	Note that in Chapter 2 the Turner (2018) reference is gvien as Kirtland Turner (2018). [Antje H. L. Voelker, Portugal]	Accepted - change made.
45427	12	17	12	17	There are more recent hypotheses on what caused the PETM, in particular the recent article by Zeebe and Lourens in Science (2019, vol. 365, issue 6456) that deserve to be included here. [Olivier Sulpis, Netherlands]	Accepted - referenced added
98595	12	17			Comment: Here Harper et al. 2019 reference could be added. [Eleni Anagnostou, Germany]	Noted
21195	12	20	12	21	".. with large consequences for many terrestrial and marine ecosystems (McInerney and Wing, 2011)" is not quite clear. I suggest to slightly change this to " .. With large consequences for the evolution of terrestrial and marine ecosystems (McInerney and Wing, 2011)" See also Speijer et al. (2012 - Austr. J. Earth Sci.) for a more expanded summary on changes in marine ecosystems, including the decline of coral reefs and rise of the lager foraminifera-dominated carbonate ramps. [Robert Speijer, Belgium]	Accepted
98603	12	20			Comment: missing reference for PETM pH is Harper et al. 2019 [Eleni Anagnostou, Germany]	Accepted
45429	12	21	12	21	"Large consequences for many ecosystems" is vague, as the consequences could be very good or very bad for the ecosystems. This should be developed a bit further. [Olivier Sulpis, Netherlands]	Accepted - clarification made
106153	12	21	12	21	Ecological consequences of the PETM are mentioned here only in passing, this being a very summary passage, and more in detail in section 5.3.1.1 on page 44 but only for the oceans. Not sure where the best location is but it is worth also pointing to the evidence of ecosystem impacts on land, namely the famous woodland-grassland-woodland sequence in the Bighorn basin (i.a. Wing and Currano, American Journal of Botany 100, 2013) and vegetation change/persistence in the (neo)tropical rainforests (notably Jaramillo et al., Science 330, 957, 2010) [Wolfgang Lucht, Germany]	Accepted
99241	12	21	12	23	large scale consequences for ecosystems does not provide some information here which supports the section. Could they expand and mention changes in region of biotic production which would be relevant? The link to the next sentence currently makes no sense if people do not have a larger knowledge as its needs comparison with another event which did not have much impact [Daniela Schmidt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted
98233	12	24	12	25	Where is the "placeholder". It is critical to explain the use of pH as a metric [Gregory Cutter, United States of America]	Accepted. The use of pH to define the rates of change has been removed

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
18193	12	27	12	27	Why is the range from high to low? To convey that the CO2 levels were decreasing? [Chelsey Baker, United Kingdom (of Great Britain and Northern Ireland)]	Not applicbable - we think this comment is miss asigned - it is not clear what is being referred to here.
71663	12	27	12	27	Is citing Zeebe et al 2016 as showing that the CO2 growth rate cannot have been as high as it is now for 66 Myr. But the text used here should mention that the time resolution for observing such large changes is constrained to 4,000 years. [Martin Manning, New Zealand]	Accepted
19927	12	27	12	28	We know this already [philippe waldteufel, France]	Rejected - the relative rates of change have not been previously assessed
68871	12	27	12	29	One of the paleo key messages focus on the unusualness of recent climate changes, including its rate of change. However, quantifying the rate of change of GHGs and other proxy-based climate indicators is controversial. It's essential to conduct a comprehensive and critical assessment of the validity of rates of change over different durations based on different natural archives. See for example, doi: 10.1029/2018PA003379. It would be very helpful if CH5 would take the lead on this in-depth assessment and would consider annually resolved records, glacier ice, and sedimentary archives. This information will be useful for other chapters that address rates of change based on proxy records. [Darrell Kaufman, United States of America]	Accepted - was taken up
9845	12	27	12	30	at any other time prior to the industrial period [Robert Kopp, United States of America]	Accepted
112301	12	27	12	30	This sentence should be reworded or removed because most of the paleorecords for the deep past don't have resolution of 300 years or less. [Jinho Ahn, Republic of Korea]	Accepted
90067	12	27	12	30	This statement uploaded by Ted Schuur on behalf of a graduate class that reviewed the chapter (this was communicated with IPCC TSU in advance): Fig. 5.2 does not show "emissions" of CO2, as stated in this sentence. [Edward Schuur, United States of America]	Accepted
2773	12	28	12	49	Line 28 states at least 10 times higher over the last 66 million years, line 48 is 6 times smaller and line 49 is 20 times smaller. To avoid any perception of contradiction it would be useful to state that the factor of 10 is for the whole period and that there is an increase in the rate of emission with time (i.e. in the last decade). I would do this after line 30, stating that the rate of emission over the last decade is X above the Industrial Era average [Stephen Wilkinson, United Arab Emirates]	Accepted
68873	12	29	12	30	CH2 does not consider the pace of change of GHGs, only the magnitudes. [Darrell Kaufman, United States of America]	Noted - this instance has been edited out
21775	12	29	12	30	The end of this sentence could be seen as editorialising and arguably the paragraph is scientifically stronger without its inclusion? [Peter Thorne, Ireland]	Accepted
52185	12	30	12	30	Figure 5.2 of Chapter 2 of the Report was not found. [Maritza Jadrijevic Girardi, Chile]	Noted - it refers to Figure 5.2 in Chapter 5 and separately to Chapter 2 but the sentence has been changed in the FGD
19929	12	32	12	33	Is there any explanation available for this decline? [philippe waldteufel, France]	Taken into account. The gradual decrease in atmopsheric CO2 concentrations has been related to increased continental weathering (e.g. Foster et al., 2017)

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
100665	12	33	12	33	Note: I can't find where Gutjahr et al. (2017) say this. [Matthew Kohn, United States of America]	Accepted - change made.
100667	12	33	12	33	Add: "…Myr-1, with an important reversal during the MCO (Foster et al., 2017)." [Matthew Kohn, United States of America]	Accepted
98597	12	33			Comment: More references are needed here to include data not included in the Foster et al. 2017 compilation. Some to consider are: Witkowski et al. 2018; Anagnostou et al. in review; Harper et al. 2019; Henehan et al. 2020; Ji et al. 2018; Swann et al. 2018; Super et al. 2018; Londoño et al. 2018; Wolfe et al. 2017; Sosdian et al., 2018; Chalk et al. 2017; Greenop et al. 2018. [Eleni Anagnostou, Germany]	Noted - included
8713	12	35	12	35	RCP8.5 could be replace with SSP5-85- SSP scenario with CO2 as high as 1000ppm [Vaishali Naik, United States of America]	Accepted
106155	12	36	12	37	"that was necessary to develop" --> "that led to the development of" [Wolfgang Lucht, Germany]	Accepted
90069	12	36	12	37	This statement uploaded by Ted Schuur on behalf of a graduate class that reviewed the chapter (this was communicated with IPCC TSU in advance): Shift ciation to end of sentence because DeConto et al. is the source for all of the content. [Edward Schuur, United States of America]	Accepted
98599	12	37			Comment: reference missing for the EOT CO2. This should be Pearson et al. 2009; Anagnostou et al. 2016; Pagani et al. 2011; Zhang et al. 2013 [Eleni Anagnostou, Germany]	accepted
35103	12	38	12	38	A paper recently published (2020) could/should be cited here about a new atmospheric CO2 record acrosss the last 23 m.y.: Cui et al., 2020, Geology,V. 48, https://doi.org/10.1130/G47681.1 [Dominique Raynaud, France]	Accepted
8715	12	38	12	49	The units of ppm yr-1 are for CO2 concentrations rather than emissions [Vaishali Naik, United States of America]	Accepted - change made.
100663	12	39	12	39	Note: Here, Pliocene pCO2 is (sort of) implied to be 400-450 ppm. Be sure this range is what is intended, or perhaps change to c. 400 ppm. [Matthew Kohn, United States of America]	Accepted
69831	12	39	12	39	The estimated CO2 level during mPWP need to be consistent that mentioned in Chap.2 Box. 2.4 (350–450 ppm, medium confidence). [Kaoru Kubota, Japan]	Accepted. Changed.
72907	12	39	12	39	Insert 'times' after 'modern' [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted
83977	12	39	12	42	Cross-Chapter Box 2.1 should be mentioned here [Marco Tulio Cabral, Brazil]	Accepted
9847	12	39	12	42	No basis is provided her efor the executive summary statement that "Current CO2 concentrations are also unprecedented in the last 2 million years". 2 million years does not appear as a significant time period here. [Robert Kopp, United States of America]	Rejected. This aspect has been assessed in CH02 and has been restated here for the sake of inter-chapter consistency
83481	12	40	12	40	A reference to Cross-Chapter Box 2.4 on the MPWP should be added here; and may be the de la Vega et al. (submitted) reference cited in Chapter 2: de la Vega et al. (submitted) cited in Chapter 2: de la Vega, E., Chalk, T. B., Hain, M. P., Wilson, P. A., and Foster, G. L. (submitted). Multi-site Late Pleistocene high resolution CO2 record using boron isotopes and constraints on CO2 climate forcing. (submitted). [Antje H. L. Voelker, Portugal]	Accepted

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
98601	12	40			Comment: an additional reference to consider here is Bartoli et al. 2011 [Eleni Anagnostou, Germany]	Rejected. AR6 focusses on publications published predominently since the last assessment report (2013). More recent studies have targeted the late Pliocene interval (e.g. de la Vega et al., 2020)
106157	12	42	12	42	"ice sheets, 2.7 Myr ago" --> "ice sheets, after 2.7 Myr ago" [Wolfgang Lucht, Germany]	Accepted
69769	12	44	12	44	replaced 'are' by 'were' in 'During the period 800,000–1 BCE, periodic oscillations in GHG concentrations are' [Gyami Shrestha, United States of America]	Accepted
19931	12	44	12	45	The way these lines are written implies that either there were no periodic oscillations in GHG concentrations before 800,000 BCE, or that such oscillations existed but were not forced by Milankovich orbital cycles. This ought to be demonstrated, unless the text be changed in order to make clear that what happened during this period was the possibility to obtain and interpret paleoarchives allowing to detect the ice cycles and associated GHG oscillations. [philippe waldteufel, France]	noted - clarification made
9849	12	44	12	53	would be useful to assess literature on pre-Industrial anthropogenic CO2 and CH4 emissions [Robert Kopp, United States of America]	Rejected - in the government approved outline, only GHG trends from the pre-industrial were considered as they related to what can be learn about carbon-climate feedbacks, and anthropogenic emissinos alone did not provide climate feedbacks for which there is enough literature to assess.
14873	12	45	12	45	Milankovich' - I did not check if the name appeared in other section/chapter. If it is the case the spelling should be made uniform. This version is not the most 'popular'. There is also Milanlovitch, Milankovic, Milanković [Marie-France Loutre, Switzerland]	Accepted
81571	12	46	12	47	These numbers for CO2 need to be carefully reconsidered and more representative values should be provided. Bereiter et al., GRL,2015, http://dx.doi.org/10.1002/2014GL061957provide a record low atmospheric CO2 concentration value of 171.6 ± 1.4 ppm. Thus the typical low range is not 160 to 180 ppm At the high end, 300 ppm is the highest measured. [Fortunat Joos, Switzerland]	Accepted
83483	12	47	12	47	add to Lüthi et al. also the following reference (see Chapter 2): Bereiter, B., Eggleston, S., Schmitt, J., Nehrbass-Ahles, C., Stocker, T. F., Fischer, H., et al. (2015). Revision of the EPICA Dome C CO2 record from 800 to 600-kyr before present. Geophys. Res. Lett. doi:10.1002/2014GL061957. [Antje H. L. Voelker, Portugal]	Accepted
90071	12	47	12	48	This statement uploaded by Ted Schuur on behalf of a graduate class that reviewed the chapter (this was communicated with IPCC TSU in advance): The unit of figure is kyr-1. It's better that the units are the same in the text as the figure. [Edward Schuur, United States of America]	Accepted
112303	12	48	12	48	0.12 ppm/yr => 12 ppm/100yrs? The WAIS Divide ice core record does not provide annual resolution. [Jinho Ahn, Republic of Korea]	Accepted

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
90073	12	49	50	48	This statement uploaded by Ted Schuur on behalf of a graduate class that reviewed the chapter (this was communicated with IPCC TSU in advance): The growth rates of prior two periods are in Fig, 5.1, but not the rate for the last decades. It would be great if this can be seen in Fig. 5.1 as well. [Edward Schuur, United States of America]	Accepted - change was made.
58553	12	51	11	53	I suggest to shorten the last sentence of this paragraph as follow: "Both the high concentrations and the growth rates of atmospheric accumulation of CO2, CH4 and N2O experienced over the last century are unprecedented relative to the past 800kyr (high confidence)." [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted
21777	12	51	12	51	the use of suggests here implies a degree of hedging / doubt. Is that really intended? Should this not be more a factual statement without hesitation? [Peter Thorne, Ireland]	Accepted
8717	12	51	12	53	The above discussion is only for CO2, assessment for past changes in CH4 and N2O have not been provided. Please clarify [Vaishali Naik, United States of America]	Accepted - clarification made
112305	12	52	12	53	The growth rate can be compared with data that have sufficient time resolution. We can compare only for specific time periods that have records with sufficient time resolutions. [Jinho Ahn, Republic of Korea]	Accepted
8719	12	55	12	55	what does resolution mean here? greater availability of data spatially or more temporal resolution? [Vaishali Naik, United States of America]	Accepted - clarification made
4393	12	55	12	56	"data show growth rates" --> "data show THAT growth rates" [Ana Bastos, Germany]	Accepted - change was made.
58555	12	55	13	6	I suggest to combine the 2 last paragraph of this section as follow: "During the period 0-2017 CE (period with the highest resolution of paleo and atmospheric records), the data show a strong shift in the GHGs growth rates of about 100 times between the periods 0-1900 and 1900-2017 (medium evidence). There are multiple evidences that make the relationship between the GHGs growth rates increase and human activities intensification virtually certain (see Sections 5.2.1.1, 5.2.2.1, 5.2.3.1)." [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted
112307	12	56	13	1	The growth rate can be compared with data that have sufficient time resolution. We may compare on multi-decadal or centennial timescales for the last 2000 years. The authors should check all the highest resolution records (not only Law Dome data) [Jinho Ahn, Republic of Korea]	Accepted
52187	12	56	13	1	They indicate higher growth rates of atmospheric CO2 during the period 1900-2017 compared to the period 0-1900, see figure 5.1. However, the figure shows the period 1900-2018. [Maritza Jadrijevic Girardi, Chile]	Accepted - inconsistencies addressed
116403	12		12		There is overlap with ch 1 and ch 2 on text and figures related to past changes in atmospheric CO2 concentration. However altogether the assessment of firn smoothing - trapping effects that can affect recorded rates of changes compared to atmospheric ones needs to be improved building on new literature available since AR5. [Valerie Masson-Delmotte, France]	Accepted
106159	13	1	13	1	"during the period of 0–1900": Is there a year 0? Or just a year 1, following -1? [Wolfgang Lucht, Germany]	Accepted
72909	13	1	13	1	Something is wrong with the dates (0-1900 is a meaningless concept). [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
83979	13	3	13	4	Does the industrial activity include transport and energy? Otherwise it does an misleading interpretation of the refered Sections (5.2.1.1, 5.2.2.1, 5.2.3.1). The refered Sections (5.2.1.1, 5.2.2.1, 5.2.3.1) do not expecify agricultural activity only but uses land use activities (which includes agriculture and forestry). It is strongly recomended to mantain coherence between this statement and the findings described in the referenced Sections. [Marco Tulio Cabral, Brazil]	Accepted - We removed 'industrial' as it was imprecise regarding as previously phrased.
106161	13	3	13	4	"GHGs atmospheric growth over the past century is consistent with the intensification of industrial and agricultural activities" --> "is concurrent with" [Wolfgang Lucht, Germany]	Taken into account. Sentence removed because it was imprecise.
71665	13	4	13	6	Suggest better wording would be “There are also multiple lines of independent evidence that make the relationship between recent growth of GHGs and human activities virtually certain (see Sections 5.2.1.1, 5.2.2.1, 5.2.3.1)." [Martin Manning, New Zealand]	Accepted - clarification made
39629	13	11	13	14	Fig 5.2. The blue arrow highlighting the WAIS Divide data is somewhat overlapping with the CO2 data, and maybe not required. [Xavier Faïn, France]	Accepted - clarification was made
98593	13	12			Comment: the compilation of Foster et al. 2017 does not include several records published since then (i.e. see comment below). Also it could be beneficial to clarify that Foster et al. 2017 is a compilation when referenced. [Eleni Anagnostou, Germany]	Accepted - change was made.
72911	13	18	13	18	Capital 'F' for 'feedbacks'. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text edited with cap
131519	13	18	14	45	Section 5.1.2. is a sub-section of section 5.1 "the paleo context". Yet sub-section 5.1.2 does only describe biogeochemical processes that drive carbon-climate feedbacks. These processes do not solely apply to the paleo context, but to many other contexts as well. I would therefore recommend to move subsection 5.1.2 or make it a separate umbrella section. [Hans Poertner and WGII TSU, Germany]	Accepted - Section 5.1 was re-organized
8721	13	20	13	20	"emission scenarios" is misplaced here. [Vaishali Naik, United States of America]	Accepted - deleted
4395	13	20	13	23	Review the sentence's grammar [Ana Bastos, Germany]	Accepted - edited for better legibility
19933	13	20	13	25	There are two ways to define the boundaries of what is called " Carbon-Climate feedbacks". In the present text, sink function themselves are considered as " major negative feedbacks", to which are added further feedback mechanisms associated to biogeochemical processes. The present reader favours an alternative view, according to which one cannot consider that ocean and land sinks are negative feed backs; their sink function in itself does not feed anything back. What is then understood from the reference to AR5 is that the carbon concentration itself may, through climate change, induce changes in the efficiency of ocean and land as sinks; then of course the modification of sinks has effects on the atmospheric concentration. Here we have feedback indeed. The definition given in the glossary (for the "Climate-carbon cycle feedback" item; there is no "carbon-climate feedback" item in the glossary) supports this more restricted interpretation. Therefore, WG1 authors are encouraged to retain a clear, consistent definition throughout the report [philippe waldteufel, France]	Noted

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
26887	13	24	13	24	it is of first importance to clearly state which part of soil (and permafrost) is taken into account into the global carbon inventories and in feedback calculation. e.g. soil respiration evaluation is not the same if based on the upper 30cm or on the whole profile. This information is missing (as it was in 2013 where only a reference to Batjes 1996 gave a clue) [Eric Brun, France]	Noted - this is discussed in more detail in the permafrost box
109637	13	25	13	27	I would add „...and sources“ at the end of the sentence. [Carolyn-Monika Görres, Germany]	Accepted - edit done
104853	13	31	13	31	Clarify what these percentages (29% and 24%) represent. Presumably the fraction of anthropogenic CO2 emissions abosrbed by the ocean over the analysis period. [Timothy DeVries, United States of America]	Accepted - % explained
115325	13	34			Replace 'quasi-linear trend characteristic' with 'approximate proportionality'. [Gillett Nathan, Canada]	Rejected - use of quasilinear is standard in the literature
38483	13	38	13	38	response to CDR -> respones to CDR and SRM [LONG CAO, China]	Accepted - … CDR and SRM
21779	13	40	13	44	My feeling is that these percentages should be reported with uncertainty ranges rather than presented as deterministic. This would also match the implied variability in the text surrounding these numbers. [Peter Thorne, Ireland]	Accepted - uncertainties added
96543	13	41	13	41	"on land (29%), oceans". There might be missing an "and". Then the sentence would make more sense. [Nicole Wilke, Germany]	Accepted -- correction made
37925	13	41	13	41	The described percentage of negative feedback of oceans (24%) is different with the number (22%) which mentioned in Figure 5.3. [Junhee Lee, Republic of Korea]	Accepted - 23% used
109639	13	41	13	41	The percentage for oceans is given as 24, but in the legend of Fig. 5.3 it is given as 22 %. In previous paragaphs, I have also seen 23 %. [Carolyn-Monika Görres, Germany]	Accepted - 23% used
9293	13	41	13	41	Suggest changing "…the negative feedbacks on land (29%), oceans (24%)…" to read "…the negative feedbacks on land (29%) and oceans (24%)…" [Christine Weldrick, Australia]	Accepted edit made
114855	13	41	13	41	Here, "partitioning of CO2 emissions between reservoirs on land (29%), oceans (24%)" is slightly different than in the Executive Summary - oceans (23%). [Roxana Bojariu, Romania]	Accepted - correction made
79935	13	47	13	47	I strongly believe that the authors should include a box on the topic of the buffering capacity of CO2 in the ocean. Given that the title of the chapter includes the word "feedback", and it has long been known (Revelle and Suess, 1957) that the buffering capacity is expected to dominate carbon feedbacks on climate, this concept of buffering capacity is central to conveying underlying mechanisms. It is the other half of the story (mechanistic) of the airborne fraction, with the airborne fraction already receiving an appropriate amount of real estate with text in the chapter. [Keith Rodgers, Republic of Korea]	Noted - This has been highlighted in Sections 5.4.2 and Cross Chapter Box 5.3 in the FGD
127687	13	50	13	51	It would probably be accurate to include "disturbances" in this list of factors; for example, large wildfires and land clearance in many regions (such as Indonesia and the Amazon) are now contributing very significantly to annual carbon emissions in those areas. [Trigg Talley, United States of America]	Accepted - added human and climate linked disturbances

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
26889	13	52	13	52	Regarding the slower processes we suggest to mention: - biomineralisation notably in ocean (that trap carbon into mineral structures for long, even million of years); pedogenesis (that balances carbon cycle, removing mineral carbon from sediment to trap organic carbon in soil - see e.g. Finke 2008 Geoderma or 2017 Geology) ; ocean circulation reorganisation (that greatly impacts carbon exchanges within ocean and with atmosphere)(ocean ventilation is only for ocean-atmosphere exchange). It is not clear that this paragraph currently focuses on processes shorter than a few decades or centuries. We suggest the references Finke P.A., Hutson J.L. (2008) Modelling soil genesis in calcareous loess. Geoderma 145, 462-479 and Finke P.A., Yin Q., Bernardini N.J., Yu Y. (2017) Climate-soil model reveals causes of differences between Marine Isotope Stage 5e and 13 paleosols. Geology 46, 99-102. [Eric Brun, France]	Noted - processes in the slow cycle were clarified
79655	13	53	13	54	Could include permafrost in the slower process (Chaudhary etal. 2017) [Nitin Chaudhary, Sweden]	Accepted - addition made
127689	14	5	14	5	Definitely add "disturbance", so the sentence would read: "...climate (heat and moisture), and disturbance (e.g. large wildfires) feedbacks, which..." [Trigg Talley, United States of America]	Accepted - added disturbance
4397	14	6	14	9	The sentence does not seem gramatically correct [Ana Bastos, Germany]	Accepted - edit done
21781	14	9	14	9	This should also reference cross-chapter box 9.2 which undertakes a joint closure of the sea level and energy budgets. [Peter Thorne, Ireland]	Accepted - link made
19937	14	10	14	12	This important result has already been indicated several times in this report; here we learn that it is going to be repeated again later in the present chapter. [philippe waldteufel, France]	Accepted - links made
7423	14	12	14	12	« global warming. which is used as » replace the '.' by a ',' [Geremy PANTHOU, France]	Accepted - edit made
9295	14	13	14	13	Suggest replacing "This climate metric maybe…" to "This climate metric may be…" [Christine Weldrick, Australia]	Accepted - change was made.
127691	14	13	14	13	Change "maybe" to "may be". [Trigg Talley, United States of America]	Accepted - change was made.
9297	14	14	14	14	Suggest replacing "climate feedbacks processes" to "climate feedback processes" [Christine Weldrick, Australia]	Accepted - change was made.
72913	14	16	14	17	References should be in chronological order (with submitted one at end of list). [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Not Applicable - The merging of citations by the Mendeley plugin is following the TSU guidelines, which is to use Frontiers Journal. Further edits on the Reference styling is responsible by TSU.
19935	14	28	14	29	See previous comment on Page 13 lines 20-25 [philippe waldteufel, France]	Noted
9299	14	35	14	36	Suggest replacing "…is build around CO2, the dominant GHG,…" with "…is build around CO2, and the dominant GHG,…" [Christine Weldrick, Australia]	Rejected - correct meaning in the text
26891	14	48	14	48	this paragraph focuses on atmospheric records whereas the title announces something broader. We suggest to address the impact (and thus feedback) on terrestrial and oceanic carbon cycle. Threshold effect and associated lag of terrestrial ecosystems could have some place here, especially since they are really complex and poorly known. This has to be raised to show that further work is needed in this area. [Eric Brun, France]	Rejected. While it would desirable to dwell into these issues in more details, the tight space allocation precludes a more detailed analysis, unfortunately. Impacts are discussed by WGII.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
112309	14	48	18	7	may need to include GHG concentration changes associated with DO-events [Jinho Ahn, Republic of Korea]	Accepted - CO2 jumps associated with DO events are now briefly mentioned in the text.
83981	14	48			The form in which many of the topics are approached feels almost as the authors are dismissing paleo records because they do not serve for direct comparsion (p.14, line 15) for nowadays processes, but what would then? [Marco Tulio Cabral, Brazil]	Noted. On the contrary, we aim to show that paleoclimatic records are useful/unique in that they can provide constraints beyond the instrumental record.
106511	14	48			Section 5.1.3 overlaps WGII CCB PALEO. Care should be taken to ensure consistency between WGs in messages and uncertainty assessments of those messages. [camille parmesan, France]	Aceepted
58559	14	48			Section 5.1.3: I suggest to reorganize this section in 3 sub-sections: 'Glacial-interglacial changes', 'Transition from the last ice age to the Holocene' and 'Holocene changes'. The current last sub-section can be condensed in 1 paragraph and be the last paragraph of the last sub-section. By such re-organization, the introduction of this section (currently 1/3 of the section) can be 1 paragraph, by for example moving the paragraph 3, 4 and 5 (potentially the 6th too) from the introduction to a specific sub-section on the transition between glacial-interglacial periods and Holocene. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - text revised
81583	14	51	14	52	What about abrupt events? Suggest to expand the disucssion on past abrupt events. For example, N2O emissions from both the ocean and land changed rapidly at the onset of the B/A likely linked to circulation changes in the NA and the tropical oceans as well as to shift in the ITCZ and moisture changes on land as discussed. Similar CH4 conc. reacted consistently and very rapidly to decadal-scale climate change (Baumgartner et al., CP, 2014 doi:10.5194/cp-10-903-2014). Similarly, some interferences could be made regarding CO2 changes during past abrupt events. Interesting in these context are also the abrupt CO2 jumps (e.g. Bauska et al., 2016, 2018). [Fortunat Joos, Switzerland]	Accepted. The discussion regarding abrupt events has been expanded.
26893	14	52	14	52	Should not it pointed out that the comparison between the CO2 record and climate record (or more likely record of climate impacts on the ecosystem) is furhtermore dependent of independent chronologies between records, which is far too rarely the case. There is often a tendency to use the "perfect synchronisation" assumption to put in time the records and to underestimate the impact of the basic assumptions in classical geochronological methods (e.g. 14C calibration). Timing is a weak point in paleoclimatology. There is too much of a tendency to think either that it is perfect or unimportant. Whereas that's the key to highlight phasing and phase-shifting and assessing change rates (dynamics). [Eric Brun, France]	Rejected. The comment is certainly valid this being said. Beeman et al., 19 (ClimPast) present a robust assessment of lead/lag relationships in ice core records. Similar analyses comparing ice core with marine sedimentary records is indeed more problematic (especially that paleoceanographic records from the Southern Hemisphere are often aligned to ice core records precluding any lead/lag relationship t be inferred)
2775	14	52	14	52	"tended to be" is weak language given the data in the previous section, I recommend "according to measurements are" [Stephen Wilkinson, United Arab Emirates]	Accepted - text revised
58557	14				Figure 5.3: mistake in the caption ; 22% to be corrected by 24% (the negative feedback of ocean on carbon concentration). [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - correction made
52189	15	1	15	4	Figure 5.1 does not show the relationship of high GHG concentrations in the past warm intervals. [Maritza Jadrijevic Girardi, Chile]	Accepted. Fig. 5.1 has been modified accordingly

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
35107	15	1	15	14	I suggest to cite somewhere in this paragraph the recent paper by Chowdhry Beeman et al. (Climate of the Past, 15,913-926, 2019) on Antarctic temperature and CO2: near-synchrony yet variable phasing during the last deglaciation. I believe it provides a state of the art in terms of methodology to infer the phase relationship between Antarctic temperature and atmospheric CO2 and provides an accurate identification of the changes in the phase relationship during the deglaciation. It confirms that during the onset of the last deglaciation the Antarctic temperature most likely led CO2 by several centuries. [Dominique Raynaud, France]	Accepted
112313	15	3	15	3	GHGs covary on orbital time scale, but may not on millennial timescales. [Jinho Ahn, Republic of Korea]	Accepted - text revised
39631	15	3	15	3	Schilt et al (2010) discuss only past N2O atmospheric levels. Maybe not relevant to cite here when mentioning CO2, CH4, and N2O [Xavier Faïn, France]	Accepted - relevant references added
90075	15	3	15	4	This statement uploaded by Ted Schuur on behalf of a graduate class that reviewed the chapter (this was communicated with IPCC TSU in advance): Fig. 5.1 does not show warming intervals of the past. Either annotate the figure to include warm vs cold or refer to CH2 where this is presented in more detial. [Edward Schuur, United States of America]	Accepted. Fig. 5.1 has been modified accordingly
106163	15	5	15	6	"reveal that temperature rise preceded the increase in atmospheric GHG concentrations, indicating that greenhouse forcing amplified"; because this is a major talking point of climate change deniers who draw an invalid conclusion from this observation, this sentence has to be worded carefully so it cannot be misconstrued when quoted out of context; here it is too technical and lends itself to such selective, in effect misleading quotation [Wolfgang Lucht, Germany]	Accepted. Argument has been reformulated to avoid confusion
35105	15	5	15	7	Unless I am mistaken, the message of Shakung et al. is just opposite to what is written here. Global warming preceded by increasing CO2 concentrations during the last deglaciaition and not global warming preceded increasing CO2 concentrations. [Dominique Raynaud, France]	Accepted - text revised
115327	15	5		10	The text seemed to say first that the temperature changes preceeded the GHG changes, and then that the GHGs led the warming at the last glacial termination. These seemed contradictory. This needs better explanation. [Gillett Nathan, Canada]	Accepted. Argument has been reformulated to avoid confusion
74189	15	6	15	6	I think one should add to with timespan the “increase in atmospheric GHG concentrations” belongs: To the end of the last glacial [Christoph Völker, Germany]	Accepted. Text revised
83485	15	7	15	7	You could add the following reference to the Shakun reference: Chowdhry Beeman, J., Gest, L., Parrenin, F., Raynaud, D., Fudge, T.J., Buizert, C., Brook, E.J., 2019. Antarctic temperature and CO2: near-synchrony yet variable phasing during the last deglaciation. Clim. Past 15, 913-926, doi: 10.5194/cp-15-913-2019. [Antje H. L. Voelker, Portugal]	Accepted - text revised
83487	15	8	15	8	To be conform with the nomenclature used in Chapter 2 and Annex II, I suggest to replace "last glacial termination" with "last deglacial transition". I myself would use "last deglaciation" since "termination" (if used correctly) refers to the half-way point of the glacial to interglacial transition. [Antje H. L. Voelker, Portugal]	Accepted - text revised

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
34895	15	10	15	12	The SOD claims that centennial scale lag of global temperature behind CO2 is consistent with the thermal inertia of the climate system associated with ocean heat uptake/release and ice sheet dynamics. The text of some lines previous indicates the opposite – that temperature rise preceded the increase in atmosphere GHG concentrations. Please see general comment #13 above. [Jim O'Brien, Ireland]	Accepted. Argument has been reformulated to avoid confusion. The text now refers to Antarctic/SH hemisphere temperature leading CO2 at the onset of the last deglacial transition, while global average temperature generally lags CO2 throughout the deglaciation (Shakun12).
69833	15	10	15	12	Is this sentence right? If my understanding is correct, CO2 lags T (e.g., Pedro et al., 2012, Clim. Past) [Kaoru Kubota, Japan]	Accepted. Argument has been reformulated to avoid confusion. The text now refers to Antarctic/SH hemisphere temperature leading CO2 at the onset of the last deglacial transition, while global average temperature generally lags CO2 throughout the deglaciation
83983	15	12	15	14	Is this valid for the whole geological timescale? [Marco Tulio Cabral, Brazil]	Accepted - text revised
39011	15	16	15	17	Is there no evidence for the contribution by freshwaters as pre-industrial CH4 sources? Freshwaters are identified as the second largest source of CH4 emissions in historical records (Fig. 5.14) 'Major pre-industrial sources of CH4 include wetlands (including subglacial environments), biomass burning and methane hydrates (clathrates) (Bock et al., 2010, 2017; Lamarche-Gagnon et al., 2019).' [Alexandra Rouillard, Sweden]	Noted. The term "wetlands" includes freshwater systems.
21783	15	16	15	18	I could not make sense of this sentence as written. It appears some critical material is missing for it to be able to make any sense. [Peter Thorne, Ireland]	Accepted. Argument has been reformulated to avoid confusion
39013	15	17	15	17	Lamarche-Gagnon et al., 2019 reference not included in the biblio [Alexandra Rouillard, Sweden]	Noted. Reference has now been added the reference list
8723	15	17	15	22	Did methane sinks play any role in palaeo methane trends? [Vaishali Naik, United States of America]	Accepted. A recent study (Kleinen et al., 20 ClimPast) suggests that the LGM-PI difference in the atmospheric methane budget can be explained uniquely by changes in methane sources. This aspect is now explicitly mentioned.
31863	15	17	15	24	should cite Hmiel, Benjamin, et al. "Preindustrial 14 CH 4 indicates greater anthropogenic fossil CH 4 emissions." Nature 578.7795 (2020): 409-412. also probably here or later Dyonisius, M. N., et al. "Old carbon reservoirs were not important in the deglacial methane budget." Science 367.6480 (2020): 907-910. [Euan G. Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - the Dyonisius20 references has now been considered
13423	15	18	15	18	Modify syntax. It is not posible to use a comma before the main verb (in this case “suggest” is the main verb). [Maria Amparo Martinez Arroyo, Mexico]	Accepted - text revised
41571	15	20	15	20	Kleinen et al. (Clim. Past, 2020) have modelled CH4 from LGM to PI, have apportioned CH4 emissions to source regions / categories -- https://doi.org/10.5194/cp-16-575-2020 [Thomas Kleinen, Germany]	Noted. Reference is now taken into consideration
8725	15	22	15	24	Also more recently Dyonisius et al (2020) https://science.sciencemag.org/content/sci/367/6480/907.full.pdf [Vaishali Naik, United States of America]	Accepted. Reference has now been considered

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
18575	15	22	15	24	With regards to permafrost, I think this statement is overstated... "negligible" permafrost emissions are up to 19% of CH4 emissions at the YD-PB transgression accoridng to Petrenko and as much as 53 Tg/year according to Dyonisius et al 2020. There are also a lot of assumptions that go into this estimate... for example, how old carbon emitted from permafrost would be. Dyonisius estimates that it's 7500 years older than contemporary carbon, bu it's based on regionally specific limited data. The way the model is set up, if the net age of the permafrost is younger, the contribution from permafrost increases. I would continue to label permafrost carbon contribution as low confidence here. From how it's written, it sounds like the the science is settled (which it is more likely to be in terms of clathrates) [Miriam Jones, United States of America]	Rejected. Several sources of the literature point towards the same conclusion. We maintain that the available evidence points towards medium confidence
58527	15	22	15	24	I think this can now be said at high confidence. Another important reference here would be Vaks et al. (2013) Speleothems reveal 500 kyr history of Siberian permafrost. Science, 340. I think that there is both high agreement and robust evidence to support the idea that global climates only slightly warmer than today are sufficient to thaw siginficant permafrost, and consequently, geological CH4 C-cycle forcings/feedbacks appear small. Therefore, I suggest the authors consider stating this at high confidence instead of medium confidence. Also note that this is consistent with the recent paper by Hmiel et al (2020) Preindustrial 14CH4 indicates greater anthropogenic fossil CH4 emissions. Nature 5787, 409-412. Revising this to high confidence also has implications for the Chapter 5 Executive summary (particularly pg. 6, line 48, and pg. 7, line 51). Therefore, if this is changed, perhaps the authors might include a palaeo statement after pg. 7, line 51? [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected. A number of studies indeed point towards similar conclusions, yet the available literature is limited and focusses on ice core measurements. We maintain that, in this context, medium confidence is warranted.
8727	15	26	15	27	it is not clear how photochemical removal in the stratosphere regulates N2O emissions. Are you talking about N2O concentrations? [Vaishali Naik, United States of America]	Accepted - text revised
72915	15	33	15	33	Capitalise 'inter tropical convergence zone' ('Inter Tropical Convergence Zone'). [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
96545	15	34	15	35	Does the word step make sense here: "About 90% of these step increases occurred"? [Nicole Wilke, Germany]	Noted. We feel it does.
103107	15	34			add: "(AMOC)" - acronym used later [Philippe Tulkens, Belgium]	Accepted - change was made.
81587	15	39	15	41	Imbalances in the weathering-burial cycle and CaCO3 compensation should in my opinion also be mentioned here as these are relevant on long time scales. [Fortunat Joos, Switzerland]	Accepted. Text revised
81585	15	43	15	43	I strongly disagree with the statement regarding the change in the land biosphere carbon inventory. It is not a consensus view that "rapid oxidation of organic carbon from land" contributed to the deglacial CO2 increase. It is also in conflict with what is said on the next page (p16) line 10 ff. [Fortunat Joos, Switzerland]	Accepted. Text revised
72917	15	44	15	44	References should be in chronological order. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial - this kind of issues will be taken care of during the production phase, which includes professional copy-editing.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
116407	15		15		The assessment related to drivers, leads and lags for the last termination is too restricted. This was done in detail in the AR5. Assessing recent literature (eg Beeman et al, Clim Past 2019, Bereiter et al, Nature 2018 etc) and providing clear conclusion is relevant. [Valerie Masson-Delmotte, France]	Accepted. Paragraph has been thoroughly revised and now includes both references
81589	16	1	16	4	Suggest to also mention here ocean sediment interactions and weathering and burial as well as changes in soil carbon (in addition to peat and permafrost) and wetlands. Notably, soil carbon is a major source for N2O and wetlands for CH4 [Fortunat Joos, Switzerland]	Accepted. Text revised
72919	16	4	16	4	Insert 'to' after 'prior'. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change was made.
74191	16	6	16	7	The minus sign in front of 190 is implicit already in the word “lower” afterwards. [Christoph Völker, Germany]	Not applicable. Sentence has been removed
85	16	6	16	8	this sentence doesn’t make much sense. Different than what? Also it is a one sentence paragraph. I suggest to delete. [Andreas Schmittner-Boesch, United States of America]	Accepted. Sentence was removed
41095	16	6		8	this sentence is very misleading, as it reads as the concentration was 90 ppm during glacial periods. Would you rephrase it a more straightforward manner? [TSU WGI, France]	Accepted. Sentence was removed
18577	16	7	16	8	"arguably different" provide some additional information here about how the interactions between climate and the global carbon cycle were different? [Miriam Jones, United States of America]	Accepted. Sentence was removed
111967	16	7			consistent unit GtCO2 [Tomas Halenka, Czech Republic]	Not applicable. Sentence has been removed
89467	16	10	15	11	Potentially relevant reference: Lingren 2018 Nature Extensive loss of past permafrost carbon but a net accumulation into present day soils https://doi.org/10.1038/s41586-018-0371-0 [Edward Schuur, United States of America]	Noted. Reference has now been considered
19939	16	10	16	18	Commas inserted after "cycle" on line 10, "dynamics" on line 17, "sea-level" on line 18, will make te sentences easier to understand. [philippe waldteufel, France]	Accepted. Commas added as recommended
18579	16	13	16	13	"arguably more realistic calculations": why are they more realistic? [Miriam Jones, United States of America]	Accepted - argument was removed
88491	16	13	16	13	Add the sign minus "-" to clarify that this is the decline of the land C inventory in glacials compared to interglacial [Damien Cardinal, France]	Accepted
88967	16	13	16	16	"PgC" and "GtC" are used. The same unit should be used for comparison. [AKIHIKO MURATA, Japan]	Accepted - change was made.
127693	16	14	16	16	Different units make for a difficult comparison (850 PgC vs 4000 GtC); suggest keeping units consistent to make it easier for the reader. [Trigg Talley, United States of America]	Not applicable. Sentence has been removed
78471	16	16	16	16	where does the figure of 4000 GtC come from? Seems to disagree with figure 5.12 [Chris Jones, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. Sentence has been removed
111969	16	16			PgC preferably to be consistent with the above unit [Tomas Halenka, Czech Republic]	Not applicable. Sentence has been removed
88493	16	20	16	25	This § relates to deglaciation while § above and after are on glacials. This is confusing. [Damien Cardinal, France]	Accepted - structure of the subsection has been thoroughly revised
83489	16	21	16	21	See comment for page 12 on use of ka and kyr. If using ka for a date is agreed upon, it should say 14.7–12.7 ka BP here. [Antje H. L. Voelker, Portugal]	Accepted - change was made.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
23681	16	23	16	24	The jargon used in this sentence with "has been proposed" and "potentilally" send the wrong message to the reader. It is a FACT that rapid warming IS destabilising permafrost and liberating vast quantitites of labile carbon. Please rephrase [Massimo Lupascu, Singapore]	Accepted - text revised
18581	16	23	16	25	Consistency of message from above... P15, line 24 you say permafrost impact is negligible and dismisses permafrost as playing a major role because of 14C in ice CH4... Petrenko et al., 2017 (and more recently Dyonisius et al. 2020 Science). Another paper that could be cited here is Turetsky et al., 2020 Nature Geoscience 13, 138-143 [Miriam Jones, United States of America]	Accepted. Sentence has been modified for the sake of clarity.
88183	16	24	16	24	"thawed permafrost" (or degraded permafrost) would be better than "destabilized permafrost" [Sharon Smith, Canada]	Accepted - text revised
71169	16	24			What do the authors mean by "destabilised permafrost"? This is also terminology, not typical for traditional permafrost science and engineering. See also Comment Nr. 2. [Lukas Arenson, Canada]	Accepted - text revised
727	16	25	16	25	Add new reference Meyer et al (2019). Full reference: Meyer, V. D.; Hefter, J.; Köhler, P.; Tiedemann, R.; Gersonde, R.; Wacker, L. & Mollenhauer, G. Permafrost-carbon mobilization in Beringia caused by deglacial meltwater runoff, sea-level rise and warming Environmental Research Letters, 2019, 14, 085003, doi: 10.1088/1748-9326/ab2653 [Peter Köhler, Germany]	Accepted - reference has been considered
87	16	28	16	28	include Khatiwala et al., 2019, doi: 10.1126/sciadv.aaw4981 [Andreas Schmittner-Boesch, United States of America]	Noted. Reference has been added
104857	16	28	16	28	Jacobel citation not in reference list [Timothy DeVries, United States of America]	Noted. Reference has now been added the reference list
93427	16	28	16	28	Jacobel et al ref is missing in the list. It probably is Jacobel, A. W., Anderson, R. F., Jaccard, S. L., McManus, J. F., Pavia, F. J., & Winckler, G. (2020). Deep Pacific storage of respired carbon during the last ice age: Perspectives from bottom water oxygen reconstructions. Quaternary Science Reviews, 230, 106065. https://doi.org/https://doi.org/10.1016/j.quascirev.2019.106065 [Carles Pelejero, Spain]	Noted. Reference has now been added the reference list
72921	16	30	16	31	colder temperatures ' is physically meaningless. It is either 'cold' or 'colder' or 'the temperature is lower'. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
65707	16	30	16	32	For improved clarity, suggest changing to: "A combination of increased CO2 solubility associated with generally colder oceanic temperatures, increased oceanic residence time of CO2 (Skinner et al., 2017), altered oceanic alkalinity (Yu et al., 2010a; Cartapanis et al., 2018; Hoogakker et al., 2018); and a generally more efficient marine biological carbon pump (Galbraith and Jaccard, 2015; Galbraith and Skinner, 2020; Yu et al., 2019) conspired to partition CO2 into the ocean interior (Anderson et al., 2019) (medium confidence)." Papers from late 2019 provide paleoclimate constraints on all three of these biogeochemical drivers of ocean-atmosphere CO2 exchange as well as regionality in source-sink behaviour. In particular paleoceanographic constraints on glacial-interglacial changes in regional source-sink modulation provides insights into modern and likely future changes in ocean uptake of CO2. For example: - Moy, A. D., M. R. Palmer, W. R. Howard, J. Bijma, M. J. Cooper, E. Calvo, C. Pelejero, M. K. Gagan, and T. B. Chalk (2019), Varied contribution of the Southern Ocean to deglacial atmospheric CO2 rise, Nat. Geosci., 12(12), 1006-1011, doi:10.1038/s41561-019-0473-9. - Shao, J., L. D. Stott, W. R. Gray, R. Greenop, I. Pecher, H. L. Neil, R. B. Coffin, B. Davy, and J. W. B. Rae (2019), Atmosphere-Ocean CO2 Exchange Across the Last Deglaciation From the Boron Isotope Proxy, Paleoceanography and Paleoclimatology(n/a), doi:10.1029/2018PA003498. [Kushla Munro, Australia]	Accepted. Sentence has been modified as recommended. Both recommended references have now been included.
89	16	30	16	34	This statement is controversial. Also an important recent study (Khatiwala et al., 2019, doi: 10.1126/sciadv.aaw4981) is missing. This study shows that the temperature effect (44-45 ppm) is much larger than previously thought, because it is amplified by an increase in physical disequilibrium carbon, an effect ignored by previous studies. It also suggests that iron fertilization (26-39 ppm) contributions were important and together with temperature account for more than three-quarters of the glacial-interglacial CO2 difference. Also, it suggests, contrary to the statement here, that the biological pump was not more efficient. It attributes the increased carbon storage to an increase in disequilibrium carbon. [Andreas Schmittner-Boesch, United States of America]	Noted. A sentence highlighting these recent results has been added to the revised text.
3913	16	33	16	33	"Yu et al. 2019" => No reference in reference list [Makio Honda, Japan]	Noted. Reference has now been added the reference list
93431	16	33	16	33	Yu et al 2019 ref is missing in the list [Carles Pelejero, Spain]	Noted. Reference has now been added the reference list
74193	16	34	16	34	I think the “temporally consistent” is an unclear way of saying “consistent between different glacial periods” [Christoph Völker, Germany]	Accepted - text revised
81591	16	34	16	37	I suggest to formulate this a bit more cautiously and more as a suggestion than as a given fact following the abstract of Galbraith and Egglestone. [Fortunat Joos, Switzerland]	Accepted - text revised
88969	16	35	16	36	"as a result of … N2 fixation (Galbraith and Eggleston, 2017)" is too conclusive expression. What is stated in the followings "as a resutl of" is one possible scenario. "as a possible resutl of" is better. [AKIHIKO MURATA, Japan]	Accepted - text revised
88495	16	35	16	36	add "marine" or "ocean": as a result of CO2 limitation of MARINE photosynthesis" [Damien Cardinal, France]	Accepted - text revised

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
83491	16	40	16	40	May be add Chowdhry Beeman, J., Gest, L., Parrenin, F., Raynaud, D., Fudge, T.J., Buizert, C., Brook, E.J., 2019. Antarctic temperature and CO2: near-synchrony yet variable phasing during the last deglaciation. Clim. Past 15, 913-926, doi: 10.5194/cp-15-913-2019. here as well. [Antje H. L. Voelker, Portugal]	Accepted - reference has now been considered
91	16	40	16	44	This statement is also controversial. Especially the reference to the Southern Ocean overturning circulation is very speculative. Missing is the study by Schmittner and Lund (2015, doi: 10.5194/cp-11-135-2015), which suggests an AMOC reduction could be the driving mechanism. [Andreas Schmittner-Boesch, United States of America]	Rejected. Several recent publications suggest that the AMOC remained relativela active and stable during this time interval (e.g. Lippold et al., 2016; Pöppelmeier et al., 2018; 2020)
58529	16	40	16	47	Are the authors aware of the recent work by Khatiwala et al. (2019) Air-sea disequilibrium enhances ocean carbon storage during glacial periods, Science Advances, 5? The same work is relevant to lines 30-34, and might motivate for high confidence in the conclusion that glacial-interglacial changes in atmospheric CO2 are primarily (three-quarters) driven by the solubility pump and biological pump? As corrollary, there are now multiple lines of high quality evidence to suggest that oceanic circulation and sea ice extent are relatively minor effects on glacial-interglacial C-cycle. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Discussion has been expanded to take these important aspects into consideration
103109	16	40			"sudden": Noting that there is a statement on p.17, line 49-55, the word should not be used. "sudden" immediately implies a change as drastical as observed now, while the increase rate seems to have been one order of magnitude lower. [Philippe Tulkens, Belgium]	Accepted. Term has been removed to avoid confusion.
103111	16	40			This seems to be the "West Antarctic Divide" event shown on figures 5-1 and 5-2. Can this term be intrioduced here? [Philippe Tulkens, Belgium]	Rejected. We feel that this aspect is superfluous.
17919	16	43	16	44	Add reference (e.g. Ernie R. Lewis Stephen E. Schwartz (2004). Sea salt aerosol production : mechanisms, methods, measurements and models. Published by the American Geophysical Union as part of the Geophysical Monograph Series, Volume 152.) [Patricia Lopez Garcia, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. Reference inappropriate
2493	16	43			As the only study to date, Ronge et al., 2020 (Paleoceanography & Paleoclimatology 35, PA003733) indicate a contribution from the Indian Sector of the Southern Ocean [Thomas Ronge, Germany]	Rejected. The study is somewhat controversial and has, in part, been questioned by Gottschalk et al., 2020.
90077	16	46	16	46	This statement uploaded by Ted Schuur on behalf of a graduate class that reviewed the chapter (this was communicated with IPCC TSU in advance): AMOC is not defined. [Edward Schuur, United States of America]	Accepted. The term in now defined

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
731	16	47	16	47	Please add as alternative explanations for the rapid CO2 rises at 14.8 and 12.9 ka: " but have alternatively been suggested to be caused by CO2 released from northern hemispheric permafrost, potentially destablized through sea level rise flooding Arctic shelf areas (Winterfeld et al., 2018; Meyer et al., 2019)." Full refererces: Winterfeld, M.; Mollenhauer, G.; Dummann, W.; Köhler, P.; Lembke-Jene, L.; Meyer, V. D.; Hefter, J.; McIntyre, C.; Wacker, L.; Kokfelt, U. & Tiedemann, R. Deglacial mobilization of pre-aged terrestrial carbon from degrading permafrost Nature Communications, 2018, 9, 3666, doi: 10.1038/s41467-018-06080-w. Meyer, V. D.; Hefter, J.; Köhler, P.; Tiedemann, R.; Gersonde, R.; Wacker, L. & Mollenhauer, G. Permafrost-carbon mobilization in Beringia caused by deglacial meltwater runoff, sea-level rise and warming Environmental Research Letters, 2019, 14, 085003, doi: 10.1088/1748-9326/ab2653. [Peter Köhler, Germany]	Noted. The references have been considered.
729	16	49	6	49	Please add new reference: Hasenclever et al. (2017), which is a model-based study on the effect of sea-level on hydrothermal activity and volcanic CO2 release, Full reference: Hasenclever, J.; Knorr, G.; Rüpke, L.; Köhler, P.; Morgan, J.; Garofalo, K.; Barker, S.; Lohmann, G. & Hall, I. Sea level fall during glaciation stabilized atmospheric CO$_2$ by enhanced volcanic degassing Nature Communications, 2017, 8, 15867, doi: 10.1038/ncomms15867. [Peter Köhler, Germany]	Accepted - reference was considered in revised document
58531	16	49	16	52	Are the authors aware that Hasenclever et al. (2015) Nature Communications 8:15867 quantified the effect of mantle degassing in response to sea level fall at 18 ppm? This work appears to be quite robust. If this study is included, it might be concluded that mantle CO2 degassing is a relatively small contribution to deglacial CO2 rise (medium confidence). [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - reference was considered in revised document
26895	16	52	16	52	what is atrributed with a 'low confidence": the geological carbon release or the mantle degassing? it is not clear here. [Eric Brun, France]	Noted. Both mechanisms are coupled
9301	16	55	17	1	Suggest replacing "reduce" to "reduced" [Christine Weldrick, Australia]	Accepted - change was made.
116409	16		16		I do not understand the statement on the importance of CO2 for the total change in RF during the last deglaciation (what about the role of ice sheet albedo and the role of change in dust). [Valerie Masson-Delmotte, France]	Accepted. Argument has been reformulated to avoid confusion
38499	17	6	17	6	should read "Earth System Models of Intermediate Complexity" [Siv K Lauvset, Norway]	Accepted - change was made. "system" was added and the words except "Earth" were capitalized (in order to follow style from previous reports, such as 1.5 Special Report).
93	17	9	17	10	again this attribution is controversial. I would argue that the attribution from the above mentioned Khatiwala et al. study is more reliable, because it is based on a data-constrained 3-dimensional model and it uses a complete and accurate carbon decomposition. Ganospolsky and Brovkin do not separate between “solubility” and ocean circulation. I suggest to remove this or to add the alternative Khatiwala study. [Andreas Schmittner-Boesch, United States of America]	Accepted. Khatiwala19 is now considered
72923	17	10	17	10	Delete , after 'outgassing'. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change was made.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
58567	17	15	17	15	The partial pressure of CO2 has been expressed in ppm up to here, where µatm is used [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. Sentence has been revised
9851	17	15	17	38	would be useful to assess literature on pre-Industrial anthropogenic CO2 and CH4 emissions [Robert Kopp, United States of America]	Accepted. Pre-industrial anthropogenic CH4 and CO2 sources are now briefly assessed
112311	17	15	17	38	There are several studies for the last 1000 or 2000 years for CO2, CH4 and N2O. High-resolution concentration and isotope dada from ice cores should be discussed. Please see Ch.2 for the references. [Jinho Ahn, Republic of Korea]	Rejected. Although a detailed assessment of GHG evolution during the last millennia would be desirable, the focus of the sub-section is the Holocene (mostly for space constraints).
5043	17	17	17	18	I query the statement that "The pre-industrial Holocene (11.7 kyr–1750) was characterised by relatively stable global climate conditions". Perhaps this is true from 10 kyr onwards, but the very early Holocene from around 11.7 kyr–10 kyr was a time of rapid climatic change involving several degrees of warming, with further instability up to c. 8 kyr. There is of course the Preboreal Oscillation (PBO) and the 8.2 kyr event which lasted for around 200 years or more. There is a range of evidence to support this early Holocene climatic instability in Europe, including ice core data (e.g. Vinther et al. 2009), with other examples including pollen-based (Davis et al., 2003) and chironomid-based (Brooks et al., 2012) temperature reconstructions. It is important that the variability of the early Holocene is not ignored as the rapid changes in environment during this time period are potentially informative of future events. I suggest a minor edit supported by suitable references (though not necessarily those listed here) is needed here at the discretion of the authors. Davis et al. (2003) The temperature of Europe during the Holocene reconstructedfrom pollen data. QSR. doi:10.1016/S0277-3791(03)00173-2 Brooks et al. (2012) High resolution Lateglacial and early-Holocene summer air temperaturerecords from Scotland inferred from chironomid assemblages. QSR. doi:10.1016/j.quascirev.2012.03.007 [Thomas Kelly, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. Sentence has been removed
83493	17	17	17	19	see previsous comments regarding refering to a date; correctly it should be "ka" in both date (age) references here. [Antje H. L. Voelker, Portugal]	Accepted
21785	17	17	17	21	This is covered in much more detail in chapter 2. It would make more sense to lift and refer to the chapter 2 finding in this regard as it would limit the possibility of folks playing spot-the-difference. [Peter Thorne, Ireland]	Accepted
10689	17	18	17	21	The "coolest temperatures of the Holocene" appear to be at the start of the Holocene, not the end, according to Cross chap box 2.1 Figure 1 [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. Sentence has been removed
72925	17	20	17	20	Replace 'coolest' with 'lowest'. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. Sentence has been removed
87683	17	20	17	21	I do not see a definition of the terms here. [Ivy Frenger, Germany]	Not applicable. Sentence has been removed
109749	17	20	17	21	The amplitude of Holocene long-term cooling is uncertain. It is larger in borehole-based estimates than stated here. [Charpentier Ljungqvist Fredrik, Sweden]	Not applicable. Sentence has been removed

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
58565	17	22	17	22	The AMOC abbreviation has already been used so doesn't need to spelled out here. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable. Sentence has been removed
78473	17	25	15	25	suggest “ppm” as common unit throughout instead of micro-atm [Chris Jones, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. Sentence has been removed
103113	17	25			use ppm (not µatm) [Philippe Tulkens, Belgium]	Not applicable. Sentence has been removed
79651	17	28	17	29	Could include the recent study in the reference list - Chaudhary, N, Westermann, S, Lamba, S, et al. Modelling past and future peatland carbon dynamics across the pan-Arctic. Glob Change Biol. 2020; 00: 1– 15. https://doi.org/10.1111/gcb.15099 [Nitin Chaudhary, Sweden]	Noted
89469	17	28	17	30	Potentially relevant reference: Lingren 2018 Nature Extensive loss of past permafrost carbon but a net accumulation into present day soils https://doi.org/10.1038/s41586-018-0371-0 [Edward Schuur, United States of America]	Accepted. Reference has now been included in the assessment.
5041	17	28	17	38	Addition: I am aware that northern peatlands store more carbon, but it seems remiss that there is no mention of tropical peatland carbon storage in here; they could represent c. 25% of global peatland carbon storage. It is also possible that our current ground-truthed estimates of peatland area/volume for regions such as tropical South America are perhaps only a third of their actual value, although this is based on remote sensing data and remains to be ground-truthed (Gumbricht et al. 2017). Our knowledge has expanded markedly over recent years, and given their C density they are extremely important at a regional level; in several cases, at a national level they store more than above ground carbon in forests. It is probably worth inserting a sentence on Line 34 something along the lines of: "Similarly, while there are substantial sources of uncertainty in tropical peatland carbon estimates (Lawson et al., 2015), recent discoveries of large peatland areas in Amazonia and the Congo Basin have added to existing estimates for Southeast Asia and suggest between 69.6–129.8 GtC is currently stored as peat across tropical regions (Page et al., 2011; Draper et al., 2104; Dargie et al., 2017) (low confidence)." Page et al. (2011) Global and regional importance of the tropical peatland carbon pool. Global Change Biology, 17:798–818 Draper et al. (2014) The distribution and amount of carbon in the largest peatland complex in Amazonia. Environ. Res. Lett. 9, 124017 Lawson et al. (2015) Improving estimates of tropical peatland area, carbon storage, and greenhouse gas fluxes. Wetlands Ecology and Management. DOI: 10.1007/s11273-014-9402-2 Dargie et al. (2017) Age, extent and carbon storage of the central Congo Basin peatland complex. Nature, 542: 86–90 Gumbricht et al. (2017) An expert system model for mapping tropical wetlands	Not applicable. Section has been removed

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
18583	17	28	17	39	In general, I thought this paragraph on peat needed work. Many of the sentences lacked clarity or were awkwardly worded. Perhaps it should be stated that peatlands take up CO2 through peat accumulation and also release CH4, but that the net balance over the deglacial and Holocene was that they were carbon reservoirs. Also, the balance of terrestrial and ocean C uptake/release seems thrown in and not specific to peatlands, or the linkage to how terrestrial (and peat) carbon uptake balances pCO2 should be clarified. [Miriam Jones, United States of America]	Accepted. Holocene section has been thoroughly revised
58569	17	29	17	29	Rates of what? ("Rates were higher under relatively higher temperatures") [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. By rates, we mean carbon accumulation.
18585	17	29	17	33	sentence beginning "Sequestration rates…" should be broken up for clarity, such as period after Loisel and Stocker citation and new sentence beginning "Rapid peat growth…" [Miriam Jones, United States of America]	Not applicable. Sentence has been removed
18587	17	31	17	33	"rapid peat growth…" this sentence could be clarified. I'm not sure what you mean by 'maintaining Holcoene atmospheric CO2 concentrations within limited bounds." limited bounds of what? Ice core data and top-down models? [Miriam Jones, United States of America]	Not applicable. Sentence has been removed
13425	17	32	17	33	Check line spacing, which is different (wider) to the rest of the text. [Maria Amparo Martinez Arroyo, Mexico]	Editorial. Professional copy-editing to be completed prior to publication. This kind of issues will be fixed then.
78475	17	33	17	33	this “within Holocene” increase in carbon is bigger than the entire glacial-inter glacial change from page 16. Are they consistent? [Chris Jones, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. Sentence has been removed
5039	17	33	17	33	Correction: I think the Nichols and Peteet (2019) estimate quoted here has not been quoted correctly. Unless I have misunderstood, 545 Gt C was an estimate based on earlier work. 1055 GtC is the 'middle' best estimate (NOT the upper end of the range as suggested). Referring to p 919 of Nichols and Peteet (2019) the correct numbers are a range of 511-1782 GtC, with a 'best estimate' of 1055 GtC. The result of this correction is roughly a doubling of the range of uncertainty, something which is important to acknowledge. [Thomas Kelly, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. Sentence has been removed
41573	17	33	17	34	Nicols & Peteet estimate is extremely high, very difficult to reconcile from ocean C cycle point of view [Thomas Kleinen, Germany]	Not applicable. Sentence has been removed
41575	17	34	17	35	I don't see a connection between the text and the Zheng et al citation. Zheng et al deal with Chinese peatlands at 42°N. This is not tropical! Zheng et al themselves write about "boreal". Furthermore to see a signature of B-A/YD, one would expect this around the Atlantic Ocean, not in Eastern China. Either the wrong paper is cited and the citation needs to be corrected, or the entire sentence should be removed. Kleinen et al., 2020 (https://doi.org/10.5194/cp-16-575-2020), do show increases in African wetland methane emissions between 15ka and 10 ka... [Thomas Kleinen, Germany]	Accepted. Sentence has been removed

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
31865	17	34	17	35	Surely this is reading much too much into the Zheng paper. It's about boreal wetland in China and the conclusion says we need to look more at the tropics. Saying tropical wetlands only marginally increased is going way beyond what Zheng et al say: their main remark is about BOREAL wetlands and (I quote) " biomarker results imply that boreal wetlands were not dominant in driving atmospheric CH4 concentrations across the last deglaciation and the Holocene." So if it's not tropics and it's not boreal, what was it? [Euan G. Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. Sentence has been removed
79653	17	34	17	38	Chaudhary etal. 2020 highlights the importance of peatland carbon dynamics at different timescales in the Arctic region. They found that peatlands would continue to act as a carbon sink under different warming scenarios but their sink capacity would substantially reduce under high warming scenario after 2050; https://onlinelibrary.wiley.com/doi/abs/10.1111/gcb.15099 [Nitin Chaudhary, Sweden]	Noted
18591	17	35	17	36	I'm a little concerned that this Zheng et al paper isn't presented with more context. It's based on a regional study that may not be representative of the tropics as a whole. Given ice core data, such as the interpolar gradient and stable isotopes of CH4, tropical wetlands had to have contributed significantly to the rise in atmospheric CH4 during deglaciation. From what is written here, it sounds like they did not. Of course, there are also other players: thawing permafrost, boreal peatland expansion, biomass burning, etc. This needs a bit more context. [Miriam Jones, United States of America]	Not applicable. Sentence has been removed
18589	17	36	17	39	is the implication that the land biosphere CO2 uptake is from peatland expansion? If not, perhaps this should be moved elsewhere, as it appears that this paragraph is primarily about peatlands and their role in CO2 and CH4uptake and release, or contextualizing the role of peatland CO2 uptake in the broader land biosphere. [Miriam Jones, United States of America]	Accepted. Subsection has been revised.
64593	17	41	17	41	Suggest alternate subsection title: "Using paleoclimate to understand the future" [Charles Curry, Canada]	Not applicable. Subsection has been removed
106165	17	41	17	41	"5.1.3.3 Past to understand the future": Past what? Improve wording [Wolfgang Lucht, Germany]	Not applicable. Subsection has been removed
74195	17	41	17	41	Has something been forgotten in the subsection header? “Past to understand” seems strange to me. “Using the Past to understand” would be clearer [Christoph Völker, Germany]	Not applicable. Subsection has been removed
19941	17	41	18	7	There is not much information in this subsection 5.1.3.3. The 1rst paragraph is just talk; giving a couple of examples of the "key insights" mentioned on line 45 would be more valuable. The second paragraph is mostly repeating information already given; moreover, while Gattegno et al stress the risks due to acidification, they link it to increase of carbon in the oceans without focusing on the rate of increase The 3rd paragraph has little to do with the past helping to understand the future… [philippe waldteufel, France]	Accepted. Subsection has been removed.
21789	17	41			This final subsection of 5.1.3 really didn't feel to me like it added anything above and beyond what had been said already. It should either be recast explicitly as a summary or removed. [Peter Thorne, Ireland]	Accepted. Subsection has been removed.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
127695	17	46	17	47	A worthwhile paper on this point is: Higgs E, DA Falk, A Guerrini, M Hall, J Harris, RJ Hobbs, ST Jackson, JM Rhemtulla, and W Throop. 2014. The changing role of history in restoration ecology. Frontiers in Ecology and the Environment 12(9): 499,Äì506. http://doi:10.1890/110267 [Trigg Talley, United States of America]	Noted. Subsection has now been removed
10951	17	52	17	55	To adjust this section as it stands, there is a specific reference for oceanic ecological shifts due to climate change, a citation for terrestrial changes might be helpful. There has been recent work summarizing future ecological shifts due to climate change (Nolan et al., 2018, 10.1126/science.aan5360). Citing other relevant sections of Chapter 5 may be helpful here as well, with section 5.4.8 covering abrupt changes to terrestrial and oceanic environments. Beyond this, this section is convolute and challenging to understand. This sentance either needs to be modifed or removed. [Joseph Thomas, United States of America]	Not applicable. Sentence has been removed
83495	17	54	17	55	Besides Gruber I would cite here also the review by Levin (2018), which is already listed in the references for this chapter. Levin, L.A., 2018. Manifestation, Drivers, and Emergence of Open Ocean Deoxygenation. Annual Review of Marine Science 10, 229-260, doi: 10.1146/annurev-marine-121916-063359. [Antje H. L. Voelker, Portugal]	Not applicable. Sentence has been removed
116411	17		17		Redundancy with ch 2 on Holocene temperature change, please check and avoid duplication. [Valerie Masson-Delmotte, France]	Accepted. Holocene section was thoroughly revised to focus on GHGs and their feedbacks
116413	17		17		Insights from section 5.1.3.3 need to be captured in the FAQs related to paleoclimate (linked to ch 2 I think). Links to SROCC and SRCCL are missing ([Valerie Masson-Delmotte, France]	Accepted. Link has been added.
21787	18	2	18	7	Chapter 4 had a very substantial segment on tipping points so I'm not sure what this paragraph, based upon a single study, adds. It would be better to point the reader to the assessment in chapter 4 or delete this paragraph I suspect. [Peter Thorne, Ireland]	Noted. Subsection has now been removed
15959	18	2	18	7	This paragraph directly contradicts the discussion points on the previous sections. This previous sections have made clear that CO2 is rising at a rate of about 2 orders of magnitude faster than anything in the paleoclimate records, and that the existing levels are beyond anything seen over the past 800k years, and it is these that will determine if tipping points are exceeded rather than a hypothetical constraining of the equilibrium temperature to below 2 degC. It is clear from today's observational measurements that tipping points are already being exceeded, e.g sea ice, methane releases from the ESAS, forest fires, etc. In its current form this paragraph erroneously diminishes the risk that we are currently facing. [Kevin Lister, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. Sentence has been removed
81023	18	6	18	7	Perhaps a brief expalnation of polar amplification needs to be provided here, or at least include a reference to a sub-section where the phenomenon is explained in more detail. [Jeffrey Philip OBBARD, Singapore]	Not applicable. Sentence has been removed
74197	18	7	18	8	Can one use the expression “polar amplification” here without further explanation? [Christoph Völker, Germany]	Noted. Subsection has now been removed

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
21791	18	10	18	10	Use of historical is somewhat misleading as you have just spent a huge amount of time discussing much more ancient proxy based records. Instrumental-era, industrial-era or something similar would be better than historical here? [Peter Thorne, Ireland]	Accepted. Industrial Era.
39669	18	10			section 5.2: The periods over which some fluxes/values are calculated is not always clearly expressed. [TSU WGI, France]	Accepted - We have added time periods in places through out the section where could have been doubt about the period cover.
72927	18	13	18	13	Replace 'its' with 'their'. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change was made.
2327	18	27		28	Why not to shed the light on more recent studies highlighting the atmospheric CO2 observations' stations? such as Apadula et al. (2019): Apadula F., Claudio Cassardo, Silvia Ferrarese, Daniela Heltai and Andrea Lanza, 2019. Thirty Years of Atmospheric CO2 Observations at the Plateau Rosa Station, Italy. Atmosphere 2019, 10, 418; doi:10.3390/atmos10070418 ; www.mdpi.com/journal/atmosphere [Abed El Rahman Hassoun, Lebanon]	Rejected. It is true we have many sites now where the measurements are being made for more than 30 years. Much of these data are used in the inverse modelling and the CO2 fluxes are presented later in this section 5.2.1. Within the limited space here we are restricted to a couple sites, which are offering a variety of supporting measurements as you can see from Fig. 5.4.
2185	18	27		28	Why not to shed the light on more recent studies highlighting the atmospheric CO2 observations' stations? such as : Apadula et al. (2019) [Abed El Rahman Hassoun, Lebanon]	Rejected. This is a repeat comment from same reviewer. Please see reply to comment #2327
2239	18	27		28	Apadula F., Claudio Cassardo, Silvia Ferrarese, Daniela Heltai and Andrea Lanza, 2019. Thirty Years of Atmospheric CO2 Observations at the Plateau Rosa Station, Italy. Atmosphere 2019, 10, 418; doi:10.3390/atmos10070418 ; www.mdpi.com/journal/atmosphere [Abed El Rahman Hassoun, Lebanon]	Rejected. This is a repeat comment from same reviewer. Please see reply to comment #2327
2283	18	27			Apadula F., Claudio Cassardo, Silvia Ferrarese, Daniela Heltai and Andrea Lanza, 2019. Thirty Years of Atmospheric CO2 Observations at the Plateau Rosa Station, Italy. Atmosphere 2019, 10, 418; doi:10.3390/atmos10070418 ; www.mdpi.com/journal/atmosphere [Abed El Rahman Hassoun, Lebanon]	Rejected. This is a repeat comment from same reviewer. Please see reply to comment #2327
78477	18	32	18	32	monthly mean – should be “annual mean” [Chris Jones, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. Fig. 5.4b is indeed showing monthly-mean values
58579	18	32	18	32	Clarify: the growth rates of what? [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Its CO2 growth rate
58581	18	33	18	33	in "...5.4b; lines" I would clarify with "...5.4b; shaded lines" [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected. We are indeed referring to the lines. The shaded background shows the ENSO cycle

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
367	18	35	18	35	I recommend to add at the end of the paragraph: Satellite CO2 observations have been shown to be consistent with the CO2 surface observations (e.g., Buchwitz et al., 2018; Reuter et al., 2020). References: Buchwitz, M., Reuter, M., Schneising, O., Noel, S., Gier, B., Bovensmann, H., Burrows, J. P., Boesch, H., Anand, J., Parker, R. J., Somkuti, P., Detmers, R. G., Hasekamp, O. P., Aben, I., Butz, A., Kuze, A., Suto, H., Yoshida, Y., Crisp, D., and O'Dell, C., Computation and analysis of atmospheric carbon dioxide annual mean growth rates from satellite observations during 2003-2016, Atmos. Chem. Phys., 18, 17355-17370, https://doi.org/10.5194/acp-18-17355-2018, 2018. Reuter, M., Buchwitz, M., Schneising, O., Noel, S., Bovensmann, H., Burrows, J. P., Boesch, H., Di Noia, A., Anand, J., Parker, R. J., Somkuti, P., Wu, L., Hasekamp, O. P., Aben, I., Kuze, A., Suto, H., Shiomi, K., Yoshida, Y., Morino, I., Crisp, D., O'Dell, C. W., Notholt, J., Petri, C., Warneke, T., Velazco, V. A., Deutscher, N. M., Griffith, D. W. T., Kivi, R., Pollard, D. F., Hase, F., Sussmann, R., Te, Y. V., Strong, K., Roche, S., Sha, M. K., De Maziere, M., Feist, D. G., Iraci, L. T., Roehl, C. M., Retscher, C., and Schepers, D.: Ensemble-based satellite-derived carbon dioxide and methane column-averaged dry-air mole fraction data sets (2003-2018) for carbon and climate applications, Atmos. Meas. Tech., 13, 789-819, https://doi.org/10.5194/amt-13-789-2020, 2020. [Michael Buchwitz, Germany]	Taken into account - we understand there are still efforts to compare the CO2 from in situ and remote sensing observations because they are not seeing the same airmass. So a statement on consistence of CO2 in situ and XCO2 total column is not required here. However, based on your comments we have inserted one sentence to mention about the new measurements are being available since the 2009.
58721	18	37	19	5	This paragraph summarizes the evidence of the dominant role of human activities in the growth of atmospheric CO2, which is also summarized by Figure 5.4. All data in Figure 5.4 were discussed in this paragraph except the ∂13C-CO2. It may be appropriate to include a statement of the significance of the decrease in ∂13C-CO2 in this paragraph as well. References for this could be, for example, Rubino et al., 2013 (A revised 1000 year atmospheric 13C-CO2 record from Law Dome and South Pole, Antarctica) or Keeling, 1979 (The Suess effect: 13Carbon-14Carbon interrelations). [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - Rubino et al. (2013) is added to the sentence "2) Measurements of the stable carbon isotope (13C-CO2) show more negative values with time because both coal and oil extracted from geological storage are depleted in 13C (Rubino et al., 2013; Keeling et al., 2017) (Figure 5.4c)". Only the recent reference is added.
58583	18	38	18	40	I would preface the numbered list of evidence of the human's role in the growth of atmospheric CO2 with something simple like: "there are now multiple lines of evidence… of atmospheric CO2, here listed." [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. The sentence is revised as "Here listed are multiple lines of evidence.."
96547	18	40	18	40	The short form MLO does not help easy reading (even though it is explained in the paragraph above). [Nicole Wilke, Germany]	Accepted - MLO expanded as Mauna Loa

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
2777	18	40	18	44	One of the reasons for the difference between the SPO and MLO is that the major carbon sinks of the tropical forests and southern ocean are in the southern hemisphere (in addition to sources in the northern hemisphere). However the systematic increase in the difference is interesting. Do the models show that this difference is due to the source, and not due to the retardation effects of CO2 crossing the equator and reaching the south pole due to the southern hemisphere CO2 sinks. Would it be possible to include a confidence statement in this section? [Stephen Wilkinson, United Arab Emirates]	Taken into account. Yes, such tests are often done and has been addressed in the previous IPCC WG1 ARs. Thus due to the space limitations we cannot discuss much details here. For example, when we plot annual mean CO2 concentration a function of the latitude at surface or in the upper heights we can see that CO2 concentrations in the southern hemisphere are generally remain at constant level, and the CO2 increases with latitude in the northern hemisphere.
58763	18	40	18	48	In this paragraph, the explanation of factors 1-3 is relatively brief while the explanation of factor 4 is relatively long. It might be useful to provide a more balanced treatment of all 4 factors. Each is important and deserving of clear introduction to readers. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. The factors 1-3 were covered already in AR5, but the factor 4 is based on a newly produced dataset covering the full period of this analysis (1958-present). Thus the longer explanation. However, we have cut one general statement "A combination of the available atmospheric 14C observations and modelling has demonstrated that in the 1960s and 1970s, the decrease was largely due to uptake of the excess 14C into the ocean interior, but in recent years dilution by the large fossil fuel emission rate has been the primary driver of ongoing 14C/12C ratio decreases. "
9303	18	41	18	41	Replace "hemispheric" with "hemisphere" [Christine Weldrick, Australia]	Accepted - change was made.
96549	18	42	18	42	The short form SPO does not help easy reading (even though it is explained in the paragraph above). [Nicole Wilke, Germany]	Accepted - SPO expanded as South Pole
45431	18	44	18	44	The reference for Figure 4a, when placed at the end of this sentence, seems to be there to support the fact that industrialized nations are in the northern hemisphere. Since this is not what this figure shows, I suggest moving the reference for this figure somewhere else. [Olivier Sulpis, Netherlands]	Accepted. Citation to the Figure moved to the middle of this sentence
84793	18	46	18	47	The stoichiometry of burning fossil fuels between CO2 and O2 is not 1 as given here, but close to 1.4. [Martin Heimann, Germany]	Accepted - revised number. Thank you
84795	18	46	18	47	It is deplorable that the oxygen method is covered only with one sentence in this chapter. It clearly is one of the most compelling evidence that the atmospheric increase is due to an oxidative process (burning) and it allows a simple separation of land vs sea sink partitioning without needing any models. [Martin Heimann, Germany]	Accepted. We have added a sentence here in line of your suggestion regarding the "burning" signal.
58761	18	46	18	47	This claim is also correct, but is not supported by the citation to Keeling et al 2017. The Keeling reference deals with 13C, not O2/N2 ratio. Perhaps this citation accidentally got switched with the one in previous sentence? Suggest to remove Keeling reference from this sentence and replace with appropriate citation to O2/N2 evidence, such as the Ishidoya et al 2012 paper. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - used Ishidoya et al. (2012) paper in the text, in addition to a different reference of Keeling and Manning. Thank you

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
103115	18	47			Statement implies that O2 decline is consitent with CO2 increase. Has this ever been checked? [Philippe Tulkens, Belgium]	Taken in to account. Yes, of course. We have given a couple of references here. Please check the Keeling and Manning (2014) book chapter
84797	18	48	18	48	The 14C evidence of fossil fuel burning as the major cause of the atmospheric CO2 increase is much older than “since AR5”. E.g. Suess 1955. The Suess effect prior to the atomic weapon tests was a key calibration/validation point in early carbon cycle models of the 1960’s and 1970’s. [Martin Heimann, Germany]	Accepted. We have revised the sentence as "Since the AR5, high quality measurements of radiocarbon (14C-CO2) at Wellington, New Zealand (Turnbull et al., 2017) and other sites around the world (Levin et al., 2010; Graven et al., 2017) showed continued long-term decrease in the 14C/12C ratio"
74199	18	48	18	52	Maybe one could add here that 14C was already used earlier to infer anthropogenic carbon increase using pre-bomb radiocarbon from tree rings (Stuiver and Quai, 1981) [Christoph Völker, Germany]	Rejected. Thank you for the note. Since the 14C discussion is already long, as pointed out by other reviewers (comment ID: 58763), we have not expanded it further.
72929	18	55	18	55	Replace 'have' with 'has'. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable - text changed.
104859	19	2	19	3	Discussion of 14C activity in Fugre 5.4: Just plotting the observed atmospheric 14C doesn't demonstrate this, because atmospheric 14C is going to decrease due to radioactive decay and the natural exchange of 14C between the atmosphere, ocean, and land reservoirs. Would need to plot the difference between the observed atmospheric 14C and that expected due to natural processes. [Timothy DeVries, United States of America]	Accepted
54981	19	8	19	8	The emission of CO2 from cement production is not entirely via fossil fuel combustion, as stated. The majority comes from the decomposition of carbonates, such as CaO and is a signficant and growing source of CO2. A good peer-reviewed reference on the issue globally is Andrew et al. (2019, https://doi.org/10.5194/essd-11-1675-2019). The statements later on the same page (Chapter 5, page 19, lines 32-35) are more accurate. [Nancy Hamzawi, Canada]	Not applicable - sorry we cannot track your comment to actual text in Fig. 5.4 or the caption
39587	19	8	19	21	Please compare Fig. 5.4 (b) with GSAT and explain the LAG of CO2 with respect to temperature as shown for example by Humlum, O., Stordahl, K., Solheim, J.E., 2013. Global & Planetary Change 100, 51, updated each month in www.climate4you.com. The absence of this comparison and its analysis is a major lack of AR6 report. The variability by a factor as large as 10 for the growth rate suggests it is mainly natural, not anthropogenic, and mainly dependent on GSAT. [François Gervais, France]	Taken in to account - this will not be shown here, but we have discussed the link between GSAT (rainfall) with CO2 flux variability in Section 5.2.1.4
58723	19	11	19	11	It was not immediately clear in panel c) of Figure 5.4 which y-axis applied to which dataset in the graph. In order to clarify, it may be helpful to edit the text in line 11 to say the following: "...c: 14C measured at Wellington and 13C isotopes..." (Also applies to page 171, line 3) [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted
131521	19	18	19	18	spell out and define "BHD" [Hans Poertner and WGII TSU, Germany]	Accepted - Barring Head spelled out in the caption
131523	19	19	19	19	spell out "MEI" [Hans Poertner and WGII TSU, Germany]	Accepted - MEI is spelled out in the caption

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
4399	19	24	19	25	" a largely unchanged airborne fraction, the fraction of anthropogenic emissions that have accumulated in the atmosphere, of 44%" --> "a largely unchanged of anthropogenic emissions that have accumulated in the atmosphere, or airborne fraction, of 44%" [Ana Bastos, Germany]	Accepted - sentence revised following comment#58765
84789	19	24	19	25	Here and also throughout the text the airborne fraction should be more carefully and consistently defined: It is the fraction of the yearly emissions that accumulate in the atmosphere. Formally it is actually just the yearly atmospheric increase divided by the corresponding yearly emissions. [Martin Heimann, Germany]	Accepted
58765	19	24	19	25	Setence structure is confusing. Suggest rewording for clarification: "Over the past six decades, the fraction of anthropogenic CO2 emissions that has accumulated in the atmosphere ("airborne fraction") has remained at approximately 44%" [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - thank you for the nice suggestion
2779	19	24	19	46	This is an editorial and substance comment. There are a few errors in this section e.g. line 42 "sinks have grown and continuous to growth". The written language of this section is a little unclear. There appears to be a contradiction line 26 "have continued to grow at a rate consistent with the growth rate of anthropogenic CO2 emissions" and line 36 "are not growing as fast as the growth in atmospheric CO2" and line 42 "both ocean and land CO2 sinks have grown and continuous to growth with the rising of atmospheric CO2 and consistent with anthropogenic emissions, but the growth of the combined ocean and land sink has been slower than the accumulation of anthropogenic CO2 in the atmosphere" I think what is being said might make sense, but the apparent contradiction could be due to language? [Stephen Wilkinson, United Arab Emirates]	Taken into account - all these sentences mentioned in the comment are revised for correctness and clarity.
21793	19	25	19	25	This percentage should be quoted alongside a very likely range. [Peter Thorne, Ireland]	Accepted
19303	19	25	19	26	incomplete sentence [Benjamin Lamptey, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The sentence is revised
9305	19	26	19	26	Replace "That suggests that the land…" to "This suggests that the land…" [Christine Weldrick, Australia]	Accepted - change was made.
104861	19	26	19	28	This is correct and seems to contradict the statement at the end of page 6 [Timothy DeVries, United States of America]	Accepted
58629	19	26			A high-confidence level could be added regarding the stable 44% airborne fraction since the end of the 60s until recent past [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected. We have given several confidence statements for the more detailed manner in the next paragraph. We feel that "over 6 decades" is already a big statement.
19943	19	28	19	28	Figure 5.5 does not give evidence concerning the prevailing role of the land sink in variability. [philippe waldteufel, France]	Accepted
58689	19	30	19	33	The sentence is hard to follow and not understandable [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. The sentence is revised
109641	19	30	19	46	The last sentence states that it is difficult to detect trends. This is for me a disconnect to the rest of the paragraph. I did not get that impression while reading it. [Carolyn-Monika Görres, Germany]	Accepted

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
103117	19	30	19	46	a stable "airborne fraction", and a decreased "sink rate" - isn't that a contradiction that should be flagged (possibly as a reseach priority?) [Philippe Tulkens, Belgium]	Accepted
115329	19	32			Insert 'emissions' after 'CO2'. [Gillett Nathan, Canada]	Not applicable - text changed.
104863	19	33	19	34	This sink rate is declining? That is not apparent from the Global Carbon Budget data as far as I can see. If it is the signal is very weak. Probably need a figure to discuss this. [Timothy DeVries, United States of America]	Accepted
19945	19	33	19	38	This passage is difficult to understand. As the sink rate is not displayed on figure 5.5, it can only (based on this figure) be inferred from the trend on the airborne fraction. Such a trend is indeed present, as illustrated by the linear fit. But the text points (line 34) "the lack of trend in the airborne fraction". Please solve this contradiction. [philippe waldteufel, France]	Accepted
87677	19	34	19	36	"This research confirms the lack of trend in the airborne fraction since 1959 but shows a declining sink rate that suggests that the combined ocean and land sinks are not growing as fast as the growth in atmospheric CO2" -> if find it difficult to understand how a constant airborne fraction goes together with a smaller growth of the ocean/land sink that lags amtospheric CO2; please try to make this overly clear. [Ivy Frenger, Germany]	Accepted
37951	19	36	19	36	"~ (Figure 5.5; (Raupach et al., 2014; Bennedsen et al., 2019)." [Junhee Lee, Republic of Korea]	Accepted - sentence revised by moving "Figure 5.5"
72931	19	41	19	41	Replace)(with ; [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change was made.
44577	19	41	19	45	This sentence is probably correct, but not clear. If the land and ocean sinks have grown *consistent with anthropogenic emissions*, then why has the growth of the sinks been slower than the accumulation of CO2 in the atmosphere? [Judith Hauck, Germany]	Accepted
45433	19	41	19	45	This sentence is poorly constructed. Replace "continuous" by "continue". Replace "and consistent with" by "due to" and mark a full stop after "anthropogenic emissions". [Olivier Sulpis, Netherlands]	Accepted - this sentence is changed
2329	19	41		45	Could you please rephrase, potentially to: "In conclusion, both ocean and land CO2 sinks have grown and continue to grow with the rising of atmospheric CO2 consistently with anthropogenic emissions, but the growth of the combined ocean and land sinks has been slower than...". [Abed El Rahman Hassoun, Lebanon]	Accepted - change was made.
2187	19	41		45	You might mean "In conclusion, both ocean and land CO2 sinks have grown and continue to grow with the rising of atmospheric CO2 consistently with anthropogenic emissions, but the growth of the combined ocean and land sinks has been slower than...". [Abed El Rahman Hassoun, Lebanon]	Accepted. Same as comment #2329
19305	19	42	19	42	the phrase "have grown and continuous growth .." should be corrected to "have grown and conitnues to grow .." [Benjamin Lamptey, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The sentence is revised
127697	19	42	19	42	Change "continuous" to "continue". [Trigg Talley, United States of America]	Accepted - change was made.
72933	19	42	19	42	Replace 'continuous to growth' with 'continue to grow'. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change was made.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
58767	19	42	19	43	Sentence structure is confusing and obscures meaning. Suggest revising, perhaps something like: 'In conclusion, both ocean and land CO2 sinks have increased in absolute terms as atmospheric CO2 has increased, but the sink growth has been relatively slower than the accumulation of anthropogenic CO2 in the atmosphere." [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. The sentence is revised
51119	19	42	19	43	Suggested edit: "continuous to growth...and consistent with" should be "continue to grow..." or other wording [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change was made.
9307	19	46	19	46	Replace "CO2" with "CO2" (subscript) [Christine Weldrick, Australia]	Accepted - change was made.
74201	19	46	19	46	“makes IT difficult” [Christoph Völker, Germany]	Accepted - change was made.
72935	19	46	19	46	Subscript 2 required in CO2 [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change was made.
2781	19	51	19	52	It would be useful to have a more detailed caption describing this figure [Stephen Wilkinson, United Arab Emirates]	Accepted
127699	20	1	20	1	There is too much emphasis on certain kinds of practices (e.g., harvesting or abandonment); the sentence should include "recovery following large wildfires". [Trigg Talley, United States of America]	Likely refers to p. 21 l. 1. Rejected. Wildfires and recovery thereafter are considered to be part of the natural land sink (see Friedlingstein et al, 2019) and thus should not be mentioned as land-use-related flux.
9855	20	1	22	10	This section has no assessment. [Robert Kopp, United States of America]	Accepted - more assessment e.g. in terms of confidence statements added.
51121	20	1	22	10	Section 5.2.1.2 includes no confidence annotations (very helpful and usually omnipresent, in italics), though it discusses uncertainties in detail. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised (confidence statements added).
40685	20	1	22	10	section 5.2.1.2: the improvements since AR5 and SRCCL are not clear and this section currently reads more as a review of the literature rather than an assessment of our current understanding of the literature. [TSU WGI, France]	Accepted - we clarified which parts go beyond SRCCL and AR5 in more detail (SRCCL did not deal with the pre-industrial, for example). We made explicit the state of knowledge of AR5. We have added confidence statements supporting the assessment character.
78479	20	1			there will be interest in the impact of the Covid-19 pandemic on CO2 (and other GHG) emissions. This is a good place to cite Le Quere (2020 Nature) and related literature that will emerge [Chris Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. A cross chapter box on Covid has been written and linked to text here.
78239	20	1			Under the light of current Covid crisis and I would find it good to include recent research about influence of covid lockdown on atmospheric CO2 emissions, so that the reader gets an idea that short therm changes can happen but that does not mean that this will have a long term effect. [Dagmar Nadja Henner, Austria]	Accepted. A cross chapter box on Covid has been written and linked to text here.
98235	20	3	20	3	Insert "combustion" after fossil fuel because the contribution of CO2 from simple fossil fuel extraction, etc is trivial. Need to emphasize the route of input. [Gregory Cutter, United States of America]	Accepted - changes made
127701	20	3	20	3	Suggest recrafting as follows: "There are two anthropogenic sources of CO2: fossil emisions and those resulting from land use change..." [Trigg Talley, United States of America]	Accepted - changes made. The suggestion was used directly

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86743	20	3	20	5	Quotation:"There are two anthropogenic sources of CO2 emissions, fossil fuels and the net flux from land use change (Friedlingstein et al., 2019), noting the different definitions used in scientific and inventory studies (Grassi et al., 2018)." Comment: Please elaborate a little more about "the net flux from the land use change". What is meant here? The cummulative net flux? temporary net flux? In what a time scale is the net flux relevant? [Oyvind Christophersen, Norway]	Taken into account ("net flux" deleted here, explanation of net as sum of gross fluxes follows on p. 20 l. 54).
29147	20	3	20	5	Unclear sentence, please revise - what is the implication of the different definitions, and why are they being mentioned here? [Helmut Haberl, Austria]	Accepted - text removed as it is discussed elsewhere as it is discussed later
58691	20	3	20	8	This statement is missleading: CO2 Emissions from cement production are sumarized under fossil fuel emissions. During cement production, most of the released CO2 stems from the raw material CaCO3 and not from the fuel. Thus, a decission maker without background knowledge could be lead to the assumption that reducing/changing the used combustion fuel could significantly change the CO2 production from cement production which is not the case. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - text changed
18197	20	3	20	9	The process of cement production produces 4 % of CO2 emissions - why is this counted as a part of fossil CO2? If the process could be run by renewable energy sources these emissions would still occur during the production. [Chelsey Baker, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text changed
115331	20	3			Cement is missing as a source here. [Gillett Nathan, Canada]	Accepted - changes made
103119	20	3			also mention cement/lime production [Philippe Tulkens, Belgium]	Accepted - changes made
8729	20	4	20	4	it is not clear what is implied here. Are there other definitions of fossil fuel and land-use CO2 emissions? [Vaishali Naik, United States of America]	Accepted - text removed as it is discussed elsewhere, see 29147
58769	20	4	20	5	The clause "noting the different definitions used in scientific and inventory studies" is not clearly introduced. Suggest either omitting this clause or introducing more clearly. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - text removed as it is discussed elsewhere, see 29147
8731	20	11	20	13	I am not sure GCP and CEDS can be counted as organizations. These are inventories based on energy consumption data reported by countries. [Vaishali Naik, United States of America]	Accepted - groups added
29149	20	12	20	12	Why is CDIAC missing in this list? [Helmut Haberl, Austria]	Accepted - now included
58589	20	12	20	12	It may take up some space, but the readers may benefit from knowing what the acronyms refer to. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Added.
8733	20	15	20	15	I think the Global Carbon Project should be highlighted by name here. [Vaishali Naik, United States of America]	Accepted. Added.
4403	20	16	20	16	"maximises" -> "maximise" [Ana Bastos, Germany]	Accepted - change was made.
9309	20	16	20	16	Replace "to maximises temporal coverage" with "to maximise temporal coverage" [Christine Weldrick, Australia]	Accepted - change was made.
51123	20	16	20	16	"to maximises" should be "to maximise" [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change was made.
74203	20	16	20	16	“to maximises” should be “to maximise” [Christoph Völker, Germany]	Accepted - change was made.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
93429	20	17	20	17	Andrew 2019 ref is not in the list. Perhaps the right ref is Andrew 2018, which is in the list, or this one from 2019: Andrew, R. M. (2019). Global CO2 emissions from cement production, 1928--2018. Earth System Science Data, 11(4), 1675–1710. https://doi.org/10.5194/essd-11-1675-2019 [Carles Pelejero, Spain]	Accepted - updated
58631	20	17			The cited source is wrong regarding the sources mentioned at the end of the chapter. It's not Andrew, 2019 but Andrew, 2018 [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - updated
2331	20	20		21	It would be great to mention the decrease we are witnessing during the COVID-19 pandemic. I'm sure this might interest many stakeholders as a couple of scientific posts have been circulating about this specific topic, for example by the NASA Earth Observatory (https://earthobservatory.nasa.gov/blogs/earthmatters/2020/03/05/how-the-coronavirus-is-and-is-not-affecting-the-environment/), SSRN (https://papers.ssrn.com/sol3/papers.cfm?abstract_id=3557738), Copernicus platforms and others. [Abed El Rahman Hassoun, Lebanon]	Accepted. A cross chapter box on Covid has been written and linked to text here.
2189	20	20		21	It would be great to mention the decrease we are witnessing during the COVID-19 pandemic as well. I'm sure this would interest the stakeholders as many scientific posts have tackled this topic recently, such as Nasa Earth Observatory: https://earthobservatory.nasa.gov/blogs/earthmatters/2020/03/05/how-the-coronavirus-is-and-is-not-affecting-the-environment/ ; SSRN: https://papers.ssrn.com/sol3/papers.cfm?abstract_id=3557738 ; Copernicus and others. [Abed El Rahman Hassoun, Lebanon]	Accepted. A cross chapter box on Covid has been written and linked to text here.
4405	20	22	20	22	add "on average", since for some years emissions were higher [Ana Bastos, Germany]	Accepted - text included
39861	20	27		32	"the slower growth ….(Friedlingstein et al, 2019)" this block is very hard to follow. Can't you follow the order in which the growth rates are presented above (i.e. increasing order) [TSU WGI, France]	Accepted - text modified
58771	20	28	20	28	Word choice: suggest replacing "coincidental" with "simultaneous". [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - text changed
21797	20	28	20	30	I would urge a degree of caution in calling out specific countries in this manner. [Peter Thorne, Ireland]	Accepted - text modified
8735	20	29	20	30	driven by growth in China will need at least one reference if not more. [Vaishali Naik, United States of America]	Accepted - text removed
38319	20	29	20	30	"The stronger growth in emissions in the 2000s was due to a rapid expansion of coal use, driven by growth China". This conclusion is inaccurate which lacks data support and does not indicate cited reference. In accordance with IPCC report preparation practices, individual countries should not be listed separately. In this regard, it is suggested to delete this conclusion. [Yaming LIU, China]	Accepted - text removed
2783	20	29	20	30	should be "growth in china" [Stephen Wilkinson, United Arab Emirates]	Accepted - text removed
2191	20	29			You need to close the sentence after "China". [Abed El Rahman Hassoun, Lebanon]	Accepted - text removed
58703	20	30	20	30	Suggestion for clarity, add "use" after "The growth in coal " and substitute "coal use" for "it" in the rest of the sentence. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - text modified
45435	20	30	20	30	Rephrase "driven by growth China" [Olivier Sulpis, Netherlands]	Accepted - text removed

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
72937	20	30	20	30	Insert 'in' before 'China'. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text removed
58591	20	30	20	31	The statement mentions that the oil and gas use have continued to grow strongly. However, in the graph 5.6a, the trend for oil shows a slight plateau for the last few years ~2014-2017. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected - this is not seen in the data, this is perhaps a visual artefact since oil sits on coal and coal has changed.
2333	20	30			You mean "in" China? [Abed El Rahman Hassoun, Lebanon]	Accepted - text removed
2193	20	30			I guess you mean "in" China? [Abed El Rahman Hassoun, Lebanon]	Accepted - text removed
104865	20	31	20	31	Probably mean to say that gas use is the dominant driver of year-to-year increases in CO2 emissions. Because coal is still the dominant driver of CO2 emissions. [Timothy DeVries, United States of America]	Accepted - text modified
38501	20	33	20	33	meaning of sentence "continue to grow steadily despite more rapid growth in the 2000s" is unclear [Siv K Lauvset, Norway]	Accepted - text modified
96551	20	33	30	34	"Process CO2 emissions from cement..." Reference is missing. [Nicole Wilke, Germany]	reference added
109643	20	55	20	55	This is the only time I see the phrase „land use and land use change“. In other parts of the text it is always just „land use change“. Maybe delete „land use“ here. [Carolyn-Monika Görres, Germany]	Taken into account (see comment 5655)
39771	20	55	20	55	"land use and land use change" is used here, while in Page 20 Line 3 it says "land use change". [TSU WGI, France]	Taken into account (see comment 5655)
127703	20	55	21	1	It's not correct to refer to all biomass burning as "forestry", which has a specific meaning. Much biomass burning comes during wildfires, some of which are human-caused but often incidental to land conversion, but many more of which are natural events caused by Earth system processes such as lightning or volcanoes. Suggest changing the word to "forest management". [Trigg Talley, United States of America]	Accepted - change was made.
4407	20	55	21	4	Switch order of the two sentences. [Ana Bastos, Germany]	Taken into account (several sentences rewritten).
5655	20	55	22	10	Please rework this text. Although you realised that land-use related emissions are not the same as land-use change related ones, you repeatedly mix up land use and land use change, and neglect land management. The rather sloppy use of (and attribution of emissions to) land use change is widespread in the modelling community (evidenced by the literature you cite), but the term "LUC" as part of "LULUCF" is a strictly defined sub-sector of GHG reporting. Only emissions from changing the land use from one land use category to another may be included here, no emissions from land management within a specific land use category. For example, deforestation and subsequent use as pasture for grazing would be "LUC", clearcut harvest and subsequent reforestation as part of forest management clearly does not constitute any "change" in use (but eventually in cover). Please make sure you adhere to the official (GHG reporting) definitions and do not include emissions from agriculture and other land management in "LUC". This has implications for the assessments you present here, too. [Joachim Rock, Germany]	Rejected: We follow the natural science definition of the net land-use change flux by the annual global carbon budget (Friedlingstein et al 2019), largely consistent with the AFOLU definition (see IPCC SRCCL Chapter 2), which includes CO2 fluxes from land management. Accepted: we clarified the terminology throughout and added more on the definitions, including an additional reference relating land-use, land-cover, land management and their changes (Pongratz et al., 2017).

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
131525	20	56	20	56	What you define as "sources" (e.g. clearing) are actually human activities (cf. IPCC Greenhouse Gas Inventories Good Practice Guidance for Land Use, Land-Use Change and Forestry. in: Penman et al, 2003). Definition of "Sources" is different: According to IPCC Good Practice Guidance, in the LULUCF sector, there are 3 main carbon pools with 5 sub-pools: 1. Living biomass (above ground, below ground), 2. Dead organic matter (Dead wood, litter) and 3. Soils (soil organic matter). The definition of such carbon pools as "Source" or. "Sink" depends on the net carbon budget of such pools (positive= source, negative= sink). [Hans Poertner and WGII TSU, Germany]	Accepted - text revised to not use "sink" and "source" to avoid potential confusion with Penman definitions.
96553	20	56	21	4	As a major source for atmospheric CO2, drained peatlands should be named as well as an example for land use change and source functions of ecosystems. [Nicole Wilke, Germany]	Accepted - change was made.
71553	20				I think the most recent dataset on anthropogenic carbon dioxide emissions, ODIAC (Oda and Maksyutov, 2015), should also be mentioned in this section. [Takashi Maki, Japan]	Noted - ODIAC is a gridded data set based on CDIAC, so not really relevant for the text here. https://www.odiac.org/index.html
131527	21	1	21	1	What you define as "sinks" (e.g. "forest re-growing") are actually human activities (cf. IPCC Greenhouse Gas Inventories Good Practice Guidelines for Greenhouse Gas Inventories. in: Penman et al, 2003). Definition of "Sinks" is different: According to IPCC Good Practice Guidance, in the LULUCF sector, there are 3 main carbon pools with 5 sub-pools: 1. Living biomass (above ground, below ground), 2. Dead organic matter (Dead wood, litter) and 3. Soils (soil organic matter). The definition of such carbon pools as "Source" or. "Sink" depends on the net carbon budget of such pools (positive= source, negative= sink). [Hans Poertner and WGII TSU, Germany]	Accepted (identical to comment 131525).
37927	21	4	21	5	Please add some examples for the “synergistic effects of land use change and environmental changes”. [Junhee Lee, Republic of Korea]	Accepted - text revised (direct reference to loss of additional sink capacity).
15395	21	4	21	10	These discrepancies about anthropogenic land CO2 emissions, in particular for different treatment under UNFCCC and science community, need more attention and should be presented in SPM. [Junichi Tsutsui, Japan]	Accepted (see 5655).
4409	21	7	21	10	Grassi et al. explains discrepancie with UNFCCC estimates, which are now shown in the Figure. Please add UNFCCC estimates in the Figure, as this is relevant for the decision makers (see IPCC SRCCL). [Ana Bastos, Germany]	Rejected - reporting of national greenhouse gas inventories to UNFCCC has been extensively covered by the SRCCL. We explain that our scientific assessment excludes GHGI reporting fluxes.
58725	21	10	21	12	I would add in a 'consequently' to the part in parenthesis to read: "which would consequently imply an underestimation of the land sink to meet…" Upon first read, I was confused by the cause-and-effect implied by the sentence and thought it was saying that the underestimation of the land sink was causing the underestimation of the net land use change, which didn't make sense. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - change was made.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
70827	21	10	21	17	Another aspect is the (partial) omission of less obvious land-management activities in most models (See Le Quere 10.5194/essd-10-2141-2018) that introduces a bias. In particular in temperate zonnes with returning forests, the cessation of forest use practices like forest grazing, litter raking, pruning and pollarding (McGrath et al., 2015 10.5194/bg-12-4291-2015) resulted in a strong grow-back of vegetation (Erb et al., 2013 10.1038/nclimate2004, Le Noe et al., 2020 10.1111/gcb.15004) - as such practices are not (fully) included in the models, the attribution between natural fluxes (Sland)and land-use induced emissions (Eluc) will be biased (in such a case LUC would be smaller and thus also the land sink). As the cessation of these land-use practices closely relates to the industrializatio process, it can be concluded that this effect is found in many regions of the world, in particular with returning forests (Gingrich et al., 2019 10.1016/j.cosust.2019.04.005). [Karlheinz Erb, Austria]	Accepted - text revised (limited to the key paper, Erb et al 2013, as the other papers' additional foci are beyond our scope).
29151	21	13	21	13	In my view it is not clear enough that "land management" in the second half of the sentence is not meant to comprise activities resulting in land-cover change, and refers only to changes in management of land remaining within one land-cover category (which seems to be the meaning of the following lines) [Helmut Haberl, Austria]	Accepted - text revised (explanation added).
96555	21	13	21	14	Please consider to hint at the significant influence of land management practices, esp. in the context of soil science / agriculture/ soil carbon. [Nicole Wilke, Germany]	Accepted - climate-smart agricultural practices have been added to the discussion of land management.
86745	21	14	21	17	Quotation: "Sensitivity studies find that practices such as wood and crop harvesting increase land use emissions (Arneth et al., 2017) and explain about half of the cumulative loss in aboveground biomass (Erb et al., 2018). However, individual practices such as fire suppression may also create carbon sinks (Andela et al., 2017; Arora and Melton, 2018)." Comment: Are we here talking about cumulative emissions or temporary emissions that will be removed by re-growth? Are these instant emissions or emissons that will be stabilized in a climate relevant time frame? [Oyvind Christophersen, Norway]	Accepted - text revised (specified to global net land use emissions, implying that regrowth is considered)..
70823	21	16	21	16	please note that the Erb et al. Paper refers to total (ie above and belowground) biomass [Karlheinz Erb, Austria]	Accepted - change was made.
96557	21	16	21	17	If single practices for the creation of carbon sinks are mentioned (like fire suppression), it should be added, that many of these practices potentially have detrimental effects on biodiversity or local stakeholders. In the mentioned case of fire suppression, e.g., it should be added, that this measure in fire-adapted ecosystems like savannas reduces biodiversity (see Abreu et al 2017: The biodiversity cost of carbon sequestration in tropical savanna). [Nicole Wilke, Germany]	Taken into account - carbon losses and sinks are used in a neutral way here, not in the context of potentially beneficial effects of carbon uptake on climate. Therefore we do not include discussion of other effects of land-use change and land management such as biodiversity, societal implications, water availability, because this would be beyond the space and scope of this chapter and is part of WGIII AR6 and the SRCCL.
4411	21	16	21	17	Add that fire supression and management are not included in these models. (exception is Houghton 2017, but only in the US) [Ana Bastos, Germany]	Accepted - change was made.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
127705	21	16	21	17	It is simplistic to conclude that ""practices such as fire suppression may also create carbon sinks""; this is a serious error not reflecting current science and needs to be corrected. A great deal of evidence suggests the opposite: When forests are thinning and maintained by prescribed burning, the total carbon loss is minimized and the larger carbon stock is far more stable. Conversely, because fire suppression leads to fuel accumulation, when wildfires do occur they are much more destructive and cause much larger carbon emissions. See for example: Wiedinmyer, C. and Hurteau, M.D., 2010. Prescribed fire as a means of reducing forest carbon emissions in the western United States. Environmental science & technology, 44(6), pp.1926-1932. Hurteau, M.D. and Brooks, M.L., 2011. Short-and long-term effects of fire on carbon in US dry temperate forest systems. BioScience, 61(2), pp.139-146. Earles, J.M., North, M.P. and Hurteau, M.D., 2014. Wildfire and drought dynamics destabilize carbon stores of fire,Äêsuppressed forests. Ecological Applications, 24(4), pp.732-740. [Trigg Talley, United States of America]	Taken into account (fire suppression is now mentioned as management practice that can create emissions and/or removals).
104867	21	17	21	17	May want to also mention here the recent study of Chen et al (2019, Nature Sustainability) demonstrating increasing CO2 sinks due to land management, primarily in India and China [Timothy DeVries, United States of America]	Rejected - Chen et al refer to leaf area, not carbon fluxes, and discussion of the linkages of the two is beyond the scope of this chapter. SRCCL discussed earlier literature on greening/browning trends.
4413	21	19	21	19	"Industrial Era of land use change estimates have been " --> "Land use change estimates in the industrial era have been…" [Ana Bastos, Germany]	Taken into account (similar to comment 64595).
64595	21	19	21	19	Suggest: "Land use change estimates over the industrial era…" [Charles Curry, Canada]	Accepted - change was made.
58773	21	19	21	19	Word choice: suggest revising beginning of sentence to read "Estimates of land use change during the industrial era …" [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account (similar to comment 64595).
74205	21	19	21	19	“Industrial Era of land use change estimates”: either leave the “of” out, or turn the order around [Christoph Völker, Germany]	Taken into account (similar to comment 64595).
29153	21	19	21	19	Unclear formulation: "Industrial Era of land use change estimates have been updated routinely" [Helmut Haberl, Austria]	Taken into account (similar to comment 64595).
72939	21	19	21	19	Delete 'of'. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account (similar to comment 64595).
19947	21	19	21	19	What does " Industrial Era of land use change estimates" mean? [philippe waldteufel, France]	Taken into account (similar to comment 64595).
72941	21	20	21	20	Replace 'bookkeeping' with 'book keeping'. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - "bookkeeping" is accepted by the dictionary.
72943	21	21	21	21	Replace 'bookkeeping' with 'book keeping'. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - "bookkeeping" is accepted by the dictionary.
96559	21	23	21	23	"and regrowth.to quantify ". Dot doesn't make sense. [Nicole Wilke, Germany]	Accepted - change was made.
37953	21	23	21	23	“~ regrowth.to ~” [Junhee Lee, Republic of Korea]	Accepted - change was made.
9311	21	23	21	23	Replace "…decay and regrowth.to quantify..." with "…decay and regrowth to quantify…" [Christine Weldrick, Australia]	Accepted - change was made.
13427	21	23	21	23	Erase the period (.) between the words “regrowth” and “to”. [Maria Amparo Martinez Arroyo, Mexico]	Accepted - change was made.
23683	21	23	21	23	remove . after "regrowth" [Massimo Lupascu, Singapore]	Accepted - change was made.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
33293	21	23			Change: “regrowth.to” by “regrowth to”. [Guiomar Rotllant, Spain]	Accepted - change was made.
4415	21	24	21	27	Please revise the sentence, bad grammar / readibility [Ana Bastos, Germany]	Accepted - text revised (sentence split in two, rephrased).
37929	21	27	21	32	Could you add more explanation? It is hard to understand the difference between estimates of DGVM and bookkeeping, described in Figure 5.6, with the current explanation. [Junhee Lee, Republic of Korea]	Accepted - text revised (loss of additional sink capacity explained in more detail).
33295	21	27			Change: “Nassikas, (2017).” By “Nassikas (2017). [Guiomar Rotllant, Spain]	Accepted - change was made.
4417	21	29	21	32	"loss of additional sink capacity" is a term only very few people will know, please explain in an additional sentence. The explanation that follows is not clear either. [Ana Bastos, Germany]	Accepted - text revised (loss of additional sink capacity explained in more detail).
58727	21	34	21	34	The intro of the sentence should include the losses that are being referred to. Thus the sentence could begin "Cumulative (pre-industrial and industrial era) carbon losses by land use activities…" [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - change was made.
4419	21	34	21	35	Move "(pre-industrial and Industrial Era)" to after "since the start of agriculture and forestry" [Ana Bastos, Germany]	Accepted - change was made.
58695	21	34	21	38	Years are sometimes reported with "CE", sometimes without. Please unify [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Editorial - this kind of issues is typically during the professonial copy-editing place taking place prior to publication
9853	21	34	21	48	This paragraph could use an assessment. [Robert Kopp, United States of America]	Accepted - text revised (confidence assessment added).
127707	21	35	21	36	Should citation be Sanderman et al. (2017)? [Trigg Talley, United States of America]	Rejected - the cited number occurs in the correction to the 2017 article, which is what is cited here.
58729	21	36	21	36	I am not sure that the 70 PgC referenced in this sentence is correct. It appears the value was obtained from Figure 2 in Sanderman et al., 2017 (I cannot find it referenced anywhere else); however Sanderman et al., 2018 corrected the Soil Organic Carbon losses in the 2017 paper (from 133 PgC to 116 PgC). Figure 2 appears to based on the 133 PgC value and thus the 70 PgC derived from this figure would be incorrect. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - Indeed, Fig. 2 in Sanderman is incorrect, but our estimate of 70PgC refers not to Fig. 2 but to data provided by Jon Sanderman to the author in response to the request for the data underlying the correction of the article. We highlight we use the corrected data and also that we applied a linear interpolation to derive 1750 values from 900 and 1800 AD values.
58633	21	36			The meaning of the numbers in brackets is not mentioned. In the quoted article, it is said (375-525) are the inner quartiles. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - change was made.
83985	21	37	21	39	Pg 21 Lines 37 - 39. The sentence is incomplete and the emission unit is missing [Marco Tulio Cabral, Brazil]	Accepted - change was made.
33297	21	37	21	39	Re-write: “Subtracting the post-1750 net land use change flux from Table 5.1 from the combined cumulative soil and vegetation losses yields pre-1750 emissions of 328 (161–501) assuming error ranges are independent).” [Guiomar Rotllant, Spain]	Accepted - text revised (sentence written more clearly).
58693	21	38	21	38	Unit is missing in "pre-1750 emissions of 328 (161–501)" [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - change was made.
40535	21	38	21	38	missing unit for 328 (161-501) [TSU WGI, France]	Accepted - change was made.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
58775	21	38	21	38	Insert units of Pg C after range [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - change was made.
51125	21	38	21	38	"emissions of 328 (...)..." is missing units of PgC after the bracket [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change was made.
13429	21	39	21	39	Eliminate parenthesis after the word “independent”. [Maria Amparo Martinez Arroyo, Mexico]	Accepted - change was made.
9313	21	44	21	44	Consider replacing "…seen in ice-core record…" with "…seen in ice-core records…" [Christine Weldrick, Australia]	Accepted. Changed.
58777	21	46	21	48	The CO2 increase is not plagued by large uncertainties; the interpretation of the processes responsible for the CO2 increase continues to have large uncertainties. Suggest to reword for clarification. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - change was made.
104869	21	46	21	48	The uncertainties in CO2 increase measured from ice cores should be highlighted and discussed in the previous discussion related to Figures 5.1 and 5.2 [Timothy DeVries, United States of America]	Accepted. This comment does not relate to this section, but to section 5.1 Introduction & paleo context. Added discussion there.
18593	21	47	17	48	It's unclear if you mean that the measurements are plagued by uncertainties or the source of the increase is uncertain [Miriam Jones, United States of America]	Accepted - change was made.
52785	21	52	21	52	The text says "...inversion products measure the net land-atmosphere fluxes..." However, inversion products are not measurements: they estimate fluxes, rather than measure. [Sergey Malyshev, United States of America]	Accepted - change was made.
127709	21	52	21		The text says "...inversion products measure the net land-atmosphere fluxes..." However, inversion products are not measurements: They estimate fluxes, rather than measure. [Trigg Talley, United States of America]	Accepted - identical to 52785.
58647	21	55			The reference "Guabert et al., 2019" is missing in the Ref of the chapter. The full reference is : "Gaubert, B., Stephens, B. B., Basu, S., Chevallier, F., Deng, F., Kort, E. A., et al. (2019). Global atmospheric CO2 inverse models converging on neutral tropical land exchange, but disagreeing on fossil fuel and atmospheric growth rate. Biogeosciences, 16(1), 117-134. doi:10.5194/bg-16-117-2019." [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - change was made.
116417	21		21		I have the impression that several sections of ch 5 are re-assessing literature already assessed in SRCCL. Can this be checked to avoid duplication, and better highlight additional aspects? Sections are also missing clear summary statements. [Valerie Masson-Delmotte, France]	Taken into account in response to TSU - we clarify the novelty over SRCCL and AR5 in more detail. Note however that due to its recent publication we partly need to re-assess SRCCL literature to provide a useful context.
72945	22	3	22	3	Replace 'to identify' with 'identification of'. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change was made.
4421	22	7	22	7	"modelling" --> "models" [Ana Bastos, Germany]	Rejected - Models is in the acronyms DGVM. However to avoid confusing with "DGVM (hence models) modelling", it was changed to "and the modelling of DGVMs".
72947	22	7	22	7	Replace 'bookkeeping' with 'book keeping'. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - "bookkeeping" is accepted by the dictionary.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
52297	22	15	22	15	does "Ocean interior" means "deep ocean"? Should be more clear. The section presents surface-water data, should be more clear. [Agneta Fransson, Norway]	Accepted - change made
82999	22	19	22	19	Suggest considering changing the wording to “coastal seas” rather than “coastal ocean”. This also matches the terminology used in the SROCC report. Also see 5.22/Row 30. [Dan Zwartz, New Zealand]	Rejected - the phrase "coastal ocean" is used throughout Chapter 5
104871	22	19	22	20	The Gruber et al, Landschutzer et al, and Rodenbeck references are not apprppriate here as they do not deal with coastal pCO2. The Landschutzer and Rodenbeck references belong after point (2) above. The Gruber reference does not belong in this list at all. [Timothy DeVries, United States of America]	Accepted - change made
72949	22	19	22	20	References should be in chronological order. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial - this kind of issues will be taken care of during the production phase, which includes professional copy-editing.
3915	22	20	22	20	"2018); Laruelle" => Eliminate ")" [Makio Honda, Japan]	Accepted - change is made.
23685	22	20	22	20	Extra) after Landschützer et al., 2016, 2018 [Massimo Lupascu, Singapore]	Accepted - change is made.
18099	22	20			An extra closed bracket was mistakenly put in [Vlad Macovei, Germany]	Accepted - change is made.
44579	22	23	22	25	talking about "a large variety of interpolation techniques", you should also cite the large variety. E.g. add Denvil-Sommer et al. 2019 https://doi.org/10.5194/gmd-12-2091-2019 and Iida et al. 2015, doi:10.1007/s10872-015-0306-4. I also suggest to put the reference to Bakker et al 2016 for SOCAT first and then the references for the interpolation schemes. I suggest to drop the references to McKinley et al 2017 and Gruber et al 2019a here; they are not primary references here [Judith Hauck, Germany]	Accepted - references add/removed, as suggested. Note that the order of citations is dictated by the TSU.
52299	22	25	22	25	add reference "Yasunaka et al. (2018)" for CO2 fluxes in the Arctic [Agneta Fransson, Norway]	Accepted - change made
104873	22	25	22	25	Again the Gruber 2019a reference is not appropriate here. That study deals with carbon accumulation in the ocean interior. [Timothy DeVries, United States of America]	Accepted - change made
63607	22	27	22	31	The paper by Gloege et al. (submitted) adds critical uncertainty assessment for the observation based products. It is referenced elsewhere and should also be referrenced here. It is again in review with PNAS. Here is the Archive link, https://www.essoar.org/doi/abs/10.1002/essoar.10502036.1 [Galen McKinley, United States of America]	Accepted - change made
58635	22	27	22	33	It is true, the development of Argo floats is starting to resolve the lack of samples in polar regions. It would be good to still mention / remind that Argo floats can't easily operate under sea-ice (that in the SO for example almost dobbles the extension of Antarctica in winter). To collect localised data and samples around antarctica, it is still needed to go there. Marine mammals equiped with sensors (ie seals) and teleguided drones are also being more and more used to collect data, data and samples (respectyively) under the sea ice. However, the polar regions still remain under-sampled. Ref : https://search.informit.com.au/documentSummary;dn=328337291357329;res=IELAPA , https://search.informit.com.au/documentSummary;dn=520443225029371;res=IELHSS, https://doi.org/10.1002/2013GL058304, https://doi.org/10.1175/JTECH-D-18-0170.1 [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected - marine mammals and drones do not (yet) have the capacity to sample carbonate system or biogeochemical parameters

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
2195	22	28		29	Please add Northcott et al. (2019): Northcott D, Sevadjian J, SanchoGallegos DA, Wahl C, Friederich J, Chavez FP (2019) Impacts of urban carbon dioxide emissions on sea-air flux and ocean acidification in nearshore waters. PLoS ONE 14(3): e0214403. https://doi.org/10.1371/journal.pone.0214403 [Abed El Rahman Hassoun, Lebanon]	Noted - however this paper does not present data representative of the global coastal ocean and so is not included in our assessment
78481	22	29	22	29	“low confidence in those fluxes” – what do you mean by having confidence in a flux? In the size? Sign? Does low confidence mean they might not exist? Or is this a relative error to magnitude statement? [Chris Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - low confidence in the magnitude of the fluxes has now been added
9315	22	30	22	30	Remove dash after "the Southern Ocean," [Christine Weldrick, Australia]	Accepted - change is made.
13431	22	30	22	30	Remove hyphen (-) between “Southern Ocean” and “the Arctic”. [Maria Amparo Martinez Arroyo, Mexico]	Accepted - change is made.
23687	22	30	22	30	Southern Ocean,- [Massimo Lupascu, Singapore]	Accepted - change is made.
2197	22	30		31	Please add Bushinsky et al. (2019): Bushinsky, S. M., Landschützer, P., Rödenbeck, C., Gray, A. R., Baker, D., Mazloff, M. R., et al. (2019). Reassessing Southern Ocean air-sea CO2 flux estimates with the addition of biogeochemical float observations. Global Biogeochemical Cycles, 33, 1370– 1388. https://doi.org/10.1029/2019GB006176 [Abed El Rahman Hassoun, Lebanon]	Accepted - change made
9317	22	31	22	31	Consider replacing "…spatially under sampled the…" with "…spatially under-sampled, the…" [Christine Weldrick, Australia]	Accepted - change is made.
19949	22	31	22	31	A comma following "sampled" will help. [philippe waldteufel, France]	Accepted - change is made.
9319	22	32	22	32	Suggest replacing "…of carbon and biogeochemically enabled Argo…" with "…of carbon- and biogeochemically-enabled Argo…" [Christine Weldrick, Australia]	Accepted - change made
36361	22	32	22	33	Filling gaps in CO2 flux observations will require a suite of different platforms, including (unlike floats) platforms that can directly measure air-sea CO2 flux. Include the role of these other platforms by modifying to: "development and deployment of carbon and biogeochemically enabled mobile autonomous platforms is starting to close the gaps" and consider adding the following citation and/or the references within (Chai et al. 2020 https://doi.org/10.1038/s43017-020-0053-y) [Adrienne Sutton, United States of America]	Accepted - change made
52243	22	33	22	40	Suggest adding : "Moreover, new time-series are established in the Arctic such as in the Arctic outflow water in the East Greenland Current (Chierici et al. submitted). Yasunaka et al 2018 summerize air-sea CO2 fluxes in the Arctic Ocean using a combination of data and self-organizing maps. " [Agneta Fransson, Norway]	Taken into account - the Yasunaka reference has been added to the text
67451	22	35	22	35	"(i.e., below the mixed layer)" Not really accurate; the interior ocean as the term as used here would usually also exclude some of the thermocline waters. But actually this parenthesis is unnecessary and could just be deleted. [James Christian, Canada]	Accepted - change made
38503	22	36	22	37	It is called Global Ocean Data Analysis Project (GLODAP). Remove "for Carbon" [Siv K Lauvset, Norway]	Accepted - change made
9321	22	40	22	40	Consider replacing "a significant advance" with "a significant advancement" [Christine Weldrick, Australia]	Accepted - change is made.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
67453	22	40	22	41	"There is high confidence that a significant advance since AR5 and SROCC is the improved characterization of the variability of the ocean CO2 storage trends in space and time". I do not think this statement is appropriate. Terms like high confidence should be reserved for statements of scientific fact. [James Christian, Canada]	Taken into account - sentence has been modified
44581	22	40	22	43	add reference to McKinley et al in press AGU advances https:/doi.org/10.1002/essoar.10501723.2 [Judith Hauck, Germany]	Accepted - change made
19951	22	40	22	43	How is it possible to point out " a significant advance since AR5 and SROCC" and to refer to SROCC as a source of information about this advance? [philippe waldteufel, France]	Taken into account - sentence has been modified
2335	22	42			Please add DeVries et al. (2017) and McKinley et al. (2020): DeVries, T., Holzer, M. & Primeau, F. Recent increase in oceanic carbon uptake driven by weaker upper-ocean overturning. Nature 542, 215–218 (2017). https://doi.org/10.1038/nature21068 ; McKinley G., Amanda Fay, Yassir Eddebbar, Lucas Gloege, Nicole Lovenduski, 2020. External forcing explains recent decadal variability of the ocean carbon sink. Earth and Space Science Open Archive, doi:10.1002/essoar.10501723.1 [Abed El Rahman Hassoun, Lebanon]	Accepted - change made
2199	22	42			Please add DeVries et al. (2017) and McKinley et al. (2020): [Abed El Rahman Hassoun, Lebanon]	Accepted - change made
2241	22	42			DeVries, T., Holzer, M. & Primeau, F. Recent increase in oceanic carbon uptake driven by weaker upper-ocean overturning. Nature 542, 215–218 (2017). https://doi.org/10.1038/nature21068 [Abed El Rahman Hassoun, Lebanon]	Accepted - change made
2243	22	42			McKinley G., Amanda Fay, Yassir Eddebbar, Lucas Gloege, Nicole Lovenduski, 2020. External forcing explains recent decadal variability of the ocean carbon sink. Earth and Space Science Open Archive, doi:10.1002/essoar.10501723.1 [Abed El Rahman Hassoun, Lebanon]	Accepted - change made
83497	22	47	22	48	I would include Perez et al. (2018) here. The reference is already in the Chapter 5 list of references: Perez, F.F., Fontela, M., García-Ibáñez, M.I., Mercier, H., Velo, A., Lherminier, P., Zunino, P., de la Paz, M., Alonso-Pérez, F., Guallart, E.F., Padin, X.A., 2018. Meridional overturning circulation conveys fast acidification to the deep Atlantic Ocean. Nature 554, 515, doi: 10.1038/nature25493. [Antje H. L. Voelker, Portugal]	Accepted - change made
44583	22	48	22	50	I can fully follow the first half of the sentence that ocean circulation is important for redistribution of carbon. I was struggling with the second half. It doesn't have much information. The idea probably was to introduce that ocean interior and surface flux data can be combined. It doesn't become very clear and it's also not clear where this will be discussed further. The least would be to add a cross-reference to the chapters where this is needed. If its only needed in one subsection, mention it there not here. [Judith Hauck, Germany]	Taken into account - sentence has been modified
58649	22	48			The reference "Bopp et al., 2015" is missing in the Ref of the chapter. The full reference is : "Pathways of anthropogenic carbon subduction in the global ocean L. Bopp M. Lévy L. Resplandy J. B. Sallée First published:14 July 2015 https://doi.org/10.1002/2015GL065073" [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - change made

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
58651	22	48			The reference "Iudicone et al., 2016" is missing in the Ref of the chapter. The full reference is : "Iudicone, D., Rodgers, K., Plancherel, Y. et al. The formation of the ocean's anthropogenic carbon reservoir. Sci Rep 6, 35473 (2016). https://doi.org/10.1038/srep35473" [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - change made
58597	22	49	22	49	Use either "air-sea flux" or "ocean-atmosphere flux", if they represent the same thing. Note that the second is used in line 5 page 24. Keep consistency on how you are calling it. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - "air-sea" is used through the rest of the chapter
104875	22	53	22	53	Here and in subsequent sections: This section deals with contemporary CO2 fluxes, not anthropogenic CO2 fluxes. The contemporary CO2 fluxes include perturbations of the natural air-sea CO2 fluxes. [Timothy DeVries, United States of America]	Rejected - combined with comment #44589. This comment seems contradictory to the comment #44589 in which inclusion of river-induced CO2 outgassing is postulated for "contemporary ocean uptake". However, this comment is correct for GOBMs that usually don't take the river-induced CO2 outgassing into account. Inconsistencies in terminology in the text have been edited throughout the section. The ocean section has adopted a uniform Net flux for the contemporary and Ocean sink Socean for the anthropogenic
2287	22			42	McKinley G., Amanda Fay, Yassir Eddebbar, Lucas Gloege, Nicole Lovenduski, 2020. External forcing explains recent decadal variability of the ocean carbon sink. Earth and Space Science Open Archive, doi:10.1002/essoar.10501723.1 [Abed El Rahman Hassoun, Lebanon]	Accepted - reference added
2285	22				DeVries, T., Holzer, M. & Primeau, F. Recent increase in oceanic carbon uptake driven by weaker upper-ocean overturning. Nature 542, 215–218 (2017). https://doi.org/10.1038/nature21068 [Abed El Rahman Hassoun, Lebanon]	Noted
44585	23	1	23	1	GOBMS --> GOBMs [Judith Hauck, Germany]	Accepted - change is made.
98237	23	1	23	22	Mixed use of GOBMS, GOBMs, and GOBM is very confusing - the acronym is GOBM and then add lower case s for plural [Gregory Cutter, United States of America]	Accepted - change is made. GOBMs was thoroughly applied in the chapter.
44587	23	2	23	4	If these references mean to cite all Global Carbon Budget models (and yes, it should), the list is not complete. From GCB2019, references to Buitenhuis et al 2013, Paulsen et al 2017, Law et al 2017 (instead of the Lenton reference), Adcroft et al 2019, and Aumont et al 2015 (instead of Aumont and Bopp, 2006). See Table 4 in Friedlingstein et al 2019, https://doi.org/10.5194/essd-11-1783-2019 [Judith Hauck, Germany]	Accepted - change made
115333	23	8		11	This sentence says that it is unequivocal that ocean uptake of CO2 was 2.5 +/-0.6 PgC/yr in 2009-2018. This implies that the stated uncertainty range is a 100% confidenc range, since 'unequivocal' implies certainty. If this is a 5-95% confidence range then the correct likelihood term would be 'very likely'. I sugegst not using 'unequivocal' here. [Gillett Nathan, Canada]	Taken into account - sentence has been rewritten

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
58599	23	9	23	10	I would introduce the synonym "ocean uptake" = "ocean sink" earlier in the paragraph. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - change made
44589	23	9	25	41	line 9: 'mean contemporary ocean uptake'…. '2.5+/- 0.6 PgC/yr'. This is WRONG. The number given is what is called S_OCEAN in the Global Carbon Budget, and this is NOT the same as the CONTEMPORARY ocean uptake. S_OCEAN does not include river-induced outgassing. The contemporary net sink is the sum of (natural steady state) + (natural non-steady state) + (anthropogenic steady-state) + (anthropogenic non-steady state) + (riverine steady state) + (riverine non steady state) fluxes. S_OCEAN is the sum of anthropogenically perturbed fluxes, i.e. (natural non-steady state) + (anthropogenic steady state) + (anthropogenic non-steady state) . These concepts and numbers are mixed up in this entire part 5.2.3.1 to 5.2.3.3 [Judith Hauck, Germany]	Accepted - the use of the word contemporary is not in the strict carbon budget sense - it was removed and all other inconsistencies in the sub-section were also edited
19953	23	10	23	10	Please move the comma from before to after "CO2" [philippe waldteufel, France]	Not Applicable - There is no comma around CO2 on line 10 of page 23.
36363	23	10	23	18	It is not clear why the 2009-2018 uptake esimate in line 10 is different from the one presented in line 17. And why do the estimates have different significant digits? Also, are the uncertainties (or standard deviations?) of the uptake estimates in lines 10-11 estimated (or calculated) differenty from those presented in line 17? The uncertainties of 0.39, 0.16, and 0.08 seem much too low. Line 13 states that the ocean uptake fraction is 24% ± 5% for 1960-2018 and Table 5.1 is referenced. However, Table 5.1 presents results from different time periods. The time periods presented should be more consistent. [Adrienne Sutton, United States of America]	Taken into account - text revised so that the difference in ocean CO2 uptake in 2009-2018 in line 10 and that in line 17 becomes clearer.
67455	23	11	23	11	More accurate to say in response to increasing atmosphere concentration; this statement would be true even if emissions had stabilized at a constant level e.g. in 2000. [James Christian, Canada]	Taken into account - sentence has been modified
78483	23	11	23	11	Technically the sinks respond to the atmospheric CO2 and don't care about the cause – anthro emissions or otherwise. [Chris Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - sentence has been modified
30561	23	13	23	13	Here you say that the ocean uptake fraction is 24% ± 5%. However, in Tabel 5.1 it is 25% for the time period from 1750 - 2018. Why are you reporting different long-term values? [nina bednarsek, United States of America]	Rejected - the ratio of ocean carbon uptake to total anthropogenic carbon emission, i.e. uptake fraction, for the recent decades of 1960-2018 are shown here. Table 5.1 shows the cumulative carbon uptake from 1750-2018.
41605	23	14	23	14	I suppose that "Table 1" refers to Table.A.1? [Katharina Meurer, Sweden]	Rejected - text refers correctly to Table 5.2
115335	23	14			The confidence level of the assessment is not given. [Gillett Nathan, Canada]	Taken into account - sentence has been modified
44591	23	15	23	15	decadal means… 6 quasi-independent methods'. This is comparing apples and oranges, not all methods use/can quantify the same thing. See previous comment on definitions and detailed comments on the figures [Judith Hauck, Germany]	Accepted - text revised

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
104877	23	16	23	18	In Fugre 5.7a: The estimates from the empirical models appear to show no difference in the mean flux from 1990-1999 and 2000-2009. Is that a mistake? [Timothy DeVries, United States of America]	Taken into account - (assuming this comment refers to Figure 5.7b) it is not a mistake. However, the results from the empirical models in Fig.5.7ab and in Table 5.A.1 was revised.
44593	23	17	23	17	The uncertainties for the decadal estimates appear to me to be very small. Is this just the standard deviation? How was the uncertainty obtained? Please make clear what it is. [Judith Hauck, Germany]	Accepted - text revised. Uncertainties in the decadal estimates are the standard deviations of the flux from the independent estimates based on indirect observations.
44595	23	17	23	17	in close agreement with GOBMs models' --> well these estimates include the models, so that's not surprising. It would be most useful to have an independent estimate based on indirect observations (without the models!) and compare this estimate to the models [Judith Hauck, Germany]	Accepted - text revised. An independent estimate based on indirect observations (without GOBMs) were compared to the results from GOBMs.
88497	23	17	23	17	add unit PgC/yr [Damien Cardinal, France]	Accepted - change made
44597	23	18	23	18	GOBMs models --> GOBMs OR models [Judith Hauck, Germany]	Accepted - change is made.
44599	23	18	23	18	variability --> temporal variability [Judith Hauck, Germany]	Accepted - change made
67457	23	19	23	20	"variability in globally- integrated flux from the GOBMs is on average lower than that of observationally-based and inverse modelled products" I don't think this is a good choice of words. I can't recall another instance of estimates from inversions being referred to in this way. How about "is on average lower than estimates from observation-based products and inverse models". But note that this is the first time inverse models are mentioned. Perhaps a basic statement of what they are is warranted, e.g., "Inversion studies estimate CO2 fluxes at the Earth's surface from the observed distribution of atmospheric CO2 concentration using atmospheric general circulation models to estimate horizontal transport." [James Christian, Canada]	Taken into account - sentence has been modified
44601	23	20	23	20	biases in the variability'. To quantify the biases of ocean models in the temporal variability of the ocean carbon sink, we would need to know the true variability. And this we don't know. Please acknowledge also the uncertainty in data-based products and ocean inverse models. [Judith Hauck, Germany]	Taken into account - sentence has been modified
58601	23	21	23	21	define ESM [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable - sentence rewritten
44603	23	21	23	23	List of references: I see this sentence supported (for seasonal variability ONLY!) by the Kessler and Tjiputra and Mongwe et al citations. I also see the benefit for citing DeVries et al 2019 on differences in decadal variability between models, data-products and ocean inverse models - but we don't know which one is right or wrong. Maybe the truth is somewhere in between? Lebehot et al 2019 is the right reference for North Atlantic trends and Goris et al for constraining future projections of the North Atlantic CO2 sink. I don't see the benefit of the McKinley et al 2017 and Gregor et al 2019 references in this sentence [Judith Hauck, Germany]	Taken into account - sentence has been modified
2201	23	27			Please add Fassbender et al. (2017): Fassbender, A. J., Sabine, C. L., and Palevsky, H. I. (2017), Nonuniform ocean acidification and attenuation of the ocean carbon sink, Geophys. Res. Lett., 44, 8404– 8413, doi:10.1002/2017GL074389 [Abed El Rahman Hassoun, Lebanon]	Accepted - change made

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
44605	23	30	23	33	This sentence again mixes up concepts. If the aim of this section is to report on the mean contemporary CO2 sink, data-products and atmospheric inversions do not need to be adjusted for riverine fluxes. The MicKinley et al 2017 and Gruber et al 2019 papers don't make any statement on river flux adjustment and should not be cited in this specific sentence. [Judith Hauck, Germany]	Taken into account - text revised to clarify what is addressed here is the increase in the ocean CO2 uptake fluxes since preindustrial era.
127711	23	30	23	33	Can you include some ranges of potential influence of river flux here? [Trigg Talley, United States of America]	Accepted - range of carbon flux through river discharges were shown.
67459	23	31	23	31	Empirical interpolation methods calculate CO2 concentration or partial pressure, not air-sea flux. Changing "calculated" to "estimated" and "methods" to "products" would probably suffice. [James Christian, Canada]	Accepted - change made
44609	23	38	23	52	The figure caption says that the numbers shown would be ' global ocean air-sea CO2 fluxes'. Again, concepts and definitions are mixed up, see more detailed comments on the figures [Judith Hauck, Germany]	Accepted - correction made
58603	23	38	23	52	I suggest including in the caption that negative values represent a gain to the reservoir (sea) [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account - caption has been altered
44607	23	39	23	40	Not all models are references and should be, see comment above. If only a subset of the GCB models is used, is should at least be stated on which basis these models were rejected. [Judith Hauck, Germany]	Accepted - all referenced
58731	23	40	23	40	The observationally-based products line in the graph is pink, not blue. (Also applies to page 174, line 8.) [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted - the figure has been redrawn
58605	23	40	23	40	the color is magenta, not blue. Correct either the caption or the figure. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted - the figure has been redrawn
9323	23	44	23	44	Consider revising the following: "Observationally-based products have been corrected differences in spatial coverage,…" [Christine Weldrick, Australia]	Accepted - the products now all have the same area correction
72951	23	47	23	47	Delete (after : [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change is made.
104879	23	51	23	51	It does not seem appropriate to correct the air-sea pCO2 fluxes for an (unknown) river effect. Better to just report the air-sea CO2 fluxes from each product. [Timothy DeVries, United States of America]	Reject - while the comment is well taken models and products reflect different flux definitions which differ by the pre-industrial steady state river linked outgassing. Making this correction makes them comparable. While there is quite a spread in the constraints for the river contribution the community has found a consensus on an average of 0.62PgCy-1. There are also other uncertainties such as the cool skin and enhanced outgassing from the SO but these are in the research realm at this stage.
72953	23	51	23	51	Delete) [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - the closing parenthesis is needed to close the one before "References" on line 47.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
87681	24	0	25	0	Section 5.2.2.1.3, the first and second paragraph talk about the same time periods which makes it hard to follow and remember in one paragraph what the other paragraph said - are the two paragraphs separate because the first is based on observations and the second on (inverse) modelling? I suggest to combine the two paragraphs. Or else try to make the points of the two paragraphs overly clear. Also, more generally, it is not very intuitive (if one does not know all the background literature) why DeVries argues with enhanced outgassing of natural CO2 due to enhanced winds in the SO overwhelming uptake of Canth, and the same time it is argued in the report that enhanced winds indeed cause enhancedcarbon uptake by subduction of anth carbon in mode and intermediate waters (as mentioned in other places). Please try to streamline and very clear across paragraphs to avoid confusion of natural and anthropogenic carbon (as this is a somewhat difficult concept for non marine biogeochemists anyhow). E.g., p58 L53 "On centennial timescales, CO2 uptake and storage is strengthened in the Southern Ocean due to intensified winds (Ito et al., 2015). On millennial timescales, weakening deep ocean circulation reduces the downward transport of CO2 from the surface to the deep ocean in high latitudes resulting in decreased CO2 uptake (Yamamoto et al., 2018)." [Ivy Frenger, Germany]	Accepted - We have clarified that the first paragraph was referring to the air sea fluxes and the second was referring to the rate of storage in the ocean interior. They cannot be combined but the point that they both show decadal scale responses is highlighted. The adjustments of the net flux by either or both enhanced outgassing of natural CO2 and uptake of anthropogenic CO2 are not mutually exclusive - both are important to understand the magnitude and timing of the variability. The concept of natural and anthropogenic CO2 was also clarified. Aparent inconsistencies such as that one pointed out elsewhere - p58 L53 have also been addressed.
2785	24	1	24	1	The bracket "(" does not appear to close anywhere [Stephen Wilkinson, United Arab Emirates]	Accepted - change is made. A bracket has been added after "changes".
33299	24	1	24	3	Re-write: “The total increase of CO2 stored in the ocean interior (net anthropogenic and natural CO2 uptake and storage changes has been evaluated as 140 ± 22 (±2σ) PgC in the year 2007, that is 28 ± 5% of total anthropogenic CO2 emissions (Gruber et al., 2019a).” [Guiomar Rotllant, Spain]	Rejected - the reviewer does not state what in this sentence should be rewritten
37955	24	1	24	3	“~ (net anthropogenic and natural CO2 uptake and storage changes has been evaluated as 140 ± 22 (±2σ) PgC in the year 2007, that is 28 ± 5% of total anthropogenic CO2 emissions (Gruber et al., 2019a). ? “)” is missing. [Junhee Lee, Republic of Korea]	Accepted - change made
58733	24	1	24	3	There seems to be an error and/or missing information in this sentence. The statement references Gruber et al., 2019a; however the values provided in this sentence could not be found in Gruber et al. On page 2 of Gruber et al., it is stated that "Adding the 34 ± 4 Pg C increase between 1994 and 2007 to the 118 ± 19 Pg C estimated for the change between the preindustrial period and 1994 yields a global ocean storage for 2007 of 152 ± 20 Pg C." Is it possible these ae the values the author refers to? [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected - the values provided in the senence have been derived from the "net ocean CO2 uptake" in Table 2 of Gruber et al. (2019a) taking both "uptake of anthropogenic CO2 by the ocean" and "loss of natural CO2 by the ocean" into account.
58735	24	1	24	3	It was not clear if the value provided for the total increase of CO2 in the year 2007 (140 ± 20 PgC) was referring to the change of CO2 stored from the start of 2007 to the end of 2007, or if it was referring to a different time span. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account - sentence has been modified
2337	24	1		3	Thank you in advancing for redistributing the parentheses to make the sentence clearer. [Abed El Rahman Hassoun, Lebanon]	Accepted - change made
2203	24	1		3	Thank you in advance for redistributing the parentheses to make this sentence clearer. [Abed El Rahman Hassoun, Lebanon]	Accepted - change made

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
44611	24	5	24	6	The 2.1 PgC/yr number given here is anthropogenic+natural CO2 flux, not taking into account river fluxes, hence it is NOT the 'global mean', i.e. contemporary flux. [Judith Hauck, Germany]	Taken into account - text revised so that it is shown that the 2.1 PgC/yr number given here is from GOBMs.
72955	24	5	24	6	Remove split of numbers and units across line break. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial - this kind of issues will be taken care of during the production phase, which includes professional copy-editing.
74209	24	6	24	6	is the plural in “increases” correct? [Christoph Völker, Germany]	Accepted - change made
72957	24	8	24	9	I don't know what Mode and Intermediate Waters means. Please check accuracy of text. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Noted - a reference to chapter 9 where water masses are discussed in detail is given
104881	24	10	24	10	The DeVries et al (2019) refernece is not appropriate, as that study did not address anthropogenic CO2 transport and storage. The study you probably mean to refer to is: DeVries, T. (2014). The oceanic anthropogenic CO2 sink: Storage, air-sea fluxes, and transports over the industrial era. Global Biogeochemical Cycles, 28(7), 631-647. [Timothy DeVries, United States of America]	Accepted - change made
58611	24	11	24	11	Since figure 5.8b is mentioned first in the text, it would make sense to show it first, so turn it into 5.8a. Unless there is another strong reason to have them in that order [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable - this sentence was moved to section 5.2.1.3.2 and Figure 5.8b is now cited after Figure 5.8a.
44613	24	13	24	13	Global and Regional Variability'. Make clear that this paragraph is about regional PATTERNS, not TEMPORAL VARIABILITY, which is discussed in the next subsection 5.2.1.3.3. is on TEMPORAL VARIABILITY. Maybe just change the title to: "Global and regional ocean fluxes and storage of anthropogenic CO2." [Judith Hauck, Germany]	Taken into account - title changed to reflect the topic is spatial variability
44615	24	13	24	13	"ANTHROPOGENIC CO2" Is it on purpose that now you want to discuss only ANTHROPOGENIC CO2 fluxes and storage in contrast to the discussion of CONTEMPORARY CO2 fluxes in the previous subsection 5.2.1.3.1 ? Most of the flux products are on contemporary, and NOT anthropogenic fluxes. [Judith Hauck, Germany]	Accepted - inconsistencies in the terminology in this sub-section have been edited
52301	24	13	24	13	same information as section above? Perhaps merge the sections. [Agneta Fransson, Norway]	Rejected - section 5.2.1.3.1 is on global trends, section 5.2.1.3.2 is on spatial variability. We have changed the section title slightly to make clearer.
19955	24	13	25	49	Please note that, unlike what is implied by lines 14-16, figure 5.8 conveys no information about variability at all. This is confirmed by the perfectly clear legend of this figure. On lines 23-25, similarly, reference to figure 5.8 to support a statement about decadal variability should be removed. There is absolutely no evidence in this subsection allowing to support statements concerning variability, at neither global nor regional scales. On the other hand, there is a lot if information concerning non-uniformity [philippe waldteufel, France]	Accepted - have deleted the reference to Fig 5.8 in the first sentence. This subsection has been clarified to refer to spatial variability at the regional scale.
9325	24	14	24	14	Suggest replacing "most important advance…" to "most important advancement…" [Christine Weldrick, Australia]	Accepted - change is made.
72959	24	14	24	14	Change 'observations' with 'observation' [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change is made.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
44617	24	14	24	16	This sentence is not helpful here, as (i) the section is supposed to be on spatial not temporal variability, (ii) Gruber et al 2019a is not on interannual variability, but decadal variability or rather long-term trends [Judith Hauck, Germany]	Accepted - sentence has been rewritten
72961	24	16	24	16	Change 'observations' with 'observation' [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change is made.
3925	24	16	24	16	"2019b, 2019a" => "2019a, 2019b" [Makio Honda, Japan]	Not applicable - sentence rewritten
104883	24	16	24	16	Referring to Figure 5.8: The juxtaposition here is jarring because the left-hand panel shows contemporary air-sea CO2 fluxes (which are dominated by natural CO2 fluxes) and the right-hand panel shows accumulation of anthropogenic carbon (which is independent of the natural air-sea CO2 fluxes).So it would be good to add in a panel showing anthropogenic air-sea CO2 fluxes (e.g. from DeVries, 2014). [Timothy DeVries, United States of America]	Noted - the caption has now been clarified
39753	24	18			"high confidence" Is not traceable [TSU WGI, France]	Taken into account - sentence has been modified
104885	24	19	24	20	Regarding the statement about the Southern Ocean CO2 sink: The Southern Ocean is not necessarily a region of natural CO2 uptake -- it tends to be neutral in that regard. It is however a region of high anthropogenic CO2 uptake south of the polar front. This is not due to its large area, but to cold waters, high wind speeds, and upwelling of deep-ocean waters. [Timothy DeVries, United States of America]	Taken into account - sentence has been modified
44619	24	20	24	20	"Southern Ocean because of its area" - well, yes of course it plays a role that the Southern Ocean is huge, but there are other regions which are huge and don't contribute in the same amount. It's the 'Southern Ocean because of its medium intensity and its area and the North Atlantic because of its high intensity' or something similar [Judith Hauck, Germany]	Taken into account - sentence has been modified
52303	24	20	24	20	"intensity" in what? Please clarify. [Agneta Fransson, Norway]	Accepted - change made
58869	24	20	24	23	Other observational-based studies also suggest this, not only for the 1994-2007 but for the whole industrial era (Khatiwala et al., 2013; and DeVries, 2014). More specifically figure 6 from DeVries (2014) show that the highest fluxes ar e in the southern ocean. doi:10.5194/bg-10-2169-2013 and doi:10.1002/2013GB004739 [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account - sentence has been modified
87679	24	21	24	21	"in both cases" -> unclear phrasing; this refers to the north Atlantic vs the Southern ocean, though at first I thought it referred to natural and anthropogenic CO2; this sentence is only about anthropogenic CO2, though, which becomes only clear late in the sentence when it says "anthropogenic CO2"; I think if you just start the sentence with anth CO2, sth like "While for anthropogenic CO2 it is water mass formation..." [Ivy Frenger, Germany]	Not applicable - sentence rewritten
35145	24	21	24	26	Should reference Chp 9 sections on water masses and air sea fluxes here, as well as SROCC [Baylor Fox-Kemper, United States of America]	Accepted - change made
44621	24	23	24	26	"upwelling regions account for decadal variability" - I haven't seen any evidence for this in publications, and in particular not in Figure 5.8 refered to, and also not in the two cited Gruber papers, which suggest a very general cooling pattern in the Pacific and enhanced stratification in the Atlantic and Indian sectors. [Judith Hauck, Germany]	Accepted - we have also updated the biome boundary so it includes a greater fraction of the tropical outgassing mainly in the Pacific ocean - this emphasizes the interannual variability with a very weak trend

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
72963	24	25	24	25	Subscript 2 required in CO2 [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change is made.
9327	24	25	24	25	Replace "CO2" with "CO2" (subscript) [Christine Weldrick, Australia]	Accepted - change is made.
44623	24	26	24	29	"by also explaining how regions may amplify or reduce the temporal variability": Three issues: (i) this paragraph is supposed to be about spatial patterns and not about temporal variability, (ii) please be more specific on how they explain the regional patterns, (iii) and again the same papers are cited as in all the previous text bits, McKinley et al 2017, Gruber et al 2019a and b - I'm not sure what they are cited for here, please be more specific or leave the citations out. [Judith Hauck, Germany]	Taken into account - i) this point is appropriate here as it points out that improved knowledge of the spatial patterns actually help explain the temporal variability, ii) the following paragraph explains the regional patterns, iii) the references have been removed
67461	24	27	24	27	delete "fluxes" after "storage" [James Christian, Canada]	Accepted - change made
69185	24	29	24	29	Although it is true that there are not enough data to be plotted, it would be a contribution to new knowledge if there is some mention on the Arctic Ocean, reflecting the increasing number of studies in recent years. For example, Yasunaka et al. (2018) estimated carbon uptake by the Arctic Ocean to be 0.18+-0.13 PgC/y without significant long-term trend due to the balance between sea ice reduction and temperature rise. The estimated uptake in the Arctic Ocean corresponds to nearly 10% of the current observations-based global estimates. Reference Yasunaka et al. (2018) Arctic Ocean CO2 uptake: an improved multiyear estimate of the air–sea CO2 flux incorporating chlorophyll a concentrations Biogeosciences, 15, 1643–1661, 2018 https://doi.org/10.5194/bg-15-1643-2018 [Kaoru Magosaki, Japan]	Taken into account - the Yasunaka study has been added in section 5.2.1.3 and 5.2.1.3.2
52311	24	29	24	29	Suggest adding reference: Yasunaka et al. (2018) for CO2 fluxes in the Arctic [Agneta Fransson, Norway]	Taken into account - the Yasunaka study has been added in section 5.2.1.3 and 5.2.1.3.2
44625	24	36	24	36	Bakker et al 2016 is on SOCAT v3. Maybe refer to (Bakker et al., 2016, updated 20xx) for SOCAT v5 [Judith Hauck, Germany]	Accepted - reference was updated
72965	24	37	24	37	Remove , after McKinley [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change is made.
72967	24	46	24	46	Delete 'time'. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change is made.
67463	24	47	24	47	"attribute" has a very specific meaning in climate science. I don't think the association of changes in storage with changes in the meridional overturning circulation is nearly as robust as is implied here. [James Christian, Canada]	Accepted - changed to "associated with"
9329	25	2	25	2	Remove comma after, "This has also highlighted…" [Christine Weldrick, Australia]	Accepted - change is made.
44627	25	2	25	5	Now again comes a sentence on the regional patterns, which should have been in the previous subsection, at least based on my understanding of the proposed structure. [Judith Hauck, Germany]	Taken into account - sentence has been altered
9331	25	3	25	3	Replace "sensitive" with "sensitivity [Christine Weldrick, Australia]	Accepted - change is made.
67465	25	3	25	3	delete "mean global" and change "sensitive" to "sensitivity" [James Christian, Canada]	Accepted - change made
103121	25	3	25	3	sensitive' should read 'sensitivity' [Philippe Tulkens, Belgium]	Accepted - change is made.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
58779	25	3	25	4	Adjust sentence structure for improved readability: "This has also highlighted which regions of the ocean account for most of the mean global variability and its sensitivity to climate change, as well as …" [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - text revised
18199	25	3	25	9	Change 'sensitive' to 'sensitivity' [Chelsey Baker, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change is made.
9333	25	4	25	4	Replace "strengthening" with "strengthened" [Christine Weldrick, Australia]	Accepted - change is made.
44629	25	4	25	5	"Earth system model evaluation", this is not done in the cited reference to Gruber et al 2019. They evaluate ocean biogeochemical models, i.e. ocean stand alone simulations, which is not an Earth System Model. Gruber et al 2019 also compare the models to the data-products which also have an uncertainty which is not really taken into account. We don't know which one is right or wrong. An evaluation should be to the SOCAT data directly which is the most direct constraint we can get. [Judith Hauck, Germany]	Accepted - meaning of this sentence was clarified. It was not meant to say that Gregor and Gruber 2019 evaluated ESM but that their study of the variability contributes to the confidence level of ESM evaluations.
44631	25	5	25	5	delete "monthly". This is not relevant on the time-scale of interannual to decadal variability [Judith Hauck, Germany]	Taken into account - sentence changed
44633	25	6	25	6	I don't see this sentence backed up by the figure 5.9. You might have to adjust the y-axis to make tropical outgassing visible. [Judith Hauck, Germany]	Accepted - we have updated the biome boundary so it includes a greater fraction of the tropical outgassing mainly in the Pacific ocean - this emphasizes the interannual variability with a very weak trend
19957	25	6	25	6	This reader is unable to detect the ocean outgassing assigned to the Tropics. The flux on figure 5.9f seems to be essentially zero. [philippe waldteufel, France]	Accepted - we have also updated the biome boundary so it includes a greater fraction of the tropical outgassing mainly in the Pacific ocean - this emphasizes the interannual variability with a very weak trend
18101	25	7			An increase in ocean CO2 uptake, for the exact period referenced in the text of the report (2002-2016) is also observed in the following study: Vlad A. Macovei, Susan E. Hartman, Ute Schuster, Sinhué Torres-Valdés, C. Mark Moore, Richard J. Sanders, Impact of physical and biological processes on temporal variations of the ocean carbon sink in the mid-latitude North Atlantic (2002–2016), Progress in Oceanography, Volume 180, 2020, 102223, ISSN 0079-6611, https://doi.org/10.1016/j.pocean.2019.102223. (http://www.sciencedirect.com/science/article/pii/S0079661119304033). While the paragraph continues to speak about the Southern Ocean, it is worth noting examples of other regions where similar trends have been observed, especially since the referenced work fits so well here. [Vlad Macovei, Germany]	Accepted - reference added
44635	25	8	25	8	I don't see this sentence back up by figure 5.9. It's not that straight-forward that just visual comparison would be enough. Any statistical tests? Other data-products supporting this? Also wrong subfigure panel referenced. [Judith Hauck, Germany]	Accepted - a correlation table is now provided in the Supplementary Material

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
116419	25	9	25	9	Here, and in other situations, there is a lack of clear link with ch 3 on model evaluation (implications of model biases related to Antarctic sea ice, southern ocean aspects, to represent key drivers of the southern ocean role in the carbon cycle). [Valerie Masson-Delmotte, France]	Accepted - have made those links with particular emphasis on the point that increasing confidence in product and inverse model variability helps constrain biases in ESMs
63609	25	10	25	10	"sensitivity to carbon concentration" is clearly indicted by McKinley et al. (AGU Advances 2020, now published https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2019AV000149). This reference should be added to this sentence. [Galen McKinley, United States of America]	Accepted - reference added
67853	25	11	25	12	Scarcity and incompleteness of data may result in incomplete results. [Ruandha Agung Sugardiman, Indonesia]	Noted.
7251	25	11	25	12	How to account for the sparseness of data? As all of research has a limitation on data availabity. Is this justified that due to data sparseness (or may be lack of data) influenced the research results. [Asaad Irawan, Indonesia]	Noted.
44637	25	12	25	12	add reference to Bushinsky et al 2019 https://doi.org/10.1029/ 2019GB006176 [Judith Hauck, Germany]	Accepted - reference added
63611	25	12	25	15	To say there is low confidence with respect to interannual variability is not fully correct. We have known for a long time that ENSO is the dominant single mode of variability (LeQuere et al. 2000, McKinley et al. 2004) and that it has a huge effect in the equatorial Paciific (Feely et al 1999 and subsequent). There needs to be some mention of our knowledge of ENSO effects here. McKinley et al. 2020 provide additional mechanistic understanding with respect to decadal variability. [Galen McKinley, United States of America]	Accepted - reference added
72969	25	15	25	15	References should be in chronological order (with submitted one at the end of the list). [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial - this kind of issues will be taken care of during the production phase, which includes professional copy-editing.
44641	25	15	25	15	The Mongwe et al papers are great, but they are about the seasonal misrepresentation in the models, not about the interannual. Not a good reference here. [Judith Hauck, Germany]	Taken into account - sentence has been modified
36365	25	20			Figure 5.9 caption needs to include how the regions presented are defined (e.g. latitude boundaries for each region). [Adrienne Sutton, United States of America]	Taken into account - figure 5.8 defines the regions. Now noted in caption
104887	25	22	25	22	Only the CSIR-ML6 product is used. Need to show more products here of the many available, for example Rodenbeck et al, Landschutzer et al, Denvil-Sommer et al., etc. [Timothy DeVries, United States of America]	Accepted - the revised figure is the mean of 6 products
44639	25	29	25	29	"during the decade after the mid 2000s", could be expressed simpler --> in the decade 2005-2015 [Judith Hauck, Germany]	Accepted - change made
103123	25	29	25	41	the role of the deep ocean would deserve some extended explanation - after all, ist pool is about two orders of magnitude larger than the atmosphere (Fig. 5-12) and the same GtC that double atmospheric concentrations would increase deep oean concentrations by 1-2%. Obviously it is a matter of characteristic mixing times and ocean circulation. Would some statement sbe possible? Also, this may be better linked to section 5.3.3 [Philippe Tulkens, Belgium]	Accepted - have clarified the storage characteristics of the deep ocean in section 5.2.1.3.2 on spatial characteristics of fluxes and storage

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
72971	25	30	25	30	Inventory' is an odd choice of word here, 'content' would be better. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - 'inventory' is the correct term, as used throughout the literature
67467	25	30	25	30	"re-invigorated" is a very odd choice of terms [James Christian, Canada]	Accepted - text revised
67469	25	34	25	37	This passage implies that storage is considerably greater in the Pacific than the Atlantic, which appears to contradict what is stated elsewhere (section 5.2.1.3.2). Possibly the Pacific estimates cited here include the Pacific sector of the Southern Ocean. [James Christian, Canada]	Taken into account - the Southern Ocean sector is included
44645	25	35	25	35	this change in the North Atlantic is huge and would merit a short explanation on what drove these changes. [Judith Hauck, Germany]	Taken into account - sentence has been modified
44643	25	36	25	37	unit should be PgC/decade [Judith Hauck, Germany]	Accepted - text revised
63613	25	39	25	41	McKinley et al. (2020) makes the case for the impact of CO2 concentration in the atmosphere and should be referenced here. [Galen McKinley, United States of America]	Accepted - reference added
35147	25	43	25	49	Useful to link to S. ocean section 9.2.3.2 [Baylor Fox-Kemper, United States of America]	Accepted - reference added
21801	25	44			This section contains very many pieces of poor grammar making it hard to parse and oftentimes difficult to properly interpret. There are too many cases to call each out individually. A more careful proofing would be very useful to ensure clarity in messaging arising from this land carbon assessment. [Peter Thorne, Ireland]	Accepted - the proofing was made for improving readability.
28281	25	44			The following reference contributes to section 5.2.1.4 on Terrestrial Carbon Dioxide: Hubau, W., Lewis, S.L., Phillips, O.L. et al. Asynchronous carbon sink saturation in African and Amazonian tropical forests. Nature 579, 80–87 (2020). https://doi.org/10.1038/s41586-020-2035-0 [Ryan Padrón, Switzerland]	Accepted - Hubau et al. (2020) was included in the revised text.
78485	25	44			what does sub section title mean “terrestrial CO2”? presumably “terrestrial carbon”? [Chris Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - the section title is changed to "Land CO2 Fluxes"
96561	25	45	27	33	The role of peatlands in the land-atmosphere CO2 exchange is not mentioned in this chapter. The function of peatlands as a long-term sink and potential source should please be included here, including new findings especially on the role of tropical peatlands (see e.g. Dargie et al 2017: Age, extent and carbon storage of the central Congo Basin peatland complex). [Nicole Wilke, Germany]	Taken into account - the potential role of peatlands as a tipping element was included with the citation to Dargie et al. (2017).
29155	25	46	27	11	In my view the disparity between satellite-derived estimates of NPP and those emerging from vegetation model runs (Fig 5.10c) needs to be better clarified in this section. There is a considerable difference between both sources, which is also highly important for understanding the difference between "residual sink" and effects of land-use change on the C balance of land; I find that that is worrying. In my view it is important to be more explicit what that difference means, in particular also for robustly assessing the implications of land-based C mitigation strategies such as bioenergy, BECCS or C sequestration in biota & soils. [Helmut Haberl, Austria]	Taken into account - Fig 5.10c was revised to plot GPP, instead of NPP. land-based C mitigation strategies were to be covered by the other WG of AR6.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
29157	25	46	27	11	While there is some discussion of the role of land-cover change and land management here, in my view it is insufficiently reflected what the large uncertainty on the full C effects of land use (land-cover change and land management, as discussed above (p21), shown in Fig 5.6b, means for the issues discussed here. It boils down to the issue that global/climate change affects the vegetation simultaneously with land use/land management/anthropogenic land-cover change, and these are difficult if not impossible to disentangle (e.g., Erb et al., 2013, Nature Climate Change, 3, 854-856). In my view it is important to be more explicit as to what this uncertainty means for interpretation of the phenomena discussed, as well as below in the section on the C balance. [Helmut Haberl, Austria]	Taken into account - at interannual and decadal scale, the large uncertainties in land-cover change and land management prevent isolating its variability in a confident way. Thus, in this section, we focused on net land CO2 sink (including land CO2 sink and net land use change emission). The dynamics of LUC since industrial revolution, where Erb et al. (2013) could be involved, was discussed in section 5.2.1.2. The more recent land use change emission as a carbon budget term was discussed in section 5.2.1.5.
4437	25	46	28	12	These two whole sections would profit from english editing [Ana Bastos, Germany]	Accepted - proof reading was performed.
78241	25	46			Whole section: It might be worthwhile to include some new research around increasing deforestation in recent years in the global South, e.g. Brazil, so that the picture is complete. [Dagmar Nadja Henner, Austria]	Taken into account - the Brazil deforestation fire in 2020 was included in the revised section.
103125	25	46			Given the recent SRCCL largely dealing with similar topics, there may be the possibility to shorten this section. Also, reference to the SRCCL might be provided [Philippe Tulkens, Belgium]	Taken into account - the SRCCL report has mentioned the global greening trend, which is referenced in the revised text.
72973	25	47	25	48	References should be in chronological order. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial - this kind of issues will be taken care of during the production phase, which includes professional copy-editing.
37907	25	47	25	51	I am not quite sure if strengthnening of the global net land CO2 sink has high confidence because of their large uncertainties of the NEP estimation resulting from model error, fire estimation, inventory estimation, and the lack of the reliable data before 1980s. Also different approaches provide different numbers and it was reported that the model did not capture current the land CO2 uptakes. Our current technologies hardly estimates NEE and NEP despite better estimation of GPP. [Junhee Lee, Republic of Korea]	Taken into account - considering the DGVMs cannot reproduce the strengthening net land CO2 sink derived from various line of observed data stream, we revised the statement into medium to high confidence.
115337	25	47			Clarify at the outset that this includes LUC emissions. [Gillett Nathan, Canada]	Accepted - it is explicitly stated that global net land CO2 sink includes both land CO2 sink and net land use change emission.
8737	25	48	25	51	There should be some effort to harmonize the budget numbers from the different studies and to use consistent time periods. How do these nubers compare with Friedlingstein et al (2019) who note that the land CO2 sink was 3.2+/- 0.6 for 2009-2018. [Vaishali Naik, United States of America]	Rejected - These numbers are consistent since net land CO2 sink includes both land CO2 sink and net land use change emission.
58737	25	49	25	51	The numbers provided in this statement for global net land CO2 sink for the 1960's (0.3 ± 0.5 PgC/yr) and the last decade (2.1 ± 0.7 PgC/yr) do not match the numbers provided in Table 5.1 or the numbers in Friedlingstein et al., 2019. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected - These numbers are consistent since net land CO2 sink includes both land CO2 sink and net land use change emission.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
2787	25	50	25	50	"0.3+/- 0.5PgCyr-1". Do the error bars mean that land could have been a source in 1960? Are these error bounds correct? [Stephen Wilkinson, United Arab Emirates]	Rejected - These numbers are consistent since net land CO2 sink includes both land CO2 sink and net land use change emission. There are years where net land CO2 sink is negative.
9335	25	51	25	51	Add "the" before "global net land CO2 sink" [Christine Weldrick, Australia]	Accepted - change was made.
9337	25	51	25	51	Replace "were" with "was" [Christine Weldrick, Australia]	Accepted - change was made.
40863	25	52	25	52	Suggest to define 'atmospheric inversion' in the glossasry [TSU WGI, France]	Not Applicable - this should be considered by the cross-chapter box, as atmospheric inversions were widely used across this chapter and the other chapters (e.g. SLCF).
109539	25	52	25	52	Seems odd to present inversions and DGVMs together on this line. I assume the inversions are separated from the other refs cited on line 47-48 due to them being available only since 1980, but DGVMs predict the sink since the industrial revolution. [Anthony Walker, United States of America]	Rejected - In fact, those refs cited on line 47-48 have used inversions and DGVMs together for decadal land CO2 sink variability.
9339	25	53	25	53	Add "the" before "global net land CO2 sink" [Christine Weldrick, Australia]	Accepted - change was made.
41607	25	53	25	54	This is a very important statement, but please give a short explanation for the differences between northern and southern hemisphere - even though it might be obvious [Katharina Meurer, Sweden]	Taken into account - Tagesson et al. (2020) was cited, which explains the diverging trends between boreal and tropical forests.
13451	25	53	25	54	It could be mentioned that physical or biogeophysical processes influence NH to contribute to an increase of net land CO2 compared to SH. Since the idea is very important but remains unfinished. [Maria Amparo Martinez Arroyo, Mexico]	Taken into account - Tagesson et al. (2020) was cited, which explains the diverging trends between boreal and tropical forests.
109537	25	53	25	54	This statement somewhat contradicts the statement below (pp 26, ln 17) on CO2 attribution that cites Schimel et al 2015 who show a strong Southern hemisphere sink. [Anthony Walker, United States of America]	Rejected - they are not contradictory as both Tagesson et al. (2020) and Schimel et al. (2015) explained, the CO2 fertilization effects lead to large sink over the tropics, but they are cancelled out by tropical LUC emissions.
127713	25	53	25	54	Perhaps mention the role of forests in the divergence in the land sink between northern and southern latitudes -- e.g., Tagesson et al. (2020, Nature Ecology and Evolution). [Trigg Talley, United States of America]	Taken into account - Tagesson et al. (2020) was cited to explain the diverging trends between boreal and tropical forests.
4423	25	54	25	54	"scale remain" --> "scales remains" [Ana Bastos, Germany]	Accepted - change was made.
9341	25	54	25	54	Suggested change to sentence: "Attributing an increased net land CO2 sink to finer regional scales remains challenging." [Christine Weldrick, Australia]	Accepted - change was made.
383	25	54	25	54	Remain → remains [Wolfgang Obermeier, Germany]	Accepted - change was made.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
4425	25	54	25	55	True that it is still challenging, but there have been several works in the paer years evaluating trends and drivers of the terrestrial sink. A key reference missing is, for examples, Fernández-Martínez et al. 2019 (Nature Climate Change) [Ana Bastos, Germany]	Taken into account - Fernández-Martínez et al. (2019) was cited in the paragraph on carbon sink sensitivity to CO2 and climate change. The regional CO2 sink pattern reported by Fernández-Martínez et al. were solely based on atmospheric CO2 inversions, which were still quite uncertain on partitioning the latitudinal distribution of land sink (Gaubert et al., 2019), and Trendy DGVMs, which failed to capture the increasing magnitude of NH sink (Ciais et al., 2019). These lines of evidence, again, highlight the challenge to assess regional-scale CO2 sink change. The ongoing studies on change in regional net land CO2 sink, coordinated by the global carbon project (RECCAP2), are very promising, but unfortunately they cannot be cited here as they are not yet published.
54983	25	54	26	2	Although the statement about limitations of flux estimates based on current satellite CO2 observations is not entirely false, it is very pessemistic and rather dismissive of an emerging methodology that is beginning to appear more promising. The paragraph fails to acknowledge or cite the work of Crowell et al. (2019, https://www.atmos-chem-phys.net/19/9797/2019/), which demonstrated that new satellite CO2 observations do provide greater constraints of regional-scale CO2 surface fluxes for the southern hemisphere in general and also for Northern Asia (both regions where in situ observational coverage is sparse). The research demonstrated this impace with data from NASA's OCO-2 satellite (launched in 2014), but by 2025 Europe's CO2M mission (3-4 satellites) could provide enhanced data quality and 150 times the data volume of OCO-2, along with numerous other new missions. The negative tone of the statement is somewhat inconsistent with a later statement (Chapter 5, page 27, lines 41-44) with a more optimistic view of satellite CO2 data. [Nancy Hamzawi, Canada]	Taken into account - we have accordingly improved the tone of the sentence and includes Crowell et al. (2019) as additional reference on satellite CO2 based inversion
72975	26	2	26	2	Insert space after 'uncertain'. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change was made.
9343	26	2	26	2	Add space after "largely uncertain" [Christine Weldrick, Australia]	Accepted - change was made.
385	26	2	26	2	add space before bracket [Wolfgang Obermeier, Germany]	Accepted - change was made.
51127	26	2	26	2	"uncertain(Hou...)" needs a space before the bracketed citation [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change was made.
74211	26	2	26	2	missing space before the bracket [Christoph Völker, Germany]	Accepted - change was made.
33301	26	2			Change: “uncertain(Houweling” by “uncertain (Houweling”. [Guiomar Rotllant, Spain]	Accepted - change was made.
9345	26	4	26	4	Add "the" before "net land CO2 sink" [Christine Weldrick, Australia]	Accepted - change was made.
74213	26	4	26	4	insert “the” before “net land” [Christoph Völker, Germany]	Accepted - change was made.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
41609	26	4	26	13	Recent research has pointed towards plants actively emitting N2O (Chang et al. 1998: Nitrous oxide emission through plants, Soil Sci Soc Am J 62:35-38; Machacova et al. 2016 Scientific Reports 6:23410, doi: 10.1038/srep23410; Diaz-Pines et al. 2016 Plant Soil, doi: 10.1007/s11104-015-2629-8; Lenhart et al. 2019 New Phytologist 221:1398-1408, doi: 10.1111/nph.15455; and this effect probably being influenced by photosynthesis (Smart & Blom 2001: Wheat leaves emit nitrous oxide during nitrate assimilation, PNAS 98:7875-7878; Bruhn et al. 2014 Atmospheric Environment 99:206-214, doi: 10.1016/j.atmosenv.2014.09.077; Schuetzenmeister et al. 2020 PEI, doi: 10.1002/pei3.10015). It is among the "hot topics" in that area at the moment and if it is true, both measurement designs and ecosystem models will have to be adapted to that. So far the data and research studies are still comparatively scarce. Still, it could be good to keep that issue in mind (for the following report?) or mention it at least with a sentence or two [Katharina Meurer, Sweden]	Rejected - the section is on CO2, while N2O is addressed in the later part of this chapter.
77737	26	4	26	13	Can carbon fertilisation be explained further as it is significant in the overall context of climate change and accumulation of carbon in terrestrail systems. The roles of this in the carbon cycles and the risks e.g. due to climate impacts could be addressed. [Emer Griffin, Ireland]	Taken into account - the CO2 fertilization effects were explained in the next paragraph.
3669	26	6	26	6	the reference Mao et al., is cited but not listed in the reference. Please add this. [Jiafu Mao, United States of America]	Accepted - change was made.
109541	26	6	26	6	Greenness is not the best indicator if photosynthesis change, it's an indicator of LAI change. Suggest leading with the estimates that are closest to a GPP "measurement." [Anthony Walker, United States of America]	Accepted - closer GPP proxies was moved ahead.
37931	26	7	26	8	Cheng et al., 2017 showed that "the terrestrial carbon uptake increase is not accompanied by a proportional increase in water use (i.e. evapotranspiration) but is largely (about 90%) driven by increased carbon uptake per unit of water use, i.e. water use efficiency". So, the author needs to revise the sentence "~ observation-driven inference of increasing photosynthesis CO2 uptake based on enhanced water use efficiency and evapotranspiration ~". [Junhee Lee, Republic of Korea]	Accepted - this was further clarified as " increasing photosynthesis CO2 uptake based mostly on enhanced water use efficiency"
4427	26	9	26	9	"enlarging" --> "enhanced" [Ana Bastos, Germany]	Accepted - change was made.
19007	26	9	26	10	might be good to cite this paper as well: Montzka, S. A., Calvert, P., Hall, B. D., Elkins, J. W., Conway, T. J., Tans, P. P., and Sweeney, C. (2007), On the global distribution, seasonality, and budget of atmospheric carbonyl sulfide (COS) and some similarities to CO2, J. Geophys. Res., 112, D09302, doi:10.1029/2006JD007665. [Mengze Li, Germany]	Rejected - this reference was on the static budget of COS, not linking COS change with GPP change.
96563	26	10	26	11	For DGVMs it may be good to cite Friedlingstein et al, 2019 global carbon budget, since this is the most up-to-date compilation of DGVMs showing enhanced carbon update over history. [Nicole Wilke, Germany]	Taken into account - this reference was added on Page 25
109543	26	11	26	13	Seems an unnecessary level of detail without a point being made. [Anthony Walker, United States of America]	Accepted - the sentence was removed.
9347	26	12	26	12	Add "the" before "1980s" [Christine Weldrick, Australia]	Accepted - change was made.
72977	26	13	26	13	Insert 'the' after 'since'. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change was made.
9349	26	13	26	13	Add "the" before "1960s" [Christine Weldrick, Australia]	Accepted - change was made.
72979	26	15	26	15	Insert 'the' after 'since'. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change was made.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
9351	26	15	26	15	Add "the" before "global net land CO2 sink" and before "1980s" [Christine Weldrick, Australia]	Accepted - change was made.
41613	26	15	26	17	has this been shown for different kind of plants? What does the increased photsynthesis refer to? I suppose forests?! [Katharina Meurer, Sweden]	Rejected - biome-specific photosynthesis change is beyond the scope of this section.
109561	26	15	26	17	Suggest revisiting “medium to high” confidence. O’Sullivan is a modeling study with just a single model, Sitch and the CO2 inference in Schimel both come from a very similar set of models. i.e. these three studies are not independent, and their inference relies on models which we know do not represent many, many processes correctly and that could change the inference that CO2 is the “main driver” (e.g. Zaehle et al., 2014; Pugh et al., 2019). Furthermore, Fernandez-Martinez suggest a CO2 related trend in the land sink that would reduce the land sink to zero in around 20 years into the past (current land sink ~2.3 PgC yr-1, Co2 effect on sink ~110 TgC yr-2). Further, their estimated change in the land sink is close to an order of magnitude higher than the model estimates, which do a reasonable job of reproducing the ‘natural’ land sink estimated from the residual (Friedlingstein et al., 2019). Suggest “medium” confidence. [Anthony Walker, United States of America]	Accepted - it was changed to "medium" confidence.
58781	26	15	26	22	This sentence considerably overstates the current level of confidence in attribution of changes in the land sink to the CO2 fertilization of photosynthesis. While there is a strong theoretical basis for the expectation that CO2 fertilization is occurring, the observational support for this expectation is indirect and claiming high confidence for attribution is inconsistent with the approaches used for attribution and the large uncertainties related to all the other processes/interactions contributing to the land sink (many of which are discussed in subsequent paragraphs). Suggest to discuss this claim more thoroughly, presenting the methods used for attribution of the land sink to CO2 fertilization and discussing their strengths as well as limitations in a more balanced way. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account - the confidence level was changed to "medium". Although contemporary literature seems consistently suggesting the dominant role of CO2 fertilization either through process modelling or through statistical modelling, considering the uncertainties in either approach, we turned down the confidence level to medium.
86747	26	15	26	22	Please consider to add some text on forest re-growth and age class effects [Oyvind Christophersen, Norway]	Taken into account - Pugh et al. (2019) was included in the discussion on land use change.
115339	26	15		16	What about reduced LUC emissions? Do they contribute? [Gillett Nathan, Canada]	Taken into account - this was addressed in Page 26 line 40.
111025	26	21	26	21	but see recent studies looking at the NH -- Bastos et al. 2019 and Wang et al (accepted in GCB) -- Bastos et al. 2019 https://doi.org/10.5194/acp-19-12361-2019; Wang et al. accepted in GCB, "Causes of slowing-down seasonal CO2 amplitude at Mauna Loa"; [Julia Nabel, Germany]	Rejected - these papers are not on the impacts of climate change on global net land CO2 sink. Even these papers mentioned show large inter-model range of climate change impacts

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
106167	26	22	26	22	A point that could be made here is that while the increase in NPP is studied using rather similar formulations between DGVMs, with the main uncertainty the strenth of carbon fertilisation and nutrient limitations, changes in carbon residence times are most strongly uncertain because of carbon outflux processes, i.e. ecological disturbances such as the roles of extremes, tissue and plant mortality, fire, insects, sapling survival etc. This is systematically documented across several DGVMs by Friend et al., PNAS 111, 3280–3285, 2014; and empirically by Yu et al., PNAS, www.pnas.org/cgi/doi/10.1073/pnas.1821387116, 2019. [Wolfgang Lucht, Germany]	Taken into account - the point proposed is much valid since the carbon turnover should be the critical uncertainties for projecting carbon cycle-climate feedbacks. However, none of the references could support that these processes were the main sources of uncertainties on trends of net land CO2 sink over past three decades. Therefore, it should not be discussed here. But it is indeed should be considered by section 5.4.3.
4429	26	24	26	24	observed" --> "has been observed" [Ana Bastos, Germany]	Rejected - "observed" has been changed to "is observed".
2789	26	24	26	24	should be "area is observed" [Stephen Wilkinson, United Arab Emirates]	Accepted - change was made.
74215	26	24	26	24	missing “is” before “observed” [Christoph Völker, Germany]	Accepted - change was made.
23689	26	24	26	28	clunky paragraph…..start the paragraph with "Satellite observations…. [Massimo Lupascu, Singapore]	Accepted - the paragraph was rearranged.
4431	26	24	26	29	consider improving the sentence readability [Ana Bastos, Germany]	Accepted - the sentences were re-written for better readability.
96565	26	24	26	37	It would make sense here to include the burnings and land use change within the Amazonian forest, that increased over the last years (report of INPE, Brazil). [Nicole Wilke, Germany]	Taken into account - the regional differences were included in the revised text.
96567	26	24	26	37	Are these scientific findings still valid in the light of the latest events of forest and bush fires in the Amazon Basin and Australia? Maybe they can be extended by first scientific insights on these events. [Nicole Wilke, Germany]	Taken into account - the regional differences were included in the revised text.
58739	26	24	26	37	It may be interesting in this discussion regarding the declining trend of global burned area to discuss changes in spatial trends of wildfires in recent decades and the impacts that may have on terrestrial carbon storage. For example, increased burning in high latitudes in recent decades disrupts highly carbon-rich storage regions such as peatlands, fen and permafrost, ultimately releasing terrestrial carbon. Possible sources for such a discussion could be: Gibson, Carolyn M., et al. "Increased deep soil respiration detected despite reduced overall respiration in permafrost peat plateaus following wildfire." Environmental Research Letters 14.12 (2019): 125001. Kim, Yongwon, and Noriyuki Tanaka. "Effect of forest fire on the fluxes of CO2, CH4 and N2O in boreal forest soils, interior Alaska." Journal of Geophysical Research: Atmospheres 108.D1 (2003): FFR-10. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account - the peatland carbon emission was included in the revised text citing Gibson et al. (2019).

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
31867	26	24	26	37	Fire discussion - this is even more uncertain than the text (which is heavily dependent on satellite rather than tropical in situ observation) indicates. ENSO cycles are massively important for inter annual variation. Fire emission is a product of fuel load as well as fire area.In la Nina years fuel load can be large. That burns in tropical winters. In el Nino years, fuel load can be small even though burn areas are large. Moreover, in the outer moist tropics a lot of the fuel load may be leaf fall from facultatively deciduous tropical woodland. There's very little real in situ measurement from key areas like Cameroon and Angola and S Sudan. We really don't know what the long term change in burning has been over a full ENSO cycle. The other problem is that the CO fire proxy has been much altered by the much better car emission catalytic clean up and the shift to diesel vehicles in the period aouond 2000-2015 - vehicle CO production plummeted as diesels emit so much less and petrol car CO was dramatically cut. Nowadays there are great numbers of cars in the tropics, not just in Mumbai but in the new super cities like Lagos and Kinshasha. So even though CO lifetime is fairly short and CO mixing ratio is falling, much of that change can probably be ascribed to changes in tropical cars: without much 13CO data it is hard to extract the fire CO signature. [Euan G. Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - we have made the statements more balanced and make it medium confidence considering the uncertainties associated with satellite burned area datasets. We agree that satellite burned area datasets are not perfect, but they are the only datasets that provide us global burned area estimates over the past two decades.
77739	26	24	26	37	Burning of vegetation has roles in many land mantainance practices. Are there insights on this practice and its roles? [Emer Griffin, Ireland]	Not Applicable - practices like "slash and burn" may have impacts on the burned area trend, but data limitations impedes us to make meaningful statements here.
86749	26	24	26	37	Please add some text on SLCP from wildfires and relationships between black carbon (BC) (as a warming agent) and organic carbon (as a cooling agent) in this respect. [Oyvind Christophersen, Norway]	Rejected - this is covered by Chap 6
127715	26	24	26	37	It may be prudent to comment on the interaction of fire and atmospheric N deposition (stimulating productivity in regions such as Africa) -- e.g., Bauters (2018, PNAS). [Trigg Talley, United States of America]	Taken into account - It is included in the revised text.
19959	26	24	26	37	The general feeling when hearing the news in 2018 and 2019 does not reflect the declining trend mentioned on line 28. While the critical question seems well formulated on lines 29-31, is not perhaps the firm conclusion given in the text somewhat hasty? [philippe waldteufel, France]	Rejected. The overall declining trend in global burned area is robust, though large fire events has attracted public attention in the past few years.
34897	26	24	26	38	It is true that observations indicate a significant decline in burnt areas globally over the last 2 decades after the peak in the 1990s. Please see also comment #12. [Jim O'Brien, Ireland]	Accepted - as it is, with additional reference by Yin et al. (2020)
109551	26	24	26	44	Seems unbalanced: 14 lines for fire effects which have “low confidence” while only 5 lines for land use which have “low to medium” confidence, and as I argue above might be better considered medium confidence. Suggest reducing fire discussion and increasing land use change discussion. [Anthony Walker, United States of America]	Taken into account - the 14 line paragraph has two major message which are declining global burned area (medium confidence) and its contribution to global net land CO2 sink. The confidence of later message was changed to low to medium confidence with recent publication (Yin et al., 2020). Therefore, it is now well balanced with the later paragraph on land use change.
72981	26	26	26	26	Replace 'warmer' with 'higher'. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change was made.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
86751	26	26	26	26	Is increased frequency of lightning really evident? Some more text and references to scientific litterature is needed to confirm this. [Oyvind Christophersen, Norway]	Accepted - It was revised to changing frequency of lightening in boreal region as discussed by Veraverbeke et al. (2017)
90139	26	26			This statement uploaded by Ted Schuur on behalf of a graduate class that reviewed the chapter (this was communicated with IPCC TSU in advance): There may be a place here to mention that, although global burned area is declining, the places where fire is increasing are globally important C sinks and places where successional trajectories could be disrupted with further feedbacks to C cycling (Walker et al. 2019, Alexander and Mack 2016) [Edward Schuur, United States of America]	Taken into account - the regional increasing fire trend has been accounted in the revised text.
127717	26	27	26	28	An important paper missing from this list of references is: Kitzberger T, DA Falk, AL Westerling, and TW Swetnam. 2017. Direct and indirect climate controls predict heterogeneous early-mid 21st century wildfire burned area across western and boreal North America. PLoS One 12(12): e0188486. https://doi.org/10.1371/journal.pone.0188486 The paper emphasizes the variability among regions (in this case, western North America) in trends and projections of area burned based on a mid-range emissions scenario. [Trigg Talley, United States of America]	Not Applicable - The projected burned area trend is beyond the scope of this section, discussing the historical trend in burned area and fire emissions of CO2.
8739	26	28	26	28	s should be lower case in Satellites and it would be more helpful to state the years instead of "past two decades" [Vaishali Naik, United States of America]	Accepted - change was made.
9353	26	28	26	28	Change "area" to "areas" [Christine Weldrick, Australia]	Accepted - change was made.
33303	26	28			Change: “Satellite” by “satellite”. [Guiomar Rotllant, Spain]	Rejected - the comma before "Satellite" was changed into a dot, hence keeping the upper case.
37933	26	28			Could you add quantitative information of the declining trend of global burned area? [Junhee Lee, Republic of Korea]	Taken into account - Qualitatively, different datasets agree on the apparent declining trend. However, since different burn area datasets show two-fold differences (-0.66% yr-2 - -1.22% yr-2) in the rate of burned area decrease (Forkel et al., 2019), we describe the trend as about 20% over past two decades.
98347	26	29	26	30	Andela et al (2017) suggested that the decline in global burned area is driven by fire management and suppression in Northern Hemisphere Africa, however Zubkova et al (2019) suggested that this only explains about one third of the decline, and the rest is attributable to increased terrestrial moisture (Zubkova, M., Boschetti, L., Abatzoglou, J. T., & Giglio, L. (2019). Changes in fire activity in Africa from 2002 to 2016 and their potential drivers. Geophysical Research Letters, 46, 7643– 7653. https://doi.org/10.1029/2019GL083469) [Chantelle Burton, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - the sentence was revised to be more balanced in order to reflect the ongoing debates on the relative importance of human and natural drivers at the global scale.
52787	26	29	26	33	Can the statement about the declining trend of global burned area be refined to include most recent fires in Amazonia and Alaska? [Sergey Malyshev, United States of America]	Taken into account - the regional perspectives were added in the end of the paragraph.
127719	26	29	26	33	Can the statement about the declining trend of global burned area be refined to include most recent fires in Amazonia and Alaska? [Trigg Talley, United States of America]	Taken into account - the regional perspectives were added in the end of the paragraph.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
9355	26	30	26	31	Suggested change to sentence: "which, at a global scale, outweighs the climate-driven change to burned areas" [Christine Weldrick, Australia]	Accepted - change was made.
8955	26	30	26	37	Assuming there is a decrease in global burned area (I agree this is only medium confidence, considering the large errors of current global BA products to detect small fires (Roteta et al., 2019 RSE), I disagree about attributing this reduction to fire management and suppresion. The most clear decrease occurs in Africa and there fire suppression is uncommon. Land use change (from range to croplands) may be one explanation, but other authors also attribute it to increase wetness in the region (Zubkova, et al. 2019. GRLs, 46, 7643-7653). [Chuvieco Emilio, Spain]	Accepted - We no longer include attribution of burn area trend in this section due to words limit and the debates on the dominant factors in the literature.
44133	26	31	26	31	wrong reference Forkel 2019a evaluates dgvms and does not analyse trends. The reference could be Forkel 2019b, but also this reference does not exactly support the statement. It shows that the increase due to temperature is balance by changes due to humans and precipitation. Teckentrup et al. 2019 show that the human factors lead to stronger trends than climate. [Gitta Lasslop, Germany]	Accepted - Teckentrup et al. (2019) was cited here and the sentence was revised to be more balanced.
44135	26	31	26	31	Giglio et al 2013 is a paper on burned area, not about emissions. [Gitta Lasslop, Germany]	Accepted - the citation was moved accordingly.
4433	26	32	26	32	"emission" --> "emissions" [Ana Bastos, Germany]	Accepted - change was made.
9357	26	33	26	33	Add "the" before "global net land CO2 sink" [Christine Weldrick, Australia]	Accepted - change was made.
44137	26	33	26	37	The overall effect of fire on climate was estimated to have a negative radiative forcing (ward et al. 2012, acp, doi:10.5194/acp-12-10857-2012), the estimates of solid pyrogenic carbon storage in soil and ocean are higher than the fire-induced losses simulated by vegetation models (Lasslop et al. 2019, CCCR, https://doi.org/10.1007/s40641-019-00128-9) [Gitta Lasslop, Germany]	Taken into account - the pyrogenic carbon is included in the revised text. However, The net radiative forcing is beyond its scope, since the section is on the land-atmosphere CO2 flux.
78487	26	36	26	36	can you refer to a specific section within chapter 6? [Chris Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - section 6.6.5 was provided here.
9359	26	37	26	37	Add "the" before "net land CO2 sink" [Christine Weldrick, Australia]	Accepted - change was made.
4435	26	39	26	39	"emission" --> "emissions" [Ana Bastos, Germany]	Accepted - change was made.
9361	26	39	26	39	Suggested change to sentence: "Evidence has also emerged that lower land use changes to emissions..." [Christine Weldrick, Australia]	Accepted - change was made.
111027	26	40	26	40	specify region and time-span of "reduced deforestation" [Julia Nabel, Germany]	Accepted - reduced deforestation was largely from the tropics.
54985	26	40	26	40	":afforestation and regrowth" could be clarified as "increased afforestion and faster regrowth" [Nancy Hamzawi, Canada]	Accepted - change was made.
9363	26	41	26	41	Suggested change to sentence: "…contributed to an acceleration of the global net land CO2 sink since the late 1990s…" [Christine Weldrick, Australia]	Accepted - change was made.
109545	26	41	26	42	Pugh et al (2019) and possibly Pan et al. (2011) should also be cited here. Pan, Y., Birdsey, R. A., Fang, J., Houghton, R., Kauppi, P. E., Kurz, W. A., et al. (2011). A Large and Persistent Carbon Sink in the World's Forests. Science, 333(6045), 988–993. Pugh, T. A. M., Lindeskog, M., Smith, B., Poulter, B., Arneth, A., Haverd, V., & Calle, L. (2019). Role of forest regrowth in global carbon sink dynamics. Proceedings of the National Academy of Sciences, 116(10), 4382–4387. [Anthony Walker, United States of America]	Accepted - Pugh et al. (2019) was cited.

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109547	26	41	26	42	What's the justification for "low to medium" confidence. The confidence in CO2 should be about equal to confidence in land use change as they are both inferred from similar methods, i.e. models. [Anthony Walker, United States of America]	Rejected - They are not at the same confidence level since increasing net land CO2 sink was supported by three independent line of evidences: atmospheric inversions, residual from carbon balance of atmosphere and ocean, land models, while the lower land use change emissions come from two lines of evidence: the bookkeeping model and the difference between process models and atmospheric inversions. Considering also the large uncertainties in land use change modelling, the low to medium confidence is appropriate.
9365	26	42	26	42	Suggested change to sentence: "…though other mechanisms explaining the acceleration of the land CO2 sink were…" [Christine Weldrick, Australia]	Accepted - change was made.
109549	26	42	26	44	This qualifier is unnecessary. The effects of CO2 on photosynthesis have already been covered above. If a statement on declining respiration needs to be made, make it as a separate possible driver rather than a potential counter to the land use driver. [Anthony Walker, United States of America]	Rejected - increasing photosynthesis was indeed covered, but not for its acceleration. It is necessary to be mentioned here.
37935	26	43	26	44	It would be better to add the reason for acceleration of photosynthesis and reduced respiration (i.e. rising atmospheric CO2 and global warming slowdown) to help readers understand better. [Junhee Lee, Republic of Korea]	Accepted - reasons provided as suggested.
72983	26	46	26	46	Replace 'progresses were' with 'progress has been'. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change was made.
9367	26	46	26	46	Add "the" before "global net land CO2 sink" [Christine Weldrick, Australia]	Accepted - change was made.
51129	26	46	26	46	"progresses were" should be "progress was" [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change was made, but using complete tense (i.e. has been).
9369	26	47	26	47	Replace "evidences" with "evidence" [Christine Weldrick, Australia]	Accepted - change was made.
51131	26	47	26	47	"reconciling evidences" should be "reconciling evidence" [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change was made.
71731	26	51			The term 'Trendy DGVM' is not explained earlier in this chapter. [Tuomo Kalliokoski, Finland]	Accepted - Trendy was removed as it is not necessary to specify them as Trendy DGVMs.
78489	26	52	26	54	This sentence implies that inclusion of nutrient cycles causes uncertainty. While the results are uncertain they are actually more constrained when nutrients are included compared to when they're not included. So a more accurate statement might be that large uncertainties exist in the response of terrestrial carbon to CO2, but this can be constrained by consideration of the role of nutrients. [Chris Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - the sentence was revised to be more clear on the message as "However, as The representations of carbon-nitrogen interactions vary greatly among models, leading to large uncertainties remains on how nitrogen cycling regulates in the response of ecosystem carbon uptake to higher atmospheric CO2."

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
44139	26	55	27	1	who is “the other group”. Not relevant that fire models have been compared. Instead many fire models have been updated from only including vegetation and climate drivers in CMIP5 (Kloster and Lasslop, 2017) to representing human ignitions and suppression (Rabin et al. 2017). Kloster, S., Lasslop, G., 2017. Historical and future fire occurrence (1850 to 2100) simulated in CMIP5 Earth System Models. Glob. Planet. Change 150, 58–69. https://doi.org/10.1016/j.gloplacha.2016.12.017 Rabin, S.S., Melton, J.R., Lasslop, G., Bachelet, D., Forrest, M., Hantson, S., Kaplan, J.O., Li, F., Mangeon, S., Ward, D.S., Yue, C., Arora, V.K., Hickler, T., Kloster, S., Knorr, W., Nieradzik, L., Spessa, A., Folberth, G.A., Sheehan, T., Voulgarakis, A., Kelley, D.I., Prentice, I.C., Sitch, S., Harrison, S., Arneth, A., 2017. The Fire Modeling Intercomparison Project (FireMIP), phase 1: experimental and analytical protocols with detailed model descriptions. Geosci. Model Dev. 10, 1175–1197. https://doi.org/10.5194/gmd-10-1175-2017 [Gitta Lasslop, Germany]	Accepted - The model development to include human ignitions and suppression were highlighted.
116421	26		26		I have the impression that several sections of ch 5 are re-assessing literature already assessed in SRCCL (fire box there). Please check. Coordination on fire x chapters (and with WGII) is needed. [Valerie Masson-Delmotte, France]	Taken into account - the fire section is further revised to reflect more recent publications.
44141	27	1	27	1	A project dedicated to the development of fire models found that the representation of human effects through population density and land use changes introduces the largest differences in historical trends for burned area (Teckentrup et al. 2019) and emissions (Li et al. 2020) as simulated by vegetation models. [Gitta Lasslop, Germany]	Taken into account - the revised text highlighted human ignitions and suppression as the major progress in fire model developments.
72985	27	6	27	6	Replace 'attentions' with 'attention' [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change was made.
51133	27	6	27	6	"attentions" should be "attention" [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change was made.
72987	27	7	27	7	Insert 'the' after 'of'. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change was made.
106169	27	7	27	7	Citing has to be selective, but if of interest: an earlier DGVM with good dynamic permafrost was already Schaphoff et al., Environm. Res. Lett., 8, 014026,doi: 10.1088/1748-9326/8/1/014026, 2013. [Wolfgang Lucht, Germany]	Rejected - here we refer to detailed developments since AR5.
88957	27	8	27	9	Can you check the use of the citation "Schurer et al 2015" here. [Schurer Andrew, United Arab Emirates]	Taken into account. The reference has been removed and crossref to section 5.4 for details on permafrost.
23691	27	9	27	9	It is Schuur NOT Schurer [Massimo Lupascu, Singapore]	Taken into account. The reference has been removed and crossref to section 5.4 for details on permafrost.
4253	27	9	27	9	We think that Schurer et al. 2015 should read Schuur et al. 2015 instead. [Claude-Michel Nzotungicimpaye, Canada]	Taken into account. The reference has been removed and crossref to section 5.4 for details on permafrost.
16061	27	9	27	9	Schurer et al: Do you mean Schuur et al? [Gerhard Krinner, France]	Taken into account. The reference has been removed and crossref to section 5.4 for details on permafrost.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
89471	27	9	27	10	The citations given: Schuur 2015, McGuire 2016 and 2018 have conclusions other than 'permafrost carbon storage sensitivity to climate is uncertain'. They both report future emissions estimates. Quantification of the 'uncertainty' is better given by ranges of estimates or standard deviations of model runs. This is a more clear way to express the range of estimates. To an average reader, uncertain indicates that scientists just don't know. Keep in mind ultimately that this text and especially the summary statements are written for non-scientist audiences. [Edward Schuur, United States of America]	Accepted - since this paragraph is on progresses on model developments, the uncertainties remain would not be the key message to be included. These papers were reviewed in more detailed by section 5.4, thus the sentence was removed here.
72989	27	10	27	10	move 'further' to after 'help'. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change was made.
26897	27	11	27	11	looking at soil in depth is a gap clearly identified and this is missing here. see e.g. Balesdent et al. 2018 Nature and Balesdent J., Basile-Doelsch I., Chadoeuf J., Cornu S., Derrien D., Fekiacova Z., Hatté C. (2018) Atmosphere-soil carbon transfer as a function of soil depth. Nature 559, 599-602. [Eric Brun, France]	Taken into account - while the study is interesting, it does not fit into this paragraph, which is on identifying model deficiencies and progresses in model developments. Balesdent et al. (2018) did not compare their results with models, but we included He et al. (2016) as additional reference for model deficiency in representing soil carbon processes.
72991	27	11	27	11	Insert 'the' after 'in'. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change was made.
4447	27	16	26	29	The figure caption of panel c does not indicate which color are the datasets (blue, AVHRR, green MODIS, yellow, DGVMs) [Ana Bastos, Germany]	Rejected - the information is included in the key box.
9371	27	16	27	16	Add "the" before "net land CO2 sink" [Christine Weldrick, Australia]	Accepted - change was made.
9373	27	17	27	17	Change "Residual net land CO2 sink" to "The residual net land CO2 sink" [Christine Weldrick, Australia]	Accepted - change was made.
37909	27	34	27	45	We should also accept that different satellite products give different interannual variabiliity of CO2 fluxes despite new satellite measurements. [Junhee Lee, Republic of Korea]	Accepted - the text was revised to reflect this point.
21803	27	35	27	36	This sentence makes no sense and anyway is not required. Suggest it be deleted. [Peter Thorne, Ireland]	Accepted - the sentence was removed.
40785	27	37	27	37	should be "process-based carbon cycle models"? [TSU WGI, France]	Taken into account - DGVMs were used here to be consistent with other sections of this chapter
72993	27	38	27	38	Insert 'a' after 'have' [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change was made.
58637	27	40			The quoted paper of Zhang et al. isn't fully described --> Zhang et al., 2018a [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Editorial - this kind of issues will be taken care of during the production phase, which includes professional copy-editing.
9375	27	41	27	41	Suggested change to sentence: "A major advancement since AR5 was a set of new satellite measurements that constrained..." [Christine Weldrick, Australia]	Accepted - change was made.
58653	27	43	27	44	Ref problem : "Liu et al., 2017" --should be changed to--> "Liu et al., 2017a" [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Editorial - this kind of issues will be taken care of during the production phase, which includes professional copy-editing.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
4439	27	44	27	44	"depths" --> "depth" [Ana Bastos, Germany]	Accepted - change was made.
58783	27	47	27	49	Suggest rewording for clarity: "Understanding the mechanisms driving interannual variability in the carbon cycle has the potential to provide insight into whether and to what extent the carbon cycle is expected to feed back to climate warming." [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - change to the text was made.
37911	27	47	27	50	Please add another reference, Hong and Kim (2011). Hong and 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. [Junhee Lee, Republic of Korea]	Rejected - the reference was not the most relevant to the paragraph and not a paper published since AR5.
72995	27	49	27	50	References should be in chronological order. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial - this kind of issues will be taken care of during the production phase, which includes professional copy-editing.
58655	27	49			Ref problem : "Cox et al., 2013a" --should be changed to--> "Cox et al., 2013" is sufficient [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Editorial - this kind of issues will be taken care of during the production phase, which includes professional copy-editing.
58657	27	50			Ref problem : "Jung et al., 2017a" --should be changed to--> "Jung et al., 2017" is sufficient [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Editorial - this kind of issues will be taken care of during the production phase, which includes professional copy-editing.
72997	27	51	27	51	Replace 'who' with 'which'. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change was made.
37957	27	51	27	51	“~ as metrics for evaluating DGVMs, who, for example, can reproduce the sensitivity” [Junhee Lee, Republic of Korea]	Accepted - change was made.
4441	27	51	27	51	"who"--> "which" (models are not people) [Ana Bastos, Germany]	Accepted - change was made.
9377	27	52	27	52	Add "the" before "global residual net land CO2 sink…" [Christine Weldrick, Australia]	Accepted - change was made.
84791	27	53	27	53	is the term gamma-IAV really needed? If so, it has to be defined. [Martin Heimann, Germany]	Accepted - as it is used only once in the text, using this term is not necessary.
127721	27	53			Recent experiments have highlighted an asynchrony in soil supply and plant uptake of nitrogen -- where increases in CO2 have supported lower supply and higher demand, while increased temperatures stimulate microbial activity and increases to supply that outpace demand. See Noyce et al. (2019, PNAS). [Trigg Talley, United States of America]	Rejected - this section is on interannual variability of land-atmosphere CO2 exchange, not on nitrogen.
37937	27	54			The number of land use change (i.e. 235 ± 75 PgC) is not equal the number described in Table 5.1. [Junhee Lee, Republic of Korea]	This is on Page 28 (carbon budget section) should be addressed to Pep
72999	27	55	27	56	References should be in chronological order. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial - this kind of issues will be taken care of during the production phase, which includes professional copy-editing.
13453	27	55	27	56	It’s recommend to explain if the anomalous land-atmosphere CO2 exchange during El Niño episodes occur due to anomalies in the precipitation or temperature? Which regions of ENSO does this anomalous land-atmosphere CO2 exchange occur? [Maria Amparo Martinez Arroyo, Mexico]	Taken into account - the follow-up sentence was on this topic, but it is yet to be completely answered by the synthesis of current literature.
4443	28	6	28	6	"two-folds" --> "two-fold" [Ana Bastos, Germany]	Accepted - change was made.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
73001	28	7	28	8	References should be in chronological order. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial - this kind of issues will be taken care of during the production phase, which includes professional copy-editing.
58659	28	7			Ref problem : "Cox et al., 2013a" --should be changed to--> "Cox et al., 2013" is sufficient [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - change on the reference style will be made by the TSU.
9379	28	8	28	8	Add "the" before "atmospheric growth rate…" [Christine Weldrick, Australia]	Accepted - change was made.
4445	28	12	28	12	"remain" --> "remains" [Ana Bastos, Germany]	Accepted - change was made.
9381	28	12	28	12	Add "the" before "tropical net land CO2 sink…" [Christine Weldrick, Australia]	Accepted - change was made.
74217	28	12	28	12	“remain” should be “remains” [Christoph Völker, Germany]	Accepted - change was made.
9383	28	17	28	17	Add "the" before "net land CO2 sink" [Christine Weldrick, Australia]	Accepted - change was made.
9385	28	19	28	19	Suggested change to sentence: "The net land CO2 sink is estimated by four…" [Christine Weldrick, Australia]	Accepted - change was made.
73003	28	22	28	22	Insert 'the' before 'net'. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change was made.
9387	28	25	28	25	Add "the" before "net land CO2 sink" [Christine Weldrick, Australia]	Accepted - change was made.
9389	28	26	28	26	Suggested change to sentence: "A 12-month running mean was taken…" [Christine Weldrick, Australia]	Accepted - change was made.
9391	28	32	28	32	Add "the" before "Climatic Research Unit (CRU)…" [Christine Weldrick, Australia]	Accepted - change was made.
109645	28	37	30	1	There are some discrepancies between some numbers listed in the text and in the table. Please recheck. [Carolyn-Monika Görres, Germany]	accepted. Revised and updated to 2019
96569	28	37	30	2	Table 5.1 and Text before: The ocean and terrestrial sink are negative for the 2000-2009 period. Please revise. If these figure are correct please provide explanation in the text. Also, the net land use change is increasing linear over time, which seems to contradict Figure 5.6 where the land use change decreases in more recent decades. If there is a difference between land use change in Figure 5.6 and net land use change in Table 5.1 (both are emissions), please explain it in the text. Also provide more in-depth information how the terrestrial sink and the net land use change are interrelated. And lastly, we are wondering, why only one scholar is referred to here. [Nicole Wilke, Germany]	Revised. Figure and table have the same values and are consistent.
4459	28	37	30	2	An important point that is missing from this section is the regional partitioning of the budget and the confidence in these fluxes. References to the regional budgets in AR5, to older RECCAP papers and to Gaubert et al. (2019), Kondo et al. (2019) and Bastos et al. (2020) would be important. A figure like Fig.2 from Cross-box 5.1 would be ideal. In fact, CO2 is the only GHG without a regional partitioning figure. [Ana Bastos, Germany]	Regional discussions are in precedent section and 5.2.4
8741	28	39	28	40	The global CO2 budget does not refer to the perturbation of the carbon budget rather it accounts for the sources and sinks of CO2 in the atmosphere. [Vaishali Naik, United States of America]	Accepted. Change made.
4449	28	39	28	40	The definition right now is reduntant. Suggestion: "The global CO2 budget (Figure 5.12, Table 5.1) refers to the perturbation of the global carbon mass balance between reservoirs since the beginning of the Industrial era, circa 1750." [Ana Bastos, Germany]	Accepted. change made.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
115341	28	40		41	According to Pg 21, ln 38 LUC emissions prior to 1750 are 161-501 PgC. The upper end of this range is close to the anthropogenic emissions since 1850. This is not consistent with the statement here that the human perturbation prior to the Industrial Era was small. This requires more careful assessment to ensure consistency. [Gillett Nathan, Canada]	Accepted. Statement deleted.
13433	28	41	28	41	The line is repetitive, use a synonym of “small”. [Maria Amparo Martinez Arroyo, Mexico]	Accepted. Rewritten
54987	28	45	28	45	It would be informative to add a statement about the value in satellite observations of Solar Induced Fluorescence (SIF) and their potential to improve our understanding of photosynthetic uptake by vegetation relative to more traditional satellite-based vegetation indices. [Nancy Hamzawi, Canada]	Accepted. We do not use SIF for the budget. SIF can help biospheric models to improve thru benchmarking and assimilation. Mentioned under land sink section.
4453	28	46	28	46	Even though this is explained in the Fig. 5.12 caption, it would be good to summarize in the text how each of the equation terms are constrained (observations for GATM, models for ELUC and Sland, inventories and EF for FF, ocean delta pCO2 -based model) [Ana Bastos, Germany]	Accepted, but further develop in the legend, not in main text as suggested. Space limitations.
8743	28	48	28	48	to be precise and consistent with Table 5.1, this should be 11.0+/- 0.8 [Vaishali Naik, United States of America]	Accepted - change made.
108823	28	48	28	51	These numbers are inconsistent with table 5.1, figure 5.3. Also check SPM pg 8, line 35-37. The numbers in TS. 2.6 'the carbon cycle' aren't consistent with section 5.2.2 or figure 5.3. [Jason Donev, Canada]	Accepted. Change made.
52789	28	48	28	51	Why the numbers and their uncertainties are different from the numbers in executive summary section, page 6 Lines 44-50? [Sergey Malyshev, United States of America]	Accepted. Corrected and updated.
28283	28	48	28	51	The values provided differ slightly from what is given in the executive summary (page 6, lines 44 to 46) and Table 5.1. Correct or explain why this is the case. [Ryan Padrón, Switzerland]	Accepted. Changed and updated
69773	28	48	28	51	reference for the 2009-2018 numbers? [Gyami Shrestha, United States of America]	Accepted. Yes, but updated to 2019
78491	28	48	28	51	these numbers disagree with exec summary [Chris Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Changed and updated to 2019
4455	28	48	28	56	For the sake of transparency, it would be important to add an explanation of how the uncertainties are calculated [Ana Bastos, Germany]	Accepted. Added.
127723	28	48	29	51	Why are the numbers and their uncertainties different from the numbers in the Executive Summary (page 6, lines 44-50)? [Trigg Talley, United States of America]	Accepted. Corrected and updated to 2019
58741	28	49	28	50	The distributions of emissions provided in this statement match the numbers provided in Table 5.1, however these numbers do not match the numbers provided in Table 6 in Friedlingstein et al., 2019. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Changed and updated to 2019
2791	28	50	28	50	The value 3.2 does not match 3.5 on page 6 from line 46 and the error bar 0.6 on this page and 0.7 on page 6, should these be the same? Note also on table 5.1 [Stephen Wilkinson, United Arab Emirates]	Accepted. corrected and updated.
73005	28	50	28	51	Remove split of numbers and units across line break. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial - this kind of issues will be taken care of during the production phase, which includes professional copy-editing.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
98349	28	50	28	51	The budget imbalance is proposed to be explained by the decline in burnt area and concurrent carbon sink enhancement (Yin, Y., Bloom, A.A., Worden, J. et al. Fire decline in dry tropical ecosystems enhances decadal land carbon sink. Nat Commun 11, 1900 (2020). https://doi.org/10.1038/s41467-020-15852-2) [Chantelle Burton, United Kingdom (of Great Britain and Northern Ireland)]	Thanks for valuable comment and reference.
4451	28	50	28	51	Please add one sentence explaining the concept of budget imbalance, as for example in Le Quéré et al. 2018b. It is fundamental to make this concept clear from a scientific perspective, as it can open the door to criticism by non-experts, especially those wanting to undermine the scientific findings summarized here. [Ana Bastos, Germany]	Accepted. added.
73007	28	53	28	53	Remove split of numbers and units across line break. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial - this kind of issues will be taken care of during the production phase, which includes professional copy-editing.
8745	28	54	28	54	Perhaps I am missing something, +/-75 is not consistent with the value of +/- 190 in Table 5.1. Please update. [Vaishali Naik, United States of America]	Accepted. Corrected.
87685	28	54	28	55	Please make very clear how the Friedlingstein et al, 2019 25% ocean carbon sink goes together with the Gruber et al, 2019 close to 30%. [Ivy Frenger, Germany]	Accepted. Discussing in ocean sink section.
4457	29	1	29	9	This paragraph is too vague, please add detail on what is mean by "source/sink dynamics due to carbon cycling in the land–ocean aquatic continuum". [Ana Bastos, Germany]	Accepted. rewritten
37749	29	1	29	9	Recent studies have revealed the imporatcne of minor carbon flows in terms of CO2 budget. For example, Ito (2019) assessed the effects of minor flows such as land-use, biomass burning, water erosion, methane emission, DOC export, wood harvest, crop harvest, and BVOCs emission. Ito A (2019) Disequilibrium of terrestrial ecosystem CO2 budget caused by disturbance-induced emissions and non-CO2 carbon export flows: a global model assessment. Earth System Dynamics 10: 685–709. DOI: 10.5194/esd-10-685-2019 [Akihiko Ito, Japan]	Accepted, included.
127725	29	1	29	9	Regional studies find anywhere between 12 and 34% of terrestrial NEP is transported from the land to inland waters (Striegl et al. 2012; Wallin et al. 2013; Butman et al. 2016) -- suggesting that this flux should attempt to be addressed. Drake et al. (2018) estimate that of the 5.1 PgC yr-1 that enter freshwaters from the terrestrial landscape, 0.95 Pg yr-1 of C is exported to the ocean (after accounting for outgassing and burial). This number for riverine C export is very uncertain. From Drake et al. (2018): “Overall, we want to emphasize that every term feeding into our simple mass-balance is accompanied by significant uncertainty … the fact that the total estimate of C delivered to inland waters has changed so dramatically over the past few decades stands testament to this high level of overall uncertainty.” [Trigg Talley, United States of America]	Accepted. We have rewritten paragraph to make clear its importance. Drake is cited. Unfortunately, we have limited new global data as good as the new data from the US. Thanks for additional suggestions, added.
73009	29	3	29	4	Remove split of numbers and units across line break. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial - this kind of issues will be taken care of during the production phase, which includes professional copy-editing.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
29159	29	4	29	7	I propose to reconsider statements regarding the uncertainty of the land-use/land-management related C flow into the atmosphere in the light of my above comments on p21, line 3 as well as p 25 l46ff (comments 5 and 6) [Helmut Haberl, Austria]	Accepted. Rewritten to consider concern.
19307	29	14	29	46	Caption for Fig 5.12 is too long [Benjamin Lamptey, United Kingdom (of Great Britain and Northern Ireland)]	Noted - Figure was changed
9393	29	35	29	36	Perhaps a word missing: "...the values of the more uncertain gross fluxes have been adjusted so that their difference matches the and Net ocean flux estimate." [Christine Weldrick, Australia]	Noted - Figure was changed
103127	29	36	29	36	matches the and Net ocean flux estimate.' is missing a word. [Philippe Tulkens, Belgium]	Noted - Figure was changed
73011	29	38	29	38	Insert . After) [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Noted - Figure was changed
9395	29	40	29	42	Sentence starting from "Permafrost region stores..." needs a period at the end. [Christine Weldrick, Australia]	Noted - Figure was changed
73013	29	46	29	46	Move (to before 2013 and delete , after al. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Noted - Figure was changed
74219	29	51	30	2	the minus signs before the numbers for the ocean and terrestrial sink in the 2000-2009 column are inconsistent with the rest of the table [Christoph Völker, Germany]	Accepted. Made consistent.
88501	29	53	30	1	The superscript "c" for ocean sink is not explained in the table caption [Damien Cardinal, France]	Accepted. Explained.
88499	29	54	29	55	Legend of Table 5.1 is incorrect since negative sign is written only for 2000-2009 decade [Damien Cardinal, France]	Accepted. Corrected.
18145	29	54	30	1	In the Table description on page 29 it states 'averaged over the 1980s, 1990s, 2000s, as well as the recent decade from 2008' but in the Table on page 30 the decade is between 2009-2018. Amend either caption or table. [Chelsey Baker, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Corrected.
90079	29	54			This statement uploaded by Ted Schuur on behalf of a graduate class that reviewed the chapter (this was communicated with IPCC TSU in advance): Table caption for Table 5.1 indicates calculations made for the recent decade from 2008, but table lists dates of 2009-2018. Need to change language ("after 2008") or change date. [Edward Schuur, United States of America]	Accepted. Corrected.
87687	29	55	29	55	Table 5.1: "By convention, a negative ocean or land to atmosphere CO2 flux is equivalent to a gain of carbon by these reservoirs." The ocean/land numbers in the table are missing some minus signs. [Ivy Frenger, Germany]	Accepted. Corrected.
3929	30	0	30	0	Table 5.1 Ocean sink: -2.1 => 2.1, Terrestrial sink: -2.7 => 2.7 [Makio Honda, Japan]	Accepted. Corrected.
58743	30	0	30	0	Comment is for Table 5.1: The following numbers do not match the numbers in Friedlingstein et al., 2019: 1750-2018 Net Land Use Change emissions; 2000-2009 Net Land Use Change emissions; 2000-2009 Ocean sink; 2000-2009 Terrestrial sink [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Corrected.
108825	30	1	30	1	These numbers are inconsistent with figure 5.3. Also check SPM pg 8, line 35-37. The numbers in TS. 2.6 'the carbon cycle' aren't consistent with section 5.2.2 or figure 5.3. [Jason Donev, Canada]	Accepted. Corrected.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
69187	30	1	30	1	The numbers of Ocean sink and Terrestrial sink for the period 2000-2009 are presented as minus. In line with the context of this table, those number must be presented as plus. [Kaoru Magosaki, Japan]	Accepted. Corrected.
18147	30	1	30	1	In the Table 'Ocean sink' has a superscript c by it but not indication as to what that refers to in the caption. Please provide the footnote for the reader. [Chelsey Baker, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Corrected.
93433	30	1	30	1	Ocean and Terrestrial sinks for the 2000-2009 period have a minus sign that needs to be removed [Carles Pelejero, Spain]	Accepted. Corrected.
8749	30	5	30	5	I think this section needs to be better organized, especially summarizing what was known about methane budget, trends and variablility in AR5, progress in SRCCL and highlighting new knowledge since and our confidence level in the advances. These changes would also help to condense the section. [Vaishali Naik, United States of America]	Taken into account. This is an introductory session to state some basic facts on CH4. We do have citations to AR5 and SRCCL at places.
66177	30	5	30	32	This discussion of CH4 is incomplete (lacking discussion of chemical feedbacks and adjusted lifetime) and is inconsistent with the later chapters and the CH4 metrics (6.2.2.4, 7.6.2.5). This chapter should drop most of this paragraph, adjust the CH4 budget tables in Chapter 5 to be consistent with the chemistry in Chapter 6, and point forward. The parallel N2O section, by contrast, is consistent through chapters 5,6,7. [Michael PRATHER, United States of America]	Taken into account - the recommendation is drastic. Some attempts are made to improve the discussion. We cannot make everything consistent with Chapter 6 in this assessment cycle mainly because the chemistry of CH4 are modelled differently in Chapter 5 and Chapter 6.
51137	30	5	30	38	Section 5.2.2 gives atmospheric CH4 burden and gain/loss rates in Tg / Tg yr-1; Section 5.2.2.1 and following uses ppb. Can the figures on line 11 be converted to ppb for comparability across the chapter (the burden should be possible)? [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. Unfortunately, the emission/loss cannot be converted to ppb units, but the burden can be by a factor of ~2.76 Tg/ppb. We think it is good to give the burden here once in Tg units for the readers to get a feeling how much is in atmosphere compared to the emissions every year.
40687	30	5	37	30	section 5.2.2 : the numbers displayed in the text, the figure and the table don't seem to always correspond, which I find confusing [TSU WGI, France]	Accepted - all numbers are checked and revised
40689	30	5	37	30	section 5.2.2 : there several are places in the section where the period during which the emission/sinks refer to is not clearly defined (e.g. p34, L24-26) [TSU WGI, France]	Not applicable - may of the natural emissions of CH4 are based on data gathered over a period of time, and are believed to occur every year in the similar rate. Thus no time period can be assigned.
40691	30	5	37	30	section 5.2.2: very little is said about the conclusion of the previous reports and, more importantly, what has improved since then. [TSU WGI, France]	Accepted - changes are made to spell out improvements since the AR5
40693	30	5	37	30	section 5.2.2: many subsections still need to be turned into an assessment [TSU WGI, France]	Accepted
40695	30	5	37	30	section 5.2.2: Please check the use of this IPCC uncertainty language term, I suspect some misuse. The IPCC guidance note on uncertainty: https://wg1.ipcc.ch/SR/documents/ar5_uncertainty-guidance-note.pdf [TSU WGI, France]	Accepted

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
58639	30	5			It could be good to quickly remind the that CH4 is a much powerful GHG than CO2 and that despite it's short life time, it's most of the time transformed into CO2 during its atmospheric degradation. Otherwise, the really small concentrations, emission rates and life time compared to those of CO2 mentioned just before don't appear as a big deal. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - we have added one sentence by highlighting its GHG and tropospheric air-chemistry roles
71667	30	5			As section 5.2.2.1 “Atmosphere” only covers variability in the growth rate, there is no subsection summarising the largest single term in any CH4 budget, i.e. removal by atmospheric OH. To balance coverage of soil removal it would make sense to either extend section 5.2.2.1 or add another subsection that summarised what is known about atmospheric removal of CH4 via OH and Cl. This could help deal with the points that I am raising for 5-30:10-29. [Martin Manning, New Zealand]	Not applicable - we have discussed the OH loss term and its variability in details in the Box 5.1, which is mentioned at the end of this paragraph
21805	30	7	30	8	Suggest being explicit here that the predominant loss is via tropospheric oxidation which leads to carbon dioxide accumulations. This needs to be clear from the outset and also justifies its consideration here instead of in chapter 6. [Peter Thorne, Ireland]	Accepted - a sentence is added
31869	30	7	30	8	net surface emissions - add the word 'net' because there is also soil uptake. [Euan G. Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Accepted
41619	30	7	30	8	I am wondering if "emission" in this sentence involves both emission and uptake? If so, then this sentence should apply to CO2 and N2O, too. If it does not involve "uptake", then the uptake of CH4 by soils is neglected here. Though I understand that it is being mentioned in Table 5.2 and page 34 ll 1-6 [Katharina Meurer, Sweden]	Accepted - changes are made to indicate that we are talking about "net emissions" here, i.e., the soil sinks included
9397	30	7	30	8	Suggested change to sentence: "The CH4 variability in the atmosphere is mainly the result of a net balance between the Earth's surface emissions and chemical losses in the atmosphere." [Christine Weldrick, Australia]	Accepted - sentence revised following your suggestion and our own reading
19961	30	7	30	8	These statements are certainly correct, but is it justified to restrict them to variability? What about concentrations themselves? [philippe waldteufel, France]	Taken into account. CH4 in atmosphere is concentration
12311	30	7	30	9	The discussion of "CH4 variability" is rather unclear and unprecise: It is not clear, if the term "CH4 variability" refers here to the variability of global average CH4 dry-air mole fractions or to the variability observed at a given location (e.g. monitoring station). Even less clear is what exactly is meant by "redistributes the CH4 variability signal to different parts of the Earth’s atmosphere". [Peter Bergamaschi, Italy]	Accepted - this sentence is revised

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
72209	30	7	30	32	The discussion in this section [5.2.2], including Cross-Chapter Box 5.1, omits several study reports that indicate a much greater contribution from anthropogenic sources to the CH4 budget than indicated in the discussion here. In particular the discussion fails to fully assess the contributions from fossil fuels sources, and in particular the contributions from the production and distribution of natural gas, particularly in North America. See these references (all omitted from the discussion): Hmiel et al 2020; Zhang et al 2020; Negron et al 2002; Plant et al. 2019; Höglund-Isaksson et al. 2020; Lan et al 2019; Sheng et al 2018; and Howarth 2019. Full references for these study reports are give in comments #19 - #26 below. In addition, the discussion omits several studies on ethane emissions that further identify a massive increase in North American methane emissions from U.S. oil and gas. See Helmig 2016; Dalsøren et al, 2018; and Hakola & Hellén 2016. References provided in comments #30-32 below. Taken all togther this missing literature provides strong evidence that North American oil and gas productions makes a much greater contribution to the CH4 budget and to the rise in CH4 levels starting in 2007 than the discussion here provides. [Hunter Cutting, United States of America]	Accepted. Some of the discussions are now included, in particular those for the higher fraction of fossil fuel emissions in the Budget subsection. Most of the references are also included in the citation.
72211	30	7	37	26	Discussion omits Hmiel et al 2020 who report that methane emissions from global fossil fuel production over most of the 20th century and the first decade of the 21st century (not including the recent surge) are under-estimated by prior studies by as much as 40%, while natural geologic sources (e.g. natural seeps) contribute correspondingly less. [this isotope analysis does not distinguish between coal-mine seeps vs. oil/gas production emissions]. As these findings indicate that natural sources have a much smaller role in the carbon budget than understood previously, anthropogenic sources are more likely to have played the major role in the recent renewed growth rate of CH4 as changes in natural sources would have to have been much greater than understood prior to play a major role in the surge since their historic emissions were much lower to begin with. These findings also indicate that addressing methane emissions from fossil-fuel production and distribution would have an greater impact on addressing global methane levels than understood prior. See: Hmiel, B., Petrenko, V.V., Dyonisius, M.N. et al. Preindustrial 14CH4 indicates greater anthropogenic fossil CH4 emissions. Nature 578, 409–412 (2020). https://doi.org/10.1038/s41586-020-1991-8 [Hunter Cutting, United States of America]	Accepted - we have discussed this issue in the Budget section 5.2.2.5. It is bit of puzzle for us. The freshwater emissions has to go up, and fossil fuel emissions go up, but without increasing the sinks we cannot accommodate more emissions in the budget. We hope these issues will be addressed by the next AR cycle

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
72213	30	7	37	26	Discussion omits Zhang et al. 2020 who estimate 3.7% leakage/venting rate in Permian basin in the United States, more than twice the prior inventory estimate for the region which accounts for roughly 15% of U.S. natural gas production. This estimate is also 60% higher than the 2.3% leakage rate estimated back in by Alvarez et al 2018.The Permian Basin's methane emissions account for about 10% of the total global increase in methane emissions from 2010 to 2020. Zhang et al. state that the shale gas and oil fields of the Permian are emitting 2.7 Tg per year of methane. The increase in global methane emissions since 2005 is ~24.7 Tg per year (Worden et al. 2017 in Reference). Since shale gas and oil were not developed in the Permian Basin (or virtually anywhere else in the world in 2005), the 2.7 Tg per year is all new, and makes up 10.9% of this global increase of 24.7 Tg per year. See: Quantifying methane emissions from the largest oil-producing basin in the United States from space. Yuzhong Zhang, Ritesh Gautam, Sudhanshu Pandey, Mark Omara, Joannes D. Maasakkers, Pankaj Sadavarte, David Lyon, Hannah Nesser, Melissa P. Sulprizio, Daniel J. Varon, Ruixiong Zhang, Sander Houweling, Daniel Zavala-Araiza, Ramon A. Alvarez, Alba Lorente, Steven P. Hamburg, Ilse Aben, Daniel J. Jacob. Science Advances. 22 Apr 2020 : eaaz5120. DOI: 10.1126/sciadv.aaz5120 [Hunter Cutting, United States of America]	Not applicable - this is probably a strong reply, but please note that it is difficult discuss point sources and such small scale emission results in this budget assessment. The emission inventories account for some these emission activities with updates, such as those in EDGAR (Crippa et al., 2020) and GAINS (Lena Höglund-Isaksson et al., 2020). The uncertainty in emissions due to various estimation methods for the USA region is much greater than these local/regional emissions
72215	30	7	37	26	Discussion omits Negron et al. 2020 who report that, for the full U.S. Gulf of Mexico, oil and gas facilities emit approximately one-half a teragram of methane each year, comparable with large emitting oil and gas basins like the Four Corners region in the southwest U.S. The effective loss rate of produced gas is roughly 2.9% (note: this is just for basin production and does include all of the supply chain losses included in the scope of Alvarez et al 2018. This leakage rate similar to large onshore basins primarily focused on oil, and twice as high as current inventory estimates. Note that Alvarez et al 2018 did not revise the estimate for off-shore production. See: Airborne Assessment of Methane Emissions from Offshore Platforms in the U.S. Gulf of Mexico. Alan M. Gorchov Negron, Eric A. Kort, Stephen A. Conley, and Mackenzie L. Smith. Environmental Science & Technology 2020 54 (8), 5112-5120. DOI: 10.1021/acs.est.0c00179 [Hunter Cutting, United States of America]	Not applicable - this is probably a strong reply, but please note that it is difficult discuss point sources and such small scale emission results in this budget assessment. The emission inventories account for some these emission activities with updates, such as those in EDGAR (Crippa et al., 2020) and GAINS (Lena Höglund-Isaksson et al., 2020). The uncertainty in emissions due to various estimation methods for the USA region is much greater than these local/regional emissions

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
72217	30	7	37	26	Discussion omits Plant et al. 2019 who report examining six old and leak-prone major cities along the East Coast of the United States, representing nearly 12% of the U.S. population and 4 of the 10 most populous cities, focusing on older, leak-prone urban centers. Finds methane emissions are more than twice EPA estimates. Estimates 0.75 Tg CH4/year is attributed to natural gas (equivalent to about 7.5% of direct leaks from production of natural gas nationally, well over triple the amount emitted by gas production in the Bakken shale formation in the U.S. Midwest.). These results highlight that current urban inventory estimates of natural gas emissions are substantially low, either due to underestimates of leakage, lack of inclusion of end-use emissions, or some combination thereof. NB: 0.75 Tg CH4/year compares to the 0.44 Tg CH4/year assumed by Alvarez et al 2018 for local distribution leaks across the entire nation, an assumption taken directly from the EPA GHGI estimate. See: Plant, G., Kort, E. A., Floerchinger, C., Gvakharia, A., Vimont, I., & Sweeney, C. (2019). Large fugitive methane emissions from urban centers along the U.S. East Coast. Geophysical Research Letters, 46, 8500– 8507. https://doi.org/10.1029/2019GL082635 [Hunter Cutting, United States of America]	Not applicable - this is probably a strong reply, but please note that it is difficult discuss point sources and such small scale emission results in this budget assessment. The emission inventories account for some these emission activities with updates, such as those in EDGAR (Crippa et al., 2020) and GAINS (Lena Höglund-Isaksson et al., 2020). The uncertainty in emissions due to various estimation methods for the USA region is much greater than these local/regional emissions
72219	30	7	37	26	Discussion omits Höglund-Isaksson et al. 2020 who conduct a bottom-up emissions inventory and report that "rapid growth in extraction of unconventional gas in North America" is one of the three primary drivers of the surge in atmospheric methane from 2007 to 2015 (end of study period). See: Technical potentials and costs for reducing global anthropogenic methane emissions in the 2050 timeframe –results from the GAINS model. Lena Höglund-Isaksson, Adriana Gómez-Sanabria, Zbigniew Klimont, Peter Rafaj, and Wolfgang Schöpp. Published 27 February 2020, Environmental Research Communications, Volume 2, Number 2 [Hunter Cutting, United States of America]	Accepted - this study on GAINS, along with the latest EDGAR, emission inventories are now used in the discussion.
72221	30	7	37	26	Discussion omits Lan et al 2019 who report that methane emission from U.S. oil and gas production increased roughly 40% from 2006 to 2015 (end of study period). See: Long-Term Measurements Show Little Evidence for Large Increases in Total U.S. Methane Emissions Over the Past Decade. Xin Lan, Pieter Tans, Colm Sweeney, Arlyn Andrews, Edward Dlugokencky, Stefan Schwietzke, Jonathan Kofler Kathryn McKain, Kirk Thoning, Molly Crotwell, Stephen Montzka. Geophysical Research Letters. First published: 25 April 2019. https://doi.org/10.1029/2018GL081731 [Hunter Cutting, United States of America]	Accepted - Lan et al. results are discussed in Box 5.1
72223	30	7	37	26	Discussion omits Sheng et al 2018 who report that "US emission trends...account for about 20 % of the observed increase in global methane over the 2010–2016 period," and proportions that contribution roughly evenly between the U.S. national oil/gas system and Midwestern livestock ("possibly swine manure management"). See: Sheng et al 2018. 2010–2016 methane trends over Canada, the United States, and Mexico observed by the GOSAT satellite: contributions from different source sectors Atmos. Chem. Phys., 18, 12257–12267, 2018. https://doi.org/10.5194/acp-18-12257-2018 [Hunter Cutting, United States of America]	Rejected - very difficult for us to reconcile/discuss these emissions in the limited space we have here. Once all these information are implemented in the emission inventories, and checked using global modelling systems a better assessment would be possible

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
72225	30	7	37	26	Discussion omits Howarth, 2019 who reports that This perspectives article analyzes istope data and the rapid rise in atmospheric methane over the past decade and argues that "that shale-gas production in North America over the past decade may have contributed more than half of all of the increased emissions from fossil fuels globally and approximately one-third of the total increased emissions from all sources globally over the past decade." This estimate stands at the high-end of the range of estimates, though it is consistent with at least one other estimate, Höglund-Isaksson et al. 2020 (Reference provided in comment #22). See: Howarth 2019: Ideas and perspectives: is shale gas a major driver of recent increase in global atmospheric methane? Biogeosciences, 16, 3033–3046, 2019. https://doi.org/10.5194/bg-16-3033-2019 [Hunter Cutting, United States of America]	Rejected - we need these kind of analysis and metadata through an emission inventory system, such as that of the Höglund-Isaksson et al. 2020
72231	30	7	37	26	Discussion omits several studies on ethane emissions that identify North American oil and gas production as a significant contributor to rising methane emissions. See Helmig 2016; Dalsøren et al, 2018; and Hakola & Hellén 2016. References provided in comments #30-32 below [Hunter Cutting, United States of America]	Rejected - unfortunately, due to the lack of space we are not going in to the debate on the use of ethane as a tracer for CH4 emission. The text relating to ethane is cut-short to accommodate the Box within 2 pages
72233	30	7	37	26	Discussion omits findings of Helmig et al 2016, who report that "Using data from a global surface network and atmospheric column observations we show that the steady decline in the ethane mole fraction that began in the 1970s1,2,3 halted between 2005 and 2010 in most of the Northern Hemisphere and has since reversed. We calculate a yearly increase in ethane emissions in the Northern Hemisphere of 0.42 (±0.19) Tg yr−1 between mid-2009 and mid-2014. The largest increases in ethane and the shorter-lived propane are seen over the central and eastern USA, with a spatial distribution that suggests North American oil and natural gas development as the primary source of increasing emissions. By including other co-emitted oil and natural gas non-methane hydrocarbons, we estimate a Northern Hemisphere total non-methane hydrocarbon yearly emission increase of 1.2 (±0.8) Tg yr−1. Atmospheric chemical transport modelling suggests that these emissions could augment summertime mean surface ozone by several nanomoles per mole near oil and natural gas production regions. Methane/ethane oil and natural gas emission ratios could suggest a significant increase in associated methane emissions..." See Helmig et al, 2016: Reversal of global atmospheric ethane and propane trends largely due to US oil and natural gas production. Nature Geoscience volume 9, pages490–495. (13 June 2016) [Hunter Cutting, United States of America]	Rejected - unfortunately, due to the lack of space we are not going in to the debate on the use of ethane as a tracer for CH4 emission. The text relating to ethane is cut-short to accommodate the Box within 2 pages

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
72235	30	7	37	26	The discussion omits the findings of Dalsøren et al, 2018, who report that "we show that observations of pre-industrial and present-day ethane and propane can be reproduced in simulations with a detailed atmospheric chemistry transport model, provided that natural geologic emissions are taken into account and anthropogenic fossil fuel emissions are assumed to be two to three times higher than is indicated in current inventories. Accounting for these enhanced ethane and propane emissions results in simulated surface ozone concentrations that are 5–13% higher than previously assumed in some polluted regions in Asia. The improved correspondence with observed ethane and propane in model simulations with greater emissions suggests that the level of fossil (geologic + fossil fuel) methane emissions in current inventories may need re-evaluation." See Dalsøren et al, 2018: Discrepancy between simulated and observed ethane and propane levels explained by underestimated fossil emissions. Nature Geoscience. 11, pages178–184 (26 Feb 2018) [Hunter Cutting, United States of America]	Rejected - unfortunately, due to the lack of space we are not going in to the debate on the use of ethane as a tracer for CH4 emission. The text relating to ethane is cut-short to accommodate the Box within 2 pages
72237	30	7	37	26	The discussion omits the analysis of Hannele Hakola & Heidi Hellén 2016 who report that "Combined with measurements of ethane that are representative of the free troposphere, and a large oil and gas field, the work by Helmig et al.2016 shows that ethane and propane concentrations in the Northern Hemisphere have taken an upward turn, largely as a by-product of the massive increase in oil and gas exploration in North America." Hakola & Heidi Hellén further report that "Time-series measurements of methane and ethane were significantly correlated during 2007–2014 (ref. 3), a period when global atmospheric methane concentrations began increasing again after stalling between 1999 and 2007. However, there was no correlation between ethane and methane concentrations in measurements earlier than 2007, or in measurements from the Southern Hemisphere, suggesting that oil and gas production have become more important sources of methane in the Northern Hemisphere since 2007. Based on the ethane-to-methane ratio of emissions and assuming that the increase in ethane emissions is entirely from oil and natural gas sources, these sources contributed at least 39% to the renewed methane increase." Hakola & Heidi Hellén further report that the Bakken shale formation alone "....is responsible for emissions on the order of 0.23 ± 0.07 terragrams of ethane per year, equivalent to 1–3% of all ethane emissions globally." See: Hannele Hakola & Heidi Hellén 2016: The returne of Ethane. Nature Geoscience volume 9, pages475–476 (13 June 2016) [Hunter Cutting, United States of America]	Rejected - unfortunately, due to the lack of space we are not going in to the debate on the use of ethane as a tracer for CH4 emission. The text relating to ethane is cut-short to accommodate the Box within 2 pages
31871	30	8	30	8	Atmospheric transport sentence is a bit confusing as remember the wind also takes the CH4 to the tropical mid-troposphere 'death zone' for methane. So transport has an active role in the variability as well as just being a passive methane mover. Maybe the simplest thing to do is delete the word 'only' from this sentence. [Euan G. Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - sentence revised and 'only' removed
58697	30	8	30	9	I do not understand what you mean by this sentence. Do you want to say that atmospheric transport evens out regional differences? [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - sentence revised following your comment
9399	30	9	30	9	Replace "between" with "from" [Christine Weldrick, Australia]	Accepted - change was made.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
12313	30	9	30	10	It is not clear, why here the "time scales relevant to CH4 process studies" are discussed. I assume that this refers to the discussion of the "CH4 variability" in the previous 2 sentences, but it is not clear which process studies exactly are meant here. And why are the time scales "for the budget estimations ... predominantly monthly to annual"? This report discusses also e.g. decadal budgets (e.g. Table 5.2). Also the reference to "Figure 6.1" is not clear. Do you really mean Figure 6.1 (from chapter 6) with the Column-averaged CH4 concentrations (XCH4) based on satellite retrievals? Furthermore, the grammar of the sentence seems not correct. [Peter Bergamaschi, Italy]	Taken into account - by this reply. Because this section is about CH4 trends and variability, and we wanted to make it clear here that CH4 has a lot of time and spatial scales of variability but we are focused here only at the monthly (rarely) and annual mean time scale because the focus is on budget. The sentence is revised for better clarity
21807	30	9	30	10	Sentence makes no logical sense as written but I can't work out what was intended to make a sensible suggestion. [Peter Thorne, Ireland]	Accepted - this sentence is revised
16505	30	9	30	10	The sentence on the methane timescales is not very clear. What point is being made here? [William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - by this reply. Because the section is about CH4 trends and variability, and we wanted to make it clear here that CH4 has a lot of time and spatial scales of variability but we are focused here only at the monthly (rarely) and annual mean time scale because the focus is on budget. The sentence is revised for better clarity
74221	30	9	30	10	the sentence has incorrect grammar [Christoph Völker, Germany]	Accepted - sentence revised
31873	30	10	30	10	hourly to seasonal? [Euan G. Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - sentence revised
58785	30	10	30	10	Suggest rewording for clarity: "The time scales relevant for CH4 process studies range between hourly to monthly, and for budget estimates between monthly to annual." [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - sentence revised following your suggestion
71669	30	10	30	29	This summary of the CH4 budget is heavily based on the Saunois et al 2019 paper, which is an excellent review of the methane budget, but there are several aspects that should be revised in this section. First, on Cl removal: the text on lines 15-17 says that Cl removal occurs in the stratosphere, whereas all relevant papers since 2001 have shown that it's effect on the CH4 budget is due to removal in the troposphere, and Table 5.2 does refer to tropospheric Cl removal. This relates to the text that cites the earlier Saunois et al 2016 paper for Cl removal which was based primarily on Allan et al (2007), as was the WG1-AR5. In contrast, while Saunois et al 2019 still mentions Allan et al it brings in significant revisions from Hossaini et al (2016: JGR) and Wang et al (2019, Atm Chem Phys). While Cl removal is small relative to OH, its much higher fractionation effect makes it very important for determining top down estimates of source δ13C. E.g. Nisbet et al 2016 noted that the Allan et al Cl removal led to anomalously low source δ13C in the southern hemisphere, whereas Nisbet et al 2019 resolved that by moving to the very different latitudinal distribution given by Hossaini et al which made the source δ13C much more in line with bottom-up budget analyses. Key points here are 1) that Cl removal in the troposphere has to be dealt with for top down analyses of δ13C/CH4/ to be useful, and 2) that differences between Hossaini et al and Wang et al estimates of Cl removal are non-trivial and should be resolved. [Martin Manning, New Zealand]	Accepted. We have discussed the issue of tropospheric Cl loss

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71671	30	10	30	29	A second concern with this summary of the CH4 budget is that it follows the Saunois et al 2019 assessment of the amount due to fossil fuel production and use. A long-standing issue has been some reluctance to consider the role of 14CH4 in determining the fossil fuel derived fraction. Evidence for 30% of atmospheric CH4 coming from fossil fuels goes back to Lowe et al (1988: Nature, 332, 522-525) with continuing updates (such as Lassey, et al, 2007: Atm Chem Phys, 7, 2119-2139). A recent revision to the fossil fuel 13C/12C isotopic ratios (Schwietzke et al, 2016, Nature, 538, 88-91.) is being cited here but the budget shown in Table 5.2 is not consistent with that. More recently, a completely independent analysis of firn air 14CH4 (Hmiel et al, 2020, Nature, 578, 409-412) has found that the fossil fuel source fraction for CH4 is almost exactly what was seen in 1988 as 30%. In contrast to this, the budget summary in Table 5.2 has top-down estimates for the fossil fuel fraction going from 17% to 19% over the four periods and with smaller estimates from the bottom-up analyses. While the upper end of the uncertainty ranges increase a lot in the last two decades, the overall summary in this table is not consistent with multiple lines of evidence for a larger fossil fuel contribution to CH4 sources. The text needs to note that there are conflicting estimates for the fossil fuel fraction of atmospheric CH4 sources and this does need to be resolved. [Martin Manning, New Zealand]	Accepted - we have now discussed the fossil fuel emission fraction in the Budget section 5.2.2.5. Thank you
31875	30	11	30	11	Is it valid or useful to cite an average value for a climbing number? Maybe better to cite two values - the burden at the start of the recent climb in 2007 and the burden in 2019? [Euan G. Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable - here we would like to give the typical values. We have chosen one of the most recent decade in this case. Yes, because the period coincide with the recent climb the 1-sigma STDEVs are larger, but that does not stop us from sending the main message. We have more details on the different time periods in the Box 5.1 Fig. 1
73015	30	11	30	12	Remove split of numbers and units across line break. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial - this kind of issues will be taken care of during the production phase, which includes professional copy-editing.
41621	30	11	30	12	what is the difference between emission and loss? [Katharina Meurer, Sweden]	Not applicable - this different remains in the atmosphere. We hope this clarify the doubt.
71673	30	11			Is this the atmospheric or tropospheric inventory? In either case the data show it is changing by 3 – 4% per decade so what is the period that these numbers apply to. [Martin Manning, New Zealand]	Not applicable - the text already states "atmospheric burden" not tropospheric
16507	30	12	30	12	This states that the uncertainty is entirely based on interannual variability. How large are the measurement uncertainties? Or systematic uncertainties in knowledge of the processes? [William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The details are given in Table 5.2. The burden calculation is accurate and should not vary from model-model if the models follow the atmospheric observations.
74223	30	13	30	13	“with A median value” [Christoph Völker, Germany]	Accepted - change was made.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
12315	30	13	30	14	I assume that the statement "The budgets are consistent..." refers to the comparison of the lifetime calculated as burden / loss (i.e. 5001 Tg / 543 Tg/yr = 9,2 yrs) with the steady-state lifetime? Would be useful to explain better the different concepts to estimate the lifetime. Furthermore, in [Saunois et al., 2019] a steady-state lifetime of 9.3 years is reported, not 9.6 years. [Peter Bergamaschi, Italy]	Taken into account - the CH4 lifetime issue has now been discussed in Chapter 6, in consultation with us, where more explanation on lifetime calculation can be found. Now we have now given the 9.3 years here from Saunois et al. (2020) as pointed out by you.
77275	30	13	30	14	The atmospheric lifetime of methane is stated as 9.6 years. This is shorter than the value provided in Chapter 7 table 7.A.3. Can this be explained? [Emer Griffin, Ireland]	Taken into account - CH4 lifetime is now discussed in great details in Chapter 6. We have cited revised value now
8751	30	14	30	14	Saunois et al have since the submission of SOD updated the value of steady state chemical lifetime to 9.3 yr so this needs to be updated. Chapter 6 uses the AR5 value of total atmospheric methane lifetime (9.1 yr) in the chapter 6. These numbers are within 2% of each other which I can live with. [Vaishali Naik, United States of America]	Accepted - change was made to reflect CH4 lifetime as 9.3 years
16509	30	14	30	14	Note that chapter 6 assess the lifetime to be 9.1+/-0.9 years. This needs to be resolved by chapters 5 and 6. If they use different methodologies, which is the more appropriate - or should an average be used? At any rate there can't be two methane lifetimes in the assessment. The central value with uncertainty needs to be shown rather than saying "about 9.6". [William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - this sentence is revised by giving the Chapter 6 assessed value
5321	30	14	30	14	90% of CH4 ‘are lost’, should be ‘is lost’ [Sheel Bansal, United States of America]	Accepted - change was made.
12317	30	14	30	17	The small CH4 sink by chlorine radicals in the troposphere (mainly marine boundary layer) should also be mentioned here (see e.g. detailed discussion in Saunois et al., 2019: "3.3.3 Tropospheric reaction with Cl" and references therein). [Peter Bergamaschi, Italy]	Accepted. A new sentence is added for the tropospheric Cl loss of CH4
19009	30	14	30	17	another potential paper to cite which is about stratosphere-troposphere CH4 oxidation with Cl and OH radicals: Li, M., Karu, E., Brenninkmeijer, C. et al. Tropospheric OH and stratospheric OH and Cl concentrations determined from CH4, CH3Cl, and SF6 measurements. npj Clim Atmos Sci 1, 29 (2018). https://doi.org/10.1038/s41612-018-0041-9 [Mengze Li, Germany]	Not applicable - this study does not estimate CH4 loss budget to be compared with the values here
31877	30	14	30	17	This sentence needs a bit of rewriting, both for English and for science. In line 15, it forgets the tropospheric Cl sink, which is especially significant because of the powerful isotopic leverage it exerts. Maybe cite Hossaini, Ryan, et al. "A global model of tropospheric chlorine chemistry: Organic versus inorganic sources and impact on methane oxidation." Journal of Geophysical Research: Atmospheres 121.23 (2016): 14-271. [Euan G. Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - the sentence is rewritten and a sentence is added for the tropospheric Cl loss
127727	30	15	30	15	Bacterial oxidation also dramatically reduces the amount of methane that ends up getting emitted to the atmosphere. The 6% cited here is thought to occur after the methane enters the atmosphere (e.g., at the air/soil interface)? [Trigg Talley, United States of America]	Taken into account - yes. Here we are discussion Loss of atmospheric CH4. The sentence is now revised for better clarity on this
83003	30	17	30	17	The reference needs to be Saunois et al 2016 a or b. [Dan Zwartz, New Zealand]	Accepted - reference updated to Saunois et al. 2020
5323	30	17	30	17	‘, atomic chlorine’ should be ‘, and atomic chlorine’ [Sheel Bansal, United States of America]	Accepted - change was made.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
58661	30	17			Ref problem : "Saunois et al., 2016" --should be changed to--> "Saunois et al., 2016a" [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - reference updated to Saunois et al. 2020
9401	30	19	30	19	Add "the" before "natural ecosystems" [Christine Weldrick, Australia]	Accepted
8753	30	20	30	20	Why add another terminology to describe CH4 sources? These are technical terms more appropriate for a more technical audience, in my opinion. [Vaishali Naik, United States of America]	Accepted - this sentence is deleted
83001	30	20	30	21	Amend the term “wetland” here to “freshwater wetlands” and likewise in Table 5.2. The data came from are Saunois et al. (2016) and in section 3.2 of their paper they state that the term “wetlands” “includes peatlands (bogs and fens), mineral wetlands (swamps and marshes), and seasonal or permanent floodplains. It excludes exposed water surfaces without emergent macrophytes, such as lakes, rivers, estuaries, ponds, and dams …. as well as rice agriculture (see Sect. 3.1.4., rice cultivation paragraph).” [Dan Zwartz, New Zealand]	Rejected - thank you for this suggestion. However, we continue to use the term wetlands here, as in Saunois et al., and the underline definition remain the same. This is for the convenience of the community
31879	30	21	30	21	freshwaters' is a perennial category problem and a likely cause of double counting in bottom up estimates. Open freshwaters probably have low emissions. Shallow closed freshwaters are wetlands. Might be wise simply to omit the word. [Euan G. Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - we agree with you on the category problem. Without further clarification on the separating these different source types it would be difficult remove the term freshwater here
5325	30	21	30	21	‘are wetlands, freshwaters and coastal oceans’ should be ‘are inland and coastal wetlands’ [Sheel Bansal, United States of America]	Not applicable - We would like to keep Wetlands as a separate entity. The CH4 emissions from wetlands are modelled widely in this definition (more in Box 5.1, Fig. 2)
83987	30	21	30	23	In order to be coerrent to the Table 5.2 and the magnitudes of emission from each category the statement should use the same class of emission when citing them (Section 5.2.2.5 e Table 5.2) [Marco Tulio Cabral, Brazil]	Accepted - thank you very much
127729	30	21	30	23	Why aren't reservoirs behind dams mentioned as an anthropogenic source of methane here? Also, should this be updated to Saunois et al., 2019? [Trigg Talley, United States of America]	Taken into account - we do not have a very good estimation of the emissions from dams, which is currently included with in the freshwater sector. Yes, the values are update to Saunois et al. 2020
14907	30	21		23	The most recent literature indicates Saunois et al. have severely underestimated fossil fuel emissions, and so the order of this sentence should be reversed, with fossil fuel emissions mentioned first, and other references added. One problem with Saunois et al. is that the assumed rather large natural emissions of fossil methane (greater than 50 Tg per year). Recent papers have shown this to be wrong, and the natural fossil emissions are near zero. As a result, fossil fuel emissions are 50+ Tg per year more than estimated by Saunois et al. See Hmiel et al. 2020 Nature 578: 409-412 and references therein. [Robert Howarth, United States of America]	Accepted - changes are made to reflect on this issue in the Budget subsection
83005	30	22	30	22	The reference needs to be Saunois et al 2016 a or b. [Dan Zwartz, New Zealand]	Accepted - reference updated to Saunois et al. 2020
31881	30	22			fossil fuels - maybe specify especially gas leaks and vents, and emissions from coal mining and use. [Euan G. Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - changed to "fossil-fuel exploitation"

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
12319	30	23	30	25	Global mean concentration of CH4 increased by 157% above pre-industrial levels (as stated in this chapter on page 6, lines 16-17). Why should then CH4 emissions have only doubled during the last 200 years? Most studies estimate that OH has changed only relatively little since pre-industrial times. If OH was constant (as e.g. assumed in the cited [Ferretti et al., 2005] paper), then emissions should follow the increase in concentration. Since here the "past two centuries" are discussed, the references should be updated, as 2 of the 3 cited studies analyzed only shorter periods: [Dalsøren et al. 2016] the period 1970 to 2012, and [Ghosh et al. 2015] the period 1910–2010. [Peter Bergamaschi, Italy]	Taken in to account. This is what we get when we convert the numbers in %, say from Fig. 1 and Fig. 2 of Ferretti et al. or very close to it. The concentrations (Tg, if converted to burden) and emissions (Tg/yr) have different units, which is why the increase of the two between two time period will be somewhat decoupled. We have added the other two papers in the citation because these are two most recent studies who have looked in to the CH4 budget for a longer time periods, and they helps to get more recent budget than that in Ferretti et al.
40673	30	23			reference to section 5.2.2.5 seems to be wrong, please check [TSU WGI, France]	Accepted - we have the infographics in Section 5.2.2.5. We agree citation to Table 5.2 at this moment.
31887	30	25			Doubling of methane - maybe mention the sustained two century trend to more 13C rich methane, from 1800 or earlier to 2007, indicating fossil fuel emissions, and that this trend has reversed in the decade since 2007. [Euan G. Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - we have similar statement while discussing Fig. 5.13. Thus we keep this simple here
9403	30	26	30	26	Change "literatures" to "literature" [Christine Weldrick, Australia]	Accepted - change was made.
31883	30	26			Renewed growth debate - this hasn't really been introduced. The text mentions the debate but doesn't give references and then the text jumps into an SRCCL note (unreferenced). I'd suggest a brief exposition of the growth debate as it has been subject to a lot of attention. Possible references would cover the discovery (Nisbet et al 2014), and the debate (Schaefer 2016, 2019, Turner2019 PNAS, Nisbet 2014,2016, 2019, Worden 2017: note Nisbet et al 2016 has a very extended Supp information discussion including the wetland and cow changes). There is a recent synopsis by Ganesan 2019.. Here is a list - Ganesan, Anita L., et al. "Advancing scientific understanding of the global methane budget in support of the Paris Agreement." Global Biogeochemical Cycles (2019).Nisbet, Euan G., Edward J. Dlugokencky, and Philippe Bousquet. "Methane on the rise—again." Science 343.6170 (2014): 493-495. Nisbet, E. G., et al. "Rising atmospheric methane: 2007–2014 growth and isotopic shift." Global Biogeochemical Cycles 30.9 (2016): 1356-1370. Schaefer, Hinrich, et al. "A 21st-century shift from fossil-fuel to biogenic methane emissions indicated by 13CH4." Science 352.6281 (2016): 80-84. Schaefer, Hinrich. "On the Causes and Consequences of Recent Trends in Atmospheric Methane." Current Climate Change Reports 5.4 (2019): 259-274.Turner, Alexander J., Christian Frankenberg, and Eric A. Kort. "Interpreting contemporary trends in atmospheric methane." Proceedings of the National Academy of Sciences 116.8 (2019): 2805-2813.Worden, John R., et al. "Reduced biomass burning emissions reconcile conflicting estimates of the post-2006 atmospheric methane budget." Nature communications 8.1 (2017): 2227 [Euan G. Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable - just in the next sentence we have referred the readers to the Box 5.1 which address this CH4 growth rate issue. Due to the lack of space we will not repeat the discussion here.
21809	30	27	30	27	Replace wholesome which has connotations around full balanced plates of food with comprehensive which is more objective. [Peter Thorne, Ireland]	Accepted - change was made.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
8755	30	27	30	27	replace wholesome with thorough [Vaishali Naik, United States of America]	Accepted - this word is changed to 'comprehensive'
31885	30	27			typo - 'an' assessment - not 'as' [Euan G. Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change was made.
31889	30	27			wholesome? - lovely but maybe the wrong word? - should this really say comprehensive [Euan G. Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - word revised
12321	30	32	30	32	For clarity I would recommend to replace "models" by "inverse models" (for the top-down estimates) [Peter Bergamaschi, Italy]	Accepted - this sentence is revised
21811	30	37	31	11	This paragraph is very hard to follow, partly because the phraseology is odd. Efforts to clarify the text would be very helpful here. The bottom line implications should be couched in confidence / likelihood language if at all possible. [Peter Thorne, Ireland]	Accepted - several changes are made and reorganised in this paragraph
5327	30	38	30	38	'the increase rate was' should be 'the increase was' [Sheel Bansal, United States of America]	Accepted - sentence revised
8757	30	38	30	41	This sentence could be revided to the following for clarity: This observed rapid CH4 growth followed the green revolution with increased crop-producion and fast rate of industrialisation characterized by rapid increases in CH4 emissions from ruminant animals, rice cultivation, landfills, coal mining, and oil and gas [Vaishali Naik, United States of America]	Accepted - sentence revised
31891	30	38			growth rates - maybe cite the primary source - NOAA CCGG database? https://www.esrl.noaa.gov/gmd/ccgg/trends_ch4/ [Euan G. Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - as this growth cover a period mostly outside that of the NOAA measurements
9405	30	39	30	39	Add "the" before "fast pace of" [Christine Weldrick, Australia]	Accepted - change was made.
65397	30	40	30	40	The sentence "xxx that period experienced rapid increases in CH4 emissions from ruminant animals, xxx" suggest to change to "xxx that period experienced rapid increases in CH4 emissions due to increase of ruminant animals, xxx" or should it be increase per animal, landfill etc? Then you can ignore my comment [Rebecca Danielsson, Sweden]	Accepted. The sentence is revised
8759	30	42	31	4	The time period being assessed in these sentences has been assessed in past IPCC reports. I think assessment from past reports should be acknowledged and new knowledge should be highlighted. [Vaishali Naik, United States of America]	Accepted - we have tried our best to refer AR5 and bring out the improvements in our understanding. Thank you
12327	30	42	31	11	The discussion of the CH4 trends since 1980 could be improved, and presented in a more systematic and comprehensive way (although this is discussed in more detail in Cross-Chapter Box 5.1): E.g. the discussion of the period 1990-2000 is limited here to the impact of Mt. Pinatubo, but should include also the studies that suggested decreasing emissions from oil and natural gas [Simpson et al., 2012; Dlugokencky et al., 2003]. Also I would recommend to discuss the different periods in chronological order - now the impact of Mt. Pinatubo is discussed only after the 2000–2006 period. [Peter Bergamaschi, Italy]	Accepted - changes are made. Thank you for the suggestions
58663	30	42			The reference "Janssens-Maenhout et al., 2017" is missing in the Ref of the chapter. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable - newer reference of Crippa et al., 2020 is used now

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
31893	30	42			This focus on agriculture downplays the enormous emissions from the Siberian gas industry especially, and the US industry also - as evidenced by the sustained isotopic trend to heavy 13C rich methane. I think this sentence needs to be rewritten to give more priority to gas and coal leaks and be consistent with the evidence from the isotopes. In addition to references cited elsewhere, could also cite Reshetnikov, A. I., N. N. Paramonova, and A. A. Shashkov. "An evaluation of historical methane emissions from the Soviet gas industry." Journal of Geophysical Research: Atmospheres 105.D3 (2000): 3517-3529. Dlugokencky, E. J., et al. "A dramatic decrease in the growth rate of atmospheric methane in the northern hemisphere during 1992." Geophysical Research Letters 21.1 (1994): 45-48. [Euan G. Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - more discussion on the CH4 emissions from oil and gas sector is given
51135	30	Table 5.1			Is there a sign error here? Only 2 of 8 comparable sink growth rates (for 2000-09) are negative. The totals balance only if they are positive. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Corrected.
8747	30		30		Table 5.1 - Why is the total cumulative CO2 emission not provided here? Why are the terrestrial and oceanic sinks for 2000-2009 negative? If the negative sign is correct it would imply that the ocean and terrestrial sink declined in which case wouldnt the atmospheric increase be greater than 4 PgC/yr? [Vaishali Naik, United States of America]	Accepted. Corrected.
116423	30		30		Table 5.1 : an asseessment of changes from 1750 to 1850 is done in the box of chapter 1 on pre industrial reference periods, please check. [Valerie Masson-Delmotte, France]	Accepted. .
31895	31	1			I don't think Stern and Kaufmann is a good reference here. The paper is fine in its own terms but it is very US centric and also honest that it is an estimate - there's essentially no original input from the immense Soviet gas industry. The huge change that took place was the shift from Soviet production-based goals (who cared what they leaked) to post-soviet sales based incentives (see Reshetnikov et al JGR ref listed above), so Gazprom invested a great deal of money and effort and very rapidly cut emission as they found the big leaks first [Euan G. Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - a read of the Reshetnikov et al. was interesting but the emission numbers they have in the paper is out of the ranges we estimate today for Russia and cannot be reconciled
9407	31	2	31	2	Add "the" before "CH4" (ie, "The causes of the termporary pause in the CH4…" [Christine Weldrick, Australia]	Accepted - change was made.
12325	31	2	31	4	The cited [Dlugokencky et al., 2003] papers suggests that the levelling off of atmospheric CH4 may imply that the global methane budget has been at steady state during this period (but that emissions may have decreased in the early 1990s, as diagnosed by a decreasing inter-hemispheric difference) [Peter Bergamaschi, Italy]	Taken into account. Now we have more results from the full modelling analysis. The results of inter-hemispheric difference as well as the inversion model results and advanced inventories are more conclusive now
31897	31	2			Maybe cite Dlugokencky et al 1994? Dlugokencky, E. J., et al. "A dramatic decrease in the growth rate of atmospheric methane in the northern hemisphere during 1992." Geophysical Research Letters 21.1 (1994): 45-48. [Euan G. Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. Steele et al. raised the issue already and Chandra et al. have now addressed it using emission inventories and inverse models.
12323	31	3	31	3	not clear, what exactly the term "persistence" refers to [Peter Bergamaschi, Italy]	Accepted - sentence revised
33305	31	4			Change: « CH4 growth » by “Methane growth”. It is better to avoid using abbreviation at the start of a sentence. [Guiomar Rotllant, Spain]	Accepted - change was made.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
8761	31	5	31	7	It is not clear reduction in CH4 growth rate in which period is being addressed here. If the temporary pause in the late 1990s is being addressed here then this sentence is not pertinent as Pinatubo was early 1991. [Vaishali Naik, United States of America]	Rejected. The cited references show that CH4 growth decrease in the 1990s was helped by the emissions reduction in 1991. Otherwise the shape of the reduction would be different.
19011	31	7	31	8	consider to add this information: since 2007, the highest growth rate was observed in 2014 (12.7 ± 0.5 ppb/yr) (Nisbet, E. G., Manning, M. R., Dlugokencky, E. J., Fisher, R. E., Lowry, D., Michel, S. E., et al. (2019). Very strong atmospheric methane growth in the 4 years 2014–2017: Implications for the Paris Agreement. Global Biogeochemical Cycles, 33, 318– 342. https://doi.org/10.1029/2018GB006009) [Mengze Li, Germany]	Accepted - sentence revised. We now cite the Box 5.1, which has the comprehensive information
14911	31	8			The report should also refer to Howarth 2019 Biogeosciences 16: 3033-3046 [Robert Howarth, United States of America]	Taken into account. Howarth 2019 is cited in the report at an appropriate place, please see section 5.2.2.2
31899	31	8			Rise in methane - not really discussed by Dlugokencky et al 2011 as it was too early, apart from a brief mention in their Fig 1 and accompanying text. At that stage it looked like a one-off event mainly in the Arctic. First real notification of a sustained global rise was Nisbet, Dlugokencky and Bousquet paper 2014 in Science. [Euan G. Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. Rigby et al. (2008) and a few other representative citations are included. It will never be a complete list here.
74225	31	10	31	10	“from A reversal” [Christoph Völker, Germany]	Accepted - change was made.
72207	31	10	31	11	This statement is contradicted by Worden et al. 2017 and van der Werf et al. 2017 (in References) who report that a reduction of biomass burning (e.g. wildfires) is responsible for the decrease in the heavy isotope content of atmospheric methane, a shift that masked the increased contribution of fossil sources to atmospheric methane as those sources are weighted towards heavy isotopes. [Hunter Cutting, United States of America]	Rejected. While biomass burning emissions helps to explain some part of the 13C observations, it is clear that the biogenic emissions increase.
14909	31	10		11	This interpretation of the 13C data has been severely challenged by several recent papers. See in particular Worden et al. 2017 Nat. Commun., 8, 2227, doi:10.1038/s41467-017-02246-0; and Howarth 2019 Biogeosciences 16: 3033-3046. A combination of changes in biomass burning over time and a different 13C value for shale gas than for conventional natural gas confound the simple intepretation given here (which is not even referenced!!). Both Worden et al. 2017 and Howarth 2019 concluded from the change in atmospheric 13C-methane that fossil fuel emissions have driven the global increase in methane since 2007. [Robert Howarth, United States of America]	Taken into account - we agree the 13C data may not give perfect answer on sectorial CH4 budgets, given the underdetermined nature of the system. The discussion is revised here
12329	31	11	31	11	I assume that the statement "the opposite of that lasted for prior 200 years" refers to increasing d13CH4 measured in ice cores (and firn) [e.g. Ferretti et al., 2005]. Reference(s) should be provided. In addition the sentence should be rephrased. [Peter Bergamaschi, Italy]	Accepted - sentences and discussions revised
74227	31	11	31	11	“lasted for THE prior 200 years” [Christoph Völker, Germany]	Accepted - change was made.
19963	31	11	31	11	This last sentence needs rewriting [philippe waldteufel, France]	Accepted - the sentence is revised
31901	31	11			Need references to the isotopic shift, in addition to Fig 5.13(c) - suggest Schaefer et al 2016, and Nisbet et al 2016, 2019, and maybe also Schwietzke, Stefan, et al. "Upward revision of global fossil fuel methane emissions based on isotope database." Nature 538.7623 (2016): 88-91. [Euan G. Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - references added

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103129	31	11			This statement implies that, according to isotope data, biogenic methane emissions should increase more strongly than fossil-derived CH4 after 2007. I cannot see this in the data provided in 5.2.2.2. Certainly it is o.k. to have a discrepancy, but one may wish to flag this [Philippe Tulkens, Belgium]	Accepted - sentence revised
73017	31	27	31	27	Enye required over n in Nino. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change was made.
109651	31	32	32	32	Certainty assessments like „high confidence“, „likely“, appear only sparsely throughout this section. The reason for this is not clear to me. If used, it should be used more often throughout the section as has also been done in other sections of this chapter. [Carolyn-Monika Görres, Germany]	Accepted - more assessment statements are given
127731	31	33	31	34	Clarify why this points to anthropogenic emissions. Is there not also geographic variability in biological emissions? Is the meaning just that the difference is due to emissions, and not due to differences in the sink? [Trigg Talley, United States of America]	Taken into account. The natural emissions are at best proportional to the land area for the major source sectors, e.g., wetlands, freshwater, wild animals, termites, fires. This means the more of the natural emissions occurs in the tropics, but still we observe continuous increase CH4 concentration between the tropics to the northern hemisphere high latitudes (Patra et al., JMSJ, 2009). Of course some part of the loss can be explained by the more loss of CH4 in the tropical region as discussed in the paper cited.
18205	31	34	31	34	I don't understand the end of line 34 which states 'and theirs'. It is not clear to the reader what this is referring too. [Chelsey Baker, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - sentence revised
74229	31	34	31	34	unclear “theirs”: maybe “their relation”? [Christoph Völker, Germany]	Accepted - sentence revised
127733	31	34	31	34	Not sure what authors mean by "theirs with". [Trigg Talley, United States of America]	Accepted - sentence revised
109647	31	34	31	35	Is there a word missing in this sentence? I don’t quite understand its meaning. [Carolyn-Monika Görres, Germany]	Accepted - sentence revised
31903	31	34	31	36	and theirs' - ?? Typo? Also some minor English issues with rest of sentence. Note also that there may be longitudinal OH heterogeneity. [Euan G. Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - the sentence is revised. The longitudinal heterogeneity would not lead to meridional gradient because the zonal mixing time scales are only a order of week(s) compared to month(s)-year in meridionally
8763	31	34	31	37	This sentence reads odd and its placement is even more odd. I would imagine that the readers of this report would be interested in knowing how much anthropogenic methane emissions have changed over the past 7-8 years since AR5. It would be pertinent to start with what we know upto AR5 and then get into what happened in the ensuing years [Vaishali Naik, United States of America]	Accepted - we have largely revised this paragraph following your comment

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71675	31	34	31	37	The text should be more consistent with the caption for Fig 5.13 as much of the data that is behind these global mean values comes from ESRL and their ways of dealing with missing data rather than just two measurement sites. [Martin Manning, New Zealand]	Taken into account. We have prepared the figure that way for discussion of the global mean and the NH-SH gradients, which provide basic information on the emissions.
19965	31	34	31	37	This compact sentence needs interpretations by the reader: i) the latitudinal gradient is due to emission rather than loss factors and ii) this favours anthropogenic origins because most of fossil fuel emissions originate in the northern hemisphere. Whether these hypotheses are correct or not, a more explicit formulation is recommended. [philippe waldteufel, France]	Accepted - the sentence is revised following your suggestions
21813	31	34	32	32	Again, I found this section very hard to follow owing to odd phraseology. Careful proofing would help. [Peter Thorne, Ireland]	Accepted - this part has undergone significant revisions
39723	31	34		35	"and theirs with the global mean CH4" what do you mean? This is unclear [TSU WGI, France]	Accepted - sentence revised
3927	31	35	31	35	CH"4" => subscript [Makio Honda, Japan]	Accepted - change was made.
65399	31	35	31	35	check 4 in CH4 [Rebecca Danielsson, Sweden]	Accepted - change was made.
23693	31	35	31	35	CH4 subscript [Massimo Lupascu, Singapore]	Accepted - change was made.
74231	31	35	31	35	CH4 without subscripted 4 [Christoph Völker, Germany]	Accepted - change was made.
127735	31	35	31	35	Where is the Trinidad Head - Cape Grim difference plotted in Figure 5.13. Was it the intention to plot that gradient in panel (a) similar to the MLO-SPO CO2 difference plotting in Figure 5.4a? [Trigg Talley, United States of America]	Taken into account - the difference between Trinidad Head and Cape Grim is very clear for CH4, unlike that is for CO2. Thus for saving space we have not the differences in a separate panel
71677	31	36	31	37	While I totally agree with Patra et al 2014, reliance on just that single reference in a summary of what is known about OH is not enough. More on what is known about the role of atmospheric chemistry in the CH4 budget would help. But some details, such as the evidence for NH – SH parity in the OH distribution, should be left to other parts of this chapter and to chapters 6 and 7 which can then be cited here. Questions about trends in OH also need to be mentioned in any summary of the methane budget and so this section should have a clear link to Box 5.1. [Martin Manning, New Zealand]	Accepted - the sentences are revised to accommodate your comments. We have cited CCMI model result
73019	31	37	31	37	Delete , after 'gas'. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change was made.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
12331	31	37	31	55	The discussion of trends in emissions from fossil fuels should include also some disucssion about potential trends from the oil and gas industry during the last decade. Several studies had suggested that CH4 emissions from oil and gas might have increased significantly (e.g. based on increasing trend in ethane/methane ratios (e.g. Hausmann et al., 2016)) especially due to the large increase of oil and gas production in the US, while other studies concluded that CH4 emissions from the US did not increase significantly (Lan et al., 2019 (and references therein)): Hausmann, P., Sussmann, R., and Smale, D.: Contribution of oil and natural gas production to renewed increase in atmospheric methane (2007–2014): top–down estimate from ethane and methane column observations, Atmos. Chem. Phys., 16, 3227–3244, https://doi.org/10.5194/acp-16-3227-2016, 2016. Lan, X., Tans, P., Sweeney, C., Andrews, A., Dlugokencky, E., Schwietzke, S., et al. (2019). Long-term measurements show little evidence for large increases in total U.S. methane emissions over the past decade. Geophysical Research Letters, 46, 4991–4999. https://doi.org/10.1029/2018GL081731 [Peter Bergamaschi, Italy]	Accepted - this paragraph is revised largely
5015	31	37	32	38	Anthropogenic CH4 emissions are underestimated. Since several years a few studies assumed that CH4 emissions from oil & gas production are way higher than previously thought. As an exemple, this study published in 2020 : https://pubs.acs.org/doi/abs/10.1021/acs.est.0c00863 (Ingraffea et al., in Environmental Science & Technology), shows that CH4 emissions from some wells are unaccounted from official CH4 inventories. It's the case in USA, but also in UK, where there is an unaccounted methane leakage (https://ueaeprints.uea.ac.uk/id/eprint/71905/, Riddick and al., 2019, in Atmospheric Chemistry and Physics). My point is that the definitive draft must adress this issue, and not only leaks from coal production which are known and even if we can measure it, it's still way below amounts of CH4 leaked from oil&gas industry. More informations about this topic are available in Chapter 6, p.15, l.9-16. A brand new study (https://advances.sciencemag.org/content/6/17/eaaz5120, Zhang and al., 2020, in Science Advances) shows how methane emissions from oil and gas production are increasing mainly because of the shale oil production, and how it will add CH4 into the atmosphere despite companies statement that this type of production does not reject a lot of methane. I really think that IPCC's report must remark this latest finding. [Olivier RAGUENES, France]	Taken into account - the oil and gas sector is indeed bigger than those from coal as given in Table 5.2. Also the increase in emissions from the oil and gas sector has increased dramatically in the recent decade compared to those in the 1990s. However, we are not able to place the point/local scale emissions in the global perspective given the uncertainties in their respective estimations.
33307	31	37			Change: « CH4 emissions» by “Methane emissions”. It is better to avoid using abbreviation at the start of a sentence. [Guiomar Rotllant, Spain]	Accepted - change was made.
31905	31	37			I'd put gas ahead of coal in this list, and order the list from biggest to least. [Euan G. Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change was made.
19967	31	38	31	38	Why insist on the part due to coal mining? [philippe waldteufel, France]	Taken into account. Because coal mining has contributed significantly and now we also discuss the oil and gas sector for completeness

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
14913	31	38		52	The material presented here is terribly out of touch with a large and growing body of evidence on methane emissions from the oil and gas industry, with more than 500 papers in the peer-reviewed literature on emissions in North America published since 2011. The vast majority of these show much larger methane emissions that indicated in the draft language here. See for example Alvarez et al. 2018 Science 361:186-188, Caulton et al. 2013 PNAS 111: 6237-6242, Howarth 2014 Energy Science & Engineering 2: 47-60, Plant et al. 2019 Geophysical Research Letters 46: 8500-8507, Miller et al. 2013 PNAS 110: 20018–20022, Vaughn et al. 2018 PNAS 115: 11712-11717,Himiel et al. 2020 Nature 578: 409-4112, and Howarth 2019 Biogeosciences 16: 3033-3046 [Robert Howarth, United States of America]	Rejected. The detailed sectorial emissions will be handled in much more detailed by WG3.
12333	31	39	31	40	Top-down / bottom-up emissions are discussed here for the period 2010-2017, while Table 5.2 reports emissions for 2008-2017 (and previous periods). It would be helpful to use consistent time periods for the discussion and the table (unless there are specific reasons to use different time periods). [Peter Bergamaschi, Italy]	Taken into account. The time periods are now matched
131529	31	39	31	40	For non-experts: briefly explain "top-down"- und "buttom up estimates" [Hans Poertner and WGII TSU, Germany]	Accepted - given in a paragraph just before subsection 5.2.2.1
23695	31	39	31	42	why the numbers do not have SD or SE?? [Massimo Lupascu, Singapore]	Accepted - range of estimations are give
31907	31	39			reference for the coal % number?? It's a very hard number to pin down. [Euan G. Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable - the fraction comes from the inventory estimations.
131531	31	40	31	40	For non-experts: briefly explain "top-down"- und "buttom up methods" [Hans Poertner and WGII TSU, Germany]	Accepted - given in a paragraph just before subsection 5.2.2.1
12335	31	40	31	42	Do you mean really [Saunois et al., 2016] (which discusses the average decadal budget) or [Saunois et al., 2017] (which discusses explicitly the change in emissions 2008–2012 compared to 2002–2006)? The latter concludes: "the top-down ensemble mean produces an emission shift between 2006 and 2008, leading to 22 [16–32] Tg CH4 yr-1 higher methane emissions over the period 2008–2012 compared to 2002–2006". This number is very different from the 7 Tg yr-1 (top–down) stated here. Saunois, M., Bousquet, P., Poulter, B., Peregon, A., Ciais, P., Canadell, J. G., et al. (2016a). The global methane budget 2000–2012. Earth Syst. Sci. Data 8, 697–751. doi:10.5194/essd-8-697-2016. Saunois, M., Bousquet, P., Poulter, B., Peregon, A., Ciais, P., Canadell, J. G., et al. (2017). Variability and quasi decadal changes in the methane budget over the period 2000–2012. Atmos. Chem. Phys. 17, 11135–11161. doi:10.5194/acp-17-11135-2017. [Peter Bergamaschi, Italy]	Accepted - the sentence is revised and we have now used more recent publications

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
38321	31	40	31	43	Peng et al. (2016) indicates that the IPCC recommended emission factors are higher than actual local values, and thus overestimate some fugitive emissions from China's coal mines. In the new EDGARv4.3.2 release, the total fugitive emissions from coal mining in China is 1.6 times lower than EDGARv4.2(Peng et al. (2016)). Therefore, it is incorrect to attribute the difference between top-down and bottom-up methods to uncertainties in fugitive emissions from China's coal mines. Furthermore, the conclusion drawn here does not accurately reflect the main finding of Peng et al. (2016). Considering these above reasons, it is suggested to delete the sentence "which can be largely explained by the uncertainties in fugitive emissions from Chinese coal mines (Peng et al., 2016)" or to replace "uncertainties" in this sentence with "overestimation". [Yaming LIU, China]	Accepted - the sentence is revised
14915	31	40		42	The increase in global methane emissions since 2018 based on top-down estimates given here, only 7 Tg per year, is far lower than that reported in many papers cited elsewhere in the chapter. For instance, Worden et al. 2017 gives a top-down value for this of 27 Tg per year, 4-times higher. [Robert Howarth, United States of America]	Not applicable. The discussion is revised significantly
131533	31	41	31	41	Define "BU" [Hans Poertner and WGII TSU, Germany]	Accepted - given in a paragraph just before subsection 5.2.2.1
58641	31	41	31	42	In the same sentence, BU (for bottom-up) and top-down (that could be shortened to TD) are used. Would be better to choose between one of the two forms. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - change was made.
39997	31	41			BU not really defined. [TSU WGI, France]	Accepted - change was made.
127737	31	41			Did not define acronym BU (bottom up). [Trigg Talley, United States of America]	Accepted - sentence revised
83007	31	42	31	42	The reference needs to be Saunois et al 2016 a or b. [Dan Zwartz, New Zealand]	Accepted - citation revised to Saunois et al. 2020
8765	31	42	31	43	I am not sure what is being explained by this paper - the different relative increase in BU and TD estiamtes or the increase in emissions from 2002-2006 to 2008-2012 [Vaishali Naik, United States of America]	Accepted - sentence revised
38323	31	43	31	45	Global Carbon Budget 2019 indicated that China's coal consumption decreased by 0.8% per year from 2013 to 2018, and in fact Chinese coal consumption has maintained a downward trend since 2013. In this regard the sentence "Emissions from the China's coal mines are likely to have continued growing" is inconsistent with the facts. It is suggested to delete this sentence "Emissions from the China's coal mines are likely to have continued growing, as suggested by top-down estimations". Reference: Global Carbon Budget 2019,Global Carbon Project. [Yaming LIU, China]	Rejected. The CH4 emissions from coal mining is decoupled from the Coal consumption because the CH4 emissions occur from mines including those abandoned within the country. The CO2 emission occur from coal burning of domestic and imported coal. It would be great if the study of Peng et al. is updated in the near future.
14919	31	43		45	This statement that methane emissions from China have increased is technically true, but is misleading when attributed to Miller et all. 2019, since as noted above, Miller et al. 2019 actually estimated these emission increases as only 1.1 Tg per year (less than 4% of the global increase in total global methane emissions reported by Worden et al. 2017 and references therein. [Robert Howarth, United States of America]	Taken into account - the sentence is revised for clarity
9409	31	44	31	44	Remove "the" before "China's coal mines…" [Christine Weldrick, Australia]	Accepted - change was made.
2793	31	44	31	44	should be "from China's" [Stephen Wilkinson, United Arab Emirates]	Accepted - change was made.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
14917	31	44			Miller et al. 2019, cited elsewhere in this chapter, uses satellite data to nail down the increase in methane emissions from coal development in China since 2008, reporting it to be fairly low (1.1 Tg per year). This is expected, since most of the increase in coal mining in China is from surface-mined coal which has low methane emissions compared to deep mines (Howarth 2019 Biogeosciences 16: 3033-3046 [Robert Howarth, United States of America]	Not applicable. Text revised
31909	31	44			delete 'the' before China's [Euan G. Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change was made.
73021	31	45	31	45	References should be in alphabetical order. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial - this kind of issues will be taken care of during the production phase, which includes professional copy-editing.
16511	31	45	31	52	There are a lot of time periods and values quoted here. So it is difficult to pick out what the AR6 assessment is. [William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. Text revised
31911	31	45			Maybe cite Thompson, Rona Louise, et al. "Methane emissions in East Asia for 2000–2011 estimated using an atmospheric Bayesian inversion." Journal of Geophysical Research: Atmospheres 120.9 (2015): 4352-4369. [Euan G. Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. The more recent papers used inventories and inversions to more clearly bring out the coal mining estimation better. The emission overestimation issue has been discussed since Patra et al., Bergamaschi et al., Tohjima et al. and host of others
12337	31	47	31	48	"...still persists between 2003–2007 in EDGARv4.3.2." It is not clear, if you really mean here between 2003 and 2007 or between (2003–2007) and (2007-2012) [Peter Bergamaschi, Italy]	Accepted - the sentence is revised
21817	31	48	31	52	This is very confusing. It implies emissions are simultaneously both increasing and decreasing as I read this text. The explanation needs modification for clarity. [Peter Thorne, Ireland]	Accepted - the sentences are revised
12339	31	48	31	55	I would recommend emphasize more clearly the uncertainties in current d13CH4 budget analyses. The [Schwietzke et al., 2016] paper heavily relies on the assumption of the global mean d13CH4 of wetlands (in that paper assumed to be in the narrow range of -61.5 ± 0.6 o/oo). However the uncertainties of wetland d13CH4 are probably much larger (given the large diversity of wetlands). Using slightly different mean d13CH4 values for wetlands would result in significantly different estimates of global fossil fuel emissions. [Peter Bergamaschi, Italy]	Taken into account . We have also discussed the used of 14C and 13C in the budget section
31913	31	48	31	55	Maybe rewrite the text a little here - it's a bit hard to follow, just for English. [Euan G. Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - the sentences are revised
14921	31	48		52	This very strong statement is based totally on Schwietzke et al. (2016); the conclusions in that paper have been highly contested by Worden et al. 2019 and Howarth 2019 (see references above), both of whom conclude the global increase in methane was driven much more heavily by fossil fuels. [Robert Howarth, United States of America]	Taken into account - the sentence is revised
9411	31	50	31	50	Replace "emission" with "emissions" [Christine Weldrick, Australia]	Accepted - change was made.
9413	31	53	31	53	Add "the" before "fossil fuel industry" [Christine Weldrick, Australia]	Accepted - change was made.
65401	31	53	31	54	suggest to add "and" in the end of the sentence "xxx biomass burning, agriculture" [Rebecca Danielsson, Sweden]	Accepted - change was made.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
14923	31	54		55	The Howarth 2019 Biogeosciences paper should be added to this list of references on the huge uncertaintly involved in using trends in 13C data to infer emission sources. [Robert Howarth, United States of America]	Rejected. This paper doesn't improve our understanding compared to those cited
31915	31	55			Perhaps mention the impact of work by Petrenko et al (and Hmiel et al and Dyonisius et al) on our assessment of fossil emissions - these are now signifcaintly bigger than previously estimated, as the natural fossil input is clearly smaller than hitherto thought [Euan G. Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We have discussed the possibility fossil fuel fraction underestimation later in the Budget subsection.
5329	31		31		After last line, add 'Given that wetland CH4 emissions are highly sensitive to temperature (e.g., Bansal et al. 2016), climate-CH4 feedback could lead to further increases in biogenic emissions from warming' Bansal, S., Tangen, B.A., and Finocchiaro, R.G., 2016, Temperature and hydrology affect methane emissions from Prairie Pothole wetlands: Wetlands, v. 36, no. 2, p. 371-381. doi: 10.1007/s13157-016-0826-8 [Sheel Bansal, United States of America]	Rejected. The discussion on climate feedback on CH4 emission is discussed in another section later in this Chapter.
23697	32	1	32	2	why the numbers do not have SD or SE?? [Massimo Lupascu, Singapore]	Accepted - time statistics is given
12341	32	1	32	3	"117 Tg 1 yr-1 in 2010–2017" I would recommend to use consistent time periods for the discussion and the Table 5.2 (where the period 2008-2017 is used) - unless there are specific reasons to use different time periods. Furthermore, I would recommend to use the average numbers from the bottom-up inventories (as compiled in Table 5.2) instead of discussing just the numbers of a single inventory (EDGAR). [Peter Bergamaschi, Italy]	Accepted - changes were made.
8767	32	1	32	3	These numbers do not match with those given in Table 5.2, please clarify. Also suggest including uncertaintiy levels for any numbers given in the text. [Vaishali Naik, United States of America]	Taken into account - the analysis time periods and numbers are now matched
77741	32	1	32	12	Could additional information be provided on the regional distribution of methane emissons and changes to these. [Emer Griffin, Ireland]	Accepted - Details are provided in Box 5.1 Fig. 2
51141	32	1	32	12	Please could you explain in this section why livestock emissions have gone up since 2000? Is this solely due to increased agricultural productivity? [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Information provided
23701	32	1	32	32	why the numbers do not have SD or SE?? [Massimo Lupascu, Singapore]	Accepted - time statistics is given
14925	32	1		12	Just as the draft language on page 31 underestimates emissions from fossil fuels, this paragraph overstates the contribution from animal agriculture. Reconciling these biogenic emissions and fossil-fuel emissions is a zero-sum game, as we know total emissions quite well. Since Petrenko et al. 2017 and Himiel et al (2020) (see referenes above) have demonstrated from radio-C14 methane data that the traditional inventories referenced here have underestimated the fossil fuel sources, these inventories are also too high for animal agriculture. The actual data on enteric emissions from cows are quite limited, and mostly come from studies in Europe and the US on very well fed cows; emissions from less-well fed cows in Africa and India are perhaps an order of magnitude lower, based on several talks given at the 8th International Symposium on Non-CO2 Greenhouse Gases, Amersterdam, June 2019. Note that cow and cattle populations have been declinding over the past decade or more in both the US and Europe, and so are unlikely to be contributing to greater fluxes. It is unbelievable to me that the IPCC authors have "high confidence" in the conlusion they give here, which I am convinced is simplly wrong. [Robert Howarth, United States of America]	Rejected. It is untrue that the fossil fuel emissions are overlooked. We are trying cover all the emission sources as much as possible. The issue of lower fossil fuel emissions, if true, has to come through an emission inventory. The processes have to be identified and mapped and then tested by atmospheric observations. Hope this cycle will be completed by the upcoming IPCC assessment cycles. We have discussed the 14C based results in the Budget subsection

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31917	32	3			see discussion of manure tanks and lagoons from intense dairying in Nisbet, E. G., et al. "Methane mitigation: methods to reduce emissions, on the path to the Paris agreement." Reviews of Geophysics 58.1 (2020): e2019RG000675. [Euan G. Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Noted - added the reference
23699	32	5	32	5	sigma should be with the greek character [Massimo Lupascu, Singapore]	Accepted - change was made.
74233	32	5	32	5	should Sigma be replaced with the greek letter? [Christoph Völker, Germany]	Accepted - change was made.
9415	32	5	32	6	Reminder of the Placeholder [Christine Weldrick, Australia]	Taken into account. Thank you
8769	32	7	32	10	Is there a difference between livestock emissions from enteric fermentation and anthropogenic ruminant emissions? If not, suggest using a single terminology to denote them. [Vaishali Naik, United States of America]	Accepted - unified the terminology
109649	32	7	32	10	What exactly is meant by production rate in this context? [Carolyn-Monika Görres, Germany]	Noted - level of milk and meat production was meant, added
115343	32	7		10	Could refer to the SRCCL here. [Gillett Nathan, Canada]	Accepted - reference added
31919	32	8			mention water buffalo with cattle, goats and sheep? [Euan G. Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - mentioned
12343	32	10	32	12	In some regions (e.g. Europe, US) significant reductions in landfill emissions between 1990 and 2018 have been reported (UNFCCC reports). Thus, at least in some global regions, landfill emissions were not "steadily increasing since 1970's". Furthermore, I am wondering about the "high confidence" in the described trend, given the generally large uncertainties is landfill emissions. [Peter Bergamaschi, Italy]	Accepted - sentence revised
8771	32	10	32	12	This is the first instance of the use of IPCC uncertainty language in the methane section, but is based on limited evidence or at least the evidence that is presented here is limited. I am not sure if we have high confidence in the trends in global methane emissions from waste management and landfills. [Vaishali Naik, United States of America]	Taken into account - we have given updated references from EDGAR and GAINS. Our inversions also support the inventories. Thus we trust the values at high confidence

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
104819	32	10	32	12	The waste emission inventory from Saunois et al., 2019 is increasing solely based on increasing waste quantities, however regionally in WestAsian countries annual municipal solid emissions are projected to decrease by up to 8.75% to 10.16% due to waste characterisation changes, as teh 12 developing countries in the region progress towards developed country status (Dumble 2017). Howver climate change impacts could increase waste emissions by 1.7% to 2.29%. Other environmental factors such as moisture levels in waste (Spokas et al 2015) indicated emissions are beng overestimated in the models used including their own CALMIM model (Spokas et al 2011, Spokas and Bognor 2011) in dry temperate climates becoming more prone to periods of drought. Dumble P. (2017). Regional development and climate change mitigation modelling of municipal solid waste emissions in Middle East, Water and Environment Journal, John Wiley, Vol3, No.2, p226-234, May 2017, DOI: 10.1111/wej.12236. Spokas K, Bogner J, Corcoran M, Walker S. (2015). From California dreaming to California data: Challenging historic models for landfill CH4 emissions.2015. Elem Sci Anth. 2015;3:51. DOI:http://doi.org/10.12952/journal.elementa.000051 Spokas KA, Bogner JE. (2011). Limits and dynamics of methane oxidation in landfill cover soils. Waste Manage., 31 (5): 823 – 832, https://doi.org/10.1016/j.wasman.2009.12.018 Spokas K, Bogner J, Chanton J. 2011 . A process-based inventory model for landfill CH4 emissions inclusive of seasonal soil microclimate and CH4 oxidation. J Geophys Res 116 (G4): G04017 https://doi.org/10.1029/2011JG001741 [Paul Dumble, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Thank you for bringing this to our notice. Dumble 2017 is discussing more about the projections in to the future while the Table 5.2 and the associated discussions are for the historical period
73023	32	11	32	11	Insert 'the' after 'since'. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change was made.
74235	32	11	32	12	since THE 1970’s [Christoph Völker, Germany]	Accepted - change was made.
31921	32	11			landfill emissions increasing? Maybe, but High confidence??….in Europe and large parts of the US there have been strong controls on landfill emission and very significant reductions in methane emissions over recent decades. Yes, landfill emissions are probably growing very fast in tropical countries but I'm not sure I'd have 'high' confidence in the net global landfill emissions budgets - we have so little real idea what the giant landfills of India, Indonesia, Nigeria and the Middle East etc etc emit. [Euan G. Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - removed the confidence statement
73025	32	12	32	12	Change 1970's to 1970s. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change was made.
58699	32	14	32	15	The period between 1980 and 2003 is described in absolute numbers (4é - 33 Tg/yr), the subsequent period in relative amounts (increase by 20%). Can you please unify this. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. The emissions are given for different periods
12345	32	14	32	16	"tended to decrease"; "increased gradually"; "as per EDGARv4,.3.2": wording should be checked / updated [Peter Bergamaschi, Italy]	Accepted - sentence revised
8773	32	14	32	16	I am confused. How can there be low agreement when the data presented here is from one source (EDGARv4.3.2)? Confidence levels should be assigned when basing the assessement frrom multiple lines of evidence. [Vaishali Naik, United States of America]	Accepted - citations added, after taking in to account other inventory and model results

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
51139	32	14	32	16	The last estimate of CH4 emissions from rice cultivation cited is from 2003 yet figures for change in emission are given to 2012. Can a recent, final emission rate be provided? [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - references added
69189	32	14	32	20	Suggest including an analysis of the trend of global rice production area over years when global CH4 emission trend from rice is discussed because this CH4 emissions are closely relating to the total area. [Kaoru Magosaki, Japan]	Taken into account. The emission inventories takes in to account the rice cultivation area as report to the FAO. Also some implementation of agricultural practices are also accounted, e.g., the intermittent irrigation
69191	32	14	32	20	CH4 emission numbers of rice production (42 Tg yr-1 and 33 Tg yr-1) do not match the numbers in table 5.2. Suggest using consistent numbers, if there is no specific reason to use different numbers. [Kaoru Magosaki, Japan]	Accepted - changes were made.
40429	32	15	32	16	incorrect use of IPCC confidence language [TSU WGI, France]	Accepted - changes are made.
73027	32	22	32	22	Change 'include' to 'including'. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change was made.
74237	32	22	32	22	“include” should be “including” [Christoph Völker, Germany]	Accepted - change was made.
12347	32	22	32	23	it is not clear to which time period the given "24 Tg CH4 yr-1" refers to [Peter Bergamaschi, Italy]	Accepted - time period is given
8775	32	22	32	23	what year? global or regional? This estimate is not given in Table 5.2. [Vaishali Naik, United States of America]	Not applicable. This emission sector is given in Table 5.2 in the Anthropogenic sources. We have revised the numbers based on the final assessment
31923	32	22			mention ENSO impact on burning Inter annual variability? Not sure how this paragraph works with the fire discussion on p 26.24-37 [Euan G. Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. No apparent contradiction
109653	32	24	32	24	Why are wildfires listed in the section about anthropogenic CH4 emissions when they are clearly identified as natural CH4 source? [Carolyn-Monika Görres, Germany]	Accepted - change was made by removing this sentence but included in parenthesis in the next sentence as the satellite observations include this sector as well.
8777	32	25	32	26	Unless clarified, the use of multiple terminology can get very confusing and difficult to keep track of. Is open burning the same as biomass burning and biofuel or is it wildfires? What does likelly signify here? [Vaishali Naik, United States of America]	Not applicable. Not clear about the source of the doubt. Open biomass burning is a fraction of the biomass burning emissions, which can be seen from satellites
19969	32	25	32	28	Looking at figure 5.13 one does not get a feeling of large variability. Also, figure 5.13b is not very convincing. One would like some statistical tests performed about possible links between methane growth rate and ENSO indices. [philippe waldteufel, France]	Accepted - we have performed correlation analysis of CH4 growth rate with ENSO
12349	32	25	32	28	I would recommend to replace "large part of the interannual variability" by e.g. "significant part of the interannual variability", since wetlands also play an important role for the interannual variability (e.g. Pandey, S., Houweling, S., Krol, M. et al. Enhanced methane emissions from tropical wetlands during the 2011 La Niña. Sci Rep 7, 45759 (2017). https://doi.org/10.1038/srep45759) [Peter Bergamaschi, Italy]	Rejected - this study is for one year of data. We are still unsure how wetland emissions affect the CH4 interannual variability over a long period of time

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
9417	32	26	32	26	Suggested sentence change: "...showed a tight link with natural climate variability (e.g., ENSO), and explained…" [Christine Weldrick, Australia]	Accepted - changes were made.
9419	32	28	32	29	Add "the" before "past two decades…" [Christine Weldrick, Australia]	Accepted - change was made.
33309	32	28			Change: « CH4 emissions» by “Methane emissions”. It is better to avoid using abbreviation at the start of a sentence. [Guiomar Rotllant, Spain]	Accepted - change was made.
127739	32	29			Savanna should not be capitalized. [Trigg Talley, United States of America]	Accepted - change was made.
8779	32	30	32	32	I am not an expert in this area but as far as I can tell, the relationship between tropical fires and climate (including precipitation) has been known for a while (for example van der Werf et al 2008 https://agupubs.onlinelibrary.wiley.com/doi/10.1029/2007GB003122). See also assessment of fire emissions (in the context of SLCFs) in Chapter 6 [Vaishali Naik, United States of America]	Taken into account. Agree, there is also Patra et al., GBC, 2005 which extensively discussed fires, drought and climate variabilities, but we think these new studies are more advanced by using more information available today.
9421	32	31	32	31	Remove "the" before "precipitation" [Christine Weldrick, Australia]	Accepted - change was made.
58643	32	37	32	42	Saunois 2016 is quoted but Saunois 2019 isn't mentioned in the sources. However, Table 5.2 on this page clearly contains columns directly taken from Saunois 2019 - Table 3 page 124. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - changed the reference to Saunois et al. 2020
12351	32	37	32	42	The table seems to include also data from the latest GCP-CH4 analysis [Saunois et al., 2019], which should be included as reference [Peter Bergamaschi, Italy]	Accepted - Saunois et al. 2020 added
73031	32	37	33	1	Table text too small. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - we have tried to improve the clarity a bit
52153	32	37	33	3	Table 5.2: It does not make sense at all to include hydrates into the 'other oceanic' category and to include 'oceans' into 'geological sources'. You should list 'geological sources (incl. hydrates)' and 'oceans'; you may split 'oceans' into open ocean and coastal oceans (i.e. estuaries etc.) [Hermann Bange, Germany]	Accepted - changes are made.
14927	32	37		42	All of my comments above apply to this table, which ignores a great deal of the pertinent recent literature. [Robert Howarth, United States of America]	Rejected. The work of Saunois et al. have tried to look in to all aspects of CH4 budget in great details as evident from the ESSD paper. We have tried some updates as a few global scale studies are published recently
88185	32	37			Table 5.2 - Permafrost row appears twice in first section [Sharon Smith, Canada]	Accepted - changes are made.
88187	32	37			Table 5.2 - Permafrost row - Lakes and wetlands are not included here but are permafrost peatlands included?. What about permafrost undergoing thaw where ponds, wetlands may be forming but permafrost is still present below them? (transitional landscapes). Are these landscapes covered under wetlands row? [Sharon Smith, Canada]	Rejected. It is unfortunate that we do not have all the information globally to make clear distinctions between various wetland types. Only the best estimations are given
88189	32	37			Geological sources - oceans are mentioned but does this include terrestrial sources such as gas hydrates? [Sharon Smith, Canada]	Accepted - correlation statistics given
40881	32	37			Table 5.2: what's in the bracket? Which level of uncertainty? [TSU WGI, France]	Taken into account. They are showing the min-max range of all available estimations. Now mentioned in the Table caption
83009	32	38	32	38	The reference needs to be Saunois et al 2016 a or b. [Dan Zwartz, New Zealand]	Not applicable - reference changed to Saunois et al. 2020

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
109655	32	38	32	38	In the text, it lists Saunois et al. (2019) as source, not Saunois et al. (2016). [Carolyn-Monika Görres, Germany]	Accepted - the references are matched
58745	32	38	32	38	The caption of Table 5.2 references Saunois et al., 2016 - I think it should be Saunois et al., 2019. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - the reference is revised
73029	32	39	32	40	References should be in chronological order. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial - this kind of issues will be taken care of during the production phase, which includes professional copy-editing.
31925	32	39			Etiope et al 2019 - mention Hmeil et al and Dyonisius et al 2019 (see refs above page 15) [Euan G. Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Reference added
12353	32	42	32	42	"molecular CH4 and measurements of carbon and hydrogen isotopes in the atmosphere": I assume that the term "molecular CH4" means here CH4 dry air mole fractions? Should be rephrased to e.g. "atmospheric CH4 and its isotopic composition" [Peter Bergamaschi, Italy]	Accepted - changes were made.
74239	32	42	32	42	unclear to me"what does "molecular" mean in this context? Is CH4 not always a molecule? [Christoph Völker, Germany]	Accepted - the phrases are revised
31927	33	0			Geological sources - likely much smaller - see Etiope/Hmiel/Dyonisius debate above p32, p15. Evidence very much on the side of the Petrenko team. [Euan G. Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - Yes, of course
18207	33	1	33	1	Table 5.2 is currently a screen shot - will this be updated into a formatted table? [Chelsey Baker, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - Yes, of course
21819	33	1	33	1	Table needs to be properly prepared and not a screencap of an excel spreadsheet for the FGD. [Peter Thorne, Ireland]	Accepted - Yes, of course
84803	33	1	33	1	This table is now no longer consistent with the text and the CH4 budget graph. Natural geological sources are much smaller than given in the table. [Martin Heimann, Germany]	Accepted - changes are made.
64597	33	1	33	1	Table 5.2 needs to be reformatted to match the appearance of the CO2 budget table. [Charles Curry, Canada]	Rejected. CH4 emissions sectors are many more than CO2 and we have both top-down and bottom-up emissions, as well as chemical loss and soil update. So Table 5.2 is more complex than the equivalent table for CO2.
109657	33	1	33	1	The period for the last columns (2008 – 2017) is listed as 2010-2017 in the preceding text. Please recheck. [Carolyn-Monika Görres, Germany]	Accepted - changes are made.
18595	33	1	33	1	Table 5.2: Permafrost (excl. lakes and wetalnds) appears twice under sources; Is the assumption that permafrost wetlands and lakes fall under the "wetlands" and "freshwater" categories? Does "Other Sources' represent a variety of sources or unknown sources? [Miriam Jones, United States of America]	Accepted - Permafrost issue is resolved. The maximum possible clarity is provided in the Table, and should be improved in the future

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
71681	33	1	33	3	The “sinks” part of Table 5.2 raises other questions. Tropospheric OH removal remains the same in Tg/yr for 2000-2009 and 2008-2017 which implies a decrease in OH because of the concentration increase between these periods. Tropospheric Cl removals for 2000-2009 and 2008-2017 are less than half those of 1980-1989 and 1990-1999 which is presumably a move to the more recent literature - rather than any change in Cl chemistry. [Martin Manning, New Zealand]	Accepted - sorry that some of the numbers were not updated for the recent decade. We have revised the numbers
87425	33	1	33	3	Table 5.2: Why do ruminants among the wild animals account for only 2% compared with the methane emissions from enteric fermentation in agriculture? [Jürg Thudium, Switzerland]	Taken into account. Their shear number and the feed E.g., meat cows are much more emission intensive that the milk cows; farmed cows are much more emission intensive than the wild animals
71679	33	1	33	3	5-33:1-3 Table 5.2 is a useful quantitative summary of what is being covered in the text. However, there is nothing equivalent to this for the δ13C/CH4/ part of a budget analysis. Given the increasing amount of isotopic data for methane and detailed reviews of source isotopic signatures (e.g. Sherwood et al, 2017, Global Inventory of Gas Geochemistry Data from Fossil Fuel, Microbial and Burning Sources. Earth System Science Data, 9, 639-656.) the chapter is not covering approaches to a budget analysis using isotopic data as now used in much of the literature. To be more specific, are the source changes from 2000-2009 to 2008-2017 consistent with observed δ13C/CH4/ in the ranges for source δ13C given in Sherwood et al? [Martin Manning, New Zealand]	Taken into account. It is a good suggestion but not easy bring in to table.
69775	33	1	200	1	please use and cite pertinent chapters of https://carbon2018.globalchange.gov for these decadal carbon dynamics and budget analyses. [Gyami Shrestha, United States of America]	Not applicable. More specific comments/suggestions are expected
18103	33	1			I assume this table will be changed to an actual formatted table, at the moment it is a screen capture of an Excel spreadsheet. [Vlad Macovei, Germany]	Accepted - Yes, of course
52155	33	6	33	6	Important publication is missing and should be cited: Al-Haj, A. N. and Fulweiler, R. W.: A synthesis of methane emissions from shallow vegetated coastal ecosystems, Global Change Biology, 2020; doi: 10.1111/gcb.15046. [Hermann Bange, Germany]	Accepted - text and citations added
58747	33	6	33	30	It may be appropriate to include a brief discussion of terrestrial permafrost and hydrates in this section. Possible sources for such a discussion could be: McCalley, C. K. et al. "Methane dynamics regulated by microbial community response to permafrost thaw." Nature, 514, 478-481, doi:10.1038/nature13798, (2014). Schuur, E. A. G. et al. "Climate change and the permafrost carbon feedback." Nature, 520, 171-179, doi:10.1038/nature14338, (2015). [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected - such discussion is not suitable in this section, feedbacks are discussed in section 5.4.7.
78493	33	8	33	8	A key uncertainty in wetland ch4 emissions is the response to CO2 which could outweigh the response to climate – this could be discussed more fully [Chris Jones, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - this is discussed in 5.4.7 "Non-CO2 feedbacks"
8781	33	8	33	9	Why are confidence levels needed here? [Vaishali Naik, United States of America]	Taken into account - because this sector is big and large differences typically exist between the estimations

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
40431	33	8	33	9	incorrect use of IPCC confidence language [TSU WGI, France]	Accepted - we have deleted confidence statement and sentence is revised
12355	33	8	33	11	Top-down / bottom-up emissions are discussed here for the period 2010-2017, while Table 5.2 reports emissions for 2008-2017 (and previous periods). It would be helpful to use consistent time periods for the discussion and the table (unless there are specific reasons to use different time periods). [Peter Bergamaschi, Italy]	Accepted - time periods are matched
8783	33	9	33	11	These numbers are not quoted in Table 5.2. Please ensure consistency. [Vaishali Naik, United States of America]	Accepted - changes are made.
39787	33	9			"medium agreement, robust evidence" this statement is not really traceable. [TSU WGI, France]	Accepted - we have deleted confidence statement and sentence is revised
83011	33	11	33	11	The reference needs to be Saunois et al 2016 a or b. [Dan Zwartz, New Zealand]	Not applicable - reference changed to Saunois et al. 2020
73033	33	12	33	12	Delete , after 'transport'. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted
127741	33	14	33	14	Saunois et al. (2019) not (2017). [Trigg Talley, United States of America]	Accepted - the sentence is revised
12357	33	14	33	15	"Both the top-down and bottom-up estimates presented in Saunois et al. (2017)" Please check reference. Saunois et al. (2017) analyzes only the period 2000–2012, while you discuss here the "last three decades" Saunois, M., Bousquet, P., Poulter, B., Peregon, A., Ciais, P., Canadell, J. G., et al. (2017). Variability and quasi decadal changes in the methane budget over the period 2000–2012. Atmos. Chem. Phys. 17, 11135–11161. doi:10.5194/acp-17-11135-2017. [Peter Bergamaschi, Italy]	Accepted - this part of discussion is revised
8785	33	14	33	16	If Saunois et al (2017) estiamtes are smaller than in AR5 then how come they depend on the new wetland maps and ecosystem models that are still being worked on? I don't think a work in progress can be cited in this report. [Vaishali Naik, United States of America]	Accepted - sentence revised
39999	33	16	33	16	Can the reference "Poulter et al., work in prog." be cited here? [TSU WGI, France]	Accepted - sentence revised, reference deleted
12359	33	16	33	16	"(Poulter et al., work in prog.)." should be replaced by approriate reference [Peter Bergamaschi, Italy]	Accepted - the sentence is revised
8787	33	16	33	17	Please provide quantifiable evidence for this. The placement of this sentence is odd [Vaishali Naik, United States of America]	Accepted - sentence revised
112021	33	16	33	18	This sentence "It is likely that 17 the bulk of the post–2006 increase in atmospheric CH4 concentration should be attributed to sources from animal farming and coal mining as the two major sectors" seems to contradict earlier statements about CH4 post 2007 (Section 5.2.2.2) that made it seem as though much debate remains regarding the driving source of emissions increases post 2007. [Cynthia Randles, United States of America]	Accepted - the sentence is revised

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
12361	33	16	33	22	I think that the discussion could be improved here, presenting the arguments more clearly and in a more logical order, e.g. along the folloing lines: (1) 13CH4 data suggest significant contribution of biogenic sources to atmospheric increase since 2007 [Schaefer et al., 2016] (2) similar / overlapping d13CH4 signature between anthropogenic biogenic sources (enteric fermentation, waste and manure management) and wetlands; therefore d13CH4 alone cannot distinguish between anthropogenic biogenic sources and wetlands (3) inventory data suggest significant increase of anthropogenic biogenic sources [Schaefer et al., 2016; Janssens-Maenhout et al., 2019]; at the same time analysis of wetlands models suggested that global wetland CH4 emissions did not increase significantly [Poulter et al., 2017]. However some studies attribute the post-2007 increase at least partly to wetlands [Nisbet et al., 2016; 2019]. [Peter Bergamaschi, Italy]	Accepted - this part of discussion is revised. We cite the Box 5.1 now
14929	33	16		18	While I agree that the increase in methane emissions over the past decade is from human-controlled sources rather than natural sources, I very strongly disagree that the evidence shows conclusively that this is from animal agriculture and coal. To the contrary, the best evidence points to fossil fuels, and to the oil and gas industry more than to coal. See Worden et al. 2017, Howarth 2019, Petrenko et al. 2017, and Hmiel et al. (2020), references provided above. [Robert Howarth, United States of America]	Taken in to account - it is a difficult discussion when it comes to coal vs oil and gas; the former is linked to China and the later to the US. We need to have more consistent and global scale studies so that global emission inventories can be developed for understanding the global CH4 cycle
71683	33	18	33	22	Says that land-surface models show that wetland emissions are probably not increasing and that the 13CH4 data support that. But this, and the last sentence in this paragraph, are misleading because wetland and agricultural emissions have very similar δ13C. To cite Schaefer et al 2016 is also misleading here because the conclusion of that paper specifically admits that the recent increase could be partly from wetlands although the authors consider it more likely to be due to agriculture. [Martin Manning, New Zealand]	Accepted - sentence revised
8789	33	19	33	21	At the same time, there are a few studies (see Nisbet et al papers, review of Turner et al 2019) that point to wetlands being an important driver of methane increases post 2006. I think a more balanced discussion is needed. Further I don't think this is the place for this discussion, would belong better in the cross-chapter Box Box 5.1 [Vaishali Naik, United States of America]	Accepted - this sentence is deleted
14933	33	21		22	This unreferenced statement reflects a naïve understanding of the factors affecting trends in the 13C signal of methane in the atmosphere, for example the influence of changes in biomass burning (Worden et al. 2017). I believe this should be deleted. [Robert Howarth, United States of America]	Accepted - sentence revised
14931	33	21			The Schaefer et al. 2016 was criticized by Schwietzke et al. 2016 for not using representative 13C emisson-source data, by Worden et al. 2017 for ingoring changes in biomass burnng over time, and by Howarth 2019 for not recognizing that the 13C signal from shale gas is more depleted in 13C than is the methane from conventional natural gas. Any one of these reasons alone severely undercuts the conclusions from Schaefer et al.2016, but collectively they should have killed the credibility of that study. It appears the IPCC authors are not aware of these criticisms, as their overall conclusions on methane emission sources seem quite driven by the Schaefer et al. 2016 paper. [Robert Howarth, United States of America]	Taken into account - we have stated in our assessment that 13C based studies remains underdetermined for separating different source sectors

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
71685	33	23			This section needs to bring in some of the studies that have looked at how some sources interact with OH. For example, a detailed model-based estimate of how wetland emissions may affect CH4-CO OH chemistry is Rowlinson et al (Rowlinson, et al, 2019, Impact of El Niño Southern Oscillation on the interannual variability of methane and tropospheric ozone. Atmospheric Chemistry and Physics Discussions). Also, Zhang et al, 2017, cited later in this chapter, found evidence for a step change in wetland emissions occurring between 2000-2006 and 2007-2014. Whether or not there is a long term trend in wetland emissions seems to remain controversial. E.g. Bridgham et al (2013: Methane emissions from wetlands: biogeochemical, microbial, and modeling perspectives from local to global scales. Global Change Biology, 19(5), 1325-1346) found that strong El Ninos reduce wetland emissions whereas Hodson et al (2011, The El Niño–Southern Oscillation and wetland methane interannual variability. Geophys. Res. Letts, 38, L08810) found an overall positive correlation. [Martin Manning, New Zealand]	Taken into account - the CH4 emission variability with ENSO is discussed here, while the OH variability issue is discussed in Box 5.1
12365	33	24	33	26	I would suggest to replace the term "photochemical reactions" by "abiotic processes" (since the exact mechanisms of these processes are not yet clear). Furthermore, I would suggest to change the order of the 3 principal mechanisms discussed here, e.g. mention first both potential production processes within the trees and then the transport. [Peter Bergamaschi, Italy]	Accepted both
58787	33	24	33	26	Replace "living parts" with "canopy" and reorganize sentence to reflect predominance of evidence for CH4 conduction from soil to atmosphere, such as: "Vegetation contributes to CH4 emissions predominantly by conducting CH4 from the soil to the atmosphere, and also with smaller contributions from methanogenesis taking place in the stems and photochemical reactions taking place in the canopies." [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account - the sentence was reorganized but not the wordings suggested
12363	33	24	33	30	would be useful to mention that the discussion here includes both wetland forests and upland forests [Peter Bergamaschi, Italy]	Accepted - mentioned
109659	33	24	33	30	I would also include a short sentence about CH4 emissions from insects in this paragraph, especially since termites are included in the CH4 budget. A possible reference is Brune A. (2018) Methanogenesis in the Digestive Tracts of Insects and Other Arthropods. In: Stams A., Sousa D. (eds) Biogenesis of Hydrocarbons. Handbook of Hydrocarbon and Lipid Microbiology. Springer, Cham [Carolyn-Monika Görres, Germany]	Accepted - changes are made.
127743	33	24	33	30	This may not be the correct sub chapter (future research sections?) but the frontier of resolving mechanism and magnitude to methane emissions from tree stems seems like an important research trajectory -- i.e Barpa et al. (2019, New Phytologist). [Trigg Talley, United States of America]	Taken into account. We are still lacking a proper dataset to separate the CH4 emissions from wetland and trees for their strong colocation. For example the emissions from the Amazon region is one of the most problematic.
11371	33	26	33	27	Please cite one or more references for this statement re: uncertain and unliely marked direct production of CH4 - e.g. Bruhn D, Møller IM, Mikkelsen TN, Ambus P. 'Minireview: Terrestrial plant methane production and emission', Physiologia Plantarum, March 2012, Vol 144, pp. 201-209 [Dan Bruhn, Denmark]	Accepted - citations added

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
115345	33	27			Don't italicize 'uncertain' since it is not calibrated language. Also if this assessment is uncertain is sufficient quantiative information really available to assess likelihood? ('unlikely' mean P<=33%). [Gillett Nathan, Canada]	Accepted - the sentence is revised
39873	33	27			"uncertain and unlikely" this statement is not really traceable and uncertain is not an IPCC uncertainty language term [TSU WGI, France]	Accepted. Changed the wording.
23703	33	30	33	30	On the top of CH4 from trees in tropical peatland (Pangala 2017), there is one new study that showed for the first time CH4 emissions from sedges in the tropics (up to 2.5% of the CH4 budget in SE Asia) similar to northern peatlands. It might be worth to mention it here. “SEDGES ARE A SIGNIFICANT SOURCE OF PLANT-MEDIATED METHANE EMISSIONS FROM DEGRADED TROPICAL PEATLANDS" by AKHTAR, HASAN1; Lupascu, Massimo1,2; Sukri, Rahayu3; Smith, Thomas4; Cobb, Alex5; Swarup, Sanjay2,6. Environmental Research Letters UNDER REVIEW. 1Department of Geography, National University of Singapore, Singapore 117570; 2NUS Environmental Research Institute, National University of Singapore, Singapore 117411; 3Institute for Biodiversity and Environmental Research, Faculty of Science, Universiti BruneiDarussalam, Brunei Darussalam BE1410; 4Department of Geography and Environment, The London School of Economics and Political Science, London, UK WC2A 2AE; 5Center for Environmental Sensing and Modeling, Singapore–MIT Alliance for Research and Technology (SMART), Singapore 13860; 6Department of Biological Sciences, National University of Singapore, Singapore 117558 [Massimo Lupascu, Singapore]	Rejected - we can only cite peer reviewed papers
5045	33	30	33	30	Comment/Addition: Further work has recently been published from Panamanian wetlands by Sjogersten et al. (2020) indicating that as much as 30% of the total methane flux is emitted through tree stems. This work adds further weight to the comments made here and in my opinion should be included here as a reference. The work by Pangala et al. (2013) from Southeast Asia, Pangala et al., (2017) in Amazonia, and now Sjogersten et al. (2020) in Central America, appear to indicate that this is an effect seen across the tropics but the estimated contribution of trees to methane emissions is highly variable from c. 30% in Panama, to 62-87% in Indonesia. A sentence may be needed to illustrate this range and to provide an indication of the current state of knowledge. Sjogersten et al. (2020) Methane emissions from tree stems in neotropical peatlands. New Phytologist https://doi.org/10.1111/nph.16178 [Thomas Kelly, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Added the reference, however no space to further discuss the different contributions observed.
127745	34	1	34	1	Authors indicated 6% earlier (page 30, line 15); check for consistency. [Trigg Talley, United States of America]	Accepted - changes are made.
8791	34	1	34	1	do you mean microbial sink is 5% of the total methane sink? for which period? [Vaishali Naik, United States of America]	Yes. For 2008-2017. Added that in the text. Actually the soil sink was 7% according to the top-down budget, which is now corrected.
31929	34	1			Microbial soil uptake by methanotrophs. Very poorly constrained %. The text does imply this but might be more outspoken in the need for research as this is a neglected topic. It's not just upland soils - any moist soil, provided it is aerobic and air can move through it as atmsopheric pressure varies meteorologically. [Euan G. Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Modified the text into this direction.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
8793	34	2	34	3	delete significantly. Would be good to note how these bottom up values are determined (model or observation based?). [Vaishali Naik, United States of America]	Accepted, deleted.
71687	34	2	34	6	This seems to be dismissing Ni and Groffman's (2018) paper on a decreasing soil sink a bit too quickly. However, my view is influenced by knowing of studies in Canada and in New Zealand that are not yet published but which both find soil sinks have been decreasing in two quite different ecosystems over decadal time scales. As we showed in Nisbet et al 2019, because of its high fractionation a decrease in soil removal could explain some of the decrease in δ13C/CH4/. [Martin Manning, New Zealand]	Noted. The assessment needs to rely on published papers, and unfortunately there are not enough of them. Hopefully bringing up the message of scarcity of papers will launch new research on this topic.
64599	34	4	34	4	The bibliographic entry corresponding to Yu et al. (2017) is incorrect. The correct reference is: Yu, L., Huang, Y., Zhang, W., Li, T., & Sun, W. (2017). Methane uptake in global forest and grassland soils from 1981 to 2010. Science of the Total Environment, 607, 1163-1172. Also, while I think the conclusion of this paragraph is appropriately conservative (i.e., no definitive change in soil methane uptake over recent decades), I find some aspects of the Ni et al. (2018) methodology concerning, namely: 1) material provided in the Supplement to Ni et al. (2018) shows that within-year sampling variability is very large, and this reviewer is not convinced it was properly accounted for in calculating error in the derived trends (i.e., it seems likely they are consistent with zero). 2) A sizable fraction of monthly CH4 measurements were included in the Ni et al. trend estimates along with annual values. This may introduce a bias at mid-latitude NH locations since CH4 consumption is much stronger in warmer months (when most of these measurements would have been collected) than during the rest of the year. I haven't looked in detail but it looks like the Yu et al. (2017) study, also a global meta-analysis, may have avoided these pitfalls. One should also keep in mind that detection of co-varying trends in both CH4 uptake and precipitation from point-scale data is extremely difficult, due to the high local variance of precipitation, I will leave it to the authors to decide whether the Yu et al. work deserves to be prioritized, e.g., by a statement such as, "There is evidence from experimental and modelling studies of increasing soil microbial uptake over recent decades, due to increasing temperature (Yu et al., 2017); although a claim of decreasing CH4 consumption, possibly linked to precipitation changes, has also been made (Ni and Groffman, 2018)." [Charles Curry, Canada]	Accepted - the suggested text as given at the end of the comment.
8795	34	4	34	6	This sentence should be placed before the previous sentence as these lines of evidence are then used to assess that there have been no changes in the estimates of microbial methane [Vaishali Naik, United States of America]	Accepted - changes are made.
73035	34	5	34	5	Quantify 'recent decades'. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable - text changed.
38505	34	9	34	9	change sub-heading to "Ocean and inland water emissions and sinks" [Siv K Lauvset, Norway]	Accepted
52149	34	9	34	20	A recent estimate of the global ocean and coastal CH4 emissions is given in Weber, T., Wiseman, N. A., and Kock, A.: Global ocean methane emissions dominated by shallow coastal waters, Nature Communications, 10, 4584, 2019. These number should be cited and used for the CH4 budget. [Hermann Bange, Germany]	Accepted - change is made by using this citation

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41023	34	9			the title of this section is confusing here, it seems to be about ocean and inland *water* [TSU WGI, France]	Accepted - change is made.
115347	34	9			Insert 'water' after 'inland'. [Gillett Nathan, Canada]	Accepted - text changed.
83389	34	9			replace inland with inland water (the passage focuses on aquatic sinks and sources, not inland in general) [Tuula Larmola, Finland]	Accepted - changes are made.
73037	34	11	34	11	Insert 'the' after 'in'. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change was made.
9423	34	11	34	12	Suggested sentence change: "…volcanos are major sources of CH4 in the marine environment, but CH4 flux measurements are sparse." [Christine Weldrick, Australia]	Accepted. The sentence is revised
127747	34	11	34	20	A clear definition of the "coastal ocean" and associated ecosystems should be included here. For example, in the global methane budget (Saunois et al., 2019), estuaries fall into the category 'coastal ocean' not 'inland waters/freshwater'. Fjords are technically estuaries, which further suggests to discuss estuaries and fjords together in this paragraph. A new global synthesis on methane emissions from all major natural, impacted and man-made aquatic ecosystems is currently submitted (Rosentreter et al., submitted). They find that the coastal ocean (estuaries, mangroves, salt-marhses, seagrasses, aquaculture ponds, tidal flats, continental shelves) contribute 4 (median) to 8 (mean) % to global aquatic methane emissions. [Trigg Talley, United States of America]	Taken into account. We have noted the suggestion and wait for the publication to come out
58701	34	11	34	20	Submarine gas hydrates are not discussed. See e.g. Mhyre et al., 2016 DOI:10.1002/2016GL068999 or Hong et al 2017 DOI: 10.1038/ncomms15745 or Dean et al 2018 DOI: 10.1002/2017RG000559 or Phlmann et al 2017 DOI: 10.5066/F7M906V0. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. Modified the text into this direction
31931	34	11			Coastal oceans the major source? Maybe but we don't know. Mangroves do emit, but there is also the open ocean phosphonate source that deserves attention. 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), 473-478. Also: del Valle, Daniela A., and David M. Karl. "Aerobic production of methane from dissolved water-column methylphosphonate and sinking particles in the North Pacific Subtropical Gyre." Aquatic Microbial Ecology 73.2 (2014): 93-105. [Euan G. Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change is made to include Mangrove
31933	34	11			The plastic source of methane in the ocean can also be mentioned. Royer, S. J., Ferron, S., Wilson, S. T., & Karl, D. M. (2018). Production of methane and ethylene from plastic in the environment. PLoS One, 13(8), e0200574. [Euan G. Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable - we are in trouble with space to bring more of such discussions
74241	34	12	34	12	“measurement” should be plural [Christoph Völker, Germany]	Accepted - change was made.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
17039	34	12	34	12	Offshore methane seeps are estimated to contribute from 5–25 Tg CH4 yr-1 to the atmosphere (Kvenvolden et al., 2001; Saunois et al., 2016). The most recent estimate using machine learning based approach narrowed this to 6-12 Tg CH4 yr-1, with a dominant sourcing from shallow coastal settings (Weber et al., 2019). This accounts for only minor (1-2%) contribution to the global atmospheric methane budget, while the ocean sediments host one of the largest methane reservoir in Earth. A primary reason for this minimal contribution is due to the efficient microbially induced Anaerobic Oxidation of Methane in sediments and Aerobic Methane Oxidation in the water column (Boetius and Wenzhöfer, 2013; Ruppel and Kessler, 2017). However, measurement of global marine CH4 fluxes are still sparse and there is an increasing amount of evidence of CH4 seepage from the Arctic shelf, possibly triggered by the loss of geological storage due to warming and thawing of permafrost and hydrate decomposition (Shakhova et al., 2010, 2017). Further, it has been shown that marine CH4 oxidation contributes to increase in oceanic CO2 (Akam et al., 2020; Wallmann 2008) Citation: Kvenvolden, K.A., Lorenson, T.D., and Reeburgh, W.S. (2001). Attention turns to naturally occurring methane seepage. Eos, Transactions American Geophysical Union 82, 457-457. Saunois, Marielle, Philippe Bousquet, Ben Poulter, Anna Peregon, Philippe Ciais, Josep G. Canadell, Edward J. Dlugokencky et al. "The global methane budget 2000–2012." Earth System Science Data 8, no. 2 (2016): 697-751. Weber, T., Wiseman, N.A., and Kock, A. (2019). Global ocean methane emissions dominated by shallow coastal waters. Nature communications 10, 1-10. Boetius, A., and Wenzhöfer, F. (2013). Seafloor oxygen consumption fuelled by methane from cold seeps. Nature Geoscience 6, 725-734. Ruppel, C.D., and Kessler, J.D. (2017). The interaction of climate change and methane hydrates. Reviews of Geophysics 55, 126-168. Akam, S.A., Coffin, R.B., Abdulla, H.a.N., and Lyons, T.W. (2020). Dissolved Inorganic Carbon Pump in Methane-Charged Shallow Marine Sediments: State of the Art and New	Accepted - the sentence is revised, Weber et al is cited
16309	34	12	34	12	However, recent study yielded a global diffusive CH4 flux of 2–6 TgCH4 yr-1 from the ocean to the atmosphere. Combined with constraints on bubble-driven ebullitive fluxes, total oceanic CH4 emissions corresponded to 6–12 TgCH4 yr-1 (Weber et al., 2019). Weber, T., Wiseman, N.A. and Kock, A., 2019. Global ocean methane emissions dominated by shallow coastal waters. Nature Communications, 10(1): 4584. [Xinghui Xia, China]	Accepted - changes are made.
8797	34	12	34	14	This is based on the work of just one group (Shakhova et al). New work by Dyonisius et al. found that methane emissions from old, cold-region carbon reservoirs like permafrost and methane hydrates may be small https://science.sciencemag.org/content/367/6480/907. Also see review by Dean et al https://agupubs.onlinelibrary.wiley.com/doi/full/10.1002/2017RG000559 [Vaishali Naik, United States of America]	Taken into account. That's true, but they have used direct measurement even though may be sporadic. Indeed we can take lessons from the past, but the what is happening at present not straightforwardly linked to deglacial period. Dean et al. is cited

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
52151	34	12	34	14	The very high CH4 emissions presented in Shakova et al. are under debate; see Thornton, B. F., Geibel, M. C., Crill, P. M., Humborg, C., and Morth, C. M.: Methane fluxes from the sea to the atmosphere across the Siberian shelf seas, Geophysical Research Letters, 43, 5869-5877, 2016. see also Thornton, B. F., Prytherch, J., Andersson, K., Brook, I. M., Salisbury, D., Tjernstrom, M., and Crill, P. M.: Shipborne eddy covariance observations of methane fluxes constrain Arctic sea emissions, Science Advances, 6, 2020. [Hermann Bange, Germany]	Accepted. The sentence is revised, but a question is left in mind that CH4 emissions from all of the Arctic shelf could be 2-3 times greater than the estimation (~3 Tg/yr) of Thornton et al for the East Siberian Arctic Shelf alone.
12367	34	12	34	16	The discussion of the CH4 emission estimates from the Arctic shelf should address in some more detail the large uncertainties of these estimates. E.g. [Shakhova et al., 2014] estimated CH4 emissions from the East Siberian Arctic Shelf (ESAS) of 17 Tg yr-1 (based on measurements of bubble flux and seawater CH4 levels), while atmospheric flux inversions reported by [Berchet et al., 2016] gave much lower estimates (0.0 to 4.5 TgCH4yr−1) for the ESAS region. Shakhova, N., Semiletov, I., Leifer, I., Sergienko, V., Salyuk, A., Kosmach, D., Chernykh, D., Stubbs, C., Nicolsky, D., Tumskoy, V., and Gustafsson, R.: Ebullition and storm-induced methane release from the East Siberian Arctic Shelf, Nat. Geosci., 7, 64–70, doi:10.1038/ngeo2007, 2014. Berchet, A., Bousquet, P., Pison, I., Locatelli, R., Chevallier, F., Paris, J.-D., Dlugokencky, E. J., Laurila, T., Hatakka, J., Viisanen, Y., Worthy, D. E. J., Nisbet, E., Fisher, R., France, J., Lowry, D., Ivakhov, V., and Hermansen, O.: Atmospheric constraints on the methane emissions from the East Siberian Shelf, Atmos. Chem. Phys., 16, 4147–4157, https://doi.org/10.5194/acp-16-4147-2016, 2016. [Peter Bergamaschi, Italy]	Accepted - changes are made.
88191	34	12	34	16	It is important to note that the warming of permafrost beneath the Arctic shelf is in large part due to marine transgression following deglaciation, i.e. these areas were above sea level during glaciation and exposed to much colder conditions which resulted in formation of thick permafrost and stability conditions for gas hydrate. [Sharon Smith, Canada]	Noted.
31935	34	14			Shakhova's papers are very controversial and not supported by in situ measurement in Svalbard etc. If the methane is being released, it is not getting into the air. Better papers to cite here are: France, James L., et al. "Measurements of δ13C 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): 14-257. Also: Berchet, Antoine, et al. "Atmospheric constraints on the methane emissions from the East Siberian Shelf." Atmospheric Chemistry and Physics 16.6 (2016): 4147-4157. Also: Fisher, Rebecca E., et al. "Measurement of the 13C isotopic signature of methane emissions from northern European wetlands." Global Biogeochemical Cycles 31.3 (2017): 605-623. [Euan G. Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - the sentence is revised, Berchet et al is cited

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31937	34	14			"These emissions are 2-9 Tg - the word 'these' seems to refer to Arctic emissions? I don't think 9Tg is supported by any recent work other than Shakhova et al. which is not consistent with the direct in situ observations from air monitoring stations. Berchet et al find 0 to 4.5 Tg and most other direct atmsopheric measurement points to the lower end of this range. [Euan G. Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text was revised to include the results of Berchet et al, but for the future assessments we need more confidence on the estimation of coastal ocean seepage using atmospheric data
16063	34	15	34	15	"...although [these emissions] are likely to increase in a warmer world..." - the description of our knowledge about the processes leading to these emissions (preceding sentence) sounds very uncertain ("possibly triggered by..."), a bit at odds with the fact that you have high confidence that these emissions will increase (which is what the likelihood statement implies) [Gerhard Krinner, France]	Accepted - sentence revised; the likelihood statement removed
58645	34	16	34	17	Would be better, for the reader's understanding, to move the "around the world" on line 17 directly after the "All geological sources" on line 16. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - changes are made.
71219	34	16	34	20	Relevant publication: Hmiel, B., Petrenko, V. V., Dyonisius, M. N., Buizert, C., Smith, A. M., Place, P. F., Harth, C., Beaudette, R., Hua, Q., Yang, B., Vimont, I., Michel, S. E., Severinghaus, J. P., Etheridge, D., Bromley, T., Schmitt, J., Faïn, X., Weiss, R. F., and Dlugokencky, E.: Preindustrial 14CH4 indicates greater anthropogenic fossil CH4 emissions, Nature, 578,409-412, 10.1038/s41586-020-1991-8, 2020. [Ragnhild Skeie, Norway]	Accepted
12369	34	16	34	20	The discussion of the geological emissions should explain that the estimates by [Etiope et al., 2019] are bottom-up estimates, while the estimate of [Petrenko et al., 2017] are based on 14CH4 in ice cores. Furthermore, the recent study of [Hmiel et al., 2020], which estimated even lower emissions of 1.6 Tg CH4 yr-1 (with upper estimate of 5.4 Tg CH4 yr-1) from 14CH4 measurements in pre-industrial firn air and ice core samples should be included here: Hmiel, B., Petrenko, V.V., Dyonisius, M.N. et al. Preindustrial 14CH4 indicates greater anthropogenic fossil CH4 emissions. Nature 578, 409–412 (2020). https://doi.org/10.1038/s41586-020-1991-8 [Peter Bergamaschi, Italy]	Accepted - changes are made.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
14935	34	16		17	The report relies on Etiope et al. 2019 to conclude that natural geological seeps are in the range of 35 to 76 Tg per year; many of us have long felt the extrapolations by Etiope and colleagues are based on too limited of a data set. And now, the work of not on Petrenko et al. (2017) -- which is mentioned in this sentence -- but also Hmiel et al. 2020 have cast severe doubt on the estimates of Etiope et al. (2019). The IPCC language here understates how much Petrenko et al. undercut Etiope's estimates, saying Petrenko et al. results show that the natural seep are likely less than 15 Tg per year; a careful read of that paper shows these natural seep emissions are likely less than 2 or 3 Tg per year. And whatever doubt there may have been from the Petrenko et al. study were settled by Hmiel et al. (2020): they showed using 14C in methane from ice core samples that natural geological seeps were negligible over the entire last 10,000 years plus. Only with the advent of the industrial revolution does the 14C-methane data start to show a fossil signal. THIS IS CRITICAL TO UNDERSTANDING THE MODERN BALANCE OF BIOGENIC AND FOSSIL FUEL METHANE EMISSIONS, AS I HAVE NOTED ABOVE. The fossil fuel emissions are 50 Tg per year or so greater than is assumed by Etiope and others who contributed to this chapter, and the anthropogenic biogenic sources must be small er by the same amount. [Robert Howarth, United States of America]	Rejected - your point well taken, but we cannot just change the anthropogenic biogenic emissions until a clear source sectors are estimated by emission inventory processes. It should be noted that the top-down emission estimations do not actually use 50 Tg/yr or so geological seeps but a much smaller amount ~10 Tg/yr
115349	34	16		19	What is the difference between 'geological sources' and 'ventilation of geological CH4' - why are these numbers different? [Gillett Nathan, Canada]	Accepted -this discussion is increased
31939	34	17			Geological sources - Etiope et al. This high estimate has been very strongly attacked by the Petrenko group - see Hmiel et al and Dyonisius et al, (cited above in p 32 l 15 comment). The debate is really important. If the Petrenko et al team are correct, fossil fuel emissions are much larger than hitherto supposed. This whole paragraph needs very substantial revision: it needs to take into account recent work on Arctic, open ocean, and Geological sources [Euan G. Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - we have tried our best to strike balance between many confusing emission sectors. Hopefully we are able to maintain neutrality
127749	34	18	34	18	It would be helpful to clarify what is meant by the ventilation of geological CH4 and how that differs from or is part of the geological sources described in the previous sentence. [Trigg Talley, United States of America]	Accepted - text is revised
41623	34	18	34	20	The term "ventilation" is not clear and should be explained [Katharina Meurer, Sweden]	Not applicable - text changed.
31945	34	22	34	39	This paragraph needs rethinking. [Euan G. Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Rejected/accepted - not sure what should be rethought. However, we have made changes to improve the subsection
9425	34	23	34	23	Add "the" before "inland water CH4 source" [Christine Weldrick, Australia]	Accepted - change was made.
8799	34	23	34	24	The numbers do not match with those in Table 5.2. Which year? [Vaishali Naik, United States of America]	Accepted - changes are made.
39017	34	25	34	25	Missing reference in the biblio for 'Stanley et al. 2016' [Alexandra Rouillard, Sweden]	Accepted
58665	34	25			The reference "Bastviken et al., 2011" is missing in the Ref of the chapter. The full reference is : "Freshwater methane emissions offset the continental carbon sink David Bastviken, Lars J Tranvik, John A Downing, Patrick M Crill, Alex Enrich-Prast 2011 Science DOI: 10.1126/science.1196808" [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - the reference was added, thank you

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
58667	34	25			The reference "Stanley et al., 2016" is missing in the Ref of the chapter. The full reference is : "The ecology of methane in streams and rivers: patterns, controls, and global significance Emily H Stanley, Nora J Casson, Samuel T Christel, John T Crawford, Luke C Loken, Samantha K Oliver Ecological Monographs, 2016 https://doi.org/10.1890/15-1027" [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - the reference was added, thank you
58669	34	25			The reference "DelSontro et al., 2018" is missing in the Ref of the chapter. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - the reference was added, thank you
16311	34	26	34	26	Current global estimates do not account for ebullition, despite it can constitute a substantial contribution to fluxes in both lentic and lotic systems (Engram et al., 2020; Zhang et al., 2020), increasing the magnitude of CH4 emissions from inland water to an even larger degree. Engram, M., Walter Anthony, K.M., Sachs, T., Kohnert, K., Serafimovich, A., Grosse, G. and Meyer, F.J., 2020. Remote sensing northern lake methane ebullition. Nature Climate Change. Zhang, L., Xia, X., Liu, S., Zhang, S., Li, S., Wang, J., Wang, G., Gao, H., Zhang, Z., Wang, Q., Wen, W., Liu, R., Yang, Z., Stanley, E.H. and Raymond, P.A., 2020. Significant methane ebullition from alpine permafrost rivers on the East Qinghai-Tibet Plateau. Nature Geoscience, 13(5): 349-354. [Xinghui Xia, China]	Accepted
73039	34	26	34	27	References should be in chronological order. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial - this kind of issues will be taken care of during the production phase, which includes professional copy-editing.
21821	34	26	34	30	uncertainty with high confidence seems to go against the grain of the uncertainty guidance. It would be better surely to say that there was low confidence in the exact value? [Peter Thorne, Ireland]	Accepted - sentence revised
8801	34	26	34	39	The bottom line is that there are large uncertainties in the estimates of CH4 emissions from inland waters that renders the budget of methane uncertain and that emission changes since AR5 are partly due to updates in the datasets. I think this can be said more succintly. [Vaishali Naik, United States of America]	Accepted - changes are made.
9427	34	27	34	27	Add "the" before "global area of them..." [Christine Weldrick, Australia]	Accepted - change was made.
58671	34	27			The reference "Crawford et al., 2017" is missing in the Ref of the chapter. The full reference is : "Spatial heterogeneity of within-stream methane concentrations John T. Crawford Luke C. Loken William E. West Benjamin Crary Seth A. Spawn Nicholas Gubbins Stuart E. Jones Robert G. Striegl Emily H. Stanley AGU JGR 2017 https://doi.org/10.1002/2016JG003698" [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - the reference was added, thank you
74243	34	29	34	30	Maybe reformulate the sentence to avoid “uncertain with high confidence”. I know what is meant, but it sounds confusing [Christoph Völker, Germany]	Not applicable - text changed.
64455	34	29	34	30	make the bottom-up CH4 emission estimate uncertain with high confidence. Consider rephrasing to: make the bottom-up CH4 emission highly uncertain [pierre regnier, Belgium]	Accepted

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
2795	34	29	34	30	I don't like the phrase "uncertain with high confidence" This may be the most appropriate way of conveying that we do not know about this. I would prefer a statement of what is thought with low confidence, as saying we are certain that we are uncertain adds little value. [Stephen Wilkinson, United Arab Emirates]	Accepted - sentence revised by removing "with high confidence"
16513	34	30	34	30	It is not obvious what "uncertain with high confidence" means? [William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - sentence revised
115351	34	30			Delete 'with high confidence'. Confidence assessment not needed here. [Gillett Nathan, Canada]	Accepted - changes are made.
31941	34	30			I'm unclear what the 'high confidence' refers to?? About our uncertainty? Yes: but a fast read for highlighted italics in the text suggests the opposite - that there is high confidence in freshwater emissions! I agree with Line 32 - that double counting of freshwaters and wetlands is arguably the biggest single cause of the bottom up vs top down discrepancy. My personal experience is that open water lakes have very low emissions, both in the Arctic and tropics. Any methane bubbles from the lake bottom are oxidised by methanotrophs within a few metres of water as they rise. [Euan G. Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - high certain statement was removed from here
16515	34	33	34	33	How is there medium confidence that there is double accounting? I would have thought there there either is or isn't double counting. [William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - sentence revised
127751	34	34	34	37	Rosentreter et al. (submitted): "Aquatic ecosystems are the most uncertain but potentially largest source of methane on Earth." [Trigg Talley, United States of America]	Taken into account. We have noted the suggestion and wait for the publication to come out
12371	34	35	34	37	"reduced double counting" should decrease the emission estimates, and not increase them [Peter Bergamaschi, Italy]	Accepted - sentence revised
39855	34	35		36	"the increase in decadal emissions… double counting" This is confusing: if you reduce the double-counting, you reduce the overestimation of methane sources, right? Therefore shouldn't you expect a slight decrease in the emissions? [TSU WGI, France]	Accepted - sentence revised

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
31943	34	35			Is the warming feeding the warming? Are tropical wetlands getting warmer and wetter and making more methane? This is a really important question and should be given much more attention here, especially in view of the evidence for expansion of the meterological tropics. Staten, P. W., Lu, J., Grise, K. M., Davis, S. M., & Birner, T. (2018). Re-examining tropical expansion. Nature Climate Change, 8(9), 768-775. The Beaulieau paper is fine but it's only a model - should cite some real information from direct measurements. The question is central to the debate on why the post-2007 rise in global methane seems to have been led from the tropics. Is it cows? see Schaefer et al 2016, 2019 Schaefer, Hinrich. "On the Causes and Consequences of Recent Trends in Atmospheric Methane." Current Climate Change Reports 5.4 (2019): 259-274.) Or is it both wetlands AND cows see Nisbet et al 2016 (especially the detailed supp info) and Nisbet et al 2019. Or is it OH? - Turner et al 2017. This is an important problem and key to the feedback debate. If wetlands in the tropics are really increasing their emissions as warming feeds warming, then our job is much tougher to bring methane under control. However, if Petrenko's team are right and Etiope's estimates of geological emissions are much too big, then the best target for mitigation, the fossil fuel industry, is much bigger and juicier so there is hope after all. [Euan G. Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - the future projections and feedbacks are assessed in section 5.4.7 "Non-CO2 feedbacks"
127753	34	37	34	39	Move estuary emissions to the paragraph above. The mean 7 Tg CH4 yr-1 estimated by Borges and Abril (2011) includes coastal wetlands and tidal flats. Rosentreter et al. (submitted) revise global emissions from rivers, lakes, estuaries, coastal wetlands, aquaculture ponds, etc., and highlight and discuss the large uncertainties, spatial and temporal variability associated with methane fluxes and the global contribution from aquatic ecosystems to atmopheric methane. [Trigg Talley, United States of America]	Taken into account. We have noted the suggestion and wait for the publication to come out
14937	34	43		49	For the reasons given above, I strongly object to this summary of the global methane budget. Further, here the authors state the increase in emissions over the past decade was 20 Tg per year ("virtually certain"), yet above they say only 7 Tg per (page 31, lines 40-42), while the recent literature indicates a higher value of 27 Tg per year (Worden et al. 2017). [Robert Howarth, United States of America]	Not applicable - We do not see much disagreement with Worden et al. and the number given here. The reference to 7 Tg/yr above may be linked to only the coal mining sector
74245	34	44	34	45	the sentence contains two wrong mixings of singular and plural [Christoph Völker, Germany]	Accepted - text changed.
31947	34	45			Should be a mention of the Geological emissions. Petrenko is probably right. This means geological emissions are vastly overestimated in the Saunois et al table. [Euan G. Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - agree with your opinion. May be we should put the values used in the atmospheric modelling
73041	34	46	34	46	Change 'freshwater' to 'freshwaters'. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change was made.
71689	34	47	34	49	“decadal mean burden increase” seems confusing here. The values given appear to be increases in the sources over each decade. But in that case the value for “2010s” is about half of what is actually given in Table 5-2 or what appears in several other recent papers. [Martin Manning, New Zealand]	Taken into account - the numbers here are about right, and we have now matched between different parts of the report. For example a mean growth rate of 7 ppb/yr would be about 20 Tg/yr burden increase

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
8803	34	47	34	49	What is the source of these numbers? Need uncertainty levels. They do not match the numbers in cross-chapter Box figure 5.1 [Vaishali Naik, United States of America]	Accepted - changes are made to match with Box 5.1
16517	34	48	34	48	It doesn't seem logical that you can be "virtually certain" that numbers are "about 42 ...". These numbers need uncertainties, and then the certainty will decrease to 90% rather than virtually certain. [William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - we have added the uncertainty which arise from the interannual variability, but it is clear that the burden changes are estimated at high accuracy, which is what meant by "virtually certain"
115353	34	48			Should not use 'about' with 'virtually certain' (P>=99%). Should specify an uncertainty range corresponding to the 99% likelihood assessment. [Gillett Nathan, Canada]	Taken into account - we have given more statistics now
127755	34	49	34	50	Why is there high confidence that inter-decadal differences are due to variability in emissions if the variability in emissions cannot be explained? Also, this seems inconsistent with the statement on page 5-36, line 32, where there is only medium (rather than high) confidence that OH isn't driving atmospheric methane concentration since 2007. [Trigg Talley, United States of America]	Taken into account - these values are derived from atmospheric concentration measurement a very high accuracy
8805	34	49	34	51	Methane growth is being driven by an imbalance in its sources and sinks. In decades with stronger methane growth, its sources outweigh its sinks...so I dont think it is accurate to say that CH4 growth rate is driven by changes in emissions rather than changes in OH (or the sum of all sinks). This is addressed in the cross-chapter box as well as chapter 6. Also see He et al 2019 https://www.atmos-chem-phys.net/20/805/2020/acp-20-805-2020.pdf. [Vaishali Naik, United States of America]	Accepted. The sentence is revised in light of Box 5.1
71691	34	49	34	52	It is important to note that variability in OH is very uncertain, but this section should be consistent with other parts of the chapter that show evidence for an increasing trend in OH. [Martin Manning, New Zealand]	Accepted - the OH related discussions are now done in collaboration with Chapter 6 and other scientists
115355	34	49		52	The likelihood assessment should probably be replaced by confidence assessments, unless underlying quantitive information exists to estimate the probabilities. [Gillett Nathan, Canada]	Accepted - changes are made.
31949	34	49			"very likely" that the growth is driven by emissions not OH. While that is my own view (and see text below on page 36 lines 19-32), I would caution here. There is strong and persuasive evidence that the oxidative capacity of the air does change, longitudinally as well as latitudinally, and while it is likely that OH is not the main cause of the recent methane growth, it is also possible that OH is changing on a longer time scale. See Nicely et al and Naus et al (cited on p36 lines 19-32) and also extensive discussion in Nisbet et al 2019. I would suggest toning this 'very likely' down by deleting the 'very' [Euan G. Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - Yes, of course
16519	34	51	34	51	"interannual or shorter". Presumably "shorter" means less than a year - which seems self evident since OH has an annual cycle. [William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable - we probably do not have to change anything here

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
58749	35	3	35	4	It is slightly confusing that the text in section 5.2.2 refers repeatedly to the timespan of 2010-2017, while Table 5.2 only gives numbers for 2008-2017, which end up being consistently different from what is reported throughout the text. Additionally, in Figure 5.14, it says that the numbers in the diagram are per Table 5.2, but they do not match, as the diagram is from 2010-2017. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - numbers and time periods in this figure and Table 5.2 for the recent decade are matched
40207	35	3			Figure 5.14: I find it confusing that the numbers in the tables don't seem to correspond to the numbers in the figure [TSU WGI, France]	Accepted - numbers and time periods in this figure and Table 5.2 for the recent decade are matched
104813	35	4	36	26	Would not atmospheric methane emission increases from 2006 be assoiciated with increases in oil and gas extraction and changes in oil and gas practices such as fracking (Industrial sources) and greater volumes of methane from permafrost melting? Fugitive fracking gas emissions have been proposed as a possible source for a rise of over 30% in US methane emissions in the decade before 2015 by linking it to a nine-fold increase in large scale fracking in the US over the period 2002 to 2014 (Turner et al. 2016). In the US, fracking development has been economically cushioned as a raw material source, significantly increasing production capacity for plastic and petrochemicals (Holden 2019). A. J. Turner D. J. Jacob J. Benmergui S. C. Wofsy J. D. Maasakkers A. Butz O. Hasekamp S. C. Biraud (2016). A large increase in U.S. methane emissions over the past decade inferred from satellite data and surface observations, AGU100, Open Access – On-line, 06 February 2016, https://agupubs.onlinelibrary.wiley.com/doi/full/10.1002/2016GL067987 Holden, Emily (2019) Will a push for plastics turn Appalachia into next 'Cancer Alley'? Will a push for plastics turn Appalachia into next 'Cancer Alley'? he Guardian, accessed 1/2/2019 at https://www.theguardian.com/environment/2019/oct/11/plastics-appalachia-next-cancer-alley-fracking-public-health-ethane [Paul Dumble, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - outside the scope. Many sectors, different for different regions, contributed to CH4 increase in the post-2006 period. We have shown regional emission trends from a global CH4 budget perspectives in Box 5.1, Fig. 2. Yes, an increase in CH4 emissions are seen in the recent decade, a mean increase of 6 Tg/yr is seen in he period 2000-2017. Unfortunately, not each regional details can be covered in this 2-page Box.
104815	35	4	36	26	The OH box theory seems to have been shown to be not significant (does it really take a page and half to come to this conclusion). The Rigby et al 2017 study only concentrates on the presence of a specific reaction species 1,1,1-trichloroethane (CH3CCl3) to estimate OH,- box models need scope out species affected by OH and relate this to the source of the OH. In the case of CH3CCl3 there is mo obvious source of recurring OH, so this will decliine as observed by authors. The likely dominant species is from a combination of radiative agents CH4 and CO2 in the atmoshere forming intermediate chain linked radicals CH3CO* and OH* . In the atmosphere the OH will remain constant in relation to the quantity of CH4 (in the presence of sufficient CO2). CH4 emitted to the atmosphere will remain linked to ground conditions (e.g moisture of soil) and activities (e.g. burning). [Paul Dumble, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - details outside scope. The reaction of CH4 with OH removes about 90% of the CH4 in the atmosphere. It is the single most important component of CH4 budget. Since the OH box model development is new since AR5, we have devoted some discussions here, keeping in mind the overall readership of the ARs. however, the OH discussion part is now shortened.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
104817	35	4	36	26	The proposed box misses out the impacts of desertification on methane emissions. In a field study of 128 landfills in temperrate climate California the methane emissions for many of sites there was aa variation of methane oxidation from O% to 100% over annual periods (Spokas et al. 2011, Spokas et al 2015). This is likely to be due to the soil/ waste moisture content (Hartz and Ham 1983) where 100% oxidation occurs below 10% moisture level As moisture loss is led by evapotranspiration and reduced rainfall, drought conditions in the State maintained since about 1990 appear to have reduced the methane emissions, though the emission reduction may be due to underground or surface fires. The impacts of seasonal permafrost changes, annual moisture changes in soil/ waste on methane emissions are more likly to account for the annual variations. Spokas K, Bogner J, Corcoran M, Walker S. (2015). From California dreaming to California data: Challenging historic models for landfill CH4 emissions.2015. Elem Sci Anth. 2015;3:51. DOI:http://doi.org/10.12952/journal.elementa.000051, Spokas K, Bogner J, Chanton J. 2011 . A process-based inventory model for landfill CH4 emissions inclusive of seasonal soil microclimate and CH4 oxidation. J Geophys Res 116 (G4): G04017 https://doi.org/10.1029/2011JG001741 Hartz, K.E., and R. K. Ham (1983) Moisture level and movement effects on methane production rates in landfill samples, Waste Management & Research, 1. 139-145, https://journals.sagepub.com/doi/pdf/10.1177/0734242X8300100116 [Paul Dumble, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - outside the scope. Our CH4 budget takes into account for the soil sink, depending on the surface conditions and atmospheric CH4 concentration. However, site specific discussion are difficult to accommodate in this assessment. Such evaluations are done by the model developers.
8807	35	11	35	11	At a minimum, this box should end with an overall confidence statement on the assessed understanding of the primary driver of methane trends in the post AR5 time period, which can then be used in the TS/SPM. [Vaishali Naik, United States of America]	Taken into account - we have done significant work with your help to simplify and summarise the Box. Thank you
112023	35	11	36	55	In this assessment of the changes in methane growth rate, I find that the authors are not clearly articulating their final assessment. They sort of finally say the growth rate post 2007 is due to agriculture and fossil fuels, but I find that this statement is a bit buried in the summaries of all the many studies on this topic. I suggest that the authors make a clear, concise statement at the end of the box that says EXACTLY what their assessment is on this topic. [Cynthia Randles, United States of America]	Accepted - changes are made to better clarify the outcome
72227	35	11	37	26	The discussion in Cross-Chapter Box 5.1 omits several study reports that indicate a much greater contribution to the CH4 Budget from anthropogenic sources than indicated in the discussion. In particular the Cross Chapter Box discussion fails to fully assess the contributions from fossil fuels sources, and in particular the contributions from the production and distribution of natural gas, particularly in North America. See these sources (all omitted from the Cross Chapter Box discussion): Hmiel et al 2020; Zhang et al 2020; Negron et al 2002; Plant et al. 2019; Höglund-Isaksson et al. 2020; Lan et al 2019; Sheng et al 2018; and Howarth 2019. Full references for all these studies are give in comments #19-#26 above. [Hunter Cutting, United States of America]	Taken into account - key references are included

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
103131	35	11			Cross chapter box: It is not clear what the function of this box is. Why can the material not be presented in the core text? It is interesting and useful, but in several instances duplication (e.g., 5.2.2.1 and 5.2.2.2) with inconsistent figures e.g. on growth rates (15 vs. 18 ppb/yr in the 1980s). [Philippe Tulkens, Belgium]	Taken into account - the Box allows us a more focused discussion on the CH4 growth rate issue. Inconsistencies in the values resolved
12373	35	20	35	20	I assume that you are referring here to Figure 5.13 (and not to "Figure 5.12") [Peter Bergamaschi, Italy]	Accepted - change was made.
8809	35	20	35	20	wrong figure number. I think this should be Figure 5.13 [Vaishali Naik, United States of America]	Accepted - change was made.
71693	35	20			(Box 5.1) – This is presumably meant to cite Fig 5.13. Also, when citing a numeric value for the growth rate, it would be more relevant to cite the data sources than this figure. [Martin Manning, New Zealand]	Accepted
73043	35	21	35	21	Change 'the' to 'these'. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change was made.
71695	35	21	35	23	When doing a similar comparison of ENSO and NOAA/ESRL CH4 growth rate during our preparation of Nisbet et al, 2019, I concluded that there was no clear correlation. Looking at Fig 5.13b now I would still make the same conclusion. For the statement to be more than a subjective comment it should bring in a quantitative metric for statistical analyses of correlation such as one of the Pearson coefficients. [Martin Manning, New Zealand]	Accepted - statistics given
31951	35	21			Is it possible to update this to include 2019? The growth rate curve should have settled by the time this draft is edited? [Euan G. Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - the time series is plotted until 2019.
127757	35	22	35	22	Delete "covering 2014-2015". It is repetitive with the last sentence. [Trigg Talley, United States of America]	Accepted - change was made.
73045	35	22	35	22	Delete 'covering 2014-2015'. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change was made.
12375	35	23	35	23	I assume that you are referring here to Figure 5.13b (and not to "Figure 5.12b") [Peter Bergamaschi, Italy]	Accepted - change was made.
127759	35	26	35	29	Why did AR5 and SR1.5 need to "anticipate" the renewed growth? CH4 has been increasing since 2007. They don't need to anticipate. Perhaps authors mean that AR5 and SR1.5 did not factor in the renewed growth in their projections because the renewed growth is not fully understood. [Trigg Talley, United States of America]	Accepted - change was made.
31953	35	31			?Mention also Nisbet et al 2020 - has a discussion of the mitigation challenges. Nisbet, E. G., et al. "Methane mitigation: methods to reduce emissions, on the path to the Paris agreement." Reviews of Geophysics 58.1 (2020): e2019RG000675. [Euan G. Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. CH4 mitigation issue is discussed explicitely in Section 5.6.2.2.5, and there this reference is cited. Here we have focussed the discussions in emission and loss processes.
14939	35	33		42	This synthesis budget is very problematic, for the reasons I raise above. I strongly urge the IPCC to revise this, and better indicate that much of the recent literature challenges the balance between biogenic and fossil fuel emissions shown here. [Robert Howarth, United States of America]	Rejected - without more concrete results on contemporary fossil fuel emission inventory, we are a bit helpless here.
58289	35	37	35	39	What is "land-based geologic"? Consider rephrasing [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - the sentence is revised
73047	35	38	35	38	Change 'geologic' to 'geology'. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable - text changed.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
73049	35	39	35	39	Change 'geologic' to 'geology'. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable - text changed.
31955	35	39			Oceans - also the plankton phosphonate source. As for geologic emissions to air from deep oceans - they are zero. Any methane bubbles are taken up within 100m of rising unless the source is truly vast. DIssolution and methanotrophy. There is excellent evidence in two examples - off Spitsbergen Westbrook et al watched big bubble trains rise from decaying hydrates and showed that nothing reached surface. We also flew over the same big plumes at 16m altitude (scary in a 4-engine jet: those waves looked big and cold) and recorded nothing. Westbrook, Graham K., et al. "Escape of methane gas from the seabed along the West Spitsbergen continental margin." Geophysical Research Letters 36.15 (2009). Secondly, the BP Deepwater Horison was a gas blow out with a bit of oil, in 1700m. The huge gas emission got up half way and then vanished into the water as dissolution and methanotrophy took hold. While the oil rose, the now-oxidised gas probably got into the Gulf Stream and made the pH drop a little as it oxidised to dissolved CO2. [Euan G. Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - the Ocean here is to account for all oceanic sources. This sentence is revised
9429	35	49	35	49	Replace "y axis" with "y-axis" [Christine Weldrick, Australia]	Accepted - change was made.
9431	35	50	35	50	Replace "y axis" with "y-axis" [Christine Weldrick, Australia]	Accepted - change was made.
12377	35	51	35	51	"CH4 in dry-air mole fractions" is not correct - should be replaced by "CH4 dry-air mole fractions" [Peter Bergamaschi, Italy]	Accepted - change was made.
12379	35	53	35	56	I assume that the "numbers under the top x-axis" give the atmospheric growth rate, and not the "bottom-up total sinks" as suggested by the current phrasing [Peter Bergamaschi, Italy]	Taken into account - this figure is now vastly revised for simplicity
77743	36	4	36	5	A clearer explanation of C-CH4 and D-CH4 isotopes and their importance would be of assistance. [Emer Griffin, Ireland]	Accepted - change was made.
103133	36	4	36	17	Text repeats argument presented on p.31, line 11. Note that inventories do not confirm increase in biogenic emissions, a fact that is worth to be noted. See also p. 37 line 16-17 [Philippe Tulkens, Belgium]	Accepted - change was made.
12381	36	5	36	6	Reference for dD-CH4 measurements needs to be checked. The Figure (I assume that you are referring here to Figure 5.13, and not to "Figure 5.12") includes d13C-CH4 measurements, but no dD-CH4 measurements. The cited reference [Rice et al., 2016] includes only measurements until 2006. [Peter Bergamaschi, Italy]	Accepted - change was made.
39015	36	5	36	6	δD' and 'D' should rather be written as 'δ2H' and '2H' accourding to the recommendated literature (revise throughout report) - where the '2' is also placed in superscript (Coplen 2011 Rapid Commun. Mass Spectrom.). [Alexandra Rouillard, Sweden]	Accepted - change was made.
31957	36	6			The Rice et al paper is a good study and does include D/H but I'm not sure it is the most appropriate reference here as it is a modelling study and its window is up to 2009, so it really misses the post 2007 growth period. Maybe to bring it more up to date on isotopes could also cite Nisbet et al 2019, which is observation-based, though it doesn't have D/H as the data base is now only a very few labs since NOAA stopped D/H in CH4 measurement? [Euan G. Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change was made.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
12383	36	9	36	11	"concurrent decrease in thermogenic and an increase in wetland and other biogenic emissions (Nisbet et al., 2016; Schwietzke et al., 2016)" - referring to [Nisbet et al., 2016] this is not correct. Instead, [Nisbet et al., 2016] concludes: "Fossil fuel emissions may also have grown, but the sustained shift to more 13C-depleted values and its significant interannual variability, and the tropical and Southern Hemisphere loci of post-2007 growth, both indicate that fossil fuel emissions have not been the dominant factor driving the increase." [Peter Bergamaschi, Italy]	Accepted - change was made.
127761	36	9	36	17	[RISK] Consider mentioning the potential for CH4 emission with increased glacial melt. See Burns et al. (2018, Scientific Reports). [Trigg Talley, United States of America]	Accepted - change was made.
14941	36	9		14	There is another issue with the 13C approach, presented in Howarth 2019 Biogeosciences (reference given above): methane from shale gas is more depleted in 13C than is conventional natural gas, due to fractionation of methane as it migrates to conventional gas reservoirs. Since 2/3rds of all increase in natural gas production globally over the past decade came from shale gas, this makes a big difference in the overall 13C signal of average atmospheric methane. Howarth used this information to conclude that one third of the total global increase in methane from all sources was due to emissions from shale gas use and development. [Robert Howarth, United States of America]	Accepted - change was made.
39807	36	9		14	"proposed hypotheses….Thompson et al 2018a)" are all the elements separated because they are from different hypotheses? or are they supposed to be acting together? This is ambiguous [TSU WGI, France]	Accepted - change was made.
31959	36	10			Also mention Nisbet et al 2019? Nisbet, E. G., et al. "Very strong atmospheric methane growth in the 4 years 2014–2017: Implications for the Paris Agreement." Global Biogeochemical Cycles 33.3 (2019): 318-342. [Euan G. Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change was made.
12385	36	11	36	12	"a concurrent reduction in pyrogenic and increase in thermogenic emissions (Worden et al., 2017)": it should be mentioned that the analysis of [Worden et al., 2017] includes also an increase in biogenic emissions. Also it might be worth mentioning that the [Worden et al., 2017] study reconciled the previously conflicting hypothesis of increasing fossil fuels (based on ethane to methane ratios) vs. increasing biogenic sources (based on d13CH4) as main drivers for the the increase since 2007. [Peter Bergamaschi, Italy]	Accepted - change was made.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
72229	36	14	36	14	This assessment mischaracterizes the findings of Thompson et al 2018a. Contrary to the characterization in the discussion here, Thompson et al 2018a affirmed the view of Worden et al 2017 on the concurent reduction in pyrogenic emissions. This is critical for understanding the role this reduction has in masking the increase in thermogenic emissions. Thompson et al 2018a report: "Noteworthy is that the biomass burning emissions, though largely unchanged compared to the prior, decreased by 3.4 ± 1.7 Tg y−1 from 2006 to 2014, which is comparable to the result of Worden et al. (2017), who found a reduction in this source of 3.7 ± 1.4 Tg y−1 based on satellite retrievals of CH4, CO, and fire activity." This findings supports the conclusions of Worden et al which found that reduced wildfire emissions masked the increase in fossil fuel emissions, explaingin the istopic shift observed in parallel with increasing fossil fuel emissions. The authors conclude by noting: "Considering that the CH4 increase is likely to be partially due to an increase in the fossil fuel source, there may be a possibility to curb the growth by mitigating these emissions." [Hunter Cutting, United States of America]	Accepted - change was made.
73051	36	14	36	14	Move the 'submitted' reference to the end of the list. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable - by the time of publication the papers will have been accepted and published.
72239	36	14	36	17	The discussion of ethane emissions should be extended through the decades after the 1990s when rising ethane emissions associated with North American oil/gas production indicate a massive increase in methane emissions from that production. See Helmig 2016; Dalsøren et al, 2018; and Hakola & Hellén 2016. References provided in comments #30-32 above. [Hunter Cutting, United States of America]	Accepted - change was made.
12387	36	14	36	17	The discussion of ethane should include also the period after 2000, for which increasing trends have been reported (e.g. Hausmann et al., 2016; Lan et al., 2019 (and references therein)): Hausmann, P., Sussmann, R., and Smale, D.: Contribution of oil and natural gas production to renewed increase in atmospheric methane (2007–2014): top–down estimate from ethane and methane column observations, Atmos. Chem. Phys., 16, 3227–3244, https://doi.org/10.5194/acp-16-3227-2016, 2016. Lan, X., Tans, P., Sweeney, C., Andrews, A., Dlugokencky, E., Schwietzke, S., et al. (2019). Long-term measurements show little evidence for large increases in total U.S. methane emissions over the past decade. Geophysical Research Letters, 46, 4991–4999. https://doi.org/10.1029/2018GL081731 [Peter Bergamaschi, Italy]	Accepted - change was made.
8811	36	14	36	17	This statement pertains to methane growth rate during 1850-2000 and does not follow a chronological order. If there is new knowledge in the trends prior to 2000 since AR5 (a paper in 2003 does not strike to me as new knowledge) then this should be discussed in the paragraph and not at the end. [Vaishali Naik, United States of America]	Accepted - change was made.
58291	36	19	36	20	First time OH is introduced but the hydroxyl has not spelled out [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - change was made.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
71697	36	19	36	32	This paragraph gives a balanced coverage of current issues around changes in OH. However, the reference to Nicely et al 2018 is not the latest result. It would be much more relevant to replace text on lines 26-27 with a reference to Niceley et al's more comprehensive review of several atmospheric chemistry models (Nicely, J.M., et al, 2020: A machine learning examination of hydroxyl radical differences among model simulations for CCMI-1. Atmospheric Chemistry and Physics, 20, 1341-1361). This concludes that the OH removal rate for CH4 has been increasing at an average rate of 1.83% per decade over 1980 - 2015. As an indication of the relative effect that this has on the budget shown in Table 5.2, the decadal rate of OH increase can be compared to the decadal increase in removal due to the concentration increase. While the latter is larger this shows that the OH trend is significant. [Martin Manning, New Zealand]	Accepted - change was made.
103135	36	19	36	32	Would the reaction with OH cause an isotopic signature? (see paragraph before, lines 4-17) [Philippe Tulkens, Belgium]	Accepted - change was made.
31961	36	22			Should probably cite Turner et al 2019. Turner, Alexander J., Christian Frankenberg, and Eric A. Kort. "Interpreting contemporary trends in atmospheric methane." Proceedings of the National Academy of Sciences 116.8 (2019): 2805-2813. Also Ganesan 2019: Ganesan, Anita L., et al. "Advancing scientific understanding of the global methane budget in support of the Paris Agreement." Global Biogeochemical Cycles (2019). [Euan G. Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change was made.
71221	36	29	36	29	The increase in OH (more than 8%) over the last three decades, can anything be said on the role of OH for the atmospheric CH4 trend prior to 2007? [Ragnhild Skeie, Norway]	Accepted - change was made.
71699	36	30	36	32	This closing sentence is confusing. I would suggest it be more like: "Removal by OH is a major part of the methane source – sink budget and there is a growing consensus that it is increasing rather than decreasing, but not by enough for this to be a primary driver of post-2006 increases in atmospheric methane (medium confidence)." [Martin Manning, New Zealand]	Accepted - change was made.
31963	36	30	36	32	I agree with this conclusion but it's controversial!! [Euan G. Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change was made.
12389	36	34	36	34	the term "their best OH estimates" might be unclear for the reader. Replace this term e.g. by "OH estimates inferred from a multispecies inversion" [Peter Bergamaschi, Italy]	Accepted - change was made.
8813	36	34	36	35	Haven't the trends in emissions been discussed in the main text already based on Saunois et al 2019 which also includes estimates from this study. [Vaishali Naik, United States of America]	Accepted - change was made.
31965	36	34	36	35	This discussion of Turner et al maybe belongs in the previous paragraph. That ended by dismissing the Turner OH hypothesis, but here it pops up again? It would be better placed in line 22 of the paragraph before, after the reference call out for Turner. [Euan G. Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change was made.
103137	36	34	36	52	also the spatial allocation does not provide any explanation for the observed isotopic signature (see paragraph above, lines 4-17) [Philippe Tulkens, Belgium]	Accepted - change was made.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
12391	36	38	36	41	"a more consistent picture emerges for the regional trends of emission rates from high-resolution inverse modelling": according to the caption of "Cross-Chapter Box 5.1 Figure 2", the results for 1988–1999 are based on "only one inversion using 19 sites only (Chandra et al., submitted)". Using only so few sites, the derived regional trends might be questionable (since the observational constraints are rather weak). [Peter Bergamaschi, Italy]	Accepted - change was made.
8815	36	38	36	49	This discussion is too detailed and belongs better in a journal article rather than an assessment. Some questions that arise - what is this high resolution inverse modeling, are the results from this inversion consistent with those derived from multiple inversions in Saunois et al, and therefore what is the assessment. [Vaishali Naik, United States of America]	Accepted - change was made.
12393	36	42	36	43	"CH4 lifetime in any latitude band" this terms is unclear - I assume that you mean here the chemical lifetime (under the hypothetical assumption that the CH4 remains in a given latitude) [Peter Bergamaschi, Italy]	Accepted - change was made.
39729	36	42		43	"CH4 lifetime in any latitude band is one year or longer," what do you mean by that? I find it unclear. [TSU WGI, France]	Accepted - change was made.
16523	36	43	36	43	Note that chapter 6 assess the lifetime to be 9.1+/-0.9 years. This needs to be resolved by chapters 5 and 6. If they use different methodologies, which is the more appropriate - or should an average be used? At any rate there can't be two methane lifetimes in the assessment. The central value with uncertainty needs to be shown rather than saying "about 9.6". [William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change was made.
31969	36	44			suggest a paragraph break here [Euan G. Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change was made.
9433	36	46	36	46	Replace "Temperate North America" with "temperate North America" [Christine Weldrick, Australia]	Accepted
37773	36	47	36	48	CH4 budget of East Asia was summarized by Ito et al. (2019). Ito A, Tohjima Y, Saito T, Umezawa T, Hajima T, Hirata R, Saito M, Terao Y (2019) Methane budget of East Asia, 1990–2015: A bottom-up evaluation. Science of the Total Environment 676: 40–52. DOI: 10.1016/j.scitotenv.2019.04.263 [Akihiko Ito, Japan]	Accepted - change was made.
12395	36	47	36	49	the time periods seem inconsistent. You discuss here the "recent growth since 2006", but attribute this growth rate to increasing regional emissions also before 2006 ("Asia (1997–2016), ... Brazil (1988–2016)"? [Peter Bergamaschi, Italy]	Accepted - change was made.
84017	36	48	36	48	Brazil is the only country individualized among several regions. Targetting countries should be avoided. In addition, the curve showing emissions from Brazil (Cross-Box 5.1, Figure 2 i) does not show a definite growth trend, quite the contrary, the graph shows that in recent years there has been a sharp decrease in CH4 emissions from Brazil. [Marco Tulio Cabral, Brazil]	Accepted - change was made.
84117	36	48			Targetting individual countries shall be avoided. This is not the commom in IPCC method. [Marco Tulio Cabral, Brazil]	Accepted - change was made.
8817	36	49	36	52	Has this study been corroborated by other studies? The conclusions of this study appear contradictory to He et al. (2019) especially the "only minor role of OH". [Vaishali Naik, United States of America]	Accepted - change was made.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
31967	36	49			The Miller et al study should be mentioned here also. 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), 1-8. [Euan G. Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change was made.
8819	37	14	37	16	As I understand, one can use either confidence level or likelihood but not both. [Vaishali Naik, United States of America]	Accepted - change was made.
96571	37	14	37	26	Please add additional references. [Nicole Wilke, Germany]	Accepted - change was made.
83019	37	16	37	17	As well as fossil fuels and livestock, the other sources of methane emissions should also be mentioned e.g. wetlands, or more general 'biogenic' emissions - see the references below: "Biogeosciences", 16, 3033–3046, 2019 https://doi.org/10.5194/bg-16-3033-2019 "Ideas and perspectives: is shale gas a major driver of recent increase in global atmospheric methane?" Robert W. Howarth Department of Ecology and Evolutionary Biology, Cornell University, Ithaca, NY 14853, USA Correspondence: Robert W. Howarth (howarth@cornell.edu)"Long-Term Measurements Show Little Evidence for Large Increases in Total U.S. Methane Emissions over the Past Decade" Xin Lan*,1,2, Pieter Tans1, Colm Sweeney1, Arlyn Andrews1, Edward Dlugokencky1, Stefan Schwietzke1,2, Jonathan Kofler1,2, Kathryn McKain1,2, Kirk Thoning1, Molly Crotwell1,2, Stephen Montzka1, Benjamin R. Miller1,2, and Sébastien C. Biraud3. Please cite this article as doi: 10.1029/2018GL0817313 "A 21st-century shift from fossil-fuel to biogenic methane emissions indicated" by 13CH4 Hinrich Schaefer,1* Sara E. Mikaloff Fletcher,1 Cordelia Veidt,2 Keith R. Lassey,1†Gordon W. Brailsford,1 Tony M. Bromley,1 Edward J. Dlugokencky,3 Sylvia E. Michel,4 John B. Miller,3 Ingeborg Levin,2 Dave C. Lowe,1‡ Ross J. Martin,1 Bruce H. Vaughn,4 James W. C. White4 80 1 APRIL 2016 • VOL 352 ISSUE 6281 sciencemag.org SCIENCE"Rising methane: A new climate challenge The amount of the greenhouse gas methane in Earth's atmosphere is rising rapidly" By Sara E. Mikaloff Fletcher and Hinrich Schaefer 932 7 JUNE 2019 • VOL 364 ISSUE 6444 Science Nisbet et al (2019). https://agupubs.onlinelibrary.wiley.com/doi/10.1029/2018GB006009 [Dan Zwartz, New Zealand]	Accepted - change was made.
39777	37	16			"likely, medium agreement" 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: https://wg1.ipcc.ch/SR/documents/ar5_uncertainty-guidance-note.pdf [TSU WGI, France]	Accepted - change was made.
31971	37	16			I'd think very likely ! [Euan G. Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change was made.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
72241	37	17	37	17	Given the additional references provided here (Hmiel et al 2020; Zhang et al 2020; Negron et al 2002; Plant et al. 2019; Höglund-Isaksson et al. 2020; Lan et al 2019; Sheng et al 2018; Howarth 2019; Helmig 2016; Dalsøren et al, 2018; and Hakola & Hellén 2016. Full references provide in comments above) the liklihood statement for the contribution of fossil fuels to the resumed CH4 growth since 2007 here is much too low. It is extremely likely that fossil fuels contributed to the resumed CH4 growth since 2007. [Hunter Cutting, United States of America]	Accepted - change was made.
71701	37	17	37	19	As noted earlier claims that short term variability in CH4 is related to ENSO should be backed up by giving a statistical analysis of the correlation. [Martin Manning, New Zealand]	Accepted - change was made.
8821	37	17	37	19	Based on which line(s) of evidence? Schaefer et al 2018 https://www.biogeosciences.net/15/6371/2018/ do not find a strng correlation of wetland emissions with ENSO. Turner etal (2018 https://www.pnas.org/content/115/36/8931) find ENSO to be the dominant mode of OH variability. Can the very likely be quantified? [Vaishali Naik, United States of America]	Accepted - change was made.
12397	37	19	37	21	which evidence can be provided that "the capacity to track “changes” in natural and anthropogenic emissions" has really "improved since the AR5"? [Peter Bergamaschi, Italy]	Accepted - change was made.
8823	37	19	37	21	Has been improved but still highly uncertain. as discussed in the main text. [Vaishali Naik, United States of America]	Accepted - change was made.
31973	37	21			but our ability to track sources has worsened with the suspension of D/H isotopic measurement by NOAA for budget reasons when the old extraction line was judged a health and safety risk. To solve global methane, models are not enough. I know it is not a popular view - funding agencies believe models rule, OK - but we really need to have just a few actual real in situ measurements and D is valuable. Methane has 3 dimensions: mole fraction, 13C and D/H. Losing one of those dimensions makes the task harder. NOAA's heroes are trying really hard on a tiny budget to sustain in situ measurement: they need support (and it's not just NOAA - I recall the European Union closed down its in situ methane C isotope measurements in the East longitude Arctic in 2007 because they supposedly could be done accurately and precisely by models backed by FTIR from space in the glorious reflected sunlight of the polar winter). We do actually need one or two real boots-on-the-ground measurements occasionally! [Euan G. Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change was made.
9435	37	22	37	22	Replace "Temperate North America" with "temperate North America" [Christine Weldrick, Australia]	Accepted - change was made.
72243	37	22	37	23	With the provision of additions references here (Hmiel et al 2020; Zhang et al 2020; Negron et al 2002; Plant et al. 2019; Höglund-Isaksson et al. 2020; Lan et al 2019; Sheng et al 2018; Howarth 2019; Helmig 2016; Dalsøren et al, 2018; and Hakola & Hellén 2016. Full references provide in comments above) there is enough evidence to be more definitive and specific,e g. increases in methane emissions from temperate North America are primarily the result of expanding oil and gas production. [Hunter Cutting, United States of America]	Accepted - change was made.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
77745	37	22	37	24	Only mentions Asia and North/South American regions. What do the datastes/inventories suggest for the other regions e.g. Europe? [Emer Griffin, Ireland]	Accepted - change was made.
84119	37	23			Targetting individual countries shall be avoided. This is not the commom in IPCC method. [Marco Tulio Cabral, Brazil]	Accepted - change was made.
31975	37	23			No mention of Africa? Huge cattle populations, enormous wetlands, world focus of biomass burning. I'd strongly suggest a mention of Lunt et al 2019 somewhere in this text Lunt, Mark F.; Palmer, Paul I.; Feng, Liang; Taylor, Christopher M.; Boesch, Hartmut; Parker, Robert J.. 2019 An increase in methane emissions from tropical Africa between 2010 and 2016 inferred from satellite data. Atmospheric Chemistry and Physics, 19 (23). 14721-14740. https://doi.org/10.5194/acp-19-14721-2019 [Euan G. Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change was made.
73053	37	24	37	24	Change 'sources' to 'source'. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change was made.
127763	37	24	37	26	Revise this sentence for clarity. [Trigg Talley, United States of America]	Accepted - change was made.
12399	37	24	37	26	regarding the requirements to improve the top-down methods for the global stock take you may include also the following references: Bergamaschi, P., A. Danila, R. F. Weiss, P. Ciais, R. L. Thompson, D. Brunner, I. Levin, Y. Meijer, F. Chevallier, G. Janssens-Maenhout, H. Bovensmann, D. Crisp, S. Basu, E. Dlugokencky, R. Engelen, C. Gerbig, D. Günther, S. Hammer, S. Henne, S. Houweling, U. Karstens, E. Kort, M. Maione, A. J. Manning, J. Miller, S. Montzka, S. Pandey, W. Peters, P. Peylin, B. Pinty, M. Ramonet, S. Reimann, T. Röckmann, M. Schmidt, M. Strogies, J. Sussams, O. Tarasova, J. van Aardenne, A. T. Vermeulen, F. Vogel, Atmospheric monitoring and inverse modelling for verification of greenhouse gas inventories, EUR 29276 EN, Publications Office of the European Union, Luxembourg, 2018, ISBN 978-92-79-88938-7, doi:10.2760/759928, JRC111789 Pinty B., G. Janssens-Maenhout, M. Dowell, H. Zunker, T. Brunhes, P. Ciais, D. Dee, H. Denier van der Gon, H. Dolman, M. Drinkwater, R. Engelen, M. Heimann, K. Holmlund, R. Husband, A. Kentarchos, Y. Meijer, P. Palmer and M. Scholze (2017) An Operational Anthropogenic CO_2 Emissions Monitoring & Verification Support capacity - Baseline Requirements, Model Components and Functional Architecture, doi: 10.2760/39384, European Commission Joint Research Centre, EUR 28736 EN. Pinty B., P. Ciais, D. Dee, H. Dolman, M. Dowell, R. Engelen, K. Holmlund, G. Janssens-Maenhout, Y. Meijer, P. Palmer, M. Scholze, H. Denier van der Gon, M. Heimann, O. Juvyns, A. Kentarchos and H. Zunker (2019) An Operational Anthropogenic CO_2 Emissions Monitoring & Verification Support Capacity – Needs and high level requirements for in situ measurements, doi: 10.2760/182790, European Commission Joint Research Centre, EUR 29817 EN.	Accepted - change was made.
8825	37	24	37	26	This would be more appropirate in the knowledge gaps section [Vaishali Naik, United States of America]	Accepted - change was made.
31977	37	25	37	26	Some English polishing needed here. [Euan G. Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change was made.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
71703	37	26			Pandey et al 2019 is a paper on satellite data about a gas well blowout. So that was a highly localised event and not particularly relevant when talking about a global stocktake. [Martin Manning, New Zealand]	Accepted - change was made.
40697	37	31	42	2	section 5.2.3: Please check the use of this IPCC uncertainty language term, I suspect some misuse. The IPCC guidance note on uncertainty: https://wg1.ipcc.ch/SR/documents/ar5_uncertainty-guidance-note.pdf [TSU WGI, France]	Accepted - uncertainty language use has been corrected
40699	37	31	42	2	section 5.2.3: some subsections still need to be turned into an assessment [TSU WGI, France]	Accepted - the section has been revised accordingly
40701	37	31	42	2	section 5.2.3: the main limiting factors are not really clearly exposed here [TSU WGI, France]	Accepted - the section has been revised accordingly
103139	37	35	37	37	More precision in wording needed: " … N2O is primarily produced as a by-product … sensitive to local conditions in the substrate like temperature, … in time and space even on small scale. [Philippe Tulkens, Belgium]	Accepted - change was made.
51143	37	40	37	40	typo: “articial fertiliser” should read “artificial” [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change was made.
103141	37	40			mineral fertilizer [Philippe Tulkens, Belgium]	Not applicable - text was revised according to comment 51134
40433	37	42	37	43	incorrect use of IPCC confidence language [TSU WGI, France]	Accepted - a reference to the comprehensive assessment by Tian et al. 2020 has been added. The statement has been revised to (robust evidence, high agreement)
39813	37	42		43	"robust evidence, high confidence" assessment not really traceable in the text [TSU WGI, France]	Accepted - a reference to the comprehensive assessment by Tian et al. 2020 has been added
51145	37	50	37	51	Here, the increase in N2O is presented as 23% since pre-industrial level, while in the summary of the chapter, a figure of 22% is mentioned (chapter 5, page 11, line 42). Please ensure similar rounding of figures. In addition, while the period of reference for figure 5.17 is not explicitely stated, one would expect a smaller relative increase of approximately 17% (226+1340)/1340. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - Figure 5.17 contained a wrong number (correct would have been 1293 for pre-industrial (1750 values). This has been changed.
103143	37	50			use 2020 values (333 ppb) [Philippe Tulkens, Belgium]	Rejected, for cross-chapter compatability (Ch 2), the 2019 value was maintained
42989	37	51	37	51	The Law Dome records of N2O published in MacFarling Meure et al. (2006) have recently been revised and should be replaced by Rubino et al. (2019) - Revised records of atmospheric trace gases CO2, CH4, N2O, and δ13C-CO2 over the last 2000 years from Law Dome, Antarctica (https://www.earth-syst-sci-data.net/11/473/2019/) [Mauro Rubino, Italy]	Accepted - reference has been updated.
23705	37	52	37	52	extra) [Massimo Lupascu, Singapore]	Accepted - change was made.
40435	37	52	37	52	incorrect use of IPCC confidence language [TSU WGI, France]	Accepted - traceability added
127765	37	53	37	53	Explain the N2O growth rate. Three different data sets (NOAA, AGAGE, and CSIRO) all yield 0.85 ppb/yr from 1999-2018, and all three networks are very consistent. [Trigg Talley, United States of America]	Accepted - an inconsistency between the text and the figure was discovered and removed. The growth rate estimate has been corrected to 0.85 ppb / yr

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
73055	37	53	37	54	Remove split of numbers and units across line break. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial - this kind of issues will be taken care of during the production phase, which includes professional copy-editing.
23707	38	1	38	1	extra) after 2018 [Massimo Lupascu, Singapore]	Accepted - change was made.
73057	38	8	38	8	Capital 'S' for 'stratosphere'. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change was made.
39869	38	8		12	"this suggests…is poor" assessment/confidence statement missing [TSU WGI, France]	Not applicable - text was removed
73059	38	9	38	9	Capital 'T' for 'troposphere'. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change was made.
2797	38	14	38	14	add commas please "firn, ice, air" [Stephen Wilkinson, United Arab Emirates]	Accepted - change was made.
40437	38	14	38	15	incorrect use of IPCC confidence language [TSU WGI, France]	Accepted - text revised
116425	38	14	38	20	check spelling for 15N [Valerie Masson-Delmotte, France]	Accepted - 15N checked
73061	38	15	38	15	15 should be superscript. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change was made.
41627	38	15	38	15	The term "15N site preference" is not clear [Katharina Meurer, Sweden]	Accepted - text revised
9437	38	15	38	15	Superscript 15: 15N [Christine Weldrick, Australia]	Accepted - change was made.
58789	38	17	38	19	Some kind of confidence assessment should be associated with the attribution of the changes in the 15N composition and site preference of N2O, to the changes in nitrification and denitrification. Considering the complexity of fractionating processes within the nitrogen cycle, and sparseness of observations to constrain global nitrogen isotope budgets, suggest presenting this attribution as a credible hypothesis, but with low confidence in its accuracy. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - the text has been revised to remove the implied certainty of the attribution of the isotopic change to underlying changes in the N cycle. The discussion of the attribution is traceable in the cited literature.
9439	38	25	38	25	Superscript 15: [Christine Weldrick, Australia]	Accepted - change was made.
19971	38	36	39	8	Having in mind that "agriculture is the largest anthropogenic source of N2O emissions" (Page 38 Line 38), the map on figure 5.16 is puzzling, because it shows most of subsaharian Africa as a strongly emitting area (same as China or central US), whereas it is known that industrial agriculture is not very developed there. However the regional subpots on figure 5.16 support this strong African contribution. [philippe waldteufel, France]	Noted - Figure 5.16 does show the sum of natural and anthropogenic emissions and not anthropogenic emissions alone
40439	38	42	38	42	incorrect use of IPCC confidence language [TSU WGI, France]	Accepted - paragraph has been revised to take this comment into account
127767	38	42	38	44	This sentence is unclear. 70% of what increase? Atmospheric N2O, the 45% increase in N2O agricultural emissions since the mid-1980s? [Trigg Talley, United States of America]	Accepted - clarified that this refers to emissions
39727	38	48			"as mentioned in the SRCCL" you mention it was discussed but not, what was the conclusion/assessment of the report [TSU WGI, France]	Accepted - paragraph has been revised to take this comment into account
103145	38	48			non-linearity of emissions should be mentioned here: "Increasing evidence from field measurements (Song et al. https://doi.org/10.1021/acs.est.8b03931) to inventories (Wang et al., doi: 10.1093/nsr/nwz087) and inverse modelling (Thompson et al., https://doi.org/10.1038/s41558-019-0613-7) point to a non-linear relationship of N2O release as a function of fertilizer application, where excess fertilizers contribute stronger to the release (Shcherbak et al.). [Philippe Tulkens, Belgium]	Accepted - references have been added. The paragraph has been revised

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
29181	39	11	39	21	There is evidence presented by Thompson et al. (2019) that emissions from adipic acid production in China may have increased since 2005, where N2O mitigation technology has not been implemented in new adipic acid production plants. [Eric Davidson, United States of America]	Accepted - reference added to point out that not all industrial emissions related to adipic acid production have declined globally
73063	39	20	39	21	Remove split of numbers and units across line break. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial - this kind of issues will be taken care of during the production phase, which includes professional copy-editing.
41631	39	21	39	21	In general, citing studies that have not gone through pee-review yet is a rather dangerous issue. In case that all studies will be accepted and published by the time the report will be published, there might not be a bigger problem, of course. However, as many statements are solely based on "submitted" studies, the paragraph becomes less credible. This also applies to the rest of the report, of course [Katharina Meurer, Sweden]	Accepted - the study was published so in this particular case the concern does not hold anymore.
40441	39	21	39	21	incorrect use of IPCC confidence language [TSU WGI, France]	Accepted - confidence language has been removed
127769	39	27	39	28	This is a similar challenge to that of attributing freshwater methane emissions as either natural or anthropogenic (since some of these emissions are due to reservoirs and canals, and emissions generally increase with more nutrient loading). This point could be made clearer in the discussion about methane sources. [Trigg Talley, United States of America]	Accepted - text has been revised to take this challenge into account
52159	39	33	39	33	Important publications on rivers and lakes and estuaries have been ignored: Lauerwald, R., Regnier, P., Figucircdo, V., Enrich-Prast, A., Bastviken, D., Lehner, B., Maavara, T., and Raymond, P.: Natural Lakes Are a Minor Global Source of N2O to the Atmosphere, Global Biogeochemical Cycles, 33, 1564-1581, 2019. Maavara, T., Lauerwald, R., Laruelle, G. G., Akbarzadeh, Z., Bouskill, N. J., Van Cappellen, P., and Regnier, P.: Nitrous oxide emissions from inland waters: Are IPCC estimates too high?, Global Change Biology, 25, 473-488, 2019. Yao, Y. Z., Tian, H. Q., Shi, H., Pan, S. F., Xu, R. T., Pan, N. Q., and Canadell, J. G.: Increased global nitrous oxide emissions from streams and rivers in the Anthropocene, Nature Climate Change, 10, 138-, 2020. [Hermann Bange, Germany]	Accepted - The Lauerwald and Yao references have been taken into account. The Maavara reference was already cited in the previous draft
52157	39	35	38	39	Important publication is missing and should be cited: Yang, S., Chang, B. X., Warner, M. J., Weber, T. S., Bourbonnais, A. M., Santoro, A. E., Kock, A., Sonnerup, R. E., Bullister, J. L., Wilson, S. T., and Bianchi, D.: Global reconstruction reduces the uncertainty of oceanic nitrous oxide emissions and reveals a vigorous seasonal cycle, Proceedings of the National Academy of Sciences, doi: 10.1073/pnas.1921914117, 2020. 201921914, 2020. [Hermann Bange, Germany]	Accepted - reference added
16313	39	35	39	36	Since AR5, new estimate of the global ocean N2O source is 4.2 (3.3–5.5) TgN yr-1 based on the largest observational synthesis from 1988 to 2017 (Yang et al., 2020), enabling a tightening of the global budget of this gas. Yang, S., Chang, B.X., Warner, M.J., Weber, T.S., Bourbonnais, A.M., Santoro, A.E., Kock, A., Sonnerup, R.E., Bullister, J.L., Wilson, S.T. and Bianchi, D., 2020. Global reconstruction reduces the uncertainty of oceanic nitrous oxide emissions and reveals a vigorous seasonal cycle. Proceedings of the National Academy of Sciences: 201921914. [Xinghui Xia, China]	Accepted - reference added

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
73065	39	46	39	46	Change in-situ to in situ. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change was made.
33311	40	4			Change: « N2O processes» by “Nitrous oxide processes”. It is better to avoid using abbreviation at the start of a sentence. [Guiomar Rotllant, Spain]	Accepted - change was made.
73067	40	5	40	5	Change 'and' to 'an'. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change was made.
9441	40	5	40	5	Replace "...account for and additional…" with "…account for an additional…" [Christine Weldrick, Australia]	Accepted - change was made.
74247	40	7	40	7	N2O without subscripted 2 [Christoph Völker, Germany]	Accepted - change was made.
73069	40	7	40	7	Subscript 2 required. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change was made.
58293	40	7	40	7	The "2" in N2O should be subscript [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - change was made.
9443	40	7	40	7	Subscript 2: N2O [Christine Weldrick, Australia]	Accepted - change was made.
64457	40	7	40	11	Suggest to start with "Inland waters and estuaries ….". Also for inland water emissions, I suggest to add references to Yao et al., Nature Climate Change 10, pages138–142(2020)https://doi.org/10.1038/s41558-019-0665-8 for streams and rivers and to Lauerwald et al., Global Biogeochemical Cycles, 33, 1564–1581. https://doi.org/ 10.1029/2019GB006261 for lakes [pierre regnier, Belgium]	Accepted - change was made.
33313	40	7			Change: « …can serve as N2O sinks….» by “….can serve as N2O sinks….”. [Guiomar Rotllant, Spain]	Accepted - change was made.
83391	40	8	40	11	Additional reference relevant here: P Kortelainen, T Larmola, M Rantakari, S Juutinen, J Alm, PJ Martikainen 2020. Lakes as nitrous oxide sources in the boreal landscape. Global change biology 26 (3), 1432-1445. this study based on 112 lakes shows 4 fold N2O emission from boreal lakes when winter time emisson included included. [Tuula Larmola, Finland]	Accepted - reference added
16315	40	10	40	11	New reference should be included, that is (Yao et al., 2020). Yao, Y., Tian, H., Shi, H., Pan, S., Xu, R., Pan, N. and Canadell, J.G., 2020. Increased global nitrous oxide emissions from streams and rivers in the Anthropocene. Nature Climate Change, 10(2): 138-142. [Xinghui Xia, China]	Accepted - reference added
96573	40	13	40	15	Please add a reference. [Nicole Wilke, Germany]	Accepted - change was made.
41643	40	18	42	1	I am missing information on N2O emissions from thawing permafrost in this part if the report. Please see my comment above (p 7 l 51 - p 8 l 4) [Katharina Meurer, Sweden]	Accepted - a reference to permafrost was added
11373	40	20	40	20	please insert 'microbial' before 'nitrification' - or perhaps mention references on also chemical N2O production in soil [Dan Bruhn, Denmark]	Accepted - change was made.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
37751	40	20	40	30	One recent finding on global N2O budget is the evaluation of contributions by nitrofication and denitrification. Inatomi et al. (2019) used a process-based nitrogen cycle model and estimated total terrestrial N2O emission as 16.8 Tg N2O yr-1 of which 25.2% is by nitrification. Remarkably, this study conducted a meta-analysis and refined the process-based model. Inatomi M, Hajima T, Ito A (2019) Fraction of nitrous oxide production in nitrification and its effect on soil emission: A meta-analysis and global-scale sensitivity analysis using a process-based model. Plos One 14: doi:10.1371/journal.pone.0219159. DOI: 10.1371/journal.pone.0219159 [Akihiko Ito, Japan]	Rejected - the VISIT model used in this study was part of the multi-model study by Tian et al. 2019 GCB, cited in this report. The parameterisation of this model is an interesting scientific exercise and improvement for one model. Since this is based on one model only, an assessment of the split between denitrification and nitrification is not reliably possible. It is unclear how this study adds to the assessment on the global N2O budget already made (including the VISIT model).
73071	40	23	40	24	Remove split of numbers and units across line break. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial - this kind of issues will be taken care of during the production phase, which includes professional copy-editing.
58295	40	25	40	27	Transition word needed after the comma [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - change was made.
103147	40	25			Please check: inventories do not include non-agricultural land, at least not non-anthropogenic emissions. Papers can and should be cited elsewhere (e.g., p.38 line 48) [Philippe Tulkens, Belgium]	Accepted - text has been removed
127771	40	29	40	20	"... do not suggest a trend over this time, ..." Over what time? [Trigg Talley, United States of America]	Accepted - sentence clarified to refer to 1980-2016.
73073	40	32	40	34	Move 'accurately' to after CO2. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change was made.
127773	40	34	40	34	"... this progress allows to separate ..." Not sure what this means. What progress? Do authors mean process models allow the separation of natural background emissions from other factors? [Trigg Talley, United States of America]	Accepted - text has been revised to clarify that progress refers to improve modelling capacity
11005	40	39	40	41	Important to note here is the paleo evidence for N2O emissions in response to climate warming. There is a growing body of evidence based on paleo atmospheric N2O reconstructions that N2O emissions increased as a result of warming during the last deglacial period (18 -11 ka). The sensitivity of N2O emissions to temperature are apparent in both reconstructed and modeled paleo emissions. Schilt et al. (2014) (doi.org/10.1038/nature13971) suggest that terrestrial emissions acted as a positive feedback on climate change during the last deglacial. Additionally, Fischer et al. (2019) (doi.org/10.5194/bg-2019-117), and Joos et al. (2019) (doi.org/10.1088/1748-9326/ab2353) also report rapid changes in terrestrial N2O emissions during the last deglacial. These studies support the statement here that recent global warming has been associated with an increase in natural background emissions, and are therefore important to note here as they provide paleo evidence of similar emission responses to global warming. [Emmy Wrobleski, United States of America]	Accepted - to remain concise, this text was added to the non-CO2 biogeochemical feedback section in Section 5.4.7. A reference to Section 5.4.7 was added.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
33315	40	40	41	43	Change: “of which about half occurred since the 1980s (limited evidence, moderate agreement (Tian et al., submitted b). As discussed in SRCCL, land-use change significantly alters the N2O emissions through emission pulses following conversions and changes in the area of intact ecosystems (Tian et al., submitted, b).” by “of which about half occurred since the 1980s (limited evidence, moderate agreement) (Tian et al., submitted b). As discussed in SRCCL, land-use change significantly alters the N2O emissions through emission pulses following conversions and changes in the area of intact ecosystems (Tian et al., submitted b).” When submitted articles choose one form to cite them, I would suggest better to do not include the comma before the letter, e.g. (Tian et al., submitted b). [Guiomar Rotllant, Spain]	Accepted - change was made.
40443	40	41	40	41	incorrect use of IPCC confidence language [TSU WGI, France]	Accepted - traceability added
41633	40	42	40	43	this is a very broad statement, as there are different types of conversions and changes of intact ecosystems. The overall statement is true, but studies that have shown this in particular and for specific cases should be included here, e.g. Neill et al. 2005 Nutr Cycl Agroecosys 71:1-15, doi: 10.1007/s10705-004-0378-9; Meurer et al. 2016 Environ Res Let 11(2):023001, doi: 10.1088/1748-9326/11/2/023001) [Katharina Meurer, Sweden]	Accepted - Text has been revised to refer to the SRCCL discussion on land-use change, which includes an assessment of the Meurer et al. 2016 paper
29183	40	42	40	43	Tian et al. (submitted) calculate that the reduced N2O emissions in degraded deforested lands are larger than the pulses in emissions following land use change. This is an important result that should be included. [Eric Davidson, United States of America]	Accepted - this discussion has been added.
73075	40	51	40	52	Remove split of numbers and units across line break. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial - this kind of issues will be taken care of during the production phase, which includes professional copy-editing.
96575	41	1	41	9	Additional references would be helpful. Please add references. [Nicole Wilke, Germany]	Full referencing is given in Table 5.13 and the caption to figure 5.17
73077	41	5	41	5	Change 'to assess' to 'assessment'. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change was made.
73079	41	8	41	9	Remove split of numbers and units across line break. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial - this kind of issues will be taken care of during the production phase, which includes professional copy-editing.
33317	41	15			Change: “…submitted, a).” by “…submitted a).”. Check the format all over the MS. [Guiomar Rotllant, Spain]	Editorial - this kind of issues will be taken care of during the production phase, which includes professional copy-editing.
127775	41	16	41	16	The "2000s" is not a very good start date for a comparison to 2007-2016, which also includes the "2000s". Do authors mean early 2000s? [Trigg Talley, United States of America]	Accepted - text was revised
74249	41	23	41	23	I think “with” should be “which” [Christoph Völker, Germany]	Accepted - change was made.
127779	41	23	41	23	"... with has a small negative..." Do authors mean "which includes a small negative ..."? [Trigg Talley, United States of America]	Accepted - change was made.
19309	41	23	41	23	change "with has a …" to "which has a .." [Benjamin Lamptey, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change was made.
8827	41	23	41	23	delete has in "with has a small negative…." [Vaishali Naik, United States of America]	Not applicable - text was revised according to comment 19301

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
41635	41	23	41	23	with = which (?) [Katharina Meurer, Sweden]	Accepted - change was made.
127777	41	23	41	25	This sentence is confusing. Revise for clarity. [Trigg Talley, United States of America]	Accepted - change was made.
127781	41	23	41	25	Move Prather et al (2015) after (116 +/- 9) to note that this is the source of the revised lifetime. [Trigg Talley, United States of America]	Accepted - change was made.
38507	41	23	41	28	The sentence is hard to understand and needs revision. Possibly just adding commas to separate clauses will help. [Siv K Lauvset, Norway]	Accepted - change was made.
9445	41	23	41	28	Entire section needs proofreading [Christine Weldrick, Australia]	Accepted - change was made.
8829	41	24	41	24	replace resulting with results [Vaishali Naik, United States of America]	Accepted - change was made.
73081	41	24	41	25	This sentence does not make (English) sense. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change was made.
8831	41	25	41	25	replace "is lower than assessed by AR5" with "compared with that assessed by AR5…" [Vaishali Naik, United States of America]	Accepted - change was made.
41637	41	25	41	25	, = . (?) [Katharina Meurer, Sweden]	Accepted - change was made.
33319	41	25			Change. "…(118–131 years), The…" by "…(118–131 years). The…" [Guiomar Rotllant, Spain]	Accepted - change was made.
73083	41	26	41	26	Capital 'S' for 'stratosphere'. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change was made.
33321	41	28			Change: "…al., submitted, a)." by "…al., submitted a).". Check the format all over the MS. [Guiomar Rotllant, Spain]	Editorial - this kind of issues will be taken care of during the production phase, which includes professional copy-editing.
33323	41	28			Change: "…abundance 4.5…" by "…abundance of 4.5…". [Guiomar Rotllant, Spain]	Accepted - change was made.
74253	41	29	41	29	I would insert an "of" before 4.5 [Christoph Völker, Germany]	Accepted - change was made.
41639	41	29	41	29	abundance "of" [Katharina Meurer, Sweden]	Accepted - change was made.
41641	41	34	42	2	the caption of the table is not very helpful and the explanation of the superscripts are missing. It is not clear to me what "surface sink" means [Katharina Meurer, Sweden]	Accepted - table has been revised and an appropriate caption has been added
18217	41	36	41	36	There are subscript letters in the left hand column but no footnotes/definitions. [Chelsey Baker, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - table has been revised and an appropriate caption has been added
73895	41	36	42	1	Table 5.3: How do you distinguish between 'inland water, estuaries, coastal zones' (listed under 'anthropogenic sources') and 'rivers, estuaries and coastal zones' (listed under 'natural sources amnd sinks'). I think you cannot do so and it is double counting of N2O emissions. Please list 'Rivers and Inland waters (incl. lakes and reservoirs)' under anthropogenic sources. List 'coastal zones' separately under 'anthropogenic sources'. [Hermann Bange, Germany]	Accepted - text and references have been added to explain this separation, which is described in detail by Tian et al. 2020, Nature, and relies on an assessment of natural, proximate conditions for pre-industrial state and the current N loading
73897	41	36	42	1	Table 5.3: Please rename 'oceans' to 'open oceans' in order to have a clear separation from coastal zones/coastal oceans. [Hermann Bange, Germany]	Accepted - table revised
58297	41	36	42	1	Not all column values in Table 5.3 add up to the totals (i.e. AR6 1980-1989 Natural Sources and Sinks) [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - errors in the propagation of minimum values have been corrected
40883	41	36			Table 5.3: why I the reference period of AR5 only 2006? Is that normal? [TSU WGI, France]	Accepted - Corrected to 2006/2011, as written in AR5
116427	41		42		please communicate revised lifetimesestimates to all chapters incl ch 2 and ch 6 [Valerie Masson-Delmotte, France]	Accepted - communication was made

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
8833	42	5	42	5	I dont think this section is needed here as quantification of the cliamte effect (in terms of forcing, climate metrics) is covered more thoroughly in Chapter 7. The numbers presented here look to be different from those in chapter 7 and this inconsistency will just lead to confusion about our assessment of the changes in ERF of GHGs. I strongly recommend that this section be eliminated. [Vaishali Naik, United States of America]	Not applicable. This section is now vastly revised in communication with scientists from Chapter 6 and Chapter 7. The figures are revised and no climate metric is discused or used.
31979	42	5	42	19	There are many problems with this paragraph summary, which is highly important and is very likely to be the "go to" paragraph for the Chapter. First I'll give specific details, then I'll follow with general remarks. SPECIFIC ISSUES 1) the text refers to the period 2007-16 but the Fig. 5.18 goes to 2018. 2) the paragraph is not consistent with the same material for WMGHG in Chaper 7. In particular, Chapter 7 and Table 7.A.1 and Section 7.3.2.3 (0.54+-0.11Wm2), on which Fig 5.18 seems to be based, accepts the Etminan et al 2016 Radiative Forcing revision for methane, but this text here seems not to (I'm guessing that from the numbers in the text). I'm finding it hard to replicate the % shares given here, and to reconcile Chapter 7 with Chapter 5. Now the rest of this paragraph here seems to reject AR5 methodology for other unexplained reasons but still use AR5 or AR4 Radiative forcings? 3) reference is made to Fig 2.9 - does this mean Fig 2.10?? But that is a very different figure. It's important to make clear the methane-related ERF forcings and how they are connected. 4) Line 17 says "CO2 is by far the single most important GHG, with greater increase of its radiative forcing since the 1960s". Well, that's a fairly loaded statement. I would qualify that by removing the words 'by far' and also maybe reconsider the word greater? 'absolute' increase....(note I haven't checked that- apologies as deadline looms, but I'd suspect many gases have % increases way beyond CO2) GENERAL COMMENTS To take first an extreme view, the present text could be read rather as if politically scripted by the White House - as an extreme first impression line 17-18 could be described as very Trumpian: it's almost a licence to "frack away, who cares about manure, global warming is China's coal CO2 problem only!" That's travesty of course, though I can see the gas and intensive dairy lobbies whooping with delight. To be specific, my objection is that the paragraph makes no mention of the knock-on impacts of methane on tropospheric ozone and	Not applicable. This section is now vastly revised in communication with scientists from Chapter 6 and Chapter 7. The figures are revised and no climate metric is discused or used.
114725	42	5	43	34	Re section 5.2.4: Please coordinate with ch7 [Jan Fuglestvedt, Norway]	Accepted - thank you for the suggestion.
96577	42	7	43	21	Np References in this part. Please add references. [Nicole Wilke, Germany]	Accepted - text revised
103149	42	12	42	13	synthetic gases - where are they covered in this report? A reference to the appropriate chapter (and if no information is available, to a previous assessment report) would be helpful [Philippe Tulkens, Belgium]	Not Applicable - text revised

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
103151	42	13	42	16	Especially in terms of this chapter, but also the whole context, attributing CH4 as a "short-lived pollutant" seems not correct. Its lifetime is an order of magnitude smaller than of N2O, but at least two orders of magnitude larger than other compounds discussed in Table 6.1 as SLCF. It is a compound that undergoes tropospheric chemistry reactions, so there may be some reason to also discuss it together with air pollutants. But that is to be explained in Chapter 6. A possible way to correctly assess could be to delete the sentence starting with "Chapter 6" and instead expand the sentence dealing with CH4: "CH4 as a well-mixed greenhouse gas is treated in this chapter, but it also undergoes chemical transformation in the troposphere and hence impacts on the spatial heterogeneity of radiative effects typical for the short-lived atmospheric compounds dealt with in Chapter 6." [Philippe Tulkens, Belgium]	See definition in Chapter 6.
127783	42	15	42	15	Fix sentence for clarity: "...is considered a short-lived pollutant but is also well mixed..." [Trigg Talley, United States of America]	See definition in Chapter 6.
73085	42	15	42	15	Insert 'is' before 'considered'. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - the sentence is revised.
16525	42	15	42	15	Note that chapter 6 assess the lifetime to be 9.1+/-0.9 years. This needs to be resolved by chapters 5 and 6. If they use different methodologies, which is the more appropriate - or should an average be used? At any rate there can't be two methane lifetimes in the assessment. The central value with uncertainty needs to be shown rather than saying "about 9.6". [William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change was made.
41645	42	15	42	15	are = is [Katharina Meurer, Sweden]	Noted - the sentence is revised.
73909	42	15	42	15	Table 7.A.3 in chapter 7 provides a lifetime of CH4 of 12,4 years, here it says 9,6 years which seems to be incorrect. [Anke Herold, Germany]	Noted - the chemical lifetime and perturbation lifetimes are differently calculated.
9447	42	15	42	16	Suggested change to sentence: "…which has a lifetime of about 9.6 years, is considered a short-lived pollutant but is well mixed in the troposphere where it plays a significant role in global radiative forcing." [Christine Weldrick, Australia]	Accepted - this sentence is revised.
33325	42	15			Change: « CH4, which has…..» by “Methane, which has….”. It is better to avoid using abbreviation at the start of a sentence. And all over the MS. [Guiomar Rotllant, Spain]	Noted - the sentence is revised.
9449	42	16	42	16	Add "the" before "atmosphere" [Christine Weldrick, Australia]	Accepted - change was made.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
71705	42	16	42	18	While much of this paragraph is a reasonable summary of the relative roles of CO2, CH4 and N2O the ending is quite subjective, particularly when saying “by far the most important”. Stressing an importance for CO2 in this way is missing the key point that to follow the Paris agreement and keep well below 2°C requires all forcing of the climate system to be reduced as rapidly as possible. As a co-author of the heavily cited Moss et al (2010: The Next Generation of Scenarios for Climate Change Research and Assessment. Nature, 463, 747-756) my concern is that, while feasibility of the RCP2.6 scenario was initially contentious, it was expected there could be a rapid decrease in CH4 emissions and because its lifetime is about 9.5 years there would be a fairly rapid drop in RF allowing enough time for global deployment of new technologies to reduce CO2. Substantive revisions made in the SSP scenarios now allow for the recent increase in CH4 but SSP119 and SSP126, the only scenarios consistent with the Paris agreement, require CH4 concentrations to be decreasing by 2022. If they are not then the need for more rapid cuts in CO2 emissions may start to look impossible in the policy process. [Martin Manning, New Zealand]	Noted - large part of the text, figures and discussion are changed. We hope this section is now sending out a better message
71707	42	16	42	19	The radiative forcing values given here and in some parts of Chapter 7 appear to be based on WG1-AR5 formulae for RF rather than on the new Etminan et al (2016) formulae that are given in Table 7.A.1. The newer RF values increase direct forcing from CH4 by 23% at its current concentrations. Also, the percentages of total forcing given here are NOT for TOTAL radiative forcing but rather for the forcing due to GHGs and the numbers are sensitive to what is actually included after CO2, CH4 and N2O. A much less ambiguous comparison using the new RF formulae would say that CH4 and N2O are adding another 33% and 9% of the RF due to CO2 alone. [Martin Manning, New Zealand]	Accepted - significant changes were made to Figure 5.18 and the related text.
74255	42	18	42	19	I think one should add here a remark on that these numbers implicitly contain the feedback via increases in the greenhouse effect of H2O driven by the long-lived greenhouse gases. [Christoph Völker, Germany]	Not applicable - text and figure changed largely.
127785	42	18	42	19	On page 43, the different metrics (and timelines) of warming potentials are discussed. It isn't clear how total radiative forcing was calculated with respect to the percentages presented here. [Trigg Talley, United States of America]	Accepted - many change were made. Please check Chapter 7 for the calculation of RFs and values
16527	42	19	42	19	Better to use "halogenated gases" rather than "sythetic" for consistency with other chapters. [William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable - text changed.
73911	42	20	42	21	It is unclear how the last sentence relates to this paragraph. Which analysis suggests that the pathways to reaching the goal of the Paris Agreement depend on the management of non-CO2 greenhouse gases? This statement seems to contradict other parts of this chapter as well as figure 5.19. The figure shows that with a choice of GTP CH4 becomes irrelevant as well as other SLCFs with the choice of GTP metrics, thus why does reaching the goals of the Paris Agreement then depends on management of non-CO2 greenhouse gases? [Anke Herold, Germany]	Not applicable - text and figure changed largely.
114699	42	22	42	27	5.18 gives an important reminder about the dominating role of CO2. But please coordinate this closely with ch7 [Jan Fuglestvedt, Norway]	Accepted - thank you for the suggestion.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
73087	42	24	42	24	Change 'Radiative' to 'radiative' [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change was made.
71709	42	24			Figure 5.18 introduces the term 'Effective Radiative Forcing' that is not used or defined anywhere else in this chapter. The reference to Chapter 7 in the figure caption refers to the short Annex in that chapter summarising the new Etminan et al formulae for the direct radiative forcing, and the figure appears to be showing values consistent with that. However, this is quite different from the ERF introduced in Chapter 7. [Martin Manning, New Zealand]	Accepted - change was made.
127787	42	24			Radiative should be lower case. [Trigg Talley, United States of America]	Accepted - change was made.
73089	42	25	42	25	Change 'Era' to 'era'. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - Throughout the chapter "Era" using with "Industrial" or "pre-industrial" holds a Capital letter, as mentioned in TSU style guide.
21823	43	1	42	3	This feels like adding a value judgement rather than being dispassionately scientific. I think removing this sentence improves the paragraph and would remove a potential contention of over-stepping remit. [Peter Thorne, Ireland]	Accepted - change was made.
78495	43	1	43	1	not clear what an "emissions-loss budget" is. The difference between emissions and loss controls the growth rate of any substance. The abundance is the time-integral of this [Chris Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text changed largely.
19973	43	1	43	2	This is gibberish. Please rewrite [philippe waldteufel, France]	Accepted - change was made.
16529	43	1	43	2	"It is well-known … GHGs are proportional to the emissions-loss budget …". What does this mean? What is the science point? The rates of increase of GHGs are *equal* to emissions-loss, but that shouldn't need saying. [William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable - text and figure changed largely.
114701	43	1	43	3	This sentence will be unclear to many readers. Please consider reformualtion [Jan Fuglestvedt, Norway]	Accepted - changes were made in the text.
19013	43	1	43	10	suggest to add this recent study somewhere in the report: Mengis, N., Matthews, H.D. Non-CO2 forcing changes will likely decrease the remaining carbon budget for 1.5 °C. npj Clim Atmos Sci 3, 19 (2020). https://doi.org/10.1038/s41612-020-0123-3 [Mengze Li, Germany]	Not applicable - text and figure changed largely.
77269	43	1	43	21	The added value of the discussion based on emissions metrics here is not clear. The regional emissions of these gases can be provided and metrics addressed in Chapter 7. [Emer Griffin, Ireland]	Accepted - text changed largely.
51147	43	2	43	2	It is well-known that the atmospheric abundance of the GHGs are proportional to the emissions-loss budget in the Earth's environment, and thus the international efforts are on to mitigate emissions of the GHGs for limiting the global warming - suggest this could be presented in a clearer way, such as: "It is well-known that the atmospheric abundance of the GHGs are proportional to the emissions-loss budget in the Earth's environment, and international efforts seek to mitigate GHG emissions to limit global warming" [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Not Applicable. This paragraph and the section as a whole is revised significantly
127789	43	2			Remove "on" (after "efforts are"). [Trigg Talley, United States of America]	Not applicable - text changed largely.
73091	43	3	43	3	Delete 'the' after 'limiting'. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change was made.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
73915	43	5	43	6	The statement that GTP and GWP are the most commonly used metrics seems wrong. Where is GTP commonly used? GTP is not used by any country apart from Brazil whi uses both GWP and GTP. It has also not been 'used' in the IPCC assessment reports. It is discussed as a metric option in chapter 7, but not 'used'. Chapter 7 requires the determination of policy and temperature goals before selecting the approrpiate metric. This guidance is not followed in this section, without a proper explanations related to the purposes and values, several metrics are calculated with their impacts. There needs to be a clear explanation what the goal is for comparing GWP with GTP50 and GTP100 in figure 5.19, as in particular the use of GTP 100 does not seem in line with the policy goals underlining the approaches in this chapter or in the chapter on SLCFs. [Anke Herold, Germany]	Not applicable - text changed largely.
85893	43	5	43	10	This discussion of emission metrics is not consistent with that discussed in chapter 7. For instance Ch 7 does not at any point suggest that GWP provides relevant information on the warming path. This discussion needs to be removed as it damages the report to have conflicting information in different chapters. [William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - changes were made in the text.
103153	43	5			Make sure to list values of GTP and GWP used also in the text: CH4, 6.7 and 32; N2O, 220 and 260. This is needed as much confusion has been created by unclear definitions in AR5. Good that now a detailed description including clear advice is available in chapter 6, but it will be helpful to be very clear what is being used here, too. [Philippe Tulkens, Belgium]	Not applicable - text and Figures are changed largely.
17861	43	6	43	7	"The GTP provides information to reach an end goal temperature (e.g., the climate goals of the Paris Agreement)": the GTP isn't actually consistent with temperature stabilization, as it is about temperature at a single-time point: I suggest making that clearer, and deleting the reference to the Paris Agreement. [Marcus Sarofim, United States of America]	Not applicable - text and figure changed largely.
114703	43	7	43	8	This sentence is not correct and I suggest deleting: "GWP at various time scales provides relevant information on the warming path (and therefore impacts) along the way to an end temperature goal". [Jan Fuglestvedt, Norway]	Accepted - changes were made in the text.
66663	43	11	43	12	The sentence describes the situation for developed countries. A further sentence should be given describing the situation for developing countries, and for the least developed. One option could be something like: "Developed countries in general have higher CO2/CH4 ratios than developing countries, because developing countries have agriculture as a larger share of their economies. This is especially true for the least-developed countries, which have high CH4/CO2 ratios compared to the rest of the world." [Dave Frame, New Zealand]	Not Applicable. This paragraph and the section as a whole is revised significantly
9451	43	12	43	13	Missing word? Or suggest removing "s" before "the emissions" and changing "overwhelm" to "overwhelmes" [Christine Weldrick, Australia]	Not Applicable. This paragraph and the section as a whole is revised significantly

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
83995	43	12	43	21	The message convened by this paragraph is worrisome. By reading the first and last sentences, it informs that the goal of the Paris Agreement does not depend on CO2 management, hence, not on actions from developed nations, where the total net CO2 overwhelms the emissions of CH4 and N2O. While in 1.3.3, 1.3.4, 1.6.3, and others, clearly states that the largest contribution for warming is the increase in the atmospheric concentration of CO2 since 1750. The paragraph should be reviewed, and when doing so, refrain from the use of superlatives such as “dwarfed” and “high” and the targeting of individual countries. The statement starting in line 14 induces the reader to conclude that there is a better metric to be used in all cases, what contradicts the text presented in WGI Ch 7 section 7.6. Individual positions or preferences should be refrained; [Marco Tulio Cabral, Brazil]	Not applicable - text changed largely.
127791	43	12	43	21	This paragraph describes the overwhelming role of CO2 flux in GHG emission, but concludes by stating that the pathway to reach the goal of the Paris Agreement depends on the management of non-CO2 greenhouse gases. Clarify. [Trigg Talley, United States of America]	Accepted - large changes were made in the text and to the figures.
15961	43	12	43	21	In the discussion on GWP and GTP, there should be recognition about effect of rate of emissions, especially in relation to methane which has a short life time. Thus, if methane emissions are increasing and are faster than the rates of decomposition, causing atmospheric concentrations to rise as they are doing today, then it is the immediate radiative forcing effect that is the most significant by a considerable margin, and this is due to the exponential shape of the methane decay curve, which has most of the total radiative forcing of a unit release of methane occurring within the first year. Thus, the limitation, and failure of the GWP and GTP measures are that they inherently assume steady state conditions or a transient response to a single impulse, and not steadily increasing emissions rates as we have today. Consequently, there is no concerted government action to focus on methane removal from the atmosphere. [Kevin Lister, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable - text and figure changed largely.
85879	43	12	43	21	This discussion of the importance of different countries is a matter for WG III and should not be discussed here. [William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - changes were made in the text.
16531	43	12	43	21	This discussion of the importance of different countries is a matter for WG III and doesn't need discussing here. [William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable - text and figure changed largely.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
73907	43	12	43	34	It is unclear how the choice of time horizon of 100 years for GTP is justified in this example. In chapter 7 it is explained that SLCFs with lifetimes less than 20 years are very sensitive to the choice of time horizon. It is obvious that not much CH4 is left after 100 years of a pulse of CH4. Several IPCC scientists explained in workshops after the release of AR5 that a appropriate time horizon for GTP in relation to the policy goals of the Paris Agreement is 40 to 60 years. The chapter on metrics explains that the choice of metric and time horizon depends on the policy goals. Therefore the choice in this section should be better explained. What meaning does a choice of GTP100 have for the comparsion of regions apart from the result that it completely neglects the contribution of SLCFs to temperature change. This also seems to be inconsistent with chapter 6 on SLCFs and it would be necessary to better explain why you chose a GTP time horizon that by choice 'dwarfs' the impact of CH4 and in relation to which values and policy goals, this is a proper choice. [Anke Herold, Germany]	Noted and thank you for this comment. We have not used any climate metric in making this plot. Now we use rather arbitrary scaling of 50 and 500 time for CH4 and N2O emissions, respectively. This was done to avoid any miscommunication, similar to the concerns you have sounded.
74257	43	13	43	13	“metric” should be “metrics” [Christoph Völker, Germany]	Accepted - change was made.
37959	43	13	43	13	“overwhelm s” [Junhee Lee, Republic of Korea]	Accepted - change was made.
73093	43	13	43	13	Close up space before 's'. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change was made.
73095	43	13	43	13	Change 'metric' to 'metrics'. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change was made.
69777	43	13	43	13	orphaned 's' [Gyami Shrestha, United States of America]	Accepted - change was made.
5319	43	13	43	13	‘overwhelm s’ should be ‘overwhelms’ [Sheel Bansal, United States of America]	Accepted - change was made.
33327	43	13			Change: “…overwhelm s the…” by “…overwhelms the…”. [Guiomar Rotllant, Spain]	Accepted - change was made.
127793	43	13			Remove "s" (after "overwhelm"). [Trigg Talley, United States of America]	Not applicable - text changed largely.
21825	43	14	43	15	It is unclear to me what is being said here and also it again tends towards the value-judgement end of the scale given how it is couched. Can it be redrafted for clarity? [Peter Thorne, Ireland]	Accepted - change was made.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
66665	43	14	43	15	This sentence is misleading. GTP100 accurately estimates the temperature effects after 100 years, because that is what it was designed to do. You might as well say "GWP100 massively overestimates the long-term temperature impacts of methane." The fact is, emissions metrics only do what they are designed to do. The segue to long-lived gases doesn't quite work because it makes it sound as though there is something wrong with GTP100 (which there isn't - it's just a different choice). A better segue while still making the point would be: "GTP100 and GWP(100) differ most significantly in the cases of short-lived gases (including methane). The reasons for this are well-understood, and have to do with differences in atmospheric residence time: some gases can be very effective at trapping radiation - and hence score highly on GWP100 - while being quite short-lived. As a consequence, even though the unit of gas has trapped a lot of radiation over the 100-year period, so little of the gas remains after 100 years that the long-term temperature implications are weak; hence the small GTP100 value. More recent emissions metrics (CGTP, GWP*) have focused on reflecting the distinction between stock and flow forcing agents, and can be used to give accurate estimates of a time-evolving inventory of gases. The distinction is less relevant for long-lived gases such as N2O, which..." [Dave Frame, New Zealand]	nice, again, but too long this time
9453	43	15	43	15	Replace "Tropical America regions" to "tropical America regions" [Christine Weldrick, Australia]	Accepted - change was made.
33329	43	15			Change: "…Brazil, Tropical America regions." By "…Brazil and Tropical America regions.". [Guiomar Rotllant, Spain]	Accepted - change was made.
127795	43	15			Add "and" (after "Brazil"). Also, what constitutes tropical America here? Central, South, and North? [Trigg Talley, United States of America]	Not applicable - text changed largely.
103155	43	15			Please check: "Because" seems to be a gross simplification here. As gases are seen in relation to CO2, effects of CO2 and the relevant gas factor into the equation [Philippe Tulkens, Belgium]	Accepted - changes were made in the text.
73097	43	16	43	16	Insert 'a' after 'has'. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change was made.
16533	43	16	43	16	Section 5.2.3 has already assessed the N2O lifetime to be 116+/- 9 years. So why is a different lifetime used here? [William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change was made.
9455	43	16	43	16	Add "a" before "chemical lifetime of…" [Christine Weldrick, Australia]	Accepted - change was made.
84801	43	16	43	20	The geological CH4 sources are now much smaller (as already reflected in the CH4 graph). See Hmiel et al., 2020. The text here must reflect this. [Martin Heimann, Germany]	Not applicable - text changed largely. The results of Hmiel et al. are discussed in Section 5.2.2.
9457	43	19	43	19	Replace "Tropical" with "tropical" [Christine Weldrick, Australia]	Accepted - change was made.
83499	43	20	43	20	Note that in Chapter 2 the Turner (2018) reference is gvien as Kirtland Turner (2018). [Antje H. L. Voelker, Portugal]	Not applicable - text changed largely.
9459	43	20	43	20	Add "the" before "Paris Agreement" [Christine Weldrick, Australia]	Accepted - change was made.
19975	43	20	43	21	This sentence is surprizing, even almost dangerous. Since the contributions of non-CO2 gases amount to 1/3, their management has definitely to be addressed. But somebody reading this sentence and concluding, naively, that managing CO2 doesn't matter would be badly in the wrong. [philippe waldteufel, France]	Accepted - change was made.
114707	43	20	43	21	I find the last sentence here uncelar and problematic. This is no analysis and management is unclear. [Jan Fuglestvedt, Norway]	Accepted - changes were made in the text.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
66667	43	20	43	21	Suggest: "...pathways to reaching the goal of Paris Agreement depend primarily on the management of CO2 (see exec summ bullet points on page 5-8), but also on the management of non-CO2 greenhouse gases." [Dave Frame, New Zealand]	Not Applicable. This paragraph and the section as a whole is revised significantly
78497	43	20	43	21	the discussion above shows that CO2 is the biggest emissions both regionally and globally, so why does this final sentence now say non-CO2 should be the focus? [Chris Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text changed largely.
114705	43	24	43	24	I struggle to see the point of fig 5.19 the way it is presented now. [Jan Fuglestvedt, Norway]	Accepted - changes were made to the Fig. 5.19. We no longer use the GWP/GTP for scaling emissions
85895	43	26	43	26	Figure 5.19 is innappropriate for a WG I report. The regional attribution of warming is a sensitive political issue and needs to be addressed in WG III. There is no reasoning given to any of the 3 metrics presented (GTP50, GWP100, GTP100). As they all give different results, from a physical science point of view this figure tells us nothing other than different metrics give different results. [William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - changes were made to the Fig. 5.19. We no longer use the GWP/GTP for scaling emissions
40163	43	26			Fig 5.19: as the information is not presented elsewhere, would it make sense to add a "global" panel, that sums up the global situation? [TSU WGI, France]	Accepted - change was made. Please see the bottom - left panel
52247	43	33	43	36	Add reference: AMAP, (2013; 2018) for ocean acidification in the Arctic Ocean [Agneta Fransson, Norway]	Not applicable - to the Figure 5.19 and associated text
45439	43	41	43	41	Why using "up to"? [Olivier Sulpis, Netherlands]	Accepted. Sentence has been revised
58571	43	41	43	42	Is there a timescale associated with this? Is this cumulative anthropogenic CO2 emissions, or current yearly anthropogenic emissions? ("The surface ocean absorbs up to a quarter of all anthropogenic CO2 emissions mainly through physical-chemical processes") [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Sentence has been revised
30563	43	42	43	42	Poor choice of references. Hoffmann et al., 2011 did not provide an estime of the anthropogenic CO2 emissions in the oceans.I would refer to papers like Sabine et al., 2004; Gruber et al., 2019; Friedlingstein et al., 2019. [nina bednarsek, United States of America]	Accepted
36367	43	42			These are not valid references for this statement. The references used here should be the most recent assessments of ocean CO2 uptake as covered on page 23 lines 8-14 earlier in this same chapter. Hoffmann et al., 2011 did not provide an estime of the anthropogenic CO2 emissions in the oceans. I would refer to papers like Sabine et al., 2004; Gruber et al., 2019; Friedlingstein et al., 2019. This statement also needs to include the time period it's referring to as uptake has changed over time as discussed on page 23. Poor choice of references. [Adrienne Sutton, United States of America]	Accepted. References were amended.
73099	43	43	43	43	Change 'turns' to 'turn'. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change was made.
90081	43	43	43	43	This statement uploaded by Ted Schuur on behalf of a graduate class that reviewed the chapter (this was communicated with IPCC TSU in advance): Change"..forms carbonic acid, that in turns..." to "forms carbonic acid. In turn, carbonic acid dissociates.." [Edward Schuur, United States of America]	Accepted - change was made.
99421	43	44	43	44	brakets missed (H+) [Isabel Seguro, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change was made.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
73101	43	44	43	44	Superscript for carbonate should be 2- [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change was made.
9461	43	44	43	44	Replace "H+ ions concentration" with "H+ ion concentration" [Christine Weldrick, Australia]	Accepted - change was made.
42791	43	44	43	45	replace"which increases water acidity" to" which has caused a shift in the carbonate chemistry towards a less basic state, so called ocean acidification (e.g Orr et al., 2005; AMAP AOA 2013; Doney et al., 2009) [Melissa Chierici, Norway]	Accepted - change was made.
52313	43	45	43	45	"decrease ocean basicity (decreased pH)" is better than "increase acidity", and reference is not Doney...? this is pure carbonate chemistry, discovered long before 2009... [Agneta Fransson, Norway]	Rejected. The literature classically refers to ocean acidity. While certainly correct, the term basicity is unfrequently used
99423	43	45	43	45	Instead of increases water acidity :"decreases pH" [Isabel Seguro, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change was made.
45441	43	45	43	45	I suggest replacing "increases the water acidity" with "acidifies the water" or "lower the water pH", since the water is not acid to start with. [Olivier Sulpis, Netherlands]	Accepted - change was made.
9463	43	48	43	48	Replace "H+ ions concentration" with "H+ ion concentration" [Christine Weldrick, Australia]	Accepted - change was made.
67855	43	49	43	50	It is stated in the text that "There is a low agreement on the effect of OA on marine organism". This statement contradicts with the SROCC. According to the SPM-SROCC (IPCC, 2019), there is a high confidence that marine biota are impacted by ocean acidification. It is stated in the document (A.6.4) that warm-water coral reefs and rocky shores dominated by immobile, calcifying (e.g., shell and skeleton producing) organisms such as corals, barnacles and mussels, are currently impacted by extreme temperatures and ocean acidification (high confidence)" . There is also a statement by Kroeker et al (2013) on the P.54 (L 7-8) that explained the effect of ocean acification on the calcification rate of marine organisms. [Ruandha Agung Sugardiman, Indonesia]	Accepted - change was made.
36401	43	49	43	50	Several recent papers have shown evidence for ocean acidfication impacts on foraminifera, pteropods, and dungeness crab larvae (Osborne et al., 2019; Bednarsek et al., 2014, 2017, 2019, 2020). [Adrienne Sutton, United States of America]	Noted
96579	43	49	43	50	"low agreement on the effects of ocean acidification on marine organisms." We are surprised about the low agreement, since it does not fit with what follows on page 44. Please check. [Nicole Wilke, Germany]	Accepted - text revised
7253	43	49	43	50	It is stated in text that "There is a low agreement on the effect of OA on marine oranism". This statement is contradicted with the SROCC. According to the SPM-SROCC (IPCC, 2016), that there is high confidence that marine biota are impacted by ocean acidification (IPCC 2019 stated that "A.6.4. Warm-water coral reefs and rocky shores dominated by immobile, calcifying (e.g., shell and skeleton producing) organisms such as corals, barnacles and mussels, are currently impacted by extreme temperatures and ocean acidification (high confidence)" . There is also a statement by Kroeker et al (2013) on the P.54 (L 7-8) that explained the effect of ocean acification to the cal calcification rate of marine organisms. [Asaad Irawan, Indonesia]	Accepted - change was made.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
30589	43	49	43	50	Several recent papers have shown evidence for ocean acidification impacts on foraminifera, pteropods, and dungeness crab larvae (Osborne et al., 2019; Bednarsek et al., 2014, 2017, 2019, 2020). [nina bednarsek, United States of America]	Noted
77279	43	49	43	50	p43 line 49 states with respect to OA ….". although there is low agreement on the effects of ocean acidification on marine organisms (Hofmann et al., 2011b; Browman, 2016; IPCC, 2019)." Such context on OA is necessary for WG1 report; but is this a conclusion as phrased more for WGII report? Also is this referring to experimental studies organism response to projected conditions or current impacts?. We also note a statement which could appear contradictory in Ch 5.3.4.1 p50 Line 48 (Coastal OA) “Nevertheless, there is medium agreement (medium evidence) that coastal and shelf seas acidification, whether induced by the increasing atmospheric CO2 or by enhancement of eutrophication, has negative effects on specific groups of marine organisms (Dupont et al., 2010), especially when combined with other stressors such as temperature and increased availability of potentially toxic metallic ions such as arsenic and copper (Millero et al., 2009; Boyd et al., 2015; Breitburg et al., 2018).” [Emer Griffin, Ireland]	Rejected - We had to add the significance for impact of OA on marine life becaue a number of previous review comments. We agreed it is WGII domain, but our references are short and concise. Second part of the comment: Accepted - We have revised the confidence statmenet and made it consistent
103157	43	49	43	50	low agreement on the effects of ocean acidification on marine organisms'. There are many scientific papers that attest to ocean acidification (OA) being a pervasive stressor, with negative effects on survival, calcification, growth and reproduction. OA harms life forms that rely on carbonate-based shells and skeletons, harms organisms sensitive to acidity and harms organisms higher up the food chain that feed on these sensitive organisms. The biological effects of ocean acidification are generally large and negative, but the variation in sensitivity amongst organisms has important implications for ecosystem responses. For example, same chapter 5, pg 55 lines 7 to 8 states: 'Ocean acidification is considered to reduce the calcification rate of marine organisms (e.g. Kroeker et al.,8 2013) (high confidence).' GESAMP2019 pg 16: 'Ocean acidification reduces the ability of marine organisms, such as corals, plankton and shellfish, to build their shells and skeletal structures. It also exacerbates existing physiological stresses and reduces growth and survival rates during the early life stages of some species;' AR6 WGIII Chapter 12, pg 22: 'For ocean alkalinity, elevated CO2 in the atmosphere acidifies the ocean, which puts stress on shell forming organisms (‘ocean acidification’). Extensive research has been conducted to understand the impact of ocean acidification on marine biota and the global carbon cycle (Doney et al. 2009).' Also chapter 5 section 5.4.8.4. on pg 72 states: 'As seawater pH lowers under accumulating CO2 in the atmosphere, there is a decrease in the saturation state of calcium carbonate, which in turn alters the marine food web.' Also chapter 5, pg 97 line 41 to 43 :'Additionally, both AOUpw and OF would enhance surface ocean acidification and perturb marine ecosystems via reorganisation of community structure (high confidence) (Oschlies, 2010; Williamson et al., 2012a). [Philippe Tulkens, Belgium]	Accepted - text revised

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
19977	43	49	44	3	Why is this way of introducing de-oxygenation slightly irritating? Because it begins by emphasizing warming, which is said to explain about 15%; And next, as a kind of afterthought, it mentions stratification recognized to contribute "most of the remaining" phenomenon. Still, this "remaining" amounts to 85% of the de-oxygenation! One would hope that the authors of WG1 report ask themselves which message is likely to stay in the memory of the readers. Later in the SOD, the authors insist on the importance of the science of science communication. They may be right! [philippe waldteufel, France]	Rejected. Increased stratification is in large parts driven by warming, as is the decrease in oxygen solubility. As such we maintain the initial structure of the argumantation.
90083	43	50	43	51	This statement uploaded by Ted Schuur on behalf of a graduate class that reviewed the chapter (this was communicated with IPCC TSU in advance): There is a reference to marine organisms in the ES and also several references in this introduction section which makes it seem like a discussion about marine organisms willl be a significant part of the section, but there are very few references to marine organisms, and the references appear limited to shelled invertebrates. Either clarify which kidns of marine organisms are affected as supported by the references, or remove. [Edward Schuur, United States of America]	Accepted - text revised
3621	43	52	44	12	I was surprised not to see anything about the impacts of deoxygenation and why this process is interesting/important to study in the context of environmental changes. I would suggest that the introduction here is a good place to have one or two sentences on the impact of deoxygenation on marine life. [Mathilde Jutras, Canada]	Rejected. Impacts are discussed in details by WGII
115357	43	52		54	Use Ch3 assessment for attribution of ocean warming - 'It is extremely likely that anthropogenic forcing has made a substantial contribution to the OHC increase over the historical period'. [Gillett Nathan, Canada]	Accepted - change was made.
37939	43	55			Could you add time information corresponding to the quantitative information (i.e. 15%). [Junhee Lee, Republic of Korea]	Accepted - change was made.
116429	43		43		Please check the coherency of the use of metrics here with ch 7 (combined GTP etc). [Valerie Masson-Delmotte, France]	Accepted - changes were made in the text and to the figures.
2205	44	1			Please add the following very holistic contribution related to the de-oxygenation: Laffoley and Baxter (2019): Laffoley, D. & Baxter, J.M. (eds.) (2019). Ocean deoxygenation: Everyone's problem - Causes, impacts, consequences and solutions. Full report. Gland, Switzerland: IUCN. 580pp. Individual chapters / sections within this report should be referenced as: Author(s). (2019). Title of chapter/ section. In 'Laffoley, D. & Baxter, J.M. (eds.) (2019). Ocean deoxygenation: Everyone's problem - Causes, impacts, consequences and solutions. Gland, Switzerland: IUCN. xxii+562pp. DOI: https://doi.org/10.2305/IUCN.CH.2019.13.en [Abed El Rahman Hassoun, Lebanon]	Rejected. Assesment reports focus on peer-reviewed literature where possible. As such the assessment draws on the multiple lines of evidence presented in the peer reviewed literature on this topic.
3617	44	4	44	4	I feel like the use of the word "projected" is not ideal to talk about something that was actually measured from observations. [Mathilde Jutras, Canada]	Accepted - text revised
90085	44	8	44	9	This statement uploaded by Ted Schuur on behalf of a graduate class that reviewed the chapter (this was communicated with IPCC TSU in advance): "... oxygen minimum zones or hypoxic costal area..." to "oxygen minimum zones, hypoxic costal areas..." or "oxygen minimum zones (hypoxic costal areas)..."so the reader knows that they are definitions of each other and not multiple things. [Edward Schuur, United States of America]	Rejected. We feel the current formulation is adequate.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
3619	44	9	44	12	The sentence "The coupled effects of acidification on de-oxygenation occur at the level of marine organism metabolism, as the excess CO2 dissolved in the oceans may lead to respiratory stress and reduction of thermal tolerance by organisms" is not very clear. [Mathilde Jutras, Canada]	Accepted. Sentence has been rephrased to avoid confusion.
39737	44	9		10	"coupled effect…metabolism" do you mean acidification AND de-oxygenation? If not, could you clarify this sentence? [TSU WGI, France]	Accepted - text revised
2339	44	9		12	It is important to highlight here that neither the occurrence nor the strength of these synergistic impacts is currently predictable, and therefore, the true threat of concurrent acidification and hypoxia to marine food webs and fisheries is still not fully understood (Gobler and Baumann, 2016). Also, elevated CO2 and decreased O2 concentrations may lead to more than respiratory stress and reduction of thermal tolerance, for example they may selectively enhance carboxylation over oxygenation catalyzed by ribulose-1,5-bisphosphate carboxylase/oxygenase and thereby benefit autotrophs. (Gao et al., 2019). Gao Kunshan, Beardall John, Häder Donat-P., Hall-Spencer Jason M., Gao Guang, Hutchins David A., 2019. Effects of Ocean Acidification on Marine Photosynthetic Organisms Under the Concurrent Influences of Warming, UV Radiation, and Deoxygenation. Frontiers in Marine Science, 6, 322. DOI=10.3389/fmars.2019.00322. https://www.frontiersin.org/article/10.3389/fmars.2019.00322 ; Gobler C.J. and Baumann H., 2016. Hypoxia and acidification in ocean ecosystems: coupled dynamics and effects on marine life. Biology Letters, 12 (5), doi:10.1098/rsbl.2015.0976. https://royalsocietypublishing.org/doi/abs/10.1098/rsbl.2015.0976 [Abed El Rahman Hassoun, Lebanon]	Rejected. Impacts are discussed in details by WGII
2207	44	9		12	It is important to highlight here that neither the occurrence nor the strength of these synergistic impacts is currently predictable, and therefore, the true threat of concurrent acidification and hypoxia to marine food webs and fisheries is still not fully understood (Gobler and Baumann, 2016). Also, elevated CO2 and decreased O2 concentrations may lead to more than respiratory stress and reduction of thermal tolerance, for example they may selectively enhance carboxylation over oxygenation catalyzed by ribulose-1,5-bisphosphate carboxylase/oxygenase and thereby benefit autotrophs. (Gao et al., 2019). [Abed El Rahman Hassoun, Lebanon]	Rejected. This current report does not focus on impacts (WGII)
2245	44	9		12	Gao Kunshan, Beardall John, Häder Donat-P., Hall-Spencer Jason M., Gao Guang, Hutchins David A., 2019. Effects of Ocean Acidification on Marine Photosynthetic Organisms Under the Concurrent Influences of Warming, UV Radiation, and Deoxygenation. Frontiers in Marine Science, 6, 322. DOI=10.3389/fmars.2019.00322. https://www.frontiersin.org/article/10.3389/fmars.2019.00322 [Abed El Rahman Hassoun, Lebanon]	Noted
2247	44	9		12	Gobler C.J. and Baumann H., 2016. Hypoxia and acidification in ocean ecosystems: coupled dynamics and effects on marine life. Biology Letters, 12 (5), doi:10.1098/rsbl.2015.0976. https://royalsocietypublishing.org/doi/abs/10.1098/rsbl.2015.0976 [Abed El Rahman Hassoun, Lebanon]	Noted

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
2289	44	9			Gao Kunshan, Beardall John, Häder Donat-P., Hall-Spencer Jason M., Gao Guang, Hutchins David A., 2019. Effects of Ocean Acidification on Marine Photosynthetic Organisms Under the Concurrent Influences of Warming, UV Radiation, and Deoxygenation. Frontiers in Marine Science, 6, 322. DOI=10.3389/fmars.2019.00322. https://www.frontiersin.org/article/10.3389/fmars.2019.00322 [Abed El Rahman Hassoun, Lebanon]	Noted
17041	44	12	44	13	Subsurface methane transport to shallow sediments and water column, a widespread phenomena along the ocean margins, also contribute to ocean acidification and deoxygenation (Biastoch et al., 2011; Boetius and Wenzhöfer, 2013; Boudreau et al., 2015; Akam et al., 2020). CO2 input from diffusive methane-charged sediments is suggested to contribute up to 6.5 Tmol year–1 towards the water column (range: 3.2–9.2 Tmol year–1) (Akam et al., 2020). Advective methane transport would induce aerobic methane consumption, resulting in oxygen consumption and CO2 production in the water column (Boetius and Wenzhöfer, 2013). These systems are suggested to imply a moderate additional perturbation to the carbonate system of the Anthropocene oceans (Boudreau et al., 2015) Citations: Boudreau, B. P., Luo, Y., Meysman, F. J., Middelburg, J. J., and Dickens, G. R. (2015). Gas hydrate dissociation prolongs acidification of the Anthropocene oceans. Geophys. Res. Lett. 42, 9337A–9344A. Boetius, A., and Wenzhöfer, F. (2013). Seafloor oxygen consumption fuelled by methane from cold seeps. Nat. Geosci. 6, 725–734. doi: 10.1038/ngeo1926 Biastoch, A., Treude, T., Rüpke, L. H., Riebesell, U., Roth, C., Burwicz, E. B., et al. (2011). Rising Arctic Ocean temperatures cause gas hydrate destabilization and ocean acidification. Geophys. Res. Lett. 38:L08602. doi: 10.1029/2011GL047222 Akam, S.A., Coffin, R.B., Abdulla, H.a.N., and Lyons, T.W. (2020). Dissolved Inorganic Carbon Pump in Methane-Charged Shallow Marine Sediments: State of the Art and New Model Perspectives. Frontiers in Marine Science 7.https://doi.org/10.3389/fmars.2020.00206 [Sajjad Abdullajintakam, United States of America]	Rejected - Detail beyond the scope of the assessment given space constraints.
21829	44	17	44	18	If I recall correctly chapter 2 had a higher estimate than that given here. Please cross-check and amend as necessary for consistency pointing to the substantive assessment in chapter 2. [Peter Thorne, Ireland]	Accepted - change was made.
9857	44	17	44	18	Suggest clarifying this is off a substantially warmer than PI base temperature (see ch 2) [Robert Kopp, United States of America]	Accepted - change was made.
99427	44	17	44	30	The paragraph speaks about CO2 realeased into the ocean-atmosphere system. But further down it tells only about release into the atmosphere. The consequences are invasion of CO2 in the ocean, and that would help to understand current invasion processes. Therefore, the first sentence in line 19 should be CO2 released into the atmosphere (and not include the ocean). [Isabel Seguro, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. It remains yet unclear where the CO2 pulse originated from. The current phrasing accounts for the possibility of CO2 released at the bottom of the ocean.
115359	44	17		18	Give a range and its likelihood (refer to CH2 if assessed there). Avoid 'possibly exceeding' and a range. [Gillett Nathan, Canada]	Accepted - text revised
39701	44	18	44	18	"...occurred 55.8 Myr ago." -> Chapter 2 actually says it occurred "55.9-55.7 Myr ago" [TSU WGI, France]	Accepted. Sentence has been revised

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
99243	44	18			chapter 2 uses different references than here (they say the same so its not a problem of substance, here the original compilation is cited, chapter 2 an update with the same outcome) [Daniela Schmidt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Sentence has been revised
99425	44	19	44	19	isotopically light CO2 is mentioned but it is not explain here why it is important to mention CO2 isotopic composition. If it is not important I would remove it. If it is important I would include a sentence explaining it. [Isabel Seguro, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Argument removed
32105	44	19			Line 19 States the PETM was driven by CO2 and quotes the modelling paper by Turner. But shifting the 13C budget using CO2 involves a very large shift in mass because the isotopic leverage of a CO2 pulse is weak. Doing it with methane, which has a much more potent isotopic leverage, involves a much smaller gas emission pulse and a potent sudden global warming impact. So surely, especially as this is an atmospheric chapter, it is worth mentioning the methane hypothesis for the PETM - the jury is out and we cannot know it was CO2? See Nisbet, E. G., S. M. Jones, J. Maclennan, G. Eagles, J. Moed, N. Warwick, Slimane Bekki, P. Braesicke, J. A. Pyle, and C. M. R. Fowler. "Kick-starting ancient warming." Nature Geoscience 2, no. 3 (2009): 156-159. [Euan G. Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The current version of the discussion does not dwell onto this level of detail.
67471	44	20	44	20	delete "Model" [James Christian, Canada]	Accepted - change was made.
21197	44	20	44	20	Turner should be Kirtland Turner (cf. other chapters) [Robert Speijer, Belgium]	Accepted
58533	44	20	44	24	There are a couple more recent references which may be very useful here; notably Jones et al. (2019) Large Igneous Province thermogenic greenhouse gas flux could have initiated Paleocene-Eocene Thermnal Maximum climate change, Nature Communications, 10:5547. I believe the emerging consensus is fairly strong that thermogenic methane and mantle-derived CO2, associated with the NAIP are by far the most probable sources. See also C-cycle modeling in Cui & Schubert (2018) GPC. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - change was made.
33331	44	25			Change: "…(i.e. 0.5–1.1 PgC yr-1;.. » by « (i.e. 0.5–1.1 PgC yr-1…". [Guiomar Rotllant, Spain]	Accepted
99245	44	26	44	26	in the context of time, the terrestrial record in this reference provides a higher resolution than the cited evidence and therefore increases the confidence in the rate Cui Y, Kump LR, Ridgwell AJ, Charles AJ, Junium CK, Diefendorf AF, et al. Slow release of fossil carbon during the Palaeocene-Eocene Thermal Maximum. Nature Geosci. 2011;4 7:481-5; doi: http://www.nature.com/ngeo/journal/v4/n7/abs/ngeo1179.html#supplementary-information. http://dx.doi.org/10.1038/ngeo1179. This assessment is also corroborated by modelling studies 1.Kirtland Turner S, Hull PM, Kump LR, Ridgwell A. A probabilistic assessment of the rapidity of PETM onset. Nature Communications. 2017;8 1:353; doi: 10.1038/s41467-017-00292-2. https://doi.org/10.1038/s41467-017-00292-2 [Daniela Schmidt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - references have been considered
115361	44	26		29	This sentence would only make sense if the permafrost or hydrates released CH4 - is that what is meant? [Gillett Nathan, Canada]	Accepted - text revised
71171	44	27			What do the authors mean by "destabilised permafrost"? This is also terminology, not typical for traditional permafrost science and engineering. See also Comment Nr. 2. [Lukas Arenson, Canada]	Accepted. Text revised

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
21199	44	29	44	30	"The PETM thus provides a test for our understanding of the ocean's response to the rapid invasion of carbon (and heat)." Rather than a test, the PETM provides a natural analog, but importantly, probably one proceeding at a much slower pace than today. Note however, that some authors rather suggested the PETM to be a non-analogue, considering the many different boundary conditions. [Robert Speijer, Belgium]	Rejected - We agree with the many boundary conditions but we keep "test" as the word of choice.
99429	44	30	44	30	change invasion for increase as it sounds also better then invasion of heat. [Isabel Seguro, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change was made.
39889	44	34		35	"with a negative surface ocean pH excursion" what does it mean? I find it unclear [TSU WGI, France]	Rejected. Negative surface ocean pH excursions means "surface ocean acidification"
73103	44	35	44	35	References should be in chronological order. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial - this kind of issues will be taken care of during the production phase, which includes professional copy-editing.
67473	44	38	44	38	Zachos 2005 should be Zachos et al (and 45/9) [James Christian, Canada]	Accepted
99247	44	38	44	40	as written at the moment, the text suggests that the expression of the PETM is the same everywhere. While the sedimentary change are similar, and the isotope excursion is it is important to remember that the local conditions change if the sediment becomes a clay or stays a carbonate and the duration of the undersaturation. I suggest rephrasing [Daniela Schmidt, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. The sentence reflects the view generally accepted in the literature
67475	44	40	44	40	change "likely globally uniform" to "global" [James Christian, Canada]	Accepted - text revised
18219	44	40	44	43	Can a yearly or decadal rate be added to the end of the sentence regarding the acidification occuring one order of magnitude slower, similar to line 25 on page 44. [Chelsey Baker, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. Given the uncertainties inherent to age models, yearly or decadal rates remain speculative.
73105	44	41	44	41	Capital 'C' for 'century' (for consistency elsewhere in chapter). [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change was made.
67477	44	42	44	42	change "occurred at one order of magnitude slower" to "occurred about an order of magnitude slower" [James Christian, Canada]	Accepted - change was made.
83501	44	45	44	46	A more recent publication on ecological impacts is: Frieling, J., Gebhardt, H., Huber, M., Adekeye, O.A., Akande, S.O., Reichart, G.-J., Middelburg, J.J., Schouten, S., Sluijs, A., 2017. Extreme warmth and heat-stressed plankton in the tropics during the Paleocene-Eocene Thermal Maximum. Science Advances 3, doi: 10.1126/sciadv.1600891. [Antje H. L. Voelker, Portugal]	Noted
21201	44	46	44	48	In the following, several scientific outcomes seem to be linked to the wrong references "Continental shelf ecosystems (Ridgwell and Schmidt, 2010; McInerney and Wing, 2011) and planktonic communities (including both phyto- and zooplankton) show reductions in diversity (Robinson, 2011)." Only McInerney and Wing (2012) deals with shelf ecosystems (see comment above). Ridgwell and Schmidt (2010) perfomed a modeling study on calcification of various plankton groups and Robinson (2011) is about an open ocean platform system and not about plankton at all. Appropriate references for the plankton communities could be Gibbs et al. (2018 - Phil. Tans. Roy. Soc, and earlier work) and work by Sluijs et al. (e.g. Sluijs and Brinkhuis 2009 - Biogeosciences) [Robert Speijer, Belgium]	Accepted - text revised

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
21203	44	54	44	54	Classical and robust ways to reveal widespread ocean de-oxygenation are equally valuable here: sedimentary data (black shales with no bioturbation) and paleontological data (fish remains and lack of benthic organisms in these black shales) from the Tethyan margins provided the first indications for widespread dysoxia during the PETM (Gavrilov et al. 1997 - Lith. Miner. Resour.; Speijer& Wagner 2002 -GSA SP356) see also Dickson et al. (2014 - Paleoceanography). [Robert Speijer, Belgium]	Rejected. While the comment is certainly valid, AR6 focuses on the new scientific evidence published since AR5
116431	44		45		There is duplication on past changes (PETM, last deglaciation) with other sections and chapters (ch 2). Check the glossary for related terms and make sure that a coherent approach is used (names of periods etc) in AR6 WGI altogether. [Valerie Masson-Delmotte, France]	Accepted - change was made.
2291	44			12	Gobler C.J. and Baumann H., 2016. Hypoxia and acidification in ocean ecosystems: coupled dynamics and effects on marine life. Biology Letters, 12 (5), doi:10.1098/rsbl.2015.0976. https://royalsocietypublishing.org/doi/abs/10.1098/rsbl.2015.0976 [Abed El Rahman Hassoun, Lebanon]	Noted
9465	45	1	45	1	Consider replacing "(anoxia)" with "(anoxic)" [Christine Weldrick, Australia]	Accepted - change was made.
51205	45	1	45	4	Unclear sentence structure here - did the oxidation of methane hydrates cause the expansion of OMZs? Suggest that breaking this sentence into two could make these points clearer. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. Sentence has been removed
33333	45	1		2	Change: “De-oxygenation affected the surface ocean globally (including the Arctic Ocean (Sluijs et al., 2006).” By “De-oxygenation affected the surface ocean globally (including the Arctic Ocean) (Sluijs et al., 2006).” [Guiomar Rotllant, Spain]	Accepted - change was made.
9467	45	2	45	2	Closed bracket missing in sentence, perhaps belongs: "…the Arctic Ocean (Sluijs et al., 2014))" [Christine Weldrick, Australia]	Accepted - change was made.
99433	45	4	45	52	Sentences in line 4 and 52 are similar but both show different conclusion? The first one say that expansion of OMZ may have stimulated N2O production while sentence in line 52 says that expansion of OMZ substancially enhace N2O. [Isabel Seguro, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. Both sentences convey a similar message. Ice core measuments covering the last deglacial transition allow infering emissions rates
67479	45	8	45	10	sentence fragment; something missing here [James Christian, Canada]	Accepted - text revised
99431	45	9	45	9	", AND rapid regrowth…." [Isabel Seguro, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change was made.
9275	45	9	45	10	Add the word "and" to the following "…(Zachos, 2005), [and] rapid regrowth…" [Christine Weldrick, Australia]	Accepted - change was made.
83503	45	13	45	13	To be conform with the nomenclature used in Chapter 2 and Annex II, may be change the subheading to "last deglacial transition". [Antje H. L. Voelker, Portugal]	Accepted - change was made.
40703	45	13		55	section 5.3.1.2 : I don't see any assessment in this section [TSU WGI, France]	Accepted. Assessment has been added.
31981	45	17			Should also mention methane rise from about 500ppb to >700ppb. Severinghaus, Jeffrey P., and Edward J. Brook. "Abrupt climate change at the end of the last glacial period inferred from trapped air in polar ice." Science 286.5441 (1999): 930-934. [Euan G. Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. The current discussion focuses on emission rates (as opposed to changes in concentrations)
86753	45	19	45	19	Please consider to insert "GHG" before the first apperance of emissions in this line, and "CO2" in front of the second apperance of emissions. [Oyvind Christophersen, Norway]	Rejected. This sentenced focuses on CO2, not GHG more broadly.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
58573	45	21	45	21	What emissions are discussed here? Total CO2 from oceans and terrestrial carbon stocks? ("Emission rates during these transient events..") [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Text revised
99439	45	24	45	24	Boron isotope lack the symbol (δ11B) [Isabel Seguro, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change was made.
83505	45	27	45	27	The study of Moy, A.D., Howard, W.R., Bray, S.G., Trull, T.W., 2009. Reduced calcification in modern Southern Ocean planktonic foraminifera. Nature Geosci 2, 276-280, doi:10.1038/ngeo460. shows a smaller change of ca. 20%. [Antje H. L. Voelker, Portugal]	Rejected. Reference deals with Holocene sediments
93435	45	27	45	27	Barker 2002 ref should be Barker and Elderfield 2002. [Carles Pelejero, Spain]	Accepted - change was made.
2505	45	27			According to the Greek origin of the word planktic is the correct term to use instead of planktonic. (Emiliani 1991, J. Paleont. 65, p 329) [Thomas Ronge, Germany]	Accepted - change was made.
83507	45	28	45	28	Termination is not the right word here because it refers to the mid-point in the transition; use deglaciation instead. [Antje H. L. Voelker, Portugal]	Accepted - change was made.
96581	45	30	45	30	Please introduce "OMZ". [Nicole Wilke, Germany]	Accepted - change was made.
98239	45	30	45	37	This paragraph is confused. It tries to connect reduced ventilation, the biological pump, and AOU. First, AOU is a calculated parameter that depends on preformed O2, so it would be simpler to use "respiration". Second, finishing with "This highlights the contribution of apparent oxygen utilization..." does not connect the ideas. I believe the point is to connect O2 supply via circulation, the amount of respirable organic C via the biological pump, and respiration, with variations in any of these affecting the amount of oxygen in the water column. [Gregory Cutter, United States of America]	Rejected. This sentence is meant to convey the idea that even though global temperatures were lower during the LGM (thereby increasing oxygen solublity), oxygenation in the ocean interior decreased as a consequence of enhanced respiration of sinking organic matter and generally more sluggish ocean circulation
83509	45	31	45	31	It would be nice to add here a reference for the Indian Ocean OMZ like Gaye, B., Böll, A., Segschneider, J., Burdanowitz, N., Emeis, K.C., Ramaswamy, V., Lahajnar, N., Lückge, A., Rixen, T., 2018. Glacial–interglacial changes and Holocene variations in Arabian Sea denitrification. Biogeosciences 15, 507-527, doi: 10.5194/bg-15-507-2018. [Antje H. L. Voelker, Portugal]	Noted
127797	45	31	45	31	The latest inventory is now EDGAR v5.0, not v4.3.2. [Trigg Talley, United States of America]	Rejected. Comment does not relate to the text on p. 45
67481	45	37	45	37	"overcompensated" is a poor choice of words here ("exceeded"? "negated"?) [James Christian, Canada]	Accepted - text revised
88539	45	40	45	41	Instead of "North Atlantic Deep Water", the term "Atlantic Meridional Overturning Circulation" would be more appropriate to make clearer link with Galbraith & Jaccard (2015) paper as well as with CO2 release into the atmosphere [Damien Cardinal, France]	Accepted - change was made.
2495	45	42			To get a better circumpolar picture I suggest to include other studies that highlighted the release of sequestered carbon from the oceans interior to the surface and atmoaphere: (Sikes et al., 2016 EPSL 438, 130pp; Skinner et al., 2010 Science 328, 1147pp; Skinner et al., 2015 EPSL 411, 45pp; Ronge et al. 2016, Nature Comm 7, 11487; Ronge et al., 2020 Paleoceanography & Paleoclimatology 35, PA003733). The Ronge 2020 study might be of particular interest as it's upt to date the only study from the Indian sector of the Southern Ocean [Thomas Ronge, Germany]	Accepted - change was made.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
98241	45	43	45	44	Intermediate waters oxygen concentrations are a result of delivery and solubility (temperature) as noted, but also the rate of respiration, which goes up exponentially with temperature. So to be complete, the stated increase in temperture affects not only chemical solubility, but also biological removal via respiration. [Gregory Cutter, United States of America]	Rejected. The statement implicitely considers the effect of temperature on solubility and repiration rates
33335	45	44			Change: “OMZs underwent” by “Oxygen minimum zones underwent”. [Guiomar Rotllant, Spain]	Accepted - change was made.
99347	45	45	45	46	Strictly speaking the B/A is a term coined for Northwest Europe and refers to pollen changes in the Nordic countries. The INTIMATE palaeoclimate group has reccomended the adoption of the Greenland event stratigraphy as a more general term for this interstadial (GI1). It is also important to note that the Bolling Allerod was in Norway separated by pollen chhanges associated with colling termed the Younger Dryas, but this cooling signal is not seen in all regions or records. It is also worth noting that the 14.7 age is based on the GICC05 Greenland timescale for GI1 and not the B/A, where in different reagons there is evidence for a lag in response. For example in lake records oxygen isotopic and insect temperature reconstructions are broadly sinchronoous with the Greenland timescale but pollen (the original definition of the interstadial) lags behind. [Simon Blockley, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. While the comment is certainly valid, we maintain the original argumentation for the sake of consistency between chapters.
19979	45	47	45	49	It is not easy to decide whether the climatic driver here is the rate of warming (beginning of the sentence) or the warming itself (end of sentence). In both cases, one wonders to which observations these suggested conclusions refer, since subsection 5.3.1.2, beyond the overall context of deglaciation, does not deal with temperature. [philippe waldteufel, France]	Accepted - change was made.
2497	45	49			I think it is worth to discuss the role of the Southern Ocean during the Younger Dryas CO2 increase. So far the data from the Atlantic and Pacific sectors indicate no oceanic contribution during this time interval. Up to today, carbon release from permafrost soils during the YD was inferred to be the main source for this increase (Köhler et al., 2014, Nature Comm 5, 5520; Winterfell et al., 2018, Natuer Comm 9, 3666). Now a first study indicates a YD contribution of CO2 release from the Indian Sector of the Southern Ocean (Ronge et al., 2020, Paleoceanography & Paleoclimatology 35, PA003733). [Thomas Ronge, Germany]	Rejected. The study is somewhat controversial and has, in part, been questioned by Gottschalk et al., 2020. Uncertainities inherent to the age model prevent robustly relating the CO2 release to the YD.
67483	45	52	45	52	change "enhance" to "enhanced" [James Christian, Canada]	Accepted - change was made.
99441	45	53	45	53	Nitrogen stable isotope lack the symbol, cannot know if it is 14 or 15 [Isabel Seguro, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. The text description is accurate.
31983	45	55			No discussion of the Younger Dryas transition? Severinghaus, Jeffrey P., et al. "Timing of abrupt climate change at the end of the Younger Dryas interval from thermally fractionated gases in polar ice." Nature 391.6663 (1998): 141-146. [Euan G. Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. The current discussion focuses on emission rates (as opposed to changes in concentrations)
40707	46	1	47	30	section 5.3.2: what about trends in deoxygenation? [TSU WGI, France]	Rejected - trends in deoxygenation have been mentioned in section 5.3.3.2.
40705	46	3			section 5.3.2.1: there is not so much information about the ranges given. Is it the likely range? The very likely one? [TSU WGI, France]	Noted - ranges given are those evaluated by in-situ observations. Sentences were rephrased accordingly.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
42793	46	5	46	10	suggest to divide and present the observed trends in each ocean domain. Now there is strong focus on subtropical, tropical, Pacific and Southern Ocean. Need to include much mmore from North Atlantic, Arctic. [Melissa Chierici, Norway]	Accepted partly - a sentence describing acidification in the interior of the Arctic Ocean was added. Acidification in subpolar N. Atlantic has already been mentioned.
73107	46	12	46	12	Insert 'from' after 'ranging'. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - changes was made.
9277	46	12	46	13	Missing value for number of decades? "…unit per decade for the last [?] decades,…" [Christine Weldrick, Australia]	Noted - lengths of record of measurements differ among sites of observation and thus number of decades are not specified.
4161	46	12	46	13	At pH8, the activity of (H+) is 10 billionths moles/kg, ie a trace concentration. With an additional 1 billionth mole/kg, (pH 7,96), the acitity is not significantly modified despite this 10% increase. For that reason, we suggest not to quantify the concentration of (H+) in terms of PERCENTAGE to avoid an apparent over-assessment of acidity. [Jean DEMONT, France]	Rejected - 10% increase of [H+] is quite large but it translates only to 0.04 for pH. To avoid an apparent underestimation, here we also quantify the concentration of H+ in terms of percentage.
58575	46	12	46	13	The number of decades should be specified ("pH has been decreasing at rates ranging 0.016 to 0.020 pH unit per decade for the last decades") [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted - lengths of the record of measurements differ among sites of observation and thus number of decades are not specified.
36413	46	12	46	16	This sentence is confusing. The decrease in aragonite saturation state should be expressed as percentage decrease per year (see Feely et al., 2012; Carter et al., 2017). [Adrienne Sutton, United States of America]	Noted - the sentence was rephrased to "the saturation state Ω (=[Ca2+][CO32-]/K'sp) of seawater with respect to …".
30599	46	12	46	16	This sentence is confusing. The decrease in aragonite saturation state should be expressed as percentage decrease per year (see Feely et al., 2012; Carter et al., 2017). [nina bednarsek, United States of America]	Noted - this comment is exactly the same as the comment no. 36413 so the response to this comment is also exactly the same as that to the comment no.36413.
74259	46	12	46	27	In the whole paragraph, the handling of whether a number is given with or without a minus sign in front seems inconsistent to me [Christoph Völker, Germany]	Accepted - rates of decrease are all shown with negative sign.
40667	46	12			ranging 0.016 to 0.020 pH unit' this way of showing pH decline is not consistent with elsewhere in the section. Here you have positive numbers while elsewhere they are negative (e.g. L21, 22, 30). Please make it consistent [TSU WGI, France]	Accepted - revised to make it consistent with elsewhere in the section.
45443	46	13	46	14	The "saturation level fo calcium carbonate mineral aragonite" has not been defined. Besides, it may be better written as "the saturation state of seawater with respect to aragonite, a common calcium carbonate mineral". [Olivier Sulpis, Netherlands]	Accepted - saturation state of calcium carbonate is defined, and the sentence is revised.
73109	46	14	46	14	Insert 'from' after 'ranging'. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - changes was made.
99435	46	15	46	32	Takahashi et al 2014 is cited twice in the text but is missed in the reference list at the end of the document. [Isabel Seguro, United Kingdom (of Great Britain and Northern Ireland)]	Accepted
93439	46	18	46	19	quasi-time-series record' needs further explanation.. [Carles Pelejero, Spain]	Accepted - the sentences was rephrased to "… from a record of repeat ocean surface CO2 measurements …".

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
93441	46	22	46	22	Add 'The' before 'decrease', and in fact, the whole paragraph needs a bit of English style polishing. [Carles Pelejero, Spain]	Accepted - changes was made.
42795	46	29	46	33	add information adn add reference Olafsson et al., 2009. Add text: "In the Iceland Sea, a 23-year long time series 1985 to 2008, the surface water pH declined of -0.0024 per year, which is twice as high as the rate found at some subtropical time serie stations. They also found decreasing pH in the deep water (>1500 m) as well as shoaling aragonite saturation horizon by 4 m/yr (Olafsson et al., 2009). [Melissa Chierici, Norway]	Accepted partly - Olafsson et al., 2009 was cited here. The pH trend at Irminger Sea that Olafsson et al., 2009, reported has been updated by Bates et al., 2014. The trends in the ocean interior is not mentioned here.
104889	46	30	46	30	Supposed to be -0.002 or -0.02? [Timothy DeVries, United States of America]	Rejected - pH trend of -0.002 +/- 0.004 per decade has been reported from ice-covered region of the Southern Ocean (Lauvset et al., 2015). pH trend of -0.026 +/- 0.006 per decade has been reported from Irminger Sea (Bates et l., 2014).
9279	46	31	46	32	Replace "near-shore region" with "near-shore regions" [Christine Weldrick, Australia]	Accepted - changes was made.
73111	46	32	46	32	Change 'region' to 'regions'. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - changes was made.
52319	46	33	46	33	please add references: Olafsson et al. (2009) and Chierici et al. (submitted) for time-series estimates in sub-polar Arctic (Olafsson) and polar Arctic (Chierici) [Agneta Fransson, Norway]	Accepted partly - Olafsson et al., 2009 was cited here. Chierici et al. (submitted) is not cited.
38509	46	33	46	33	The reference to Lauvset et al (2015) is missing from the reference list. Lauvset, S. K., N. Gruber, P. Landschützer, A. Olsen, and J. Tjiputra (2015), Trends and drivers in global surface ocean pH over the past 3 decades, Biogeosciences, 12(5), 1285-1298, doi:10.5194/bg-12-1285-2015. [Siv K Lauvset, Norway]	Accepted
93443	46	34	46	36	I'm not sure Brown et al study should be included here as the only exception on acidification trends, since the work does not show data on pH, only on pCO2, and all discussion revolves around this parameter and not on pH. [Carles Pelejero, Spain]	Accepted - the sentence here was deleted.
52329	46	35	46	36	Add "effects on ocean acidification and aragonite saturation due to meltwater from Antarctic ice shelves in the Pacific sector of the Southern Ocean (Mattsdotter-Björk et a. 2014)" [Agneta Fransson, Norway]	Accepted - low saturation state of aragonite in surface layer near glacier fronts in the Southern Ocean was mentioned and Mattsdotter Björk et al., 2014, was cited.
67485	46	35	46	36	"sea-ice dynamics" vague; could state more clearly what changes in sea ice are relevant [James Christian, Canada]	Not applicable - the sentence here was deleted (see the response to the comment 93443.
52321	46	36	46	36	Extended information on times-series on ocean acidification and data is needed. There are times series on the CO2 system in subpolar Arctic, such as Icelandic Sea (Olafsson et al. 2009), and a newly established time-series in polar Arctic in western Fram Strait (Chierici et al. submitted) [Agneta Fransson, Norway]	Rejected - not supported by the peer-reviewed published literature.

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52237	46	36	46	37	Add "Time-series in the Icelandic Sea show decreased pH and aragonite saturation in the deeper layers, xxxx [Agneta Fransson, Norway]	Noted - acidification in deep layers is not the scope of section 5.3.2.1 but that of section 5.3.3.1. Olaffson et al. (2009) which demonstrates the progress of ocean acidification in the deep Iceland Sea was cited in section 5.3.3.1.
42797	46	36	46	37	Add text" The Arctic Ocean is especially vulnerable to OA, mainly due to ist already low carbonate ion concentrations and cold water promoting solubility. The observational evidence lacks behind the surface ocean time series stations in the open oceans. Howevere, recent adnvances and increased observational evidence shows evidence for increased colume and spread of low pH and low aragonite saturated waters from the western Arctic (Qi et al., 2017). Moreover, reent data from a 8-yr ttime series in the Arctic outlflow waters (50 to 150 me layer) in the East Greenland Current show drastic decline in pH of about 0.006/yr and increased pCO2 (Chierici et al., submitted). Other studies reveal ocean acidification and increased CO2 in the internediate layers in teh Arctic interior (Ericson et al., 2014). [Melissa Chierici, Norway]	Rejected - not supported by the peer-reviewed published literature. Acidification trend in the ocean interior is mentioned in section 5.3.3.
67487	46	36	46	37	Possibly true but have a look at Giesbrecht et al 2014 (10.5194/essd-6-91-2014). [James Christian, Canada]	Rejected - Giesbrecht et al 2014 provides a great data set of carbon chemistry in the Canadian Arctic for 1974–2009, but no trend has been analysed for ocean acidification on the basis of the dataset.
52309	46	36	46	42	An eight-year times-series (2011-2019) in the Arctic outflow water on the Greenland shelf and in the East Greenland Current (EGC; 3-11°W) shows a significant pH decrease of 0.006/year (0.06 per decade) in the depth interval of 50-150m, coinciding with increased pCO2 of 5 µatm/year and increased temperature of 0.06/year (Chierici et al., submitted). This Arctic outflow water has decreased buffering capacity, which is transported further south (Chierici et al., submitted). [Agneta Fransson, Norway]	Rejected - not supported by the peer-reviewed published literature.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
2341	46	36		42	Nevertheless, many oceanographic cruises have been conducted in the Arctic and a bunch of studies, not cited here, have highlighted few intresting conclusions that would be great to be mentionned here, such as Rajasakaren et al. (2019) who studied the anthropogenic carbon (Cant) trends in the Arctic and estimated an Cant accumulation of 25 Tg C yr−1 therein, and Ulfsbo et al. (2018) who also estimated the anthropogenic carbon storage and ocean acidification in the intermediate layers of the Eurasian Arctic, and Woosley and Millero (2020) who concluded that changes in the freshwater budget of the western Arctic override its uptake potential, resulting in a weak sink, or possibly source of CO2. In addition, Bellerby (2017) also concluded that the rate and extent of Arctic Ocean acidification is enhanced through increased transport from the North Pacific. Whereas other studies have illustrated how local biogeochemical processes and climate variability can modify projected rates of OA within a coastal shelf system such as in the Bering Sea (Pilcher et al., 2019), mainly due to riverine carbon and nutrient delivery (Terhaar et al., 2019). Also , the reductions in pH and aragonite saturation state in the Arctic surface waters are significantly affected by the difference in future projections for sea-ice reduction rate according to Yamamoto et al. (2012). Yamamoto A., M. Kawamiya, A. Ishida, Y. Yamanaka, and S. Watanabe, 2012. Impact of rapid sea-ice reduction in the Arctic Ocean on the rate of ocean acidification. Biogeosciences, 9, 2365–2375, doi:10.5194/bg-9-2365-2012. www.biogeosciences.net/9/2365/2012/ ; Ulfsbo, A., Jones, E. M., Casacuberta, N., Korhonen, M., Rabe, B., Karcher, M., & van Heuven, S. M. A. C. (2018). Rapid changes in anthropogenic carbon storage and ocean acidification in the intermediate layers of the Eurasian Arctic Ocean: 1996–2015. Global Biogeochemical Cycles, 32, 1254– 1275. https://doi.org/10.1029/2017GB005738 ; Bellerby, R., 2017. Ocean acidification without borders. Nature Clim Change 7, 241–242 (2017). https://doi.org/10.1038/nclimate3247 ; Rajasakaren B., Emil Jeansson, Are Olsen, Toste Tanhua, Truls Johannessen, W.M. Smethie, 2019.	Taken into account – a result of Ulfsbo et al. (2018) was mentioned in section 5.3.3.1. A result of Yamamoto et al., (2012) and Terhaar et al. (2019) were mentioned in section 5.3.3.3. Belleby et al. (2017) was not cited but Qi et al. (2017) was cited in section 5.3.3.1. Rajasakaren et al (2019), Pilcher et al. (2019), and Woosely et al. (2020) are also interesting papers but it was not possible to mention them because they don't fit with the context of this section or a bit too detailed to mention.
2209	46	36		42	Nevertheless, many oceanographic cruises have been conducted in the Arctic and a bunch of studies, not cited here, have highlighted few interesting conclusions that would be great to be mentioned here, such as studies by Rajasakaren et al. (2019) who studied the anthropogenic carbon (Cant) trends in the Arctic and estimated an Cant accumulation of 25 Tg C yr−1 therein, and Ulfsbo et al. (2018) who also estimated the anthropogenic carbon storage and ocean acidification in the intermediate layers of the Eurasian Arctic, and Woosley and Millero (2020) who concluded that changes in the freshwater budget of the western Arctic override its uptake potential, resulting in a weak sink, or possibly source of CO2. In addition, Bellerby (2017) also concluded that the rate and extent of Arctic Ocean acidification is enhanced through increased transport from the North Pacific. Whereas other studies have illustrated how local biogeochemical processes and climate variability can modify projected rates of OA within a coastal shelf system such as in the Bering Sea (Pilcher et al., 2019), mainly due to riverine carbon and nutrient delivery (Terhaar et al., 2019). Also, the reductions in pH and aragonite saturation state in the Arctic surface waters are significantly affected by the difference in future projections for sea-ice reduction rate according to Yamamoto et al. (2012). [Abed El Rahman Hassoun, Lebanon]	Taken into account - this comment is the same as the comment 2341. Response was made to the comment 2341.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
2249	46	36		42	Yamamoto A., M. Kawamiya, A. Ishida, Y. Yamanaka, and S. Watanabe, 2012. Impact of rapid sea-ice reduction in the Arctic Ocean on the rate of ocean acidification. Biogeosciences, 9, 2365–2375, doi:10.5194/bg-9-2365-2012. www.biogeosciences.net/9/2365/2012/ [Abed El Rahman Hassoun, Lebanon]	Taken into account - see the response to the comment 2341.
2251	46	36		42	Ulfsbo, A., Jones, E. M., Casacuberta, N., Korhonen, M., Rabe, B., Karcher, M., & van Heuven, S. M. A. C. (2018). Rapid changes in anthropogenic carbon storage and ocean acidification in the intermediate layers of the Eurasian Arctic Ocean: 1996–2015. Global Biogeochemical Cycles, 32, 1254– 1275. https://doi.org/10.1029/2017GB005738 [Abed El Rahman Hassoun, Lebanon]	Taken into account - see the response to the comment 2341.
2253	46	36		42	Bellerby, R., 2017. Ocean acidification without borders. Nature Clim Change 7, 241–242 (2017). https://doi.org/10.1038/nclimate3247 [Abed El Rahman Hassoun, Lebanon]	Taken into account - see the response to the comment 2341.
2255	46	36		42	Rajasakaren B., Emil Jeansson, Are Olsen, Toste Tanhua, Truls Johannessen, W.M. Smethie, 2019. Trends in anthropogenic carbon in the Arctic Ocean. Progress in Oceanography, Volume 178, 102177, ISSN 0079-6611, https://doi.org/10.1016/j.pocean.2019.102177. (http://www.sciencedirect.com/science/article/pii/S007966111830168X) [Abed El Rahman Hassoun, Lebanon]	Rejected - see the response to the comment 2341.
2257	46	36		42	Terhaar, J., Orr, J. C., Ethé, C., Regnier, P., & Bopp, L. (2019). Simulated Arctic Ocean response to doubling of riverine carbon and nutrient delivery. Global Biogeochemical Cycles, 33, 1048– 1070. https://doi.org/10.1029/2019GB006200 [Abed El Rahman Hassoun, Lebanon]	Taken into account - see the response to the comment 2341.
2259	46	36		42	Pilcher Darren J., Naiman Danielle M., Cross Jessica N., Hermann Albert J., Siedlecki Samantha A., Gibson Georgina A., Mathis Jeremy T., 2019. Modeled Effect of Coastal Biogeochemical Processes, Climate Variability, and Ocean Acidification on Aragonite Saturation State in the Bering Sea. Frontiers in Marine Science, 5, 508, DOI: 10.3389/fmars.2018.00508 https://www.frontiersin.org/article/10.3389/fmars.2018.00508, [Abed El Rahman Hassoun, Lebanon]	Rejected - see the response to the comment 2341.
2261	46	36		42	Woosley, R.J. and Millero, F.J. (2020), Freshening of the western Arctic negates anthropogenic carbon uptake potential. Limnol Oceanogr. doi:10.1002/lno.11421 [Abed El Rahman Hassoun, Lebanon]	Rejected - see the response to the comment 2341.
2293	46	36			Yamamoto A., M. Kawamiya, A. Ishida, Y. Yamanaka, and S. Watanabe, 2012. Impact of rapid sea-ice reduction in the Arctic Ocean on the rate of ocean acidification. Biogeosciences, 9, 2365–2375, doi:10.5194/bg-9-2365-2012. www.biogeosciences.net/9/2365/2012/ [Abed El Rahman Hassoun, Lebanon]	Taken into account - see the response to the comment 2341.
74261	46	37	46	37	Avoid having twice “robust” in the sentence [Christoph Völker, Germany]	Accepted - text revised.
52325	46	40	46	41	Suggested information on effects due to " As a result of dissolved bedrock-derived carbonate minerals in glacial meltwater in Svalbard and Greenland fjords, the buffering capacity increased and impacted the potential for ocean CO2 uptake in the surfac water (Fransson et al. 2015; Meire et al. 2017; Hopwood et al. 2020) [Agneta Fransson, Norway]	Rejected - an interesting finding, but the impact of the increased freshwater input on the ocean CO2 uptake is outside the scope of this section.
52327	46	41	46	42	Information on CO2 uptake due to freshwater could be added: "The ocean CO2 uptake on the freshwater-influenced shelves in the Canadian Arctic Archipelago in the Arctic Ocean and glacier-influenced Svalbard fjords increased due to sea-ice meltwater and glacial meltwater (Fransson et al. 2009; 2015) " [Agneta Fransson, Norway]	Rejected - an interesting finding, but the impact of the increased freshwater input on the ocean CO2 uptake is outside the scope of this section.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
52331	46	41	46	42	after "…river discharges (high confidence) (Bates et al. 2013)" add "and melting of glacial ice (Fransson et al. 2015; Meire et al. 2015; Hopwood et al. 2020)" regarding impact of freshwater on ocean acidification [Agneta Fransson, Norway]	Accepted - melting of glacial ice was mentioned and Fransson et al. 2015 was cited.
116433	46		49		please add a short summary statement at the end of sections. [Valerie Masson-Delmotte, France]	Accepted - short summary statement was added to the each of subsections.
2295	46			42	Ulfsbo, A., Jones, E. M., Casacuberta, N., Korhonen, M., Rabe, B., Karcher, M., & van Heuven, S. M. A. C. (2018). Rapid changes in anthropogenic carbon storage and ocean acidification in the intermediate layers of the Eurasian Arctic Ocean: 1996–2015. Global Biogeochemical Cycles, 32, 1254– 1275. https://doi.org/10.1029/2017GB005738 [Abed El Rahman Hassoun, Lebanon]	Taken into account - see the response to the comment 2341.
2305	46				Woosley, R.J. and Millero, F.J. (2020), Freshening of the western Arctic negates anthropogenic carbon uptake potential. Limnol Oceanogr. doi:10.1002/lno.11421 [Abed El Rahman Hassoun, Lebanon]	Rejected - see the response to the comment 2341.
2297	46				Bellerby, R., 2017. Ocean acidification without borders. Nature Clim Change 7, 241–242 (2017). https://doi.org/10.1038/nclimate3247 [Abed El Rahman Hassoun, Lebanon]	Taken into account - see the response to the comment 2341.
2299	46				Rajasakaren B., Emil Jeansson, Are Olsen, Toste Tanhua, Truls Johannessen, W.M. Smethie, 2019. Trends in anthropogenic carbon in the Arctic Ocean. Progress in Oceanography, Volume 178, 102177, ISSN 0079-6611, https://doi.org/10.1016/j.pocean.2019.102177. (http://www.sciencedirect.com/science/article/pii/S007966111830168X) [Abed El Rahman Hassoun, Lebanon]	Rejected - see the response to the comment 2341.
2301	46				Terhaar, J., Orr, J. C., Ethé, C., Regnier, P., & Bopp, L. (2019). Simulated Arctic Ocean response to doubling of riverine carbon and nutrient delivery. Global Biogeochemical Cycles, 33, 1048– 1070. https://doi.org/10.1029/2019GB006200 [Abed El Rahman Hassoun, Lebanon]	Taken into account - see the response to the comment 2341.
2303	46				Pilcher Darren J., Naiman Danielle M., Cross Jessica N., Hermann Albert J., Siedlecki Samantha A., Gibson Georgina A., Mathis Jeremy T., 2019. Modeled Effect of Coastal Biogeochemical Processes, Climate Variability, and Ocean Acidification on Aragonite Saturation State in the Bering Sea. Frontiers in Marine Science, 5, 508, DOI: 10.3389/fmars.2018.00508 https://www.frontiersin.org/article/10.3389/fmars.2018.00508, [Abed El Rahman Hassoun, Lebanon]	Rejected - see the response to the comment 2341.
9281	47	3	47	3	"time series" not hyphenated here, but hyphenated earlier in document. Need to be consistent. [Christine Weldrick, Australia]	Accepted - thank you for pointing this inconsistency. The version "time series" (without hyphen) has been adopted throughout the chapter.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
93445	47	3	47	28	I am repeating here my concerns raised during the FOD revision, which were mostly not taken into consideration. I totally disagree with the first sentence of this paragraph claiming that "d11B records evidence a prominent ocean acidification trend since mid-20th century (high confidence)". This assertion is very incorrect. 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, there are also typos (e.g. 'pH units' in plural, please), missing spaces and odd phrases. For example, the sentence "A distinct feature of coral δ11B records is ocean acidification trends since the mid-20th century albeit having wide-range values" does not read properly. The last sentences were rephrased according to a suggestion I made, but the writer changed two crucial words ''some' by 'most' and 'may be' by 'is' making an assertion which, as I indicate at the beginning of this comment, is totally incorrect: so far only one d11B record seems to record evidence of acidification. Please, remove 'most' and 'is', which are totally misleading. The proper sentence should be: '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.' And, as the last sentence, I suggest the following: 'To be able to better detect recent decreases in pH and attribute them to human-induced pressures, further work is needed including better calibrations and constraints on the use of this paleo-pH proxy in different tropical coral species and reefs'. Note also that Pelejero (2005) ref should be Pelejero et al (2005). [Carles Pelejero, Spain]	Accepted for editorial point - thank you for pointing the error in our Mendeley for Liu et al. (2014) and other typos. While we acknowledge uncertainties associated with coral d11B-based pH reconstructions and strong decadal variations, the majority of published coral d11B records exhibit a clear trend since mid-20th. We do not assign high confidence for period before mid 20th century.
73113	47	4	47	4	Capital 'C' for 'century' (for consistency elsewhere in chapter). [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - TSU style guide requests "21st century" without capital C. (It was made consistent now to not use the capital C).
2343	47	9		12	Thanks for mentioning whether the pH changes are positive or negative (+ or -). [Abed El Rahman Hassoun, Lebanon]	Noted.
2211	47	9		12	Thanks in advance for mentioning whether the pH changes are positive or negative (+ or -). [Abed El Rahman Hassoun, Lebanon]	Noted.
38511	47	12	47	12	rewrite to: "Decadal variations in the South China Sea ocean pH changes of ..." [Siv K Lauvset, Norway]	Accepted.
67489	47	12	47	12	delete "setting" [James Christian, Canada]	Accepted - changes was made.
74263	47	13	47	13	space missing after comma [Christoph Völker, Germany]	Accepted - changes was made.
9283	47	13	47	13	Add a space after comma: "…East Asian Monsoon (Liu et al., 2015; Wei et al., 2015),[add space here]as a…" [Christine Weldrick, Australia]	Accepted - changes was made.
73115	47	13	47	13	Space reuquired before 'as'. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - changes was made.
33337	47	13			Change: "…(Liu et al., 2015; Wei et al., 2015),as a… » by « … (Liu et al., 2015; Wei et al., 2015), as a…". [Guiomar Rotllant, Spain]	Accepted - changes was made.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
58561	47	15	47	15	I suggest to add a sentence to explain the dependence of the pH seasonality not only to the seasonal [H+] but also to the annual global mean [H+], simultaneously increasing by 117+/- 3% over the 21st century under RCP 8.5. The counter intuitive opposite variation of the seasonal amplitudes can surprise the reader as I have been. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected - we appreciate the suggestion, however, the suggestion is outside the scope of the subsection on coral-based paleo pH records.
73117	47	18	47	18	Capital 'C' for 'century' (for consistency elsewhere in chapter). [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - TSU style guide requests "21st century" without capital C. (It was made consistent now to not use the capital C).
73119	47	21	47	21	Capital 'C' for 'century' (for consistency elsewhere in chapter). [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - TSU style guide requests "21st century" without capital C. (It was made consistent now to not use the capital C).
38513	47	21	47	23	unclear sentence [Siv K Lauvset, Norway]	Accepted - text revised.
67491	47	21	47	23	"Concurrent coral carbon isotopic (δ13C) measurements infer the ocean uptake of anthropogenic CO2 from the combustion of fossil fuel characterised based on the isotopically depleted δ13 C of fossil fuels." Concurrent coral carbon isotopic (δ13C) measurements infer ocean uptake of anthropogenic CO2 from the combustion of fossil fuel, based on the lower abundance of 13C in fossil fuel carbon. [James Christian, Canada]	Accepted -change was made.
73121	47	22	47	22	Delete 'characterised'. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - changes was made.
73123	47	24	47	24	Capital 'C' for 'century' x2 (for consistency elsewhere in chapter). [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - TSU style guide requests "21st century" without capital C. (It was made consistent now to not use the capital C).
73125	47	24	47	24	th' should be superscript [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - TSU style guide requests to put terms like "21st century" without putting "st" in superscript. Hence this is applicable for the 20th century.
115363	47	25		27	This wasn't completely clear to me. Is this text saying that there is only medium confidence that the decline in ocean pH is due to human activities? Ch3 currently assesses this as 'virtually certain'. If this isn't the meaning, clarify the text here. If Ch5 really assesses attribution of ocean acidification to human activity to only be 'medium confidence' please coordinate with Chapter 3, and we can discuss our assessment. [Gillett Nathan, Canada]	Rejected - thank you for the comment. However, the suggestion is outside the scope of the subsection on coral-based paleo pH records.
67493	47	26	47	26	change "oscillating" to "variable" [James Christian, Canada]	Accepted.
67495	47	38	47	38	change ""accompanying the removal of dissolved oxygen" to "removing dissolved oxygen" [James Christian, Canada]	Accepted - text revised.
36379	47	43	47	43	The Lauvset et al paper has been published. The proper reference is: Lauvset, S.K., B.R. Carter, F.F. Perez, L.-Q. Jiang, R.A. Feely, A. Velo, and A. Olsen (2020): Processes driving global interior ocean pH distribution. Global Biogeochem. Cycles, 34(1), e2019GB006229, doi: 10.1029/2019GB006229 [Adrienne Sutton, United States of America]	Accepted - the citations of Lauvset et al. in the text and the caption of Figure 5.21 were amended.
30565	47	43	47	43	The Lauvset et al paper has been published. The proper reference is: Lauvset, S.K., B.R. Carter, F.F. Perez, L.-Q. Jiang, R.A. Feely, A. Velo, and A. Olsen (2020): Processes driving global interior ocean pH distribution. Global Biogeochem. Cycles, 34(1), e2019GB006229, doi: 10.1029/2019GB006229 [nina bednarsek, United States of America]	Accepted - the citations of Lauvset et al. in the text and the caption of Figure 5.21 were amended.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
45445	47	43	47	43	The "Lauvset et al. submitted" reference is not up to date, and does not appear in the reference list. This should be corrected here and after. [Olivier Sulpis, Netherlands]	Accepted - the citations of Lauvset et al. in the text and the caption of Figure 5.21 were amended.
67497	47	44	47	45	"largely connecting with the structure of the three-dimensional ocean circulation" vague. Not clear what is meant here. [James Christian, Canada]	Accepted - text revised.
36381	47	47	47	47	You should add a references to: Carter, B.R., R.A. Feely, R. Wanninkhof, S. Kouketsu, R.E. Sonnerup, P.C. Pardo, C.L. Sabine, G.C. Johnson, B.M. Sloyan, A. Murata, S. Mecking, B. Tillbrook, K. Speer, L.D. Talley, F.J. Millero, S.E. Wijffels, A.M. Macdonald, N. Gruber, and J.L. Bullister (2019): Pacific anthropogenic carbon between 1991 and 2017. Global Biogeochem. Cycles, 33(5), 597–617, doi: 10.1029/2018GB00615 [Adrienne Sutton, United States of America]	Accepted - Carter et al. (2019) was also cited here.
30567	47	47	47	47	You should add a references to; Carter, B.R., R.A. Feely, R. Wanninkhof, S. Kouketsu, R.E. Sonnerup, P.C. Pardo, C.L. Sabine, G.C. Johnson, B.M. Sloyan, A. Murata, S. Mecking, B. Tillbrook, K. Speer, L.D. Talley, F.J. Millero, S.E. Wijffels, A.M. Macdonald, N. Gruber, and J.L. Bullister (2019): Pacific anthropogenic carbon between 1991 and 2017. Global Biogeochem. Cycles, 33(5), 597–617, doi: 10.1029/2018GB00615 [nina bednarsek, United States of America]	Accepted - the same as the response to the comment 36381.
73127	47	47	47	47	Capital 'T' for tropics [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - small "t" for tropics is commonly used.
52241	47	48	47	51	I suggest to add a time-series in sub-polar ocean: "Morover, time-series from the period of 1984-2008 in the Icelandic Sea show decreased surface-water pH by 0.002/year and aragonite saturation (Ωar) by 0.007/year, and increased pCO2 by 2.1 ppm/year in the deeper layers (Olafsson et al 2009). Deep water Ωar horizon shoaled 4m/yr (from 1763 to 1710m; Olafsson et al. 2009)" [Agneta Fransson, Norway]	Taken into account - citation of Olaffson et al. (2009) was added here and the shoaling of aragonite saturation horizon in the Iceland Sea was mentioned in brief.
52323	47	48	47	51	Please add studies in the Icelandic Sea, subpolar Arctic: Olafsson et al. (2009) time-series of the CO2 system, where aragonite saturation and pH decreased both in the surface and deeper, during a more than 20-years study. During the period 1984-2008 decreased surface-water pH by 0.002/year and aragonite saturation (Ωar) by 0.007/year were observed, and increased pCO2 by 2.1 ppm/year in the deeper layers (Olafsson et al 2009). Deep water Ωaragonite horizon shoaling of 4m/yr (from 1763 to 1710m) Olafsson et al. (2009) [Agneta Fransson, Norway]	Taken into account - see the response to the comment 52241.
73129	47	49	47	49	Delete 'time' and 'of'. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - changes was made.
42799	47	51	47	51	Add" This is larger than the shoaling of 4 m/yr found in teh Iceland Sea in the period 1985 to 2008 (Olafsson et al., 2009), which may indicate a speed up of shoaling." [Melissa Chierici, Norway]	Accepted - text was revised taking this comment into account.
90087	47		52		This statement uploaded by Ted Schuur on behalf of a graduate class that reviewed the chapter (this was communicated with IPCC TSU in advance): There were at least 20 uses of likelihood language in 5.3.3, and five instances in 5.3.4, this seems unbalanced. [Edward Schuur, United States of America]	Taken into account - the text in section 5.3.4 has been fully re-written but not extended in length. In the revised version, the use of likelihood language (calibrated) increased and the sections 5.3.3 and 5.3.4 are more balanced.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
87689	48	10	48	10	The Resplandy et al, 2013, http://doi.wiley.com/10.1002/grl.50414, may be a reference to be added in this paragraph. [Ivy Frenger, Germany]	Taken into account - the results from Resplandy et al. (2013) is outside the scope of this section but are mentioned in section 5.3.3.3.
74265	48	11	48	13	there are several works one could cite here for that, among them Panassa et al, 2018 (https://doi.org/10.1007/s10236-018-1131-2), and Salt et al., 2015 (https://doi.org/ 10.5194/bg-12-1387-2015) [Christoph Völker, Germany]	Taken into account - Salt et al. (2015) was cited in section 5.3.3.3. Panassa et al. (2018) was not cited in this section because it doesn't address ocean acidification.
67499	48	13	48	13	water mass name should be upper case as on line 21 [James Christian, Canada]	Rejected - upper case are used for proper nouns.
73131	48	15	48	16	References should be in chronological order. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial - this kind of issues will be taken care of during the production phase, which includes professional copy-editing.
41649	48	20	48	23	This sentence is difficult to understand [Katharina Meurer, Sweden]	Accepted - the sentence was revised.
73133	48	21	48	21	Replace 'the' with 'a'. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - changes was made.
3917	48	26	48	26	How about adding the following sentence: "In the western Pacific subtropical region (30N, 145E), based on observation between 2004 and 2011, aragonite and calcite saturation horizon at around 700 m and 900 m in 2004, respectively, has been shoaling at a rate of 7 m and 9 m per year, respectively (Honda et al. 2017).* Honda et al. J Oceanogr (2017) 73:647-667 DOI 10.1007/s10872-017-0423-3 [Makio Honda, Japan]	Accepted - the shoaling of the aragonite saturation horizon in the North Pacific subtropics was mentioned in the earlier paragraph of this section.
3623	48	29	49	16	Following the introduction that quickly presents deoxygenation, this is the first section talking about deoxygenation. It focuses specifically on implications for GHGs, namely for N2O in this case. I think that to make the whole story clearer, a section or a paragraph about the causes of deoxygenation would be needed before going into the details of the effect on N2O, since there are many other processes happening. A description of the causes is interesting as it is linked to various consequences of climate change: the increase in temperature that decreases solubility, the increase in stratification/reduction in ventilation that increases the residence time of deep waters and hence the time for oxygen consumption, and the slow-down of some circulation patterns due to climate change. This report is not the place to go into the details, but here are some additional details just to add some references: about 15% of global deoxygenation can be attributed to the mixed layer temperature increase (Schmidtko et al, Nature, 2017; Helm et al., GRL, 2011). Ventilation and circulation changes explain most of the variations in oxygen concentrations (Deutsch et al, JGR: Oceans, 2006; Long et al., GBC, 2016).While increased stratification increases the residence time of waters, it also reduces the nutrient supply towards the surface, similarly to what is discussed for N2O in the second paragraph of this section. The two effects partially compensate each other (Ito et al., GRL, 2017; Tagklis et al., Biogeosciences, 2020). [Mathilde Jutras, Canada]	Accepted - causes of deoxygenation and importance of solubility reduction due to warming were mentioned briefly as they are repetition of SROCC.
112487	48	37	48	38	Suggested references: Stramma et al. 2010 doi: 10.1029/2009JC005976 ; Deutsch et al., 2011 doi: 10.1126/science.1202422 ; Llanillo et al., 2013 doi: 10.5194/bg-10-6339-2013 [Pedro LLanillo del Rio, Germany]	Accepted - Deutsch et al., 2011 and Llanillo et al., 2013 were cited. Stramma et al., 2010 was not.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
67501	48	44	48	44	oxidation misspelled [James Christian, Canada]	Accepted - changes was made.
131535	48	46	48	46	Do you mean "CO2" or "N2O?" in this phrase: "CO2 from deeper water masses is emitted to the atmosphere". Context indicates it should be N2O [Hans Poertner and WGII TSU, Germany]	Noted - we mean CO2 as described. Tyrrell and Lucas (2002) cited here suggests that denitrification in OMZ deplete nitrate and thus primary production is reduced when oMZ water upwelled thereby enhancing the CO2 enrichment in surface layer and CO2 emission to the atmosphere.
83997	48	47	48	47	Please insert data local anthropogenic and acidity of marine sediments in Bays with foraminiferal calcium carbonate shells being dissolved and replaced by agglutinated invasive forms which uses sediment grains to compose its tests (Eichler et al., 2014 and 2018). [Marco Tulio Cabral, Brazil]	Rejected - outside the scope of this subsection.
115365	48	49			Total N2O production in the ocean? Clarify. [Gillett Nathan, Canada]	Accepted - "in the ocean" was added in the text.
26899	48	53	48	53	We suggest to add the reference Paulmier et Ruiz-Pino 2008 [Eric Brun, France]	Rejected - here we refer to the oxygen decrease in the OMZs of the tropical oceans reported in Stramma et al (2008).
19981	48	53	48	55	OMZ have been introduced in subsection 5.3.1.1 [philippe waldteufel, France]	Accepted - text revised.
67503	48	53	49	16	This paragraph repeats some fairly basic information and appears to have been spliced in after having been written by someone who was not familiar with the rest of the text. "regions of lowest oxygen, known as oxygen minimum zones (OMZs)" appears three pages after the term is first defined, with 5 or 6 occurrences in between. [James Christian, Canada]	Accepted - text revised.
3625	48	54	48	54	To be more precise when we say "found in the tropical oceans", we should say in the gyres of the subtropical oceans. [Mathilde Jutras, Canada]	Rejected - here we refer to the work of Stramma et al (2008) and followed the expression of this work.
73135	49	4	49	4	Remove , before 'and'. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - changes was made.
99443	49	7	49	7	Nitrogen and methane quantities in between brackets, but not CO2 quantity [Isabel Seguro, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - indeed all references quoted allow to provide a bracket estimate for both CH4 and N2O covering most OMZs but the scarcity of CO2 data in OMZs prevent for providing a similar estimate for CO2.
26901	49	8	49	8	We suggest to add the reference Paulmier et al 2008 [Eric Brun, France]	Accepted - Paulmier et al. (2008) was also cited here.
73137	49	9	49	9	Insert 'the' before 'RCP8.5'. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - changes was made.
41651	49	9	49	12	It is not clear to me of the expansion of the OMZ is expected to happen around 2100? In this sentence, it is first about an increase in oceanic N2O emissions, which are expected to compensate for a decrease in ocean N2O emissions over the same time. Why compensate? [Katharina Meurer, Sweden]	Taken into account - text was revised to "Modelling projections suggest a global decrease of 4 to 12% in oceanic N2O emissions from 2005 to 2100 under RCP8.5 emission scenario, despite a tendency to increased N2O production in the OMZs associated primarily to denitrification (low confidence) (Martinez-Rey et al., 2015). "

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
87691	49	9	49	13	"According to modelling projections of oceanic N2O emissions in 2100 under RCP8.5 emission scenario, the expansion of OMZs could thus increase N2O production, associated primarily to denitrification, and partly compensate for the decrease in global ocean N2O emissions from 2005 to 2100 (low confidence) (Martinez-Rey et al., 2015). It is yet unclear whether N2O production from bacterial nitrification increases exponentially or linearly with decreasing oxygen and whether a threshold oxygen value exists below which" -> difficult to follow, could you please rephrase to clarify? [Ivy Frenger, Germany]	Accepted - text revised.
67505	49	12	49	14	The way this sentence is written makes it sound like there is an N2O sink associated with nitrification. But the latter part actually refers to denitrification (cf. Babbin et al; note that there is only one Babbin et al 2015 in the ref list but Babbin et al 2015, 2015a, and 2015b are all cited in the text). [James Christian, Canada]	Accepted - text was revised to "It is difficult to single out the contribution of nitrification and denitrification which can occur simultaneously. A rigorous separation of these two processes would require more mechanistic parameterizations that have been hindered by the still large conceptual and parametric uncertainties (Babbin et al., 2015; Trimmer et al., 2016; Landolfi et al., 2017)." Citation of Babbin et al 2015 was corrected.
89359	49	19	50	8	The acidification of the ocean interior is discussed in terms of observations (sec.5.3.3.1), but not predictions. In Replandy et al. (2013) and Watanabe and Kawamiya (2017), using CMIP5 models data, acidification of mode and intermediate waters (0.3 by the end of this century under RCP 8.5 scenario) is reported. It would be good to introduce these. (doi:10.1002/grl.50414, doi: 10.1007/s10872-017-0431-3) [Michio Watanabe, Japan]	Accepted - faster pH decline in mode waters is mentioned, and Replandy et al. (2013) and Watanabe and Kawamiya (2017) are cited here.
45447	49	19	50	8	Future projections of ocean acidification in the ocean interior are absent of this paragraph, even though recent research has covered this topic. Sulpis et al. (2019, Global Biogeochemical Cycles 33) compared predictions of ocean acidification in the 21st century from different earth system models, for the RCP8.5 scenario and showed that during the 21st century: 1) the world-average calcite saturation depth is projected to rise by ~150 meters, 2) the world-average bottom-water DIC is projected to rise by ~9 µmol/kg, and in the Labrador Sea and at the southern edge of Greenland, the bottom-water DIC increase by the end-of the century relative to the beginning are projected to be as high as +70 µmol/kg. This may be worth mentioning here, as it also follows up on section 5.3.3.1. [Olivier Sulpis, Netherlands]	Accepted - future projections of ocean acidification in the bottom waters are mentioned.
78499	49	19			a figure could be useful here – lots of info. Need to coordinate with chapter 4 who show global ocean ph changes in projections [Chris Jones, United Kingdom (of Great Britain and Northern Ireland)]	Noted - a figure could be useful but no more space to put the figure is allowed in this section. Instead, coordination with chapter 4 was made.
36415	49	21	49	27	The authors need to discuss the new projections for the global surface ocean pH distributions by Jiang et al 2019. The changing Revelle Factors with increasing pCO2 makes the acidity responses nonlinear. [Adrienne Sutton, United States of America]	Taken into account - the effect of changing Revel factor is discussed in 5.4.3. Jiang et al., 2019 is cited in this section as a reference to the rapid pH decline in polar oceans.

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30601	49	21	49	27	The authors need to discuss the new projections for the global surface ocean pH distributions by Jiang et al 2019. The changing Revelle Factors with increasing pCO2 makes the acidity responses nonlinear. [nina bednarsek, United States of America]	Taken into account - the effect of changing Revel factor is discussed in 5.4.3. Jiang et al., 2019 is cited in this section as a reference to the rapid pH decline in polar oceans.
41125	49	21			unequivocally is not part of the IPCC uncertainty language and therefore should not be italicised. [TSU WGI, France]	Accepted - the text was revised.
115367	49	21			The word 'unequivocally' is not needed here. [Gillett Nathan, Canada]	Accepted - the text was revised.
39691	49	24		25	" The Arctic Ocean is an exception" this is unclear. Do you mean it's different from the SROCC conclusions? Could you clarify? [TSU WGI, France]	Accepted - text revised. "Exception" meant that the freshwater input is another major controlling factor for the acidification in the Arctic Ocean.
36403	49	26	49	26	The authors also need to reference the recent paper by Zhang, Y., Yamamoto-Kawai, M. & Williams, W. J. Two decades of ocean acidification in the surface waters of the Beaufort Gyre, Arctic Ocean: effects of sea ice melt and retreat from 1997-2016. Geophys. Res. Lett. 47, e60119 (2020). [Adrienne Sutton, United States of America]	Taken into account - this published literature was rather cited in section 5.3.2.1.
30587	49	26	49	26	The authors also need to reference the recent paper by Zhang, Y., Yamamoto-Kawai, M. & Williams, W. J. Two decades of ocean acidification in the surface waters of the Beaufort Gyre, Arctic Ocean: effects of sea ice melt and retreat from 1997-2016. Geophys. Res. Lett. 47, e60119 (2020). [nina bednarsek, United States of America]	Taken into account - this published literature was rather cited in section 5.3.2.1.
42801	49	26	49	27	Add references "Steinacher et al., 2005; AMAP AOA 2013:2018" before Qi et al., 2017 [Melissa Chierici, Norway]	Taken into account - Steinacher et al. (2009) was added to citations.
21831	49	32	49	32	very low is a value judgement and not a term associated with RCP2.6 elsewhere in the report. Suggest to delete this qualifier. [Peter Thorne, Ireland]	Accepted - "very low" was deleted.
67507	49	32	49	32	delete "very low" [James Christian, Canada]	Accepted - "very low" was deleted.
73139	49	32	49	32	Insert 'the' after 'except'. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - changes was made.
42803	49	32	49	33	Add" This agrees well with projections based on obervations showing that the surface water in summer aragonite saturation in the Ross and Amundsen Sea will be undersaturated already in year 2026-2030, (Mattsdotter-Björk et al., 2014)." [Melissa Chierici, Norway]	Taken into account - this published literature was rather cited in section 5.3.2.1.
67509	49	37	49	39	"These long term projections are modulated at interannual timescales by large-scale climate modes (Ríos et al., 2015), such as the representation of the El Niño Southern Oscillation and the Southern Annular Mode in models" I see what they are trying to say here but this is a poor choice of words. The easiest fix would be to delete "the representation of" and "in models". But I would say this whole sentence is expendable. [James Christian, Canada]	Accepted - "the representation of" and "in models" were deleted.
67511	49	41	49	42	another very awkward sentence; meaning unclear; modulated by what? [James Christian, Canada]	Accepted - text was revised.
88971	49	42	49	43	It is not easily understandable for persons not familiar with pH why [H+] is hgih variability while pH is low. A sentence like "As pH is logarithm of [H+], changes of pH is associated not only with seasonal amplitudes of [H+] but also annual mean [H+] (Kwiatlowski and Orr, 2018). [AKIHIKO MURATA, Japan]	Accepted - text was revised. A sentence explaining for the decrease of pH seasonality with the increase of [H+] seasonality was added.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
2799	49	42	49	43	given that pH is the negative logarithm of [H+], how can pH amplitude decrease when [H+] amplitude increases surely these are linked, also page 55 line 2&3 [Stephen Wilkinson, United Arab Emirates]	Accepted - text was revised. A sentence explaining for the decrease of pH seasonality with the increase of [H+] seasonality was added.
73141	49	45	49	45	Capital 'C' for 'century' (for consistency elsewhere in chapter). [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - changes was made.
103159	49	51	49	55	While calcification of juvenile crustose coralline algae is initially highly sensitive to ocean acidification, after six generations of exposure the effects of ocean acidification disappear. (Cornwall, C.E., Comeau, S., DeCarlo, T.M. et al. A coralline alga gains tolerance to ocean acidification over multiple generations of exposure. Nat. Clim. Chang. 10, 143–146 (2020). https://doi.org/10.1038/s41558-019-0681-8) [Philippe Tulkens, Belgium]	Rejected - the impact of ocean acidification on marine organisms and ecosystems is out of the scope of this chapter.
26903	49	53	49	53	We suggest to revise the confidence level. This is well known that all of those process modulate the local variability, but we dont know how, and the confidence in quantitification is very low [Eric Brun, France]	Accepted - confidence level was lowered to "medium" because this is with high agreement but low evidence.
116435	50	1	50	8	This is an important statement, but it is very difficult to understand how to make use of it in relationship to projections reported in chapter 4 (direction of changes implied by missing feedbacks, relative magnitude of amplifying effects). [Valerie Masson-Delmotte, France]	Taken into account - text revised. This statement was substituted to the more simple one stating that ocean acidification will continue to grow but magnitude and sign of many of ocean carbon-climate feedbacks that affect the trend of ocean acidification are still poorly constrained.
115371	50	2		5	If the models are used to diganose physical climate changes consistent with particular concentration changes, and also used to infer emissions changes compatible with those concentration changes, then carbon-climate feedbacks are not overlooked - to the extent they are represented in the models used, they are reflected in the diagnosed emissions. [Gillett Nathan, Canada]	Accepted - text was revised.
93449	50	5	50	5	Zhang et al., 2018a ref in the list doesn't seem the proper one, please double check this. [Carles Pelejero, Spain]	Accepted - citation was revised.
67513	50	7	50	7	delete "nevertheless" [James Christian, Canada]	Accepted - changes was made.
18221	50	11	50	11	I would expand the initialism to remind the reader of its meaning as it has not been mentioned for several sections. [Chelsey Baker, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change was made.
127799	50	22	50	22	Remove "in" between "value" and "about". [Trigg Talley, United States of America]	Accepted - changes was made.
127801	50	25	50	27	Is it possible to give a date by when the global ocean pH returns to preindustrial level? [Trigg Talley, United States of America]	Noted - the following sentence notes that ocean pH returns to pre-industrial levels around 2700, even with the most extreme CO2 removal rates
127803	50	25	50	27	Provide more information about what "CO2 extraction" means in this context? [Trigg Talley, United States of America]	Accepted - the sentence has been altered
51149	50	26	50	26	does the "extraction rate" refer to CO2 removed from the occean, or the atmosphere? Please clarify. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - the sentence has been altered
73143	50	27	50	27	Replace 'by' with 'until'. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - changes was made.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
67515	50	29	50	32	I think this paragraph needs some literature references. [James Christian, Canada]	Noted - this paragraph has been combined with the previous one
51151	50	29	50	32	Both sentences of this paragraph seem to be re-stating the point made in the previous paragraph, although it's not clear what is meant by "reversal of the atmospheric CO2 increase". Suggest deletion of this paragraph. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Noted - this paragraph has been combined with the previous one
52163	50	35	50	35	In this section I am missing a dicussion about coastal deoxygenation and N2O/CH4 production, see Ma, X., Lennartz, S. T., and Bange, H. W.: A multi-year observation of nitrous oxide at the Boknis Eck Time Series Station in the Eckernförde Bay (southwestern Baltic Sea), Biogeosciences, 16, 4097-4111, 2019; Farías, L., Besoain, V., and García-Loyola, S.: Presence of nitrous oxide hotspots in the coastal upwelling area off central Chile: an anlysis of temproral variability based on ten years of a biogeochemiocal time series, Environmental Research Letters, 10, 044017, 2015.; Naqvi, S. W. A., Jayakumar, D. A., Narveka, P. V., Naik, H., Sarma, V. V. S. S., D'Souza, W., Joseph, S., and George, M. D.: Increased marine production of N2O due to intensifying anoxia on the Indian continental shelf, Nature, 408, 346-349, 2000.; Capelle, D. W., Hallam, S. J., and Tortell, P. D.: Time-series CH4 measurements from Saanich Inlet, BC, a seasonally anoxic fjord, Mar. Chem., 215, 103664, https://doi.org/10.1016/j.marchem.2019.103664, 2019; Capelle, D. W., Hawley, A. K., Hallam, S. J., and Tortell, P. D.: A multi-year time-series of N2O dynamics in a seasonally anoxic fjord: Saanich Inlet, British Columbia, Limnology and Oceanography, 63, 524-539, 2018. [Hermann Bange, Germany]	Noted - the section went under major revision and the impact on coastal de-oxygenation and N2O and CH4 production is going to be mentioned. However, ocean and coastal emissions of N2O, and trends, are discussed in section "5.2.3.3 Emissions from Ocean, Inland water bodies and Estuaries" in this chapter. According to this report's assessment, the role of coastal low-oxygen areas in N2O emissions not yet well constrained. Regarding CH4, the emissions and sources (open and coastal ocean) are assessed in section "5.2.2.4 Ocean and inland emissions and sinks". For N2O, although it is a consensus that coastal de-oxygenation may enhance its emission, the lack of longer coastal time series such as those already in place for the Baltic Sea, California Current System or East China Sea do not allow a complete assessment regarding trends or spatial variability for the global coastal ocean. For CH4, the interplay between low-O2/anoxia in coastal waters and consequent CH4 production (and accumulation in the water column) does not seem to be fully understood yet, as in (Naqvi, S. W. A., Bange, H. W., Farías, L., Monteiro, P. M. S.,
42805	50	35	50	37	better to discuss drivers in general which is valid for all ocean regions and then specifically address some regional drivers which are more important in one region than another. Drivers should be mentioned and valid for all ocean regions. A division into ocean regions as proposed erlier would make it clearer and easier to include regional specific important drivers. [Melissa Chierici, Norway]	Noted - Please note that unfortunately there is not available literature to assess coastal ocean acidification and de-oxygenation equally in all ocean regions. For this reason, the regions where the observations and models allowed an assessment were included in the revised-text. The following paragraph was added to the end of this section:

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
40709	50	35	52	38	section 5.3.4: a wrap-up/conclusion to insist on the key messages of the section is missing [TSU WGI, France]	Accepted - section 5.3.4 has been completely re-written and the closing message now is: "Scientific literature specific to regional trends in coastal ocean acidification and de-oxygenation is emerging since the beginning of this assessment. This gap highlights that, despite high spatial variability and consequent longer time of emergence for acidification and de-oxygenation, the coastal ocean is an area demanding improved understanding of carbon and oxygen budgets."
96583	50	35	52	38	In 5.3.4 „Coastal ocean acidification and de-oxygenation“ an important area between land and ocean is described. It is good to recognise that eutrophication is included due to its regional prevalence. [Nicole Wilke, Germany]	Accepted - it is indeed important to mention the exacerbating effect that eutrophication has on both anthropogenic CO2-induced coastal ocean acidification and to hypoxia. The section text has been re-written and there is now more emphasis to this aspect.
18053	50	37	50	53	This section is titled Drivers but really only discusses acidification. Deoxygenation seems to be missing. [Lisa Levin, United States of America]	Accepted - Coastal de-oxygenation drivers were added to this section inthe 2nd paragraph. In addition to the drivers, an assessment of coastal ocean de-oxygenation was added regarding the status since SROCC.
131537	50	37	50	53	Section 5.4.3.1 should be about "Drivers" Yet, the content focusses on the vulnerability of different ecosystems - which is mandate of WGII. [Hans Poertner and WGII TSU, Germany]	Taken into account - this section text went through major revision to avoid the overlap with WGII. A cross-reference to WGII, chapter 03, was included where necessary in the revised text.
3627	50	39	50	53	The effect of eutrophication is discussed relatively to acidification, but not to deoxygenation. Since this is the only paragraph in this section on drivers of coastal acidification and deoxygenation, I think it would be appropriate to develop on this or at least mention it. [Mathilde Jutras, Canada]	Taken into account - this section text went through major revision to avoid the overlap with WGII. A cross-reference to WGII, chapter 03, was included where necessary in the revised text.
40899	50	40			the assessment "high agreement robust evidence" is not really traceable [TSU WGI, France]	Accepted - the text was re-written accordingly
88973	50	41	50	41	Not "saturated" but "supersaturaed"? [AKIHIKO MURATA, Japan]	Accepted - change was made
67517	50	41	50	41	"inner seas" odd choice of terms; not clear what it means [James Christian, Canada]	Accepted - change was made and now in the text there is "Although some exceptions exist, typically coastal ocean surface waters are ..."
67519	50	42	50	42	change "prevails over" to "exceeds" [James Christian, Canada]	Accepted - change was made.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
17043	50	43	50	44	Methane flux settings from subsurface towards the seafloor, a widespread phenomenon along coastal settings, is also shown to contribute to coastal carbon budgets (e.g., Dickens, 2003; Coffin et al., 2014; Aleksandra and Katarzyna, 2018). Recent global compilation suggests that diffusive methane charged marine sediments contribute an estimated 6.5 Tmol year–1 inorganic carbon, dominantly as CO2, to the water column and 5.3 Tmol year–1 of this inorganic carbon flux is occurring in shelf settings (Akam et al., 2020) Citation: Dickens, G. R. (2003). Rethinking the global carbon cycle with a large, dynamic and microbially mediated gas hydrate capacitor. Earth Planet. Sci. Lett. 213, 169–183. doi: 10.1016/s0012-821x(03)00325-x Coffin, R., Hamdan, L., Smith, J., Rose, P., Plummer, R., Yoza, B., et al. (2014). Contribution of vertical methane flux to shallow sediment carbon pools across Porangahau Ridge, New Zealand. Energies 7, 5332–5356. doi: 10.3390/en7085332 Akam, S.A., Coffin, R.B., Abdulla, H.a.N., and Lyons, T.W. (2020). Dissolved Inorganic Carbon Pump in Methane-Charged Shallow Marine Sediments: State of the Art and New Model Perspectives. Frontiers in Marine Science 7.https://doi.org/10.3389/fmars.2020.00206 [Sajjad Abdullajintakam, United States of America]	Taken into account - the section is going under major revision and the impact on coastal de-oxygenation and N2O and CH4 production is going to be mentioned. Regarding CH4, the emissions and sources (open and coastal ocean) are assessed in section "5.2.2.4 Ocean and inland emissions and sinks". For CH4, the interplay between low-O2/anoxia in coastal waters and consequent CH4 production (and accumulation in the water column) does not seem to be fully understood yet, as in (Naqvi, S. W. A., Bange, H. W., Farías, L., Monteiro, P. M. S., Scranton, M. I., and Zhang, J. (2010). doi:10.5194/bg-7-2159-2010) or (Ma, X., Sun, M., Lennartz, S. T., and Bange, H. W doi: 10.5194/bg-17-3427-2020, 2020.). In a tropical eutrophic bay, (Cotovicz, L. C., Knoppers, B. A., Brandini, N., Poirier, D., Costa Santos, S. J., and Abril, G. (2016) doi:10.1002/lno.10298.) didn't find significantly higher CH4 concentration in bottom hypoxic waters than in oxygenated surface waters, suggesting that sulphate reduction may be an important process. It is not intended though to diminish the importance of coastal low oxygen areas, or
67521	50	46	50	46	delete "a sink yield of" [James Christian, Canada]	Accepted - change was made.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
2345	50	47			Could you cite few examples. I can suggest the combined effects on reef sponge bioerosion rates (Webb et al., 2017), on the ecophysiology of macroalgae (Kang and Chung, 2017 ; Gao et al., 2018). Webb A.E., van Heuven Steven M. A. C., de Bakker Didier M., van Duyl Fleur C., Reichart Gert-Jan, de Nooijer Lennart J., 2017. Combined Effects of Experimental Acidification and Eutrophication on Reef Sponge Bioerosion Rates. Frontiers in Marine Science, 4, 311, DOI: 10.3389/fmars.2017.00311. https://www.frontiersin.org/article/10.3389/fmars.2017.00311 ; Kang, J.W., Chung, I.K. The effects of eutrophication and acidification on the ecophysiology of Ulva pertusa Kjellman. J Appl Phycol 29, 2675–2683 (2017). https://doi.org/10.1007/s10811-017-1087-5 ; Gao G., Anthony S. Clare, Eleni Chatzidimitriou, Craig Rose, Gary Caldwell, 2018. Effects of ocean warming and acidification, combined with nutrient enrichment, on chemical composition and functional properties of Ulva rigida. Food Chemistry, 258, 71-78, ISSN 0308-8146, https://doi.org/10.1016/j.foodchem.2018.03.040, (http://www.sciencedirect.com/science/article/pii/S0308814618304709) [Abed El Rahman Hassoun, Lebanon]	Here the reviewer only cited a reference to an article. The section in p. 50 from l. 34 to line 40, regardin impact in the marine biota now is: "Since AR5, and in agreement with SROCC, there is now high agreement (robust evidence) that coastal ocean acidification, whether induced only by increasing atmospheric CO2 or locally exacerbated by eutrophication or upwelling processes, has negative effects on specific groups of marine organisms such as reef-building corals, crabs, pteropods, and sessile organisms (Dupont et al., 2010; Bindoff et al., 2019b; Bednaršek et al., 2020; Osborne et al., 2020) + CROSS-REF to WG2-Ch03), especially when combined with other stressors such as temperature and de-oxygenation, and potentially increased bioavailability of toxic elements such as arsenic and copper (Millero et al., 2009; Boyd et al., 2015; Breitburg et al., 2018)."
2213	50	47			Could you cite few examples. I can suggest the combined effects on reef sponge bioerosion rates (Webb et al., 2017), and on the ecophysiology of macroalgae (Kang and Chung, 2017 ; Gao et al., 2018) [Abed El Rahman Hassoun, Lebanon]	p. 50 l. 34 to l. 40 now is: "Since AR5, and in agreement with SROCC, there is now high agreement (robust evidence) that coastal ocean acidification, whether induced only by increasing atmospheric CO2 or locally exacerbated by eutrophication or upwelling processes, has negative effects on specific groups of marine organisms such as reef-building corals, crabs, pteropods, and sessile organisms (Dupont et al., 2010; Bindoff et al., 2019b; Bednaršek et al., 2020; Osborne et al., 2020) + CROSS-REF to WG2-Ch03), especially when combined with other stressors such as temperature and de-oxygenation, and potentially increased bioavailability of toxic elements such as arsenic and copper (Millero et al., 2009; Boyd et al., 2015; Breitburg et al., 2018)."

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
2263	50	47			Webb A.E., van Heuven Steven M. A. C., de Bakker Didier M., van Duyl Fleur C., Reichart Gert-Jan, de Nooijer Lennart J., 2017. Combined Effects of Experimental Acidification and Eutrophication on Reef Sponge Bioerosion Rates. Frontiers in Marine Science, 4, 311, DOI: 10.3389/fmars.2017.00311. https://www.frontiersin.org/article/10.3389/fmars.2017.00311 [Abed El Rahman Hassoun, Lebanon]	Here the reviewer only cited a reference to an article. The section in p. 50 from l. 34 to line 40, regardin impact in the marine biota now is: "Since AR5, and in agreement with SROCC, there is now high agreement (robust evidence) that coastal ocean acidification, whether induced only by increasing atmospheric CO2 or locally exacerbated by eutrophication or upwelling processes, has negative effects on specific groups of marine organisms such as reef-building corals, crabs, pteropods, and sessile organisms (Dupont et al., 2010; Bindoff et al., 2019b; Bednaršek et al., 2020; Osborne et al., 2020) + CROSS-REF to WG2-Ch03), especially when combined with other stressors such as temperature and de-oxygenation, and potentially increased bioavailability of toxic elements such as arsenic and copper (Millero et al., 2009; Boyd et al., 2015; Breitburg et al., 2018)."
2265	50	47			Kang, J.W., Chung, I.K. The effects of eutrophication and acidification on the ecophysiology of Ulva pertusa Kjellman. J Appl Phycol 29, 2675–2683 (2017). https://doi.org/10.1007/s10811-017-1087-5 [Abed El Rahman Hassoun, Lebanon]	Here the reviewer only cited a reference to an article. The section in p. 50 from l. 34 to line 40, regardin impact in the marine biota now is: "Since AR5, and in agreement with SROCC, there is now high agreement (robust evidence) that coastal ocean acidification, whether induced only by increasing atmospheric CO2 or locally exacerbated by eutrophication or upwelling processes, has negative effects on specific groups of marine organisms such as reef-building corals, crabs, pteropods, and sessile organisms (Dupont et al., 2010; Bindoff et al., 2019b; Bednaršek et al., 2020; Osborne et al., 2020) + CROSS-REF to WG2-Ch03), especially when combined with other stressors such as temperature and de-oxygenation, and potentially increased bioavailability of toxic elements such as arsenic and copper (Millero et al., 2009; Boyd et al., 2015; Breitburg et al., 2018)."

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
2267	50	47			Gao G., Anthony S. Clare, Eleni Chatzidimitriou, Craig Rose, Gary Caldwell, 2018. Effects of ocean warming and acidification, combined with nutrient enrichment, on chemical composition and functional properties of Ulva rigida. Food Chemistry, 258, 71-78, ISSN 0308-8146, https://doi.org/10.1016/j.foodchem.2018.03.040 (http://www.sciencedirect.com/science/article/pii/S0308814618304709) [Abed El Rahman Hassoun, Lebanon]	Here the reviewer only cited a reference to an article. The section in p. 50 from l. 34 to line 40, regardin impact in the marine biota now is: "Since AR5, and in agreement with SROCC, there is now high agreement (robust evidence) that coastal ocean acidification, whether induced only by increasing atmospheric CO2 or locally exacerbated by eutrophication or upwelling processes, has negative effects on specific groups of marine organisms such as reef-building corals, crabs, pteropods, and sessile organisms (Dupont et al., 2010; Bindoff et al., 2019b; Bednaršek et al., 2020; Osborne et al., 2020) + CROSS-REF to WG2-Ch03), especially when combined with other stressors such as temperature and de-oxygenation, and potentially increased bioavailability of toxic elements such as arsenic and copper (Millero et al., 2009; Boyd et al., 2015; Breitburg et al., 2018)."
67523	50	48	50	51	Dupont et al 2010. Surely there are more recent references that could be cited for such a general statement. The literature on biological impacts of ocean acidification has exploded in the last 10 years. [James Christian, Canada]	Accepted - the text was changed and now includes updated references (including SROCC). The new text is: "Nevertheless, since AR5 and in agreement with SROCC there is now high agreement (medium to high evidence) that coastal ocean acidification, whether induced only by the increasing atmospheric CO2 or locally exacerbated by eutrophication or upwelling processes, has negative effects on specific groups of marine organisms such as reef-building corals, crabs, pteropods, and sessile organisms (Dupont et al., 2010)Bindoff et al., 2019; Bednarsek et al. 2020, Osborne et al. 2020, CROSS-REF to WG2-Ch03) , especially when combined with other stressors such as temperature and deoxygenation, and potentially increased bioavailability of toxic elements such as arsenic and copper (Millero et al., 2009; Boyd et al., 2015; Breitburg et al., 2018)."

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67525	50	51	50	53	I see what they are trying to say here, but again the choice of words is poor. Arsenic is a metalloid, not a metal (Millero et al do refer to it as a metal); arsenic and copper are elements, not ions; none of the cited references asserts that acidification will necessarily increase As bioavailability. Breitburg et al state that "Hypoxia increases conversion of As(V) to the more toxic As(III)", but the preceding text does not mention hypoxia. Millero et al state that "Metal solubility in seawater is a strong function of pH. Most trivalent metals like Fe(III), Al(III), and As(III) are more soluble in acidic and basic solutions with a minimum somewhere in between. The location of that minimum will determine whether the metal will increase or decrease in solubility with the expected change in pH." but do not state which side of the minimum we are currently on for As. How about "when combined with other stressors such as temperature and deoxygenation, and potentially increased bioavailability of toxic elements such as arsenic and copper"? [James Christian, Canada]	Accepted - change was made and included the phrasing suggested by the reviewer.
127805	50	52	50	54	The sentences here imply that the new ESMs include nutrient feedbacks, but Table 5.4 only lists two models for CMIP5 having nutrients included. Considere rephrasing this if the majorty of CMIP6 models do not include nutrients. [Trigg Talley, United States of America]	Here it seems the reviewer has commented on section 5.4.1, and not 5.3.4.1. This comment was forwarded to the chapter group in charge of section 5.4 during the review.
52307	50	70	50	70	add ...."are changing"? missing ending of sentence, important for the understanding [Agneta Fransson, Norway]	Not Applicable - Line 70 of page 50 doesn't exist.
116437	50		50		outcomes of 5.3.3.4 are relevant / irreversibility (also for TS - SPM). [Valerie Masson-Delmotte, France]	Noted
2307	50				Webb A.E., van Heuven Steven M. A. C., de Bakker Didier M., van Duyl Fleur C., Reichart Gert-Jan, de Nooijer Lennart J., 2017. Combined Effects of Experimental Acidification and Eutrophication on Reef Sponge Bioerosion Rates. Frontiers in Marine Science, 4, 311, DOI: 10.3389/fmars.2017.00311. https://www.frontiersin.org/article/10.3389/fmars.2017.00311 [Abed El Rahman Hassoun, Lebanon]	Rejected - what this comment suggests is unclear. Rejection is also the case if this is the suggestion to cite this literature. The impact of acidification on marine organisms and ecosystems is outside the scope of this chapter. This is rather within the scope of WG II.
2309	50				Kang, J.W., Chung, I.K. The effects of eutrophication and acidification on the ecophysiology of Ulva pertusa Kjellman. J Appl Phycol 29, 2675–2683 (2017). https://doi.org/10.1007/s10811-017-1087-5 [Abed El Rahman Hassoun, Lebanon]	Rejected - what this comment suggests is unclear. Rejection is also the case if this is the suggestion to cite this literature. The impact of acidification on marine organisms and ecosystems is outside the scope of this chapter. This is rather within the scope of WG II.
2311	50				Gao G., Anthony S. Clare, Eleni Chatzidimitriou, Craig Rose, Gary Caldwell, 2018. Effects of ocean warming and acidification, combined with nutrient enrichment, on chemical composition and functional properties of Ulva rigida. Food Chemistry, 258, 71-78, ISSN 0308-8146, https://doi.org/10.1016/j.foodchem.2018.03.040 (http://www.sciencedirect.com/science/article/pii/S0308814618304709) [Abed El Rahman Hassoun, Lebanon]	Rejected - What this comment suggests is unclear. Rejection is also the case if this is the suggestion to cite this literature. The impact of acidification on marine organisms and ecosystems is outside the scope of this chapter. This is rather within the scope of WG II.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
18057	51	1	51	1	This section does not (and SROCC) did not convey a growing understanding of vulnerability of coral reef environments to deoxygenation via climate change. See papers by Andrew Altieri and others. (Nelson and Altieri 2019; Altieri et al. 2017). [Lisa Levin, United States of America]	Accepted. The new version of section 5.3.4.2 has a general reference to WGII - Chapter 03 regarding the impacts on organisms. Additionally there was not, by the time the assessment was revised, extensive literature on the regional distribution of de-oxygenation trends (only anoxia/hypoxia occurence as in the Levin, breitburg, Diaz papers). (p.53 l. 24) "Scientific literature specific to regional trends in coastal ocean acidification and de-oxygenation is only just emerging since the beginning of this assessment. These new results highlights that, despite high spatial variability and consequent longer time of emergence for acidification and de-oxygenation, the coastal ocean is an area demanding improved understanding of carbon and oxygen budgets, with special attention to the areas already affected by heavy anthropogenic eutrophication
19983	51	1	52	38	Here we have a subsection where basically every of the 8 paragraphs supplies interesting information and mentions interesting studies; at the same time the overall impact is limited because these paragraphs are not linked together in a clear, common approach. This is not a unique case in the SOD, though. [philippe waldteufel, France]	Accepted. The whole section was re-written. I have added this question here in case it would be necessary to paste the entire section as an answer to the comment.
115373	51	1	52	38	This discussion seemed somewhat US-centric. Many studies on US coastal waters were assessed, but none for example on coastal waters off Africa, Australia, the Arcitc, the Antarctic etc. [Gillett Nathan, Canada]	The revised section 5.3.4.2 (Spatial characteristics) has been enlarged and improved, covering as many coastal areas as possible, according to the latest available literature. The assessment on spatial characteristics continued until the last draft version.
58577	51	1	52	38	This section describes riverine input to coastal zones and impacts on pCO2 and buffering capacity. Another vector for terrestrial solutes and organic matter that may be worthy of mention here is submarine groundwater discharge, which is an important component of coastal carbon and chemical cycling in many regions (e.g. Luijendijk et al. 2020 Fresh groundwater discharge insignificant for the world's oceans but important for coastal ecosystems; doi: https://doi.org/10.1038/s41467-020-15064-8). While groundwater contributions to coastal systems are poorly characterized compared to riverine contributions, they will respond to drivers such as sea level rise and land use change, with potential impacts to coastal carbon budgets. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - The following sentence was added: "Locally, submarine groundwater discharge may play an important role in increasing the eutrophication state (low agreement, limited evidence, Luijendijk et al., 2020). " . The paper certainly highlights that locally SGD is of importance, but the available data in the literature do not yet allow a robust assessment.
73145	51	3	51	3	Change 'heterogenous' (American spelling) to 'heterogeneous' (British spelling) for parity with language useage within the chapter. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - changed in the text

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127807	51	3	51	5	Add Baumann and Smith, 2017 (Quantifying Metabolically Driven pH and Oxygen Fluctuations in US Nearshore Habitats at Diel to Interannual Time Scales). Also add Sutton et al., 2016. [Trigg Talley, United States of America]	Noted - This section was completely re-written, and the reference to Dr. A. Sutton's more recent articles were added. According to other reviewer's comments, this section only considered for the assessment results from studies that utilize standardized methodologies of the oceanographic community that produce measurements of defined data quality (Dickson et al. 2007; IOC 2018).
131539	51	3	51	8	logical structure of argument on "heterogenity" is unclear. How does sentence two result from sentence one? [Hans Poertner and WGII TSU, Germany]	Accepted - The initial sentence was changed to: "The coastal ocean , from the shore line to the isobath of 200 m, including bays and estuaries as defined in SROCC (Bindoff et al. 2019a), is highly heterogeneous due to the complex interplay between physical, biogeochemical and anthropogenic factors (Gattuso et al. 1998; Chen and Borges 2009; Dürr et al. 2011; Laruelle et al. 2014; McCormack et al. 2016). ". Please note that the whole section was re-written.
3629	51	4	51	5	I would add a reference to Fennel, K., & Testa, J. M. (2019). Biogeochemical controls on coastal hypoxia. Annual review of marine science, 11, 105-130. [Mathilde Jutras, Canada]	Accepted - The reference was added to this assessment.
52245	51	10	51	20	Suggest adding "Upwelling in southern Bering Sea caused increased pCO2 in the surface water and outgassing of CO2 from ocean to atmosphere, particularily evident during cold years with strong winds, in both the deep ocean and on the shelf, in the period of 1995-2001 (Fransson et al. 2006)." [Agneta Fransson, Norway]	Noted - there are now references to the coastal portion of the Bering Sea and the drivers of acidification.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
3631	51	10	51	20	This paragraph discusses the effect of upwelling in coastal areas for acidification, but there is also something to be said for deoxygenation. Upwelling of oxygen-low and nutrient-rich waters leads to deoxygenation below the surface waters upon an increase in biological activity, and increased wind under climate change in some regions are expected to increase coastal deoxygenation. References: Breitburg et al., Science, 2018.; Sydeman et al., Science, 2014; Chan et al., Science, 2008; Wang et al., Nature, 2015. [Mathilde Jutras, Canada]	Noted - the section has been re-written, and the new text is "Spatial distribution of hypoxic areas is highly heterogeneous in the coastal ocean, and there is high agreement, robust evidence that more severe hypoxia or anoxia may occur more often in highly populated coastal areas, or in regions where local water circulation and upwelling, seasonal water column stratification and wind patterns lead to an accumulation of organic matter at subsurface (Ciais et al., 2013; Rabalais et al., 2014; Breitburg et al., 2018b; Bindoff et al., 2019b) (Li et al., 2016) (SROCC Chapter 5). The causes and the signal for coastal de-oxygenation can be only assessed by making available long-term time series combined to regional modelling (Fennel and Testa, 2019b), as in the California Current System (Wang et al., 2017), the East China Sea (Chen et al., 2007; Qian et al., 2017), the Namibian shelf or along the north-western Atlantic shelf (Claret et al., 2018). Other coastal upwelling sites such as the Arabian Sea display seasonal hypoxia but no worsening trends (Gupta et al., 2016)."
127809	51	10	51	41	This section is almost entirely focused on the United States. Aren't there papers/datasets from Europe, Chile, or New Zealand which might support these same findings? [Trigg Talley, United States of America]	Accepted - although it is indisputable that there is a large amount of high quality peer-reviewed literature related to the NW Atlantic and the NE Pacific coasts that enables a more robust assessment for these areas. The text has also been rewritten so now it encompasses large regions, and there is no more direct citation of countries.
2501	51	21			Hertzberg et al. (2016, GRL 43, GL07023) give a detailed account on the role of the biological pump during key events of the glacial termination (HS1 and YD). [Thomas Ronge, Germany]	Taken into account however this comment refers to Section 5.3.3.1. THis paper was considered in the assessment there.
73147	51	22	51	22	Capital 'H' for 'hemisphere' [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial - this kind of issues will be taken care of during the production phase, which includes professional copy-editing.

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67527	51	27	51	27	change "combined to" to "combined with"; change "lower " to "declining" and delete "ecosystem" [James Christian, Canada]	Accepted - the text now is "combined with an open ocean signal of acidification, lead to a declining buffering capacity along the decreasing salinity gradient from the ocean to the inner estuary". NOTE: this may change with major rewriting in this section.
36369	51	28	51	30	Wallace et al. 2014 is not a valid reference to use here. Some of the measurements presented in Wallace et al. 2014 are from glass electrode autonomous sensors, which are not considered one of the standard methodologies of the oceanographic community. 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 change in these coastal environments from measurement error without explicit interrogation of the methodologies. 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 (http://goa-on.org/resources/sdg_14.3.1_indicator.php). 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) or considered an ocean carbon sensor by the International Ocean Carbon Coordination Project (http://www.ioccp.org/index.php/instruments-and-sensors). This sentence should simply be removed. [Adrienne Sutton, United States of America]	Accepted. this section was re-written and a new assessment was added in p. 51 l. 50: "There is high agreement that long time series of observations utilising standard methods are needed to distinguish the climate change signal in pH or saturation state from the large natural variability typical of coastal sites (Duarte et al., 2013b; IOC, 2018; Salisbury and Jönsson, 2018; Sutton et al., 2019; Tilbrook et al., 2019; Turk et al., 2019). Despite increasing availability of pH and other marine carbonate parameter datasets and sea-air CO2 flux budgets for the coastal ocean (ref to this chapter Section 5.3.4.1), additional long-term observations are required to constrain the time of emergence of anthropogenic CO2-driven acidification in coastal areas. There is high agreement (medium evidence) that, for the subtropical to temperate coastal northeast Pacific and northwest Atlantic, the mean time of emergence for acidification in these areas is above two decades (Sutton et al., 2019; Turk et al., 2019). "
74267	51	30	51	31	The sentence that begins with a bracketed citation “(Rheuban et al..” seems unclear to me. [Christoph Völker, Germany]	Accepted - the text now is "Rheuban et al. (2019) empirical model suggests that for the former estuaries, aragonite saturation is controlled by nutrient loading, reinforcing that eutrophication is the main driver for exacerbating acidification in non-upwelling areas." - NOTE THAT the final text is probably going to be change after major revision in this section;

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
33339	51	30	51	31	Change: “(Rheuban et al., 2019) empirical model… » by « Rheuban et al. (2019) empirical model…”. [Guiomar Rotllant, Spain]	Accepted - the text now is "Rheuban et al. (2019) empirical model suggests that for the former estuaries, aragonite saturation is controlled by nutrient loading, reinforcing that eutrophication is the main driver for exacerbating acidification in non-upwelling areas." - NOTE THAT the final text is probably going to be change after major revision in this section;
73149	51	30	51	31	Change to 'Rheuban et al.'s (2019)' [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - the text now is "Rheuban et al. (2019) empirical model suggests that for the former estuaries, aragonite saturation is controlled by nutrient loading, reinforcing that eutrophication is the main driver for exacerbating acidification in non-upwelling areas." - NOTE THAT the final text is probably going to be change after major revision in this section;
67529	51	30	51	32	"aragonite saturation is controlled by nitrogen loading" possibly an overgeneralization [James Christian, Canada]	Taken into account: The whole section has undergone many changes as other reviewers have pointed out that not all studies about coastal ocean acidification were emplying the recommended best practices for the marine carbonate system. The text now is (p.52): "Temperate, non-upwelling coastal areas along the northwest Atlantic display a trend of decreasing seawater pH, mainly attributed to the combined effects of ecreasing seawater buffering capacity and eutrophication (high agreement, robust evidence). Observations show an increasing north to south gradient of aragonite saturation state (Sutton et al., 2016; Fennel et al., 2019; Cai et al., 2020). Local conditions of low alkalinity and total inorganic carbon concentration in coastal areas, combined with an open ocean signal of acidification, lead to a declining buffering capacity along the decreasing salinity gradient from the ocean to estuarine areas (Cai et al., 2017, 2020). Empirical models suggest that for northwest Atlantic estuarine areas, the aragonite saturation state is seasonally controlled by utrient availability and

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
36371	51	33	51	36	Robbins and Lisle 2018 also utilize non-standard glass electrode measurements for ocean acidification research. This is even more problemactic than Wallace et al. 2014 because Robbins and Lisle are calculating trends from these data. It is impossible to separate true pH long-term change using these estuarine measurements from measurement error without explicit interrogation of the methodologies, which this study does not do. As described previously for Wallace et al. 2014, this study should not be referenced by IPCC, and this sentence should be removed. [Adrienne Sutton, United States of America]	Accepted - the references were removed, and the importance of the use of STANDARD METHODS was added to the text e.g. p.51 l. 50 "There is high agreement that long time series of observations utilising standard methods are needed to distinguish the climate change signal in pH or saturation state from the large natural variability typical of coastal sites (Duarte et al., 2013b; IOC, 2018; Salisbury and Jönsson, 2018; Sutton et al., 2019; Tilbrook et al., 2019; Turk et al., 2019). Despite increasing availability of pH and other marine carbonate parameter datasets and sea-air CO2 flux budgets for the coastal ocean (ref to this chapter Section 5.3.4.1), additional long-term observations are required to constrain the time of emergence of anthropogenic CO2-driven acidification in coastal areas. There is high agreement (medium evidence) that, for the subtropical to temperate coastal northeast Pacific and northwest Atlantic, the mean time of emergence for acidification in these areas is above two decades (Sutton et al., 2019; Turk et al., 2019). ".
74269	51	34	51	35	the numbers are unclear: does 7.3 also have a factor of 10 to the power of minus 4? [Christoph Völker, Germany]	Accepted - yes, now it is 7.3 10-4 to 5.0 10-4
2801	51	34	51	35	This should be "7.3 10-4 to 5.0 10-4" [Stephen Wilkinson, United Arab Emirates]	Accepted - yes, now it is 7.3 10-4 to 5.0 10-4
67531	51	36	51	36	"predict" is not appropriate terminology here; the cited reference describes a hindcast not a forecast [James Christian, Canada]	Noted - the text is rewritten.
73151	51	43	51	43	Change 'heterogenous' (American spelling) to 'heterogeneous' (British spelling) for parity with language useage within the chapter. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - it was changed.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
52333	51	43	51	54	Suggest adding information from polar regions on influence of freshwater, for example: freshwater-influenced shelves and fjords due to sea-ice or glacial meltwater in the Arctic show decrease in alkalinity and buffering capacity and increased ocean acidification (Chierici and Fransson, 2009; Fransson et al. 2009; 2015; Meire et al. 2015; Hopwood et al. 2020) [Agneta Fransson, Norway]	Noted - there is a paragraph on acidification vs freshwater in high latitudes (both Arctic and Southern Ocean): "High latitude coastal areas in the Southern Ocean such as the North-western Antarctic Peninsula are likely at risk of being undersaturated in relation to aragonite by 2060 (medium evidence, (Lencina-Avila et al., 2018; Monteiro et al., 2020a). The effects of multiple drivers such as warming, meltwater, sea-air CO2 equilibrium and local circulation may, however, to some extent offset the trends in coastal ocean acidification in Antarctica (Henley et al., 2020). In the coastal western Arctic Ocean and the corresponding ocean basin, ocean acidification is very likely driven by sea-air CO2 fluxes and sea-ice melt, together with increasing intrusions since the 1990s of low-alkalinity Pacific Water, lowering aragonite saturation state (Qi et al., 2017). In the north-eastern Pacific, the coastal Bering Sea also shows decreasing trends in aragonite saturation state in the water column, related to the increasing atmospheric CO2 uptake combined to riverine freshwater and carbon inputs
67533	51	43	51	54	This paragraph appears to repeat some fairly basic information that has already been discussed in this section. [James Christian, Canada]	Accepted - text in this section has been completely rewritten.
2043	51	43	52	38	This comment is a repetition of my comment that I added to the first order draft and think this is very important: 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 (https://doi.org/10.5194/bg-11-1215-2014) should be added to refer to any details on causes and consequences of hypoxia in lakes, oceans, fjords, lagoons, Black Sea, Baltic Sea, etc. 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]	Noted - the reference was used to validate the assessment on the importance of long-term time series to detect the emergence of de-oxygenation trends in the coastal ocean.
36373	51	50	51	51	As described in previous comments, the Wallace et al. 2014 and Lowe et al. 2019 citation should be removed as they utilize non-standard methodologies for measuring seawater pH. [Adrienne Sutton, United States of America]	Accepted - the text is going to be changed accordingly.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
116439	51		52		Why not use SROCC as a starting point? The section needs to be sharpened to focus on an assessement (beyond the literature review) and develop a summary of the new knowledge (with use of the calibrated language when relevant). [Valerie Masson-Delmotte, France]	Accepted. The section was re-written, and used the SROCC assessment as a starting point (e.g. p.51 l. 4-7 "Since SROCC (Bindoff et al., 2019a), there is further evidence that anthropogenic eutrophication via continental runoff and atmospheric nutrient inputs and ocean warming are very likely the main drivers of de-oxygenation in coastal areas (Levin and Breitburg, 2015; Levin et al., 2015; Royer et al., 2016; Breitburg et al., 2018a; Cocquempot et al., 2019; Fagundes et al., 2020; Limburg et al., 2020).") The calibrated language was used in all assessments, and the section starts with a summary assessment defining what is considered as the coastal ocean as well. (e.g. p. 50 l. 37: "The coastal ocean, from the shore line to the isobath of 200 m, is highly heterogeneous due to the complex interplay between physical, biogeochemical and anthropogenic factors (Gattuso et al., 1998; Chen and Borges, 2009b; Dürr et al., 2011; Laruelle et al., 2014a; McCormack et al., 2016). These areas, according to SROCC (Bindoff et al., 2019a) are, with high confidence, already affected by ocean acidification and de-
36375	52	1	52	8	This paragraph is highly problematic. First, high variability in the coastal zone does not mean there is higher uncertainty in the data. It means that more observations are required to constrain the temporal and spatial variability. As in earlier comments, climate assessments should only utilize data of defined quality. Replace this paragraph with: Despite the increasing availability of pH and other marine carbonate parameters datasets for the coastal oceans, it is important to highlight that more observations are required to constrain temporal and spatial variability of ocean acidification conditions in coasts and estuaries. In addition, this section only includes results from studies that utilize standardized methodologies of the oceanographic community that produce measurements of defined data quality (Dickson et al. 2007; IOC 2018). References: Dickson, A. G., et al., Eds. (2007). Guide to Best Practices for Ocean CO2 Measurements, North Pacific Marine Science Organization. Intergovernmental Oceanographic Commission (IOC). 2018. Indicator Methodology for 14.3.1., IOC/EC-LI/2 Annex 6 rev. http://goa-on.org/resources/sdg_14.3.1_indicator.php [Adrienne Sutton, United States of America]	Accepted - the suggested text was included, replacing the former version.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
2215	52	1		5	Are you considering here the Mediterranean area or just the coastal areas of the open oceans?! [Abed El Rahman Hassoun, Lebanon]	Accepted - in this assessment we have used the SROCC (2019) definition for coastal ocean i.e. the ocean area from the coastline to 200 m water depth contour, thus the literature review included the Mediterranean coastal area.
127811	52	1			Remove "s" from "parameters". [Trigg Talley, United States of America]	Accepted - change was made.
52249	52	5	52	6	perhaps add what methods "such as hydrogen-ion-senstitive electrode and spectrophotometic method using a pH-sensitive dye", the mwthod descriptions could be in a separate section where uncertainty is described and discussed. This is valid for both presenting observations and models results. I assume that the data shown (such as pH time series) use correct data. [Agneta Fransson, Norway]	Accepted - this part of the text has changed substantially, and we now consider only standardized methods as those recommended by the IOC. There is a new paragraph that replaces the former version: "Despite the increasing availability of pH and other marine carbonate parameters datasets for the coastal oceans, it is important to highlight that more observations are required to constrain temporal and spatial variability of ocean acidification conditions in coasts and estuaries. In addition, this section only includes results from studies that utilize standardized methodologies of the oceanographic community that produce measurements of defined data quality (Dickson et al. 2007; IOC 2018)."
3633	52	10	52	13	Physical characteristics of the systems are also important. For instance, stratification is essential, even if the nutrient import was very high (again Fennel et al., Ann. Reviews of Marine Science, 2019). [Mathilde Jutras, Canada]	Noted - The text was re-written and stratification (thermal or haline, this last one important in estuaries) is mentioned.
90089	52	10	52	38	This statement uploaded by Ted Schuur on behalf of a graduate class that reviewed the chapter (this was communicated with IPCC TSU in advance): This paragraph seemed to include a lot of examples on hypoxia; this could be condensed to save space [Edward Schuur, United States of America]	Accepted - the text in this section has been fully rewritten.
83999	52	13	52	13	please insert (Eichler et al., 2018) [Marco Tulio Cabral, Brazil]	Not accepted - I am afraid I did not understand the comment. Was the reference made to this paper: "Eichler, P. P. B., McGann, M., Rodrigues, A. R., Mendonca, A., Amorim, A., Bonetti, C., et al. (2018). The occurrence of the invasive foraminifera Trochammina hadai Uchio in Flamengo Inlet, Ubatuba, São Paulo State, Brazil. Micropaleontology 64, 391–402."? If yes, it is not clear enough how local results from a study on benthic invasive foraminifera would add to the assessment of hypoxia in coastal areas.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
73153	52	14	52	14	Change 'heterogenous' (American spelling) to 'heterogeneous' (British spelling) for parity with language useage within the chapter. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - the text has been revised and now uses the British spelling.
73155	52	15	52	15	Replace 'many times not obtainable' with 'rarely obtainable' [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change was made.
127813	52	17	52	30	If a connection to CO2 and climate change cannot be made, maybe delete this section or significantly shorten. [Trigg Talley, United States of America]	Not accepted - There is a clear connection between increasing atmospheric CO2, open and coastal ocean acidification and climate change. The point is that the time of emergence of acidification and warming in coastal areas is longer because of their natural variability - from diel to seasonal to decadal.
21833	52	24	52	25	Sould Southern Oscillation be replaced with ENSO for whole report consistency and is a reference to the technical annex required? [Peter Thorne, Ireland]	Noted - the text has been rewritten and the mention to ENSO does not appear in the text anymore.
13435	52	25	52	26	Check line spacing, which is different (wider) to the rest of the text. [Maria Amparo Martinez Arroyo, Mexico]	Editorial - this kind of issues will be taken care of during the production phase, which includes professional copy-editing.
33341	52	28	52	29	Change: “(Claret et al., 2018) in…” by “Claret et al. (2018) in…”. [Guiomar Rotllant, Spain]	Accepted - changed in the text - Text may change after major revision
73157	52	28	52	29	Change to 'Claret et al. (2018)' [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - changed in the text - Text may change after major revision
3635	52	28	52	30	In this list of most important accounts of coastal deoxygenation, I would also include the deoxygenation that took place in the St. Lawrence Gulf and Estuary and is well documented, and moreover motivated the study of Claret et al. 2018 cited here. Oxygen concentrations in the St Lawrence Lower Estuary where divided by half (from ~150 micromol/L to less than 60 micromol/L) between the 1930s and present time, reaching hypoxic conditions. Reference: Gilbert, D., Sundby, B., Gobeil, C., Mucci, A., & Tremblay, G. H. (2005). A seventy-two-year record of diminishing deep-water oxygen in the St. Lawrence estuary: The northwest Atlantic connection. Limnology and Oceanography, 50(5), 1654-1666. [Mathilde Jutras, Canada]	Accepted - Reference was added to the text.
73159	52	37	52	37	Cumbersome expression, replace 'present' with 'currently'. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - changed in the text - Text may change after major revision
38475	52	41	58	50	It might be more logical to discuss direct CO2 and climate effect on land carbon uptake, and then discuss direct CO2 and climate effect on ocean carbon uptake. [LONG CAO, China]	Noted - however we feel that there are pros and cons of both ways of organising these subsections. Based-on within chapter discussions and previous reviewer comments we have therefore decided to retain the current structure

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
5635	52	41	74	49	The whole Chapter separately discussed the effect of CO2, climate and other essential factors(e.g., nutrient). However, these factors occuring at the same time. I thus strongly suggest a new subsection to discuss the interactions between these factors, although it is a difficult one. [Enzai Du, China]	Taken into account - the combination of the various drivers is dealt with in the ESM section 5.4.5. We have made that clearer in the revised introduction to section 5.4.5, by adding: "ESMs include interactions between many of the processes and feedbacks described in Sections 5.4.1 - 5.4.4".
40711	52	49	54	20	section 5.4.1: nicely written! [TSU WGI, France]	noted
40507	52	49	54	20	lack of confidence level/uncertainty assessments in this section (5.4.1) [TSU WGI, France]	Accepted - text revised
70829	52	49	54	20	I am missing refernce to Körner who made several contributions to this issue, e.g. https://doi.org/10.1016/j.pbi.2015.05.003. [Karlheinz Erb, Austria]	Rejected - this is not a comprehensive review but an assessment. The point that
17873	52	49	54	20	This section jumps around between different processes and from field experiments to models, making it difficult to follow. Could you include subheadings for the different processes, then go through what the field experiments show, and then note whether they are included in ESMs? [Katherine Calvin, United States of America]	Taken into account. The section has been revised for clarity, however, a full separation of theory, empirical support and model studies is beyond the length constraint, because it would require triplicate mention of individual processes. The authors believe that its important to combine observational and model-based knowledge for each topic
17875	52	49	54	20	Can you provide quantitative information in this section? It is difficult to judge how large these potential effects are from the text. [Katherine Calvin, United States of America]	Accepted - for clarity, these two sentences have been moved to the end of the section, where the quantitative information is given
37913	52	51	53	4	We do not have extensive study about CMIP6 model intercomparison. Many CMIP6 models consder nitrogen cycle (thought this is too much simplistic) and we must consider their results in this part. [Junhee Lee, Republic of Korea]	Noted - this is what the section intends to cover.
127815	52	51			It seems important to introduce or directly mention the growing belief that mycorrhizal associates modulate the eCO2 effect on forest biomass (see Song and Terrer, 2019). [Trigg Talley, United States of America]	Noted - the SOD text in L53 L53-57 does exactly this. The suggested reference is incomplete and was not found. However, literature is cited to document this effect
8835	52	54	52	55	Any figure to show this? [Vaishali Naik, United States of America]	Accepted - A reference to Figure 5.28 has been added
33343	52				Avoid starring sentences with abbreviations AR5, CMIP or ESM. Check all over the MS. [Guiomar Rotllant, Spain]	Accepted - the text has been rewritten
40509	53	1		4	Literature citation is lacking: the statements are not traceable [TSU WGI, France]	Accepted - Literature added
8837	53	2	53	2	Please give the precise number of CMIP6 models that account for what nutrient cycles. Perhaps this information could be included in a table [Vaishali Naik, United States of America]	Accepted - number and reference to added
109519	53	3	53	3	“CO2 fertilisation effect” is ambiguous. If I understand right the fertilisation effects refers to the effect on photosynthesis while in this sentence it’s the downstream effect on ecosystem carbon storage that’s relevant. [Anthony Walker, United States of America]	Accepted - language corrected

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
54989	53	6	53	34	Within this section, there should be more coverage of the methodological uncertainty (both in terms of analysis and data collection bias) uncertainty associated wtith corroborating experimental and/or modelling studies with large scale field observations from tree-ring networks and forest inventory plots. Example references for Canada include: Girardin, M.P., Bouriaud, O., Hogg, E.H., Kurz, W.A., Zimmermann, N.E., Metsaranta, J., deJong, R., Frank, D.C., Esper, J., Büntgen, U., Guo, X.J., Bhatti, J., 2016a. No growth stimulation of Canada's boreal forest under half-century of combined warming and CO2 fertilization. Proc. Nat. Acad. Sci. USA 113, E8406–E8414 Hember, R.A., W.A. Kurz and N.C. Coops. 2017b. Increasing net ecosystem biomass production of Canada's boreal and temperate forests despite decline in dry climates. Glob. Biogeochem. Cycles 31(1): 134–158. Duchesne, L., Houle, D., Ouimet, R. et al. Large apparent growth increases in boreal forests inferred from tree-rings are an artefact of sampling biases. Sci Rep 9, 6832 (2019). Searle, E.B., Chen, H.Y.H. 2017. Tree size thresholds produce biased estimates of forest biomass dynamics. For. Ecol. Man. 400:468-474. [Nancy Hamzawi, Canada]	Rejected - This comment refers to detecting and estimating trends in observed forest growth and carbon stock during the historical period. It does not address processes involved in future climate feedbacks. Without doubt, all methods are subject to uncertainties and sampling biases, but these are acknowledged in this section
8839	53	7	53	7	this reference is not needed here as this sentence notes the assessment from AR5 and SRCCL [Vaishali Naik, United States of America]	Accepted - reference removed
7161	53	7			Add the references after the sentence " New studies since AR5 add evidence that the leaf-level CO2 fertilisation is modulated by acclimation of photosynthesis to long term CO2 exposure, growth temperature, seasonal drought, and nutrient availability (Penuelas et al 2017;2020)" Peñuelas, J., Ciais, P., Canadell, JG., Janssens, I., Fernandez-Martinez, M., Carnicer, J., Obersteiner, M., Piao S., Vautard, R., Sardans, J. (2017). Shifting from a fertilization-dominated to a warming-dominated period. Nature Ecology & Evolution 1, 1438–1445. doi:10.1038/s41559-017-0274-8 Penuelas, J., Fernández-Martínez, M., Vallicrosa, H., Maspons, J., Zuccarini, P., Carnicer, J., Sanders, T., Krüger, I., Obersteiner, M.I., Janssens, I.A., Ciais, P., Sardans, J. 2020. Increasing atmospheric CO2 concentrations correlate with declining nutritional status of European forests. Communications Biology, (2020) 3:125. Doi:10.1038/s42003-020-0839-y [Josep Penuelas, Spain]	Accepted - the first reference was added to the text, the second does not strictly refer to the modulation of the CO2 fertilisation effect on growth as modulated by nutrient constraints, but rather a documentation of the effect of CO2 fertilisation on forest nutrition

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
11379	53	8	53	9	In leaf-level CO2 fertilization is modulated by acclimation of photosynthesis to long term CO2 exposure,…., 1) the proposition...is controlled by ….is more accurate and frequently used in the subject matter than …is modulated by. 2) Leaf structure (mainly porosity, surface area and functional composition) is a very strong determinant of catalytic processes underlying such a fertilization. It determines the rate and efficiency of photosynthesis. Nevertheless, it has not been highlighted in the section. Among other important benefits, leaf structure dictates the focusing of light (in lens-like epidermal cells), the scattering and absorption of light within the porous networks and diffusion of light within the palisade parenchyma. The structure determines even the flows of CO2 and water molecules, the flow of the nutrient referred to in the section as well as the transpiration occurring through the leaf. This shortcoming should be addressed in the section and across the chapter. [LOUIS LUBANGO Mitondo, Ethiopia]	Rejected - this detailed assessment is beyond the scope of the IPCC. Also no relevant literature is provided to consider this effect. Similar effects are discussed in the literature underlying the assessment of AR5 regarding the photosynthesis response to CO2
11381	53	8	54	20	There are also CO2 efflux resulting from catalysis (metabolism) in the leaf cells, which have not be indicated in this section. Such efflux can be very high, depending of the leaf structure. The work of Zhang et al. (2012) that has reported that global leaf respiration amounted to 21-28 Pg C per year can serve as a good reerence. These fluxes are not the ones exiting from the roots and wooden tissues, which are more documented. How did the various models cited in this section account for such leaf-level efflux? Gross leaf efflux of xylem-transported CO2 could approach ½ rate of leaf respiration in the light when transportation rates and branch inorganic carbon concentration are high irrespective of grossly different petiole morphology. This aspect has been overlooked in this section (Stutz et all , J. Experimental Botany 2017, 68 (11), pp. 2849-2857). [LOUIS LUBANGO Mitondo, Ethiopia]	Rejected -This comment addresses a physiological process that affects the interpretation of leaf-level measurements. It is not the purpose of this section to provide a detailed review of all physiological processes affecting the plant carbon balance. It is without doubt important to recognise these processes, but why they are of particular relevant to the direct effect of CO2 on land carbon is unclear from the comment.
44989	53	8	54	20	There are also CO2 efflux resulting from catalysis (metabolism) in the leaf cells, which have not be indicated in this section. Such efflux can be very high, depending of the leaf structure. The work of Zhang et al. (2012) that has reported that global leaf respiration amounted to 21-28 Pg C per year can serve as a good reference. These fluxes are not the ones exiting from the roots and wooden tissues, which are more documented. How did the various models cited in this section account for such leaf-level efflux? Gross leaf efflux of xylem-transported CO2 could approach ½ rate of leaf respiration in the light when transportation rates and branch inorganic carbon concentration are high irrespective of grossly different petiole morphology. This aspect has been overlooked in this section (Stutz et all , J. Experimental Botany 2017, 68 (11), pp. 2849-2857). [LOUIS LUBANGO Mitondo, Ethiopia]	Rejected - duplication of comment 11381
58879	53	11	53	17	For how carbon fertilization is modulated by various other factors - consider Drake et al., Global Change Biology (2016). Warming actually increased drought tolerance and/or partial pressure of CO2 in some plants, depending on environment. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - however, the paper in question is Drake et al. 2017, GCB (first published 2016)

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
26905	53	12	53	12	Please precise on which time scale. Does the carbon storage increase, noted today, remain the same over the time? This notion of durability is always forgotten. Carbon storage in soils is not linear and an increase at a time t can only be transitory and, through an increase in the priming effect for example, may be a loss of carbon in total over time.vegetation response to CO2 increase is short and stable in time but not the soil response. It is thus very difficult to talk about ecosystem by only considering the way the vegetation responds to CO2 forcing. priming effect is mentioned once in the soil part, a bit later. [Eric Brun, France]	Accepted - text clarified
109521	53	12	53	12	Suggest citing Walker (in revision) here. That study is an unprecedented cross-evidence review that includes evidence from eCO2 experiments, tree rings, forest inventories, flux towers, satellites, abd various other data streams. Walker et al (in revision) Integrating the evidence for a terrestrial carbon sink caused by increasing atmospheric CO2. New Phtytologist. This study provides support for the statement that “the long-term effect of elevated CO2 on plant growth and ecosystem carbon storage is generally positive.” Can provide full reference, email walkerap@ornl.gov [Anthony Walker, United States of America]	Accepted - reference added
26141	53	13	53	13	Could add a meta-analysis that assesses CO2 by phosphorus interaction effect on plant photosynthesis and growth to the citation list: M. Jiang, S. Caldararu, H. Zhang, J. Yang, K.Y. Crous, K. Fleischer, M.G. De Kauwe, D.S. Ellsworth, P. Reich, D.T. Tissue, S. Zaehle, and B.E. Medlyn. Low phosphorus supply constrains plant responses to elevated CO2 – a meta-analysis. Global Change Biology. Submitted. [Mingkai Jiang, Australia]	Accepted - reference added
109523	53	13	53	13	Suggest citing Walker 2019 here. It’s one of the few syntheses that addresses long-term effects of eCO2 on vegetation carbon storage [Anthony Walker, United States of America]	Accepted - reference added
127817	53	13			[RISK] There is a new synthesis of 138 CO2 experiments, which shows that soil nutrients strongly regulate the CO2 fertilization effect, with a decreasing effect across experiments when nitrogen or phosphorus availability decreases. Citation: Terrer, C., Jackson, R.B., Prentice, I.C. et al. Nitrogen and phosphorus constrain the CO2 fertilization of global plant biomass. Nat. Clim. Chang. 9, 684-689 (2019). https://doi.org/10.1038/s41558-019-0545-2 It seems appropriate to cite it here because the paper is a synthesis of experiments highlighting the importance of nutrients in regulating the CO2 fertilization effect. [Trigg Talley, United States of America]	Accepted - reference added, however, a sentence later (effects on growth and C storage), where it belongs.
7163	53	13			Add references (Penuelas et al 2017;2020)" Peñuelas, J., Ciais, P., Canadell, JG., Janssens, I., Fernandez-Martinez, M., Carnicer, J., Obersteiner, M., Piao S., Vautard, R., Sardans, J. (2017). Shifting from a fertilization-dominated to a warming-dominated period. Nature Ecology & Evolution 1, 1438–1445. doi:10.1038/s41559-017-0274-8 Penuelas, J., Fernández-Martínez, M., Vallicrosa, H., Maspons, J., Zuccarini, P., Carnicer, J., Sanders, T., Krüger, I., Obersteiner, M.I., Janssens, I.A., Ciais, P., Sardans, J. 2020. Increasing atmospheric CO2 concentrations correlate with declining nutritional status of European forests. Communications Biology, (2020) 3:125. Doi:10.1038/s42003-020-0839-y [Josep Penuelas, Spain]	Rejected - repeats accepted comment 7161

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
28409	53	14	53	16	Important recent publications on modeling allocation and vegetation dynamics (which in itself could be its own section; maybe for AR7 ;) are Franklin O, Harrison SP, Dewar R et al. (2020) Organizing principles for vegetation dynamics. Nature Plants.; Fisher RA, Koven CD, Anderegg WRL et al. (2018) Vegetation demographics in Earth System Models: A review of progress and priorities. Global change biology, 24, 35–54. [Nicholas Smith, United States of America]	Noted - a review of the details of representation of vegetation demography is beyond the scope of this assessment. It is also not specific to the question of CO2 fertilisation
106171	53	14	53	17	This important point could be strengthened by pointing out more explicitly that the main uncertainty in changes in carbon residence times are not in the uptake but in the carbon outflux processes, i.e. ecological disturbances such as the roles of extremes, tissue and plant mortality, fire, insects, sapling survival etc. This is systematically documented across several DGVMs by Friend et al., PNAS 111, 3280–3285, 2014; and empirically by Yu et al., PNAS, www.pnas.org/cgi/doi/10.1073/pnas.1821387116, 2019. The point is made in the text, but the text is still mostly focussed on the uptake; from a perspective of C residence times it could be more explicit on the C outfluxes because it is a quite important one where the models are still very weak. Could also be on p. 54 line 8. [Wolfgang Lucht, Germany]	Accepted - reference added
37915	53	16	53	16	Please add another reference, Hong and Kim (2011) Hong and 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. [Junhee Lee, Republic of Korea]	Rejected - irrelevant to the question of the direct CO2 effect and its relation ship with carbon-water cycles
74121	53	18	53	18	observations of IN SITU CO2 fertilization are rare, but there are LOTS of lab studies. [Daniel J. Short Gianotti, United States of America]	Accepted - text revised to specify "field experiments"
109525	53	21	53	23	The list of references here suggests a large amount of evidence for the statement that initial vegetation responses to CO2 may decline over time (progressive N limitation, PNL). Really PNL has only been observed at two sites – ORNL and Biocon – both experiments in which nitrogen was also causing a decline in production in the ambient CO2 treatments. Suggest just citing Norby and Reich here, possibly Hungate (but I find their annual NPP data very uncertain, in many years plot-level NPP is strongly negative). Clearly N and P are important in moderating the CO2 response, but PNL or PPL is not often seen. [Anthony Walker, United States of America]	Taken into account. Text has been modify to make clear that PNL is a possibility, not a fact. Also note changes to the preceeding paragaphs that clearly state that the obervational support for PNL is limited

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
5633	53	21	54	20	Du et al (2020) has recently developed a framework for testing nutrient limitation using plant leaf nutrient resorption efficiencies and mapped terrestrial nitrogen and phosphorus limitation using soil, climate and vegetation variables. This work provides a potential benchmark of nitrogen and phosphorus limitation for models to constrain predictions of the terrestrial carbon sink. The authors have found plants growing on 43 percent of Earth's natural land surface are limited by inadequate phosphorus, while plants on 18 percent of land are limited by inadequate nitrogen. The results imply a stronger and wider limitation of phosphorus on CO2 fertilization effect. Considering that climate warming will partially release nitrogen limitation via accelerating nitrogen mineralization and biological nitrogen fixation in high-latitude and high-altitude ecosystems, the limiting effect of inadequate phosphorus (mainly from weathering) will become more prevalent and dominant over nitrogen limitation on the global scale. Therefore, including C-P interactions in models is extremely essential for the projection of future carbon dynamics (but so few models have done so). This message might be included in the Chapter. The framework proposed by Du et al. (2020) might provide some clues to improve representation of nitrogen and phosphorus limitation in models. Reference: Du, E., Terrer, C., Pellegrini, A.F.A., Ahlström, A., van Lissa C.J., Zhao, X., Xia, N., Wu, X., Jackson, R.B. 2020. Global patterns of terrestrial nitrogen and phosphorus limitation. Nature Geoscience, 13, 221–226. [Enzai Du, China]	Accepted - reference added as example for the growing evidence base for nutrient limitation in the terrestrial biosphere. However, the paper does not allow to make direct inferences about the CO2 fertilisation effect. It is therefore added later in the Section w.r.t. to the ability to test global C-N-P models
26907	53	30	53	30	We suggest to mention the soil microorganism activity (priming effect - Fontaine et al. 2015)) ... Fontaine S., Barot S., Barré P., Bdioui N., Mary B., Rumpel C. (2007) Stability of organic carbon in deep soil layers controlled by fresh carbon supply. Nature 450, 277-280. [Eric Brun, France]	Accepted - test revised. Content already covered by literature cited
109527	53	30	53	31	The mycorrhizal control on biomass responses to CO2 suggested by Terrer 2016 is indistinguishable from control by woody vs non-woody plants (unpublished analysis using their dataset). Clearly woody plants have greater capacity to store additional carbon compared with non-woody plants. Point raised by Norby et al (2017). ESMs do consider those differences. [Anthony Walker, United States of America]	Taken into account. The revised text makes clear that mycorrhizal activity may have an effect on below-ground C storage. It does not make any reference to above-ground storage capacity, which is the topic of Norby et al. 2017.
74123	53	31	53	31	I don't see how reduced C accumulation in soils follow from the previous discussion (increased root C allocation). The has to be about the balance between the INCREASING soil carbon from plants and changes in soil respiration (which is not mentioned at all in this discussion). Seems like we should either remove the "Consistent with expectations from these observations" bit or add some respiration discussion. [Daniel J. Short Gianotti, United States of America]	Accepted - text revised
109529	53	31	53	34	Sentence is long and seems overly wordy. Suggest: "One global model that attempts to represent these processes suggests that elevated CO2 related carbon accumulation is reduced in soils but increased in vegetation relative to more conventional models (Sulman et al., 2019)." [Anthony Walker, United States of America]	Accepted - text revised

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
109531	53	36	53	36	Calling out Australia seems imbalanced given the importance ans scale of the tropical; zone. Suggest replacing with “other regions with highly weathered soils” [Anthony Walker, United States of America]	Accepted - text revised
127819	53	36			[RISK] Consider opening this paragraph with "Our understanding of the effects of P limitation is less developed than N, but a growing body of literature suggests it is as important (co-limitation/N and P interactions)." [Trigg Talley, United States of America]	Accepted - text revised, noting however that the evidence base is limited
127821	53	36			[RISK] There is a new synthesis more robustly inferring the areas of the planet that are nitrogen or phosphorus limited. Citation: Du, E. et al. Global patterns of terrestrial nitrogen and phosphorus limitation. Nat. Geosci. 13, 221,Äì226 (2020). https://doi.org/10.1038/s41561-019-0530-4 [Trigg Talley, United States of America]	Accepted - text revised
127823	53	37			Is Grandy (2013) the correct citation here? Maybe Terrer et al. (2019) instead? [Trigg Talley, United States of America]	Accepted - text revised
127825	53	37			[RISK] Previous research from CO2 experiments report the constraining role of soil P on the CO2 fertilization effect, e.g.: Hoosbeek, M.R. Elevated CO2 increased phosphorous loss from decomposing litter and soil organic matter at two FACE experiments with trees. Biogeochemistry 127, 89-97 (2016). https://doi.org/10.1007/s10533-015-0169-1 The two experiments discussed in Hoosbeek (2016) are free-air CO2 enrichment (FACE) experiments conducted with tree species, so the statement that the study by Jiang et al. is the first FACE experiment under low P is not accurate. Jiang et al.'s study is a mature forest indeed, but many CO2 experiments have been conducted under low P before, which is the topic sentence here. A synthesis of 138 CO2 experiments around the globe indeed shows that N and P are the most important drivers of the CO2 fertilization effect: Terrer, C., Jackson, R.B., Prentice, I.C. et al. Nitrogen and phosphorus constrain the CO2 fertilization of global plant biomass. Nat. Clim. Chang. 9, 684-689 (2019). https://doi.org/10.1038/s41558-019-0545-2 It is important to acknowledge global evidence on the role of P, instead of singling out one experiment carried out under particular conditions not necessarily representative of extensive areas of the planet. An example: CO2 experiments collectively show that soil phosphorus is an important constraint on the CO2 fertilization effect on plant biomass, as found in a synthesis of 138 experiments (Terrer et al., 2019). Indeed, a free-air CO2 enrichment experiment in a phosphorus-limited mature forest ecosystem did not find an increase in biomass growth despite increases in photosynthesis after three four of CO2 exposure (Jiang et al., 2020). The lack of free-air CO2 enrichment experiments in phosphorus-limited tropical forests, however, limits our understanding on the role of P availability in constraining the CO2 fertilization effect globally (Norby et al., 2015): Norby, R.J., De Kauwe, M.G., Domingues, T.F., Duursma, R.A., Ellsworth, D.S., Goll, D.S., Lapola, D.M., Luus, K.A., MacKenzie, A.R., Medlyn, B.E., Pavlick, R., Rammig, A., Smith, B., Thomas, R., Thonicke, K., Walker, A.P., Yang, X.	Accepted - text revised
74125	53	38	53	38	"an increase in biomass growth" Does this mean an increase in biomass? Or an increase in primary production (growth)? Also, is this aboveground biomass specifically? [Daniel J. Short Gianotti, United States of America]	Accepted - text revised
109533	53	38	53	38	What is meant by “biomass growth”? Suggest replacing with “biomass production” or “ecosystem carbon storage” [Anthony Walker, United States of America]	Accepted - text revised

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
26143	53	39	53	39	Update on the citation: M. Jiang, B.E. Medlyn, J.E. Drake, R.A. Duursma, I.C. Anderson, C.V.M. Barton, et al. 2020. The fate of carbon in a mature forest under carbon dioxide enrichment. Nature, 580, 227-231. [Mingkai Jiang, Australia]	Accepted - text revised
17877	53	39	53	42	Some additional references for P limitation: https://agupubs.onlinelibrary.wiley.com/doi/full/10.1002/2016GL069241, https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2019JG005082 [Katherine Calvin, United States of America]	Accepted - text revised
615	53	39			The CO2 fertilization effect was also absent in a Eucalypt forest on low P soil. See Ellsworth, David S., Anderson, Ian C., Crous, Kristine Y., Cooke, J., Drake, John E., Gherlenda, Andrew N., . . . Reich, Peter B. (2017). Elevated CO2 does not increase eucalypt forest productivity on a low-phosphorus soil. Nature Climate Change, 7, 279. doi:10.1038/nclimate3235 [Vicca Sara, Belgium]	Rejected - This reference is superseded by Jiang et al. 2020, who demonstrate that despite a CO" fertilisation effect on Photosynthesis, there was no increase in biomass production or ecosystem carbon storage
58881	53	44	53	49	See above comment - consider Drake et al. Global Change Biology (2016) and the effects of CO2 on water use efficiency. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted - it is unclear what this comment refers to directly, and how Drake et al. 2016 (10.1111/gcb.13109) is relevant to the question of WUE efficiency response of CO2. However, the WUE section has been expanded by the Quotation of Walker eta l. 2020, who provide a comprehensive review of these effects
74127	53	46	53	46	"at a rate that generally consistent…" either add "is" or remove "that" [Daniel J. Short Gianotti, United States of America]	Accepted - text revised
74129	53	46	53	47	"despite notable spread in the different observations that so far remain partially unexplained" What is the "spread" in (i.e., what variable -- it's not a "spread in obserations")? The slopes of WUE to CO2 concentrations? Similarly, what is partially unexplained? [Daniel J. Short Gianotti, United States of America]	Accepted - text revised. It is the spread in the slope of WUE to CO2 across different types of observations
7165	53	50			Add reference Peñuelas J., Canadell J., Ogaya R. 2011. Increased water-use efficiency during the 20th century did not translate into enhanced tree growth. Global Ecology and Biogeography 20: 597-608. [Josep Penuelas, Spain]	Rejected - This literature was already available for the AR5 assessment quoted initially
109535	53	52	53	52	Consider also adding Donohue et al 2013 as a reference. Donohue, R. J., Roderick, M. L., McVicar, T. R., & Farquhar, G. D. (2013). Impact of CO2 fertilization on maximum foliage cover across the globe's warm, arid environments. Geophysical Research Letters, 40(12), 3031–3035. https://doi.org/10.1002/grl.50563 [Anthony Walker, United States of America]	Accepted - text revised
116441	53		53		Good link to chapter 8 on water use efficiency but it could make sense to improve the coordination on energy - carbon - water flux links. [Valerie Masson-Delmotte, France]	Noted - A cross-chapter box has been proposed to expand this link

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
70831	54	1	54	8	In this passage, the large uncertainties introduced by land use, e.g. the turnover-accelerating effects (Erb et al. 2016 10.1038/ngeo2782) and the potential bias in the attribution of fluxes to natural (Sland) and land-use (Eluc) forces (see my comment above) is missing . The cessation of strong C-stock-reducing land use practices might be resonsible for a part of the calculated Sland, because it is not captured by the models (neither bookkeeping nor DGVMs) which results potentially in an attribution of the flux to SLand while it would be Eluc. This is in line with the many papers that do cast doubt on the strenght of the CO2 fertilization effect on C-stocks (nb, not fluxes; fluxes can be enhanced without proportional changes in stocks if turnover is accelerated, see e.g. Körner, Mahli on this issue). [Karlheinz Erb, Austria]	Rejected - The importance of turnover time and its changes has been mentioned in the preceding paragraphs. The effect of CO2 on the historical C balance and its attribution question is subject matter to Section 5.2. A combined assessment of different C cycle drivers in determining the total C balance is presented in Section 5.4.5 ff.
8841	54	2	54	2	Figure 5.28 is being referred to before 5.23 through 5.27. A renumbering is required I guess [Vaishali Naik, United States of America]	Noted - subject to final editing of the draft
8843	54	3	54	4	over what time period? [Vaishali Naik, United States of America]	Noted - the time is given period is given at the end of the sentence
21835	54	12	54	12	I may have forgotten the early part of the chapter but I think this may be first use of this ensemble in the chapter. If so does it either need to be introduced here briefly or a reference made to another chapter where it is introduced? I don't think reader knowledge of what this MIP is can be taken as a given? [Peter Thorne, Ireland]	Accepted - text changed
387	54	14	54	14	add „decreased“ before „nutrient availability“ [Wolfgang Obermeier, Germany]	Accepted - text revised
17879	54	17	54	20	Isn't there also limited information on the vertical distribution of P? Depending on assumptions about the vertical distribution you could different results. There is a paper by Burrows et al in review at JAMES that includes two different ways of initializing the vertical distribution of P from the same data set. [Katherine Calvin, United States of America]	Noted - This may be relevant, but we cannot consider papers in review
42807	54	23	54	23	Not sure I understand the title of the chapter. What is different from some of the other discussions on CO2 utake by the ocean? And is this only valid for Coast? Lots of repeat from previous, [Melissa Chierici, Norway]	Noted - feedback mechanisms and their impacts are addressed in this section. Not limited to the coastal zone but rather focused on open waters.
96585	54	23	55	17	The chapter 5.4.2 „Direct CO2 effects on ocean carbon uptake“ describes the potential limits of the ocean as natural CO2 sink. Would it be possible to connect those future restrictions of the natural uptake somehow with the potential effects of additional CO2 input by ocean-related CDR techniques? [Nicole Wilke, Germany]	Accepted - the link of buffering capacity with CDR has been made in section 5.6.2.2.3
83825	54	23	55	17	This section refers the direct CO2 effects on the ocean carbon uptake, only the physical and chemical effects have been assessed, causing ocean acidification and relative effexts. Oceans also uptake CO2 in bilogical way, mainly via the microbes, such as the well known biological carbon pump and microbial carbon pump, which uptake and sequestrate CO2 without or likely not causing acidification. Thus, at least brief assessment of biological aspect is recommended. [Nianzhi Jiao, China]	Rejected - biological pump is addressed in section 5.4.4.2.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
42809	54	25	54	36	Lots of repeat in this section already explained in previous chapter 5.3. For example what occurs when CO2 is taken up by the ocean, as well as how much the ocean has taken up. This has already been addressed. There is something with the structure of the whole report that makes it difficult to comment on since there are many places bringin up the same things and issues. Suggest to restrcuture into chapters for ex Atmosph, Lan, Ocean; subdivide into regions, global, drivers, observational evidence and modelled. [Melissa Chierici, Norway]	Taken into account - changes in the amplitude of seasonal cycle of surface-water hydrogen ion concentration and pH were deleted because they are addressed in section 5.3.3.3. Some repetitions are unavoidable as introductory remarks for this section assessing the carbonate chemistry feedback on ocean carbon uptake.
58867	54	25	54	44	It should be relevant to add something about the scenarios necessary to reach the 1,5 degree warming, which 6 out of 10 ESM follow a negative emission scenario. In this scenario the subtropical mode waters show a large reemergence of anthropogenic carbon at relatively short renewal timescales (< 10 years) emission availability of this due to the short term reservoir (Schwinger and Tjiputra (2018), Global Carbon and other Biogeochemical Cycles and Feedbacks). https://doi.org/10.1029/2018GL077790. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected - a feedback of seawater carbonate chemistry to climate is addressed in this section.
2347	54	25		32	I would suggest to add few references for the pCO2 definition and formulas. Examples: Copin-Montégut C, 1988. A new formula for the effect of temperature on the partial pressure of CO2 in seawater. Marine Chemistry, 25 (1), 29-37, ISSN 0304-4203, https://doi.org/10.1016/0304-4203(88)90012-6 (http://www.sciencedirect.com/science/article/pii/0304420388900126) ; Goyet C., Frank J. Millero, Alain Poisson, Deborah K. Shafer, 1993. Temperature dependence of CO2 fugacity in seawater, Marine Chemistry, 44 (2–4), 205-219, ISSN 0304-4203, https://doi.org/10.1016/0304-4203(93)90203-Z (http://www.sciencedirect.com/science/article/pii/030442039390203Z) ; Goyet C. and , Peltzer E., 1994. Comparison of the August–September 1991 and 1979 surface partial pressure of CO2 distribution in the Equatorial Pacific Ocean near 150°W. Marine Chemistry, 45 (4), 257-266, ISSN 0304-4203, https://doi.org/10.1016/0304-4203(94)90073-6 (http://www.sciencedirect.com/science/article/pii/0304420394900736). [Abed El Rahman Hassoun, Lebanon]	Rejected - pCO2 definition and formulas are not given in this report as in AR5 and SROCC.
2217	54	25		32	I would suggest to add few references for the pCO2 definition and formulas. Examples: [Abed El Rahman Hassoun, Lebanon]	Rejected - the same as the response to the comment 2347.
2269	54	25		32	Copin-Montégut C, 1988. A new formula for the effect of temperature on the partial pressure of CO2 in seawater. Marine Chemistry, 25 (1), 29-37, ISSN 0304-4203, https://doi.org/10.1016/0304-4203(88)90012-6 (http://www.sciencedirect.com/science/article/pii/0304420388900126) [Abed El Rahman Hassoun, Lebanon]	Rejected - the same as the response to the comment 2347.
2271	54	25		32	Goyet C., Frank J. Millero, Alain Poisson, Deborah K. Shafer, 1993. Temperature dependence of CO2 fugacity in seawater, Marine Chemistry, 44 (2–4), 205-219, ISSN 0304-4203, https://doi.org/10.1016/0304-4203(93)90203-Z. (http://www.sciencedirect.com/science/article/pii/030442039390203Z) [Abed El Rahman Hassoun, Lebanon]	Rejected - the same as the response to the comment 2347.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
2273	54	25		32	Goyet C. and , Peltzer E., 1994. Comparison of the August–September 1991 and 1979 surface partial pressure of CO2 distribution in the Equatorial Pacific Ocean near 150°W. Marine Chemistry, 45 (4), 257-266, ISSN 0304-4203, https://doi.org/10.1016/0304-4203(94)90073-6 (http://www.sciencedirect.com/science/article/pii/0304420394900736) [Abed El Rahman Hassoun, Lebanon]	Rejected - the same as the response to the comment 2347.
127827	54	26	54	26	Something wrong with grammar of this sentence. [Trigg Talley, United States of America]	Accepted - the sentence was rephrased following the comment 58883.
58883	54	26	54	27	This is phrased awkwardly. Consider "the difference between the partial pressure of CO2 in the air that is in air-sea equilbrium with surface seawater and the partial pressure in the overlying atmosphere…" [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - the sentence was rephrased following the comment.
36383	54	32	54	32	Where did this value of 28% ± 5 come from? The long-term average in Table 5.1 of this document is 25% ± 5 is from Friedlingstein for the time frame from 1750 to 2018. [Adrienne Sutton, United States of America]	Accepted - text was revised based on the latest global carbon budget of Friedlingstein et al., 2019.
58885	54	32	54	36	There is data available (from the American Meteorological Society, and elsewhere) on annual CO2 uptake by the ocean. It seems counter-intuitive to report continued growth in anthropogenic CO2 emissions from 1994-2007 and then say "the ocean continued to absorb essentially the same ratio" when that ratio is a running average from that same time period. Is it possible to leverage time-series data for both pieces of data to better show steady oceanic CO2 absorption rates in relation to increasing emissions? See, for example, Friedlingstein et al. (2019) Earth Syst. Sci Data, https://doi.org/10.5194/essd-11-1783-2019 [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - changes in the ocean anthropogenic CO2 uptake fraction was evaluated with the estimates in Global Carbon Budget 2019 by Friedlingstein et al. 2019.
30569	54	33	54	53	Where did this value of 28% ± 5 come from? The long-term average in Table 5.1 of this document is 25% ± 5 is from Friedlingstein for the time frame from 1750 to 2018. [nina bednarsek, United States of America]	Accepted - the same as the response to the comment 36383.
44995	54	34	54	34	Please correct…essentially the same rates 26 ± 5 %. Instead use approximately not the same given that 26 ± 5 % is smaller than 28 ± 5 %. [LOUIS LUBANGO Mitondo, Ethiopia]	Accepted - text was revised but with different quantification of ocean anthropogenic CO2 uptake fractions.
73161	54	34	54	34	Delete 'years of'. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - changes was made.
44993	54	38	54	39	The description is confusing. Please rephrase it starting with the cause. The following can help. Naturally, the oceans absorb/dissolve large amounts of CO2 emitted in the atmosphere. Over the past years, the ocean capacity to dissolve CO2 has been decreasing significantly subsequent to higher rate of saturation by such CO2…. [LOUIS LUBANGO Mitondo, Ethiopia]	Accepted - text was revised.
104891	54	39	54	40	Very confusing statement about the ocean CO2 sink. I get what you mean, but should reword it to clarify that the ocean will continue to take up CO2, but the ratio of the annual ocean CO2 uptake to annual atmospheric CO2 growth will decrease (or something). [Timothy DeVries, United States of America]	Accepted - text was revised.
36385	54	41	54	42	This statement is highly problematic. Table 5.1 of this document suggests that the long-term mean ocean fraction is 25% ± 5 and has a value of 23% ± 5 for the period between 2009-2018. So how can the "CO2 uptake decrease from 32% in the 1990s to 23% over the 21st century." It seems to me we are are already there! [Adrienne Sutton, United States of America]	Accepted - text was revised.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
30571	54	41	54	42	This statement is highly problematic. Table 5.1 of this document suggests that the long-term mean ocean fraction is 25% ± 5 and has a value of 23% 5 for the period between 2009-2018. So how can the "CO2 uptake decrease from 32% in the 1990a to 23 % over the 21st century." It seems to me we are are already there! [nina bednarsek, United States of America]	Accepted - text was revised.
44991	54	42	54	42	The description CO2 buffering capacity of seawater does represent the exact process that is being referred to. The correct description is …. solubility of CO2 in sea waters, not the buffering capacity…. [LOUIS LUBANGO Mitondo, Ethiopia]	Rejected - the comment unclear. The factor that will dominantly control the future ocean CO2 uptake is the reduction of buffering capacity. The effect of the reduction of solubility due to ocean warming is minor.
127829	54	42	54	44	Sentence not clear. [Trigg Talley, United States of America]	Accepted - sentence was revised.
73163	54	47	54	47	Insert 'the' after 'since'. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised.
78501	54	51	54	51	why only cite results from 1 ESM. Even if this study used just one, results are available for many to verify if this is true or not. [Chris Jones, United Kingdom (of Great Britain and Northern Ireland)]	Noted - this is because outputs from a large ensemble of single ESM have been used in the peer-reviewed literature (Schluneger et al. NCC, 2019). However, the mechanism is clearly explained in the literature, and three other ESMs, analysed internally, give consistent results with Schlunegger et al., 2019. Therefore we revised the text and put "medium confidence" on this conclusion.
115375	54	52			How can the seasonal variation be largest in summer? [Gillett Nathan, Canada]	Noted - the reason for this has been illustrated in Schlunegger et al. NCC, 2019. Text was revised to show the essence of the reason.
7333	54	53	55	1	Sentence could be clearer. Maybe add why increased biological CO2 assimilation does not automatically lead to increased production. [Svenja Halfter, Australia]	Accepted - text was revised.
52335	54	55	54	55	do you really mean "lower buffering capacity results in a larger draw-down of pCO2sea"? Don't you mean the opposite, higher buffering capacity will increase potential for ocean CO2 uptake? [Agneta Fransson, Norway]	Taken into account - text was revised. A phrase "when DIC draw-down due to" was inserted. "Lower buffering capacity results in a larger draw-down of pCO2sea" is correct.
2313	54				Copin-Montégut C, 1988. A new formula for the effect of temperature on the partial pressure of CO2 in seawater. Marine Chemistry, 25 (1), 29-37, ISSN 0304-4203, https://doi.org/10.1016/0304-4203(88)90012-6 (http://www.sciencedirect.com/science/article/pii/0304420388900126) [Abed El Rahman Hassoun, Lebanon]	Rejected - Section 5.4.1 is about terrestrial carbon cycle. Also, these references are not new
2315	54				Goyet C., Frank J. Millero, Alain Poisson, Deborah K. Shafer, 1993. Temperature dependence of CO2 fugacity in seawater, Marine Chemistry, 44 (2–4), 205-219, ISSN 0304-4203, https://doi.org/10.1016/0304-4203(93)90203-Z. (http://www.sciencedirect.com/science/article/pii/030442039390203Z) [Abed El Rahman Hassoun, Lebanon]	Rejected - Section 5.4.1 is about terrestrial carbon cycle. Also, these references are not new

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2317	54				Goyet C. and , Peltzer E., 1994. Comparison of the August–September 1991 and 1979 surface partial pressure of CO2 distribution in the Equatorial Pacific Ocean near 150°W. Marine Chemistry, 45 (4), 257-266, ISSN 0304-4203, https://doi.org/10.1016/0304-4203(94)90073-6 (http://www.sciencedirect.com/science/article/pii/0304420394900736) [Abed El Rahman Hassoun, Lebanon]	Rejected - Section 5.4.1 is about terrestrial carbon cycle. Also, these references are not new
44997	55	1	55	1	…after remains unchanged… Add additional uptake of CO2 from the atmosphere is likely to impact both the total concentration of inorganic carbon (H2CO3, HCO3 -, CO3 2- and CO2), amplify the seasonal cycles of surface-water hydrogen ion concentrations… [LOUIS LUBANGO Mitondo, Ethiopia]	Rejected - but text was revised so that the explanation of the mechanism becomes clearer.
18223	55	1	55	4	This sentence is confusing. Make is clearer to the reader in which direction (i.e. positive/ negative, increase/ decrease) the changes are occuring. The number (+81 %) for the amplification of the seasonal cycle is not explained well. Does the concentration range in the seasonal cycle increase by 81%? I would also replace the word attenuating with a different word to improve the clarity of the sentence. [Chelsey Baker, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text was revised based on the results of from CMIP6 and direction was clarified.
42811	55	1	55	5	the principle behind Ocean acidfication addressed again and that it has changed pH and may have sonsequences for marine life. This has been addressed in 5.3 page 43. Suggest to combine and have one section discussing OA and trends in different regions, use more observational evidence as well as modelled. [Melissa Chierici, Norway]	Accepted - descriptions on changes in the amplitudes of seasonal variability of [H+] and pH were removed. They are now only in section 5.3.3.3.
78503	55	2	55	3	when making “likelihood” statements for future projections need to be clear for which scenario as results are clearly dependent on that choice [Chris Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - descriptions on changes in the amplitudes of seasonal variation of [H+] and pH were removed from this section, but the descriptions in section 3.3.3.3 was revised.
41653	55	3	55	3	The information on the seasonal cycle of pH will confuse the reader if expressed in % [Katharina Meurer, Sweden]	Rejected - the comment makes sense in light of that pH is a logarithmic value. However, the description here follows that of peer-reviewed literature (Kwiatkowski et al., 2020) and seasonal variation in [H+] are also described to avoid misinterpretations. Changes in the amplitudes of seasonal variation of [H+] and pH are now described only in the section 5.3.3.3.
73165	55	3	55	3	Capital 'C' for 'century'. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - TSU style guide requests "21st century" without capital C. (It was made consistent now to not use the capital C).
44999	55	4	55	5	Delete depending…….Instead use..The magnitude of the impact on ocean's acidity and marine organisms is likely to be mediated by regional and seasonal differences. [LOUIS LUBANGO Mitondo, Ethiopia]	Not applicable - changes in the amplitudes of seasonal variation of [H+] and pH are now described only in the section 5.3.3.3. The impact of acidification on marine organisms is outside the scope of WGI.

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17045	55	6	55	7	Dissolved inorganic carbon contribution as alkalinity and/or CO2 from the marine sediments to the water column has gained considerable attention recently as is important for oceanic CO2 balance (Hu and Cai., 2011, Akam et al., 2020). Alkalinity contribution from the sediments to the water column can reduce the ocean acidification effect and even enhance the CO2 absorption capacity of surface water (Chen and Wang, 1999; Chen, 2002; Thomas et al., 2008; Hu and Cai, 2011; Krumins et al., 2013; Brenner et al., 2016). Citation:Hu, X., and Cai, W. J. (2011). An assessment of ocean margin anaerobic processes on oceanic alkalinity budget. Global Biogeochem. Cycles 25:GB3003. doi: 10.1029/2010GB003859 Akam, S.A., Coffin, R.B., Abdulla, H.a.N., and Lyons, T.W. (2020). Dissolved Inorganic Carbon Pump in Methane-Charged Shallow Marine Sediments: State of the Art and New Model Perspectives. Frontiers in Marine Science 7.https://doi.org/10.3389/fmars.2020.00206 Chen, C. T. A., and Wang, S. L. (1999). Carbon, alkalinity and nutrient budgets on the East China Sea continental shelf. J. Geophys. Res. Oceans 104, 20675–20686. doi: 10.1029/1999jc900055 Krumins, V., Gehlen, M., Arndt, S., Cappellen, P. V., and Regnier, P. (2013). Dissolved inorganic carbon and alkalinity fluxes from coastal marine sediments: model estimates for different shelf environments and sensitivity to global change. Biogeosciences 10, 371–398. doi: 10.5194/bg-10-371-2013 Brenner, H., Braeckman, U., Le Guitton, M., and Meysman, F. J. (2016). The impact of sedimentary alkalinity release on the water column CO 2 system in the North Sea. Biogeosciences 13, 841–863. doi: 10.5194/bg-13-841-2016 [Sajjad Abdullajintakam, United States of America]	Noted - this effect is not included here as a large portion of the efflux of alkalinity from sediments is in the form of reduced solutes that are highly unstable in oxygenated waters. Therefore it is unlikely to act as a significant buffer against increasing atmospheric CO2 (Andersson et al., 2005; 10.2475/ajs.305.9.875 and Krumins et al., 2013; 10.5194/bg-10-371-2013)
45001	55	7	55	8	Use the following description. Subsequently, the ability of such ocean acidification to reduce the calcification rate of miarine organismsm (….) constitutes a negative feedback on atmospheric CO2….. [LOUIS LUBANGO Mitondo, Ethiopia]	Taken into account - sentence has been modified
36387	55	7	55	17	This paragraph needs to be written more carefully. As stated it is unclear whether or not the authors are referring to both reduced calcification (a negative feedback) and increased dissolution (a negative feedback). The problem is that the reduced calcification occurs in the surface waters while the increased dissolution occurs in deep waters so their impacts on climate feedbacks occur over differnet time scales, with the former being immediate and the latter occuring over longer time scales. This needs to be described more fully in the text. [Adrienne Sutton, United States of America]	Taken into account - paragraph has been modified

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
2349	55	7		8	I would propose to mention that OA may reduce the calcification rate in "few" marine organisms, instead of generalizing as many studies have shown that some corals are able to maintain their calcification rates despite shifts in their calcifying fluid carbonate chemistry (Comeau et al., 2017) and have linked between Coral resistance and calcium at the site of calcification in OA conditions (DeCarlo et al., 2018), whereas other studies highlights that OA impairs the calcification process and corrodes inner shell surfaces of blood clams (Zhao et al., 2017) and affects foraminiferal survival rate and growth/calcification (mainly through test weight reduction ; Guama´n-Guevara et al., 2019). Zhao X., Wei Shi, Yu Han, Saixi Liu, Cheng Guo, Wandong Fu, Xueliang Chai, Guangxu Liu, 2017. Ocean acidification adversely influences metabolism, extracellular pH and calcification of an economically important marine bivalve, Tegillarca granosa. Marine Environmental Research, 125, 82-89, https://doi.org/10.1016/j.marenvres.2017.01.007 (http://www.sciencedirect.com/science/article/pii/S0141113616303300) ; Comeau, S., Cornwall, C.E. & McCulloch, M.T., 2017. Decoupling between the response of coral calcifying fluid pH and calcification to ocean acidification. Sci Rep 7, 7573. https://doi.org/10.1038/s41598-017-08003-z ; DeCarlo T.M., S. Comeau, C. E. Cornwall and M. T. McCulloch 2018. Coral resistance to ocean acidification linked to increased calcium at the site of calcification. Proc. R. Soc. B. 28520180564 http://doi.org/10.1098/rspb.2018.0564 ; Guama´n-Guevara F, Austin H, Hicks N, Streeter R, Austin WEN, 2019. Impacts of ocean acidification on intertidal benthic foraminiferal growth and calcification. PLoS ONE 14(8): e0220046. https://doi.org/10.1371/journal.pone.0220046. [Abed El Rahman Hassoun, Lebanon]	Taken into account - sentence has been modified
2219	55	7		8	I would propose to mention that OA may reduce the calcification rate in "few" marine organisms, instead of generalizing as many studies have shown that some corals are able to maintain their calcification rates despite shifts in their calcifying fluid carbonate chemistry (Comeau et al., 2017) and have linked between Coral resistance and calcium at the site of calcification in OA conditions (DeCarlo et al., 2018), whereas other studies highlights that OA impairs the calcification process and corrodes inner shell surfaces of blood clams (Zhao et al., 2017) and affects foraminiferal survival rate and growth/calcification (mainly through test weight reduction ; Guama´n-Guevara et al., 2019). [Abed El Rahman Hassoun, Lebanon]	Taken into account - sentence has been modified
2275	55	7		8	Zhao X., Wei Shi, Yu Han, Saixi Liu, Cheng Guo, Wandong Fu, Xueliang Chai, Guangxu Liu, 2017. Ocean acidification adversely influences metabolism, extracellular pH and calcification of an economically important marine bivalve, Tegillarca granosa. Marine Environmental Research, 125, 82-89, https://doi.org/10.1016/j.marenvres.2017.01.007. (http://www.sciencedirect.com/science/article/pii/S0141113616303300) [Abed El Rahman Hassoun, Lebanon]	Taken into account - repeat of comments 2349 and 2219
2277	55	7		8	Comeau, S., Cornwall, C.E. & McCulloch, M.T., 2017. Decoupling between the response of coral calcifying fluid pH and calcification to ocean acidification. Sci Rep 7, 7573. https://doi.org/10.1038/s41598-017-08003-z [Abed El Rahman Hassoun, Lebanon]	Taken into account - repeat of comments 2349 and 2219

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2279	55	7		8	DeCarlo T.M., S. Comeau, C. E. Cornwall and M. T. McCulloch 2018. Coral resistance to ocean acidification linked to increased calcium at the site of calcification. Proc. R. Soc. B. 28520180564 http://doi.org/10.1098/rspb.2018.0564 [Abed El Rahman Hassoun, Lebanon]	Taken into account - repeat of comments 2349 and 2219
2281	55	7		8	Guama´n-Guevara F, Austin H, Hicks N, Streeter R, Austin WEN, 2019. Impacts of ocean acidification on intertidal benthic foraminiferal growth and calcification. PLoS ONE 14(8): e0220046. https://doi.org/10.1371/journal.pone.0220046 [Abed El Rahman Hassoun, Lebanon]	Taken into account - repeat of comments 2349 and 2219
45449	55	10	55	10	This sentence is confusing, as it implies that CaCO3 dissolution decreases with ocean acidification. [Olivier Sulpis, Netherlands]	Taken into account - sentence has been modified
30573	55	10	55	11	This paragraph needs to be written more carefully. As stated is unclear whether or not the authors are referring to both reduced calcification (a negative feedback) and increased dissolution (a negative feedback). The problem is that the reduced calcification occurs in the surface waters while the increased dissolution occurs in deep waters so their impacts on climate feedbacks occur over differnet time scales, with the former being immediate and the latter occuring over longer time scales. This needs to be described more fully in the text. [nina bednarsek, United States of America]	Taken into account - paragraph has been modified
37917	55	12	55	15	It looks fascinating to mention ENSO-related changes in carbon fluxes in this report but I am not quite sure if such relationship needs to be address in this 5.4.3.1 plant physiology. This chapter needs to focus on direct evidence on impacts on plant physiology and its related what we know and what we do not know. [Junhee Lee, Republic of Korea]	Taken into account - we discuss ENSO-related changes in order to put into context the main drivers of climate-driven changes to terrestrial carbon stocks, and since ENSO-timescale variability is useful for understanding the longer-term response.
2351	55	15		17	I would suggest to add a brief definition of the "Ballast effect" between parentheses (Sanders et al., 2010): Sanders, R., Morris, P. J., Poulton, A. J., Stinchcombe, M. C., Charalampopoulou, A., Lucas, M. I., and Thomalla, S. J. (2010), Does a ballast effect occur in the surface ocean? Geophys. Res. Lett., 37, L08602, doi:10.1029/2010GL042574. [Abed El Rahman Hassoun, Lebanon]	Taken into account - although there isn't space to include a description of the ballast effect, a reference to the original study has been included
2221	55	15		17	I would suggest to add a brief definition of the "Ballast effect" between parentheses (Sanders et al., 2010). Sanders, R., Morris, P. J., Poulton, A. J., Stinchcombe, M. C., Charalampopoulou, A., Lucas, M. I., and Thomalla, S. J. (2010), Does a ballast effect occur in the surface ocean? Geophys. Res. Lett., 37, L08602, doi:10.1029/2010GL042574. [Abed El Rahman Hassoun, Lebanon]	Taken into account - although there isn't space to include a description of the ballast effect, a reference to the original study has been included
89473	55	20	55	21	A useful citation could be: Walker, X.J., J.L. Baltzer, S.G. Cumming, N.J. Day, C. Ebert, S. Goetz, J.F. Johnstone, S. Potter, B.M. Rogers, E.A.G. Schuur, M.R. Turetsky, and M.C. Mack. 2019. Increasing wildfires threaten historic carbon sink of boreal forest soils. Nature, 572(7770): 520-523, https://doi.org/10.1038/s41586-019-1474-y [Edward Schuur, United States of America]	Accepted: replaced Mack et al citation with Walker et al citation
58887	55	22	55	22	Is this decrease in land carbon uptake a decrease in absolute carbon uptake (i.e. raw mass of carbon) or proportion of carbon uptake (i.e. percentage of total emissions)? This is an important distinction. For more quantitative analysis, see, for example, Tharammal et al. (2019) Env Res Letters, https://doi.org/10.1088/1748-9326/ab3012 [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	rejected: This comment refers to AR5 assessment, which we cannot edit.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
58889	55	22	55	36	This section begins by saying future climate change will decrease land carbon uptake, but ends by saying the net effect is a net increase in global land carbon storage out to 2100. Whether this is absolute magnitude or proportion of emissions is important, and the distinction between long-term (e.g. past 2100) and short-term (until 2100) changes could be more clear. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	accepted: reworded to make more clear
77747	55	22	55	36	It was previously stated in the Chapter that carbon fertilisation and increased rainfall will result in additional greening of vegetation in certain regions. Is there a need to clarify here that while the additional greening referred to earlier will occur the overall net carbon balance will be negative as a result of the factors discussed in this paragraph? [Emer Griffin, Ireland]	rejected: we do not discuss greening per se here.
17881	55	33	55	36	Across all emissions scenarios? [Katherine Calvin, United States of America]	accepted: clarified to state that this is under all scenarios
37919	55	39	56	15	We need to discuss changes in water use efficiency with warming climate and increasing CO2 environment. [Junhee Lee, Republic of Korea]	rejected: this is discussed in section 5.4.1 and chapter 7
7211	55	41	55	45	In addition to changing the growing season, warming will reduce chilling requirements of plants, that could reduce production of plants, and it should be mentioned as a negative impact. (e.g. Campoy, J. A., Darbyshire, R., Dirlewanger, E., Quero-García, J., & Wenden, B. (2019). Yield potential definition of the chilling requirement reveals likely underestimation of the risk of climate change on winter chill accumulation. International journal of biometeorology, 63(2), 183-192.) [Seyed Muhammadreza Tabatabaei, Iran]	rejected: the relationship between chilling requirements and overall biospheric productivity is less clear than that between chilling and fruit production
58933	55	41	56	14	I think it is importamt to mentioned the impact of increased freeze-thaw cycles on cold climate plants. If appropriate, the following sentence could be added" "Warmer temperatures are likely to increase the number of freeze-thaw cycles in cold regions (high confidence). This is likely to increase plant tissue damage during winter when ice crystal formation causes cells to rupture, leading to the release of inter- and intracellular solutes (high confidence) (Jones, 2013). A maximum amount of biomass P is often released from the damaged cellular tissues once a certain number of FTCs occurs (Bechmann et al., 2005; Øgaard, 2015; Cober et al., 2018), an effect that varies with crop species and can be seen as similar to that achieved with chemical kill (Cober et al., 2018). Bechmann et al. (2005), for instance, observed a maximum release of biomass P after five FTCs during a hard frost (−18°C).” [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	rejected -- too much detail for this section
28399	55	45	55	45	The following recent paper also directly addresses this point: Liu, Z., Chen, L., Smith, N. G., Yuan, W., Chen, X., Zhou, G., ... & Chu, C. (2019). Global divergent responses of primary productivity to water, energy, and CO2. Environmental Research Letters, 14(12), 124044. [Nicholas Smith, United States of America]	accepted: added citation to this paper

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
28401	55	47	55	49	I don't think is true. For instance, a meta-analysis by Yamori et al. (2014) indicates that responses are quite variable in response to direct warming across a wide variety of plant species and are flat on average for C3 species. The Pau et al. study cited does not explore direct warming but rather seasonal changes, which may be influenced by a wide variety of climate factors. The warming response of photosynthesis is likely to be positive, negative, or negligible depending on context. This will depend on the interplay between the stimulation of biochemical processes by warming (Smith and Dukes 2017) and the negative effects of warming on stomatal conductance related to an increase in vapor pressure deficit (Grossiar et al., 2020). This interplay is nicely reviewed in Lin et al. (2015). [CITATIONS] Yamori, W., Hikosaka, K., & Way, D. A. (2014). Temperature response of photosynthesis in C 3, C 4, and CAM plants: temperature acclimation and temperature adaptation. Photosynthesis research, 119(1-2), 101-117.; Smith, N. G., & Dukes, J. S. (2017). Short-term acclimation to warmer temperatures accelerates leaf carbon exchange processes across plant types. Global change biology, 23(11), 4840-4853.; Grossiord, C., Buckley, T. N., Cernusak, L. A., Novick, K. A., Poulter, B., Siegwolf, R. T., ... & McDowell, N. G. (2020). Plant responses to rising vapor pressure deficit. New Phytologist, 226(6), 1550-1566.; Lin Y-S, Medlyn BE, Ellsworth DS (2012) Temperature responses of leaf net photosynthesis: the role of component processes. Tree Physiology, 32, 219–231. [Nicholas Smith, United States of America]	accepted -- changed these sentences and incorporated suggested citations.
7213	55	47	56	5	In addition to reducing photosynthetic rates, some extreme of high temperatures could cause plant death (Menezes-Silva, P. E., Loram-Lourenço, L., Alves, R. D. F. B., Sousa, L. F., Almeida, S. E. D. S., & Farnese, F. S. (2019). Different ways to die in a changing world: Consequences of climate change for tree species performance and survival through an ecophysiological perspective. Ecology and evolution, 9(20), 11979-11999.) [Seyed Muhammadreza Tabatabaei, Iran]	rejected -- too much detail for this section
7167	55	47			This statement is not always true: " high temperatures are observed to correlate with reduced photosynthetic rates " especially in cold regions [Josep Penuelas, Spain]	accepted -- this sentence has been changed
74131	55	49	55	51	Not just higher VPD (which questionably controls photosynthetic processes), but probably more importantly lower moisture availability with soils that is coupled with VPD as well as being coupled with surface-level air temperature. I'd recommend: "A key question is whether the observed relationships are due to the exceedance of temperature thresholds in photosynthetic biochemistry itself, or to reduced moisture supply in soils and increased atmospheric moisture demand correlated with high temperatures." Potential citation if necessary is "Satellite-Based Assessment of Land Surface Energy Partitioning–Soil Moisture Relationships and Effects of Confounding Variables" by Feldman et al. (2019) https://doi.org/10.1029/2019WR025874 [Daniel J. Short Gianotti, United States of America]	accepted -- this sentence has been changed
28403	55	55	55	55	It should be noted that this is "canopy" photosynthesis [Nicholas Smith, United States of America]	accepted

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
28407	55	55	55	55	Here it might be worth noting that parameters representing the temperature responses of plant physiology have been noted as the most sensitive in models that do not include acclimation, further emphasizing the importance of including temperature acclimation in model simulations. See Booth BBB, Chris DJ, Mat C et al. (2012) High sensitivity of future global warming to land carbon cycle processes. Environ. Res. Lett., 7, 24002. [Nicholas Smith, United States of America]	rejected -- whether or not this reduces parametric uncertainty is not yet clear, as acclimation adds its own set of parameters.
116443	55		55		How are air quality (ozone, aerosol) effects on land carbon uptake addressed in ch 5 (link to the assessment in chapter 6)? [Valerie Masson-Delmotte, France]	Taken into account - covered in section 5.4.
2319	55				Comeau, S., Cornwall, C.E. & McCulloch, M.T., 2017. Decoupling between the response of coral calcifying fluid pH and calcification to ocean acidification. Sci Rep 7, 7573. https://doi.org/10.1038/s41598-017-08003-z [Abed El Rahman Hassoun, Lebanon]	Rejected - what this comment suggests is unclear. Rejection is also the case when this is the suggestion to cite this literature. The feedback of ocean anthropogenic CO2 uptake (or acidification) to the atmospheric CO2 increase is addressed in this subsection.
2321	55				DeCarlo T.M., S. Comeau, C. E. Cornwall and M. T. McCulloch 2018. Coral resistance to ocean acidification linked to increased calcium at the site of calcification. Proc. R. Soc. B. 28520180564 http://doi.org/10.1098/rspb.2018.0564 [Abed El Rahman Hassoun, Lebanon]	Rejected - the response to this comment is the same as that to the comment 2319.
2323	55				Guama´n-Guevara F, Austin H, Hicks N, Streeter R, Austin WEN, 2019. Impacts of ocean acidification on intertidal benthic foraminiferal growth and calcification. PLoS ONE 14(8): e0220046. https://doi.org/10.1371/journal.pone.0220046 [Abed El Rahman Hassoun, Lebanon]	Rejected - the response to this comment is the same as that to the comment 2319.
28405	56	2	56	2	The Smith et al. (2015) publication noted in the previous line also includes temperature acclimation of autotrophic respiration in the NOAA land surface model LM3. Note that this publcation indicates that temperature acclimation improves simulations of canopy photosynthesis [Nicholas Smith, United States of America]	accepted -- reworded to say that acclimation had been applied to photosynthesis and/or autotrophic respiration and grouped references together.
90091	56	7	56	10	This statement uploaded by Ted Schuur on behalf of a graduate class that reviewed the chapter (this was communicated with IPCC TSU in advance): These two sentences cover much of the same subject and could be shortened to "Since AR5, research has found that much of the local scale year-to-year variability in global vegetation production and net land carbon uptake is associated with interannual variability in total and seasonal precipitation and therefore the extent of drought (see also Section 8.4.2.7)." [Edward Schuur, United States of America]	accepted
7215	56	7	56	15	Extreme rainfall will increase flood risk that could cause plant death, and the subject should be mentioned (Menezes-Silva, P. E., Loram-Lourenço, L., Alves, R. D. F. B., Sousa, L. F., Almeida, S. E. D. S., & Farnese, F. S. (2019). Different ways to die in a changing world: Consequences of climate change for tree species performance and survival through an ecophysiological perspective. Ecology and evolution, 9(20), 11979-11999.) [Seyed Muhammadreza Tabatabaei, Iran]	rejected -- its not clear what the magnitude of flood impacts is, based on the suggested paper
96587	56	12	56	12	Please introduce "ENSO". [Nicole Wilke, Germany]	Rejected -- this acronym is defined earlier in the chapter (section 5.2.1.1)

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
127831	56	12	56	13	Missing reference? [Trigg Talley, United States of America]	accepted -- added reference to Liu et al., 2017 Science paper
74133	56	13	56	15	Some evidence of this in online historical runs too: "Satellite and station observations demonstrate water availability's effect on continental-scale evaporative and photosynthetic land surface dynamics DJ Short Gianotti, AJ Rigden, GD Salvucci, D Entekhabi- Water Resources Research, 2019" [Daniel J. Short Gianotti, United States of America]	accepted, reworded and added reference.
2803	56	18	56	18	Given the large areas of fires in several locations in 2019 parts of this section may already be out of date [Stephen Wilkinson, United Arab Emirates]	Accepted: Rewrote subsection taking into account this and other comments. Some newer references were added to discuss recent high-fire years.
86755	56	18	56	54	If lightning will increase in frequency, this will increase forest and wildfires in some regions. Please see page 26-line 26 [Oyvind Christophersen, Norway]	Rejected. Subsection rewritten but did not include this citation, as length and number of citations already increased.
58865	56	18	56	54	Have you thought about mentioning the potential influence of high latitude fires on permafrost in this section? For example Holloway, JE, Lewkowicz, AG, Douglas, TA, et al. Impact of wildfire on permafrost landscapes: A review of recent advances and future prospects. Permafrost and Periglac Process. 2020; 1–12. https://doi-org.vu-nl.idm.oclc.org/10.1002/ppp.2048 [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted: Rewrote subsection taking into account this and other comments. A reference to this paper was included.
785	56	18	56	55	The whole subchapter is no up-to-date (following the large scale fires in the USA and Australia). There are numerouse new publications and input in carbon flux , carbon storage and carbon uptake following large scale fires. As examples: Song, X., Wang, G., Hu, Z., Ran, F. and Chen, X., 2018. Boreal forest soil CO2 and CH4 fluxes following fire and their responses to experimental warming and drying. Science of the total environment, 644, pp.862-872; Sheehan, T., Bachelet, D. and Ferschweiler, K., 2019. Fire, CO2, and climate effects on modeled vegetation and carbon dynamics in western Oregon and Washington. PloS one, 14(1); Hope, P., Black, M.T., Lim, E.P., Dowdy, A., Wang, G., Fawcett, R.J. and Pepler, A.S., 2019. On Determining the Impact of Increasing Atmospheric CO2 on the Record Fire Weather in Eastern Australia in February 2017. Bulletin of the American Meteorological Society, 100(1), pp.S111-S117; Todd-Brown, K.E., Baltzer, J.L. and Turetsky, M.R., 2019. Modeling interactions between fire behaviour and post-fire decomposition on boreal forest carbon stocks. AGUFM, 2019, pp.B23I-2542.. These are only samples from the literature. [Baruch Rinkevich, Israel]	Accepted: Rewrote subsection taking into account this and other comments.
131541	56	18	58	18	Chapter 5.4.3.2 does not take into account emissions through human disturbance and activities such as deforestation and bogland drainage. Is there any evidence on the effect of such activities? Please explain [Hans Poertner and WGII TSU, Germany]	accepted -- added some discussion of peatlands in section 5.4.3.3
106513	56	18			WGII ch2 "Terrestial and freshwater ecosystems" has sections on observed and projected changes in wildfire globally, as well as an FAQ on wildflire, that overlap with section5.4.3.2 . Care should be taken to ensure consistency between WGs in messages and uncertainty assessments of those messages. [camille parmesan, France]	Taken in to account. Care was taken to ensure consistency across WGs in assessments of uncertainty. Since the goal here was to quantify fire as a feedback rather than an impact, the level of uncertainty is higher here than in WG2.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
30619	56	20	56	21	Current citations are mainly focused on fire increases in the Arctic and Russia. Some citations to the fire projections in Boreal North America would be helpful (For example: Yue, X. et al., 2015. Impact of 2050 climate change on North American wildfire: consequences for ozone air quality. Atmospheric Chemistry and Physics 15, 10033-10055.) [Hong Liao, China]	Rejected. Subsection rewritten but did not include this citation, as length and number of citations already increased.
127833	56	20	56	23	Very strong evidence of projected future area burned in western North America using robust fine-scale modeling is provided in this paper, which should be cited: Kitzberger T, DA Falk, AL Westerling, and TW Swetnam. 2017. Direct and indirect climate controls predict heterogeneous early-mid 21st century wildfire burned area across western and boreal North America. PLoS One 12(12): e0188486. https://doi.org/10.1371/journal.pone.0188486 [Trigg Talley, United States of America]	Rejected. Subsection rewritten but did not include this citation, as length and number of citations already increased.
74353	56	20	56	26	In tropics region, peatland is very important to be protected from the occurrence of fire due to warmed climate. It is not only part as a groundwater in the complex ecosystem, but it also as a stock carbon, especially for depth greater than 3 meter [Yulizar Yulizar, Indonesia]	partially accepted -- added discussion pf peatland carbon feedbacks in 5.4.3.3
115377	56	20	56	33	Is Arora and Melton (2018) (already cited in the chapter) relevant here? [Gillett Nathan, Canada]	Rewrote subsection taking into account this and other comments.
98345	56	22	56	22	An additional reference for increasing fire with climate change in tropical ecosystems: P. M. Brando, B. Soares-Filho, L. Rodrigues, A. Assunção, D. Morton, D. Tuchschneider, E. C. M. Fernandes, M. N. Macedo, U. Oliveira, M. T. Coe Science Advances10 Jan 2020 : eaay1632 [Chantelle Burton, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. Subsection rewritten but did not include this citation, as length and number of citations already increased.
31985	56	22			"degraded tropical forest'. This makes no mention of the very widespread African tropical woodlands which are facultatively deciduous fire ecologies. Look at any satellite map of fires and you will see how important they are in global fire distribution. These are quasi natural fire ecologies shaped by half a million years of human-lit fires. Many may now be degraded but I've flown over vast tracts that are still functioning as they have for millennia. The C in the smoke plumes is isotopically light-ish (-30s per mil, not -15 grassfire) - this indicates leaf litter C3 fuel, not grass C4 burning. Likewise in Australia the spinifex fires are in land that has been modified by deliberate burning over tens of millennia by the native Australian people [Euan G. Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. This is greater detail than we had space for in this section.
77749	56	26	56	26	Further explanation of what these "fire management practices" refer to would be beneficial [Emer Griffin, Ireland]	Accepted: Rewrote subsection taking into account this and other comments.
67857	56	26	56	27	Is there any reason not to include fire disturbance on all of the CMIP6? Fire is one important factor that affect forest ecosystem, and the frequency and intensity are increasing. In 2019, wildfires have burned more than 11 million ha of forest in Australia, 0.9 million ha of Amazonian forest in Brazil, about 1,6 million ha of tropical forest in Indonesia. [Ruandha Agung Sugardiman, Indonesia]	Rejected. It is not possible for IPCC to add processes into ESMs, so while this is a good point, it is beyond the scope of this assessment report.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
7255	56	26	56	27	Are there any reasons to not include Fire disturbance on all of the CMIP6? Fire is one of the most disturbance factors to forest ecosystem, and the frequency and intensity are increasing. In the year 2019, wildfires have burned more than 11 million ha of forest in Australia, 0.9 million ha of Amazonian forest in Brazil, about 1,6 million ha of tropical forest in Indonesia. [Asaad Irawan, Indonesia]	Rejected. It is not possible for IPCC to add processes into ESMs, so while this is a good point, it is beyond the scope of this assessment report.
44143	56	27	56	29	Several vegetation models separate the source of ignition. A model based comparison of natural global fire occurrence finds a reduced burned area in temperate regions largely balanced by increases in burned area in tropical regions due to anthropogenic effects. There is higher confidence in that humans suppress fire in highly populated regions than in the effects of humans in remote areas. Lasslop, G., Kloster, S., 2017. Human impact on wildfires varies between regions and with vegetation productivity. Environ. Res. Lett. 12, 115011. https://doi.org/10.1088/1748-9326/aa8c82 [Gitta Lasslop, Germany]	Accepted: Rewrote subsection taking into account this and other comments. A reference to this paper was included.
44145	56	29	56	29	Teckentrup et al is not CMIP5. Suggest rewrite: In CMIP5 fire processes are included in some but not all models (Kloster and Lasslop 2017). These models did not yet include population density as direct human driver, but land use was an important factor explaining differences between the models (Kloster and Lasslop 2017). The CMIP6 models are partly updated to recent model versions similar to those used in the fire model intercomparison project (offline simulations with prescribed meteo forcing). With these models spatial patterns can be captured for present day (Teckentrup et al.2019, Hantson et al. 2020). The model ensemble median outperforms any individual model (Lasslop et al. 2020). Still the historical trends diverge and human drivers (land use change and population density) are the main reasons (Teckentrup et al. 2020). Climate did not induce clear trends in burned area over the 20th century but was a strong driver of interannual variability (Teckentrup et al. 2020). The longer satellite observation record indicates a stronger suppression due to human acting than currently included in the models (Andela et al. 2017). Hantson, S., Kelley, D.I., Arneth, A., Harrison, S.P., Archibald, S., Bachelet, D., Forrest, M., Hickler, T., Lasslop, G., Li, F., Mangeon, S., Melton, J.R., Nieradzik, L., Rabin, S.S., Prentice, I.C., Sheehan, T., Sitch, S., Teckentrup, L., Voulgarakis, A., Yue, C., 2020. Quantitative assessment of fire and vegetation properties in historical simulations with fire-enabled vegetation models from the Fire Model Intercomparison Project. Geosci. Model Dev. Discuss. 2020, 1–25. https://doi.org/10.5194/gmd-2019-261 (under review) Lasslop, G., Hantson, S., Harrison, S.P., Bachelet, D., Burton, C., Forkel, M., Forrest, M., Li, F., Melton, J.R., Yue, C., Archibald, S., Scheiter, S., Arneth, A., Hickler, T., Sitch, S., 2020. Global ecosystems and fire: multi-model assessment of	Accepted: Rewrote subsection taking into account this and other comments.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
30689	56	29	56	30	Also add reference to the analyses of Earl, N., and I. Simmonds, 2018: Spatial and temporal variability and trends in 2001-2016 global fire activity. Journal of Geophysical Research, 123, 2524-2536, doi: 10.1002/2017JD027749 AND Earl, N., and I. Simmonds (2017), Variability, trends, and drivers of regional fluctuations in Australian fire activity, J. Geophys. Res., 122, 7445-7460, doi: 10.1002/2016JD026312. [Ian Simmonds, Australia]	Rejected. Subsection rewritten but did not include this citation, as length and number of citations already increased.
8957	56	29	56	30	Observations from Andela et al. are based on the MCD64 product, which has shown to have high omission (70%) and commission errors (40%) in a recent validation paper conducted by Boschetti et al. (2019, RSE), with similar values in other independent validation studies (Chuvieco et al., 2018, ESSD). A significant part of this error comes from small fires (<100 ha), which are not well detected by course resolution sensors. Discrepancy of detections obtained from medium-resolution sensors may be as high as 80% of total burned area (Roteta et al, 2019, RSE). [Chuvieco Emilio, Spain]	Taken into account. More context for Andela's result and disagreement between models and observations was added in revised section.
44147	56	30	56	30	New study: The effects of fire on the vegetation distribution and terrestrial carbon cycle was estimated based on seven vegetation models (Lasslop et al. 2020). A clear (10%) and consistent reduction of tree cover and vegetation carbon storage was found. The impact is lower on total carbon storage and on vegetation productivity. The uncertainty is especially high for soil carbon as peatlands and permafrost processes were not represented in the models. The impact is highest in the savanna regions especially on the African continent. Lasslop, G., Hantson, S., Harrison, S.P., Bachelet, D., Burton, C., Forkel, M., Forrest, M., Li, F., Melton, J.R., Yue, C., Archibald, S., Scheiter, S., Arneth, A., Hickler, T., Sitch, S., 2020. Global ecosystems and fire: multi-model assessment of fire-induced tree cover and carbon storage reduction. Glob. Chang. Biol. gcb.15160. https://doi.org/10.1111/gcb.15160 [Gitta Lasslop, Germany]	Accepted: Rewrote subsection taking into account this and other comments. A reference to this paper was included.
15963	56	30	56	32	The statement "There is limited evidence and low confidence for a positive feedback mechanism between fire emissions and climate change" does not reflect observational evidence with wild fires spanning the boreal forests circling the Arctic circle and also wide-scale fires in the tropical regions. It is also of note that the three references cited are all from papers published before the wide scale fires of recent years. [Kevin Lister, United Kingdom (of Great Britain and Northern Ireland)]	Accepted: Rewrote subsection taking into account this and other comments.
44149	56	33	56	33	Lasslop et al. 2019 reviews the effect of fire on carbon cycle and climate. Most atmospheric modelling studies show a negative radiative forcing due to fires. One that includes also domestic biomass burning shows a positive radiative forcing. the negative radiative forcing is mainly caused by the indirect aerosol effects. [Gitta Lasslop, Germany]	Rejected. Since the focus of this section is on the carbon feedbacks associated with fire, not mention of aerosol radiative effects provided here.
132099	56	35	56	54	On carbon cycle feedbacks of changing disturbance regimes in forests pls consider Seidl R, Spies TA, Peterson DL, Stephens SL & Hicke, JA 2016: Searching for resilience: addressing the impacts of changing disturbance regimes on forest ecosystem services. Journal of Applied Ecology 2016, 53, 120–129. [Mikulas Cernota, Slovakia]	Partially accepted: WE have expanded the discussion of disturbance and climate extremes a bit, but with a more specific focus on tree mortality drivers than the suggested reference.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
29187	56	35	56	54	This paragraph ignores the growing high risk of climate-induced fire. See Brando et al. 2020. Sci. Adv. 2020; 6 : eaay1632 10 January 2020. It was submitted and accepted in 2019. [Eric Davidson, United States of America]	Take into account. We did not add this reference for space reasons, however we do address the climate effect on wildfire.
44151	56	38	56	38	move Lasslop et al. 2016 to tropical, low potential for multistability is found in temperate and boreal regions [Gitta Lasslop, Germany]	Accepted: Rewrote subsection taking into account this and other comments. A reference to this paper was included.
70825	56	43	56	54	the role of land-use induced changes in C turnover-times, on top of the variability of turnover-time, is discussed also here: Erb et al., 2016 10.1038/ngeo2782, also pointing to a systemic effect between land-use intensity and accelerated turnover time in vegetation. [Karlheinz Erb, Austria]	Rejected: this section is on climate drivers rather than land use
31987	56	43			Is it worth mentioning the extremely rapid deforestation of moist tropical woodlands in countries like Zambia, where extraordinarily rapid cutting is taking place on a vast scale to burn for charcoal for the DRC? [Euan G. Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Rejected: this section is on climate drivers rather than land use
13437	56	51	56	51	It is suggested to use parethesis instead commas in phrase “like the CMIP5 models”, to avoid confusion while reading. [Maria Amparo Martinez Arroyo, Mexico]	Partially accepted -- changed wording to clarify
39881	56	52	56	52	"unlikely" should be italic [TSU WGI, France]	Accepted - change was made.
116445	56		56		Reference to SRCCL and SROCC for disturbance and fire would be relevant (as a starting point). X chapter coordination on fire is needed. [Valerie Masson-Delmotte, France]	accepted -- now starting with SRCCL assessment
77751	57	1	57	53	The role of farming practices, mineral fertilisation, the loss of SOM/humus the impact of changing weather systems on soil biology etc are all worthy of inclusion. These may be addressed in WGIII report. This is important for policy the non specialist about the many factors which affect soils ability to absorb and retain carbon. [Emer Griffin, Ireland]	Rejected. Discussion of land use (which includes farming practices) as a carbon driver is handled elsewhere in the AR6.
37949	57	1	58	18	In this chapter, recent finding from Barrow, Alaska (Jeong et al. 2018) should be included. Jeong et al shows decreasing carbon residence time in permafrost regions by having data-model fusion. This is an important piece of carbon dynamics changes related to climate change. But, it was not seriously addressed. Jeong, S.J, A. A. Bloom, D. Schimel, C. Sweeney, N. C. Parazoo, D. Medvigy, G. Schaepman-Strub, C. Zheng, C. R. Schwalm, D. N. Huntzinger, A. M. Michalak, C. E. Miller, 2018, Accelerating rates of Arctic carbon cycling revealed by long-term atmospheric CO2 measurements, Science Advances, 4, eaao1167. In this chapter, recent finding from Barrow, Alaska (Jeong et al. 2018) should be included. Jeong et al shows decreasing carbon residence time in permafrost regions by having data-model fusion. This is an important piece of carbon dynamics changes related to climate change. But, it was not seriously addressed. [Junhee Lee, Republic of Korea]	Rejected. Interpreting this result would have required more detail than we had space for in this section. The reason for this is that the result in the suggested paper is ambiguous as to whether or not permafrost carbon is being released due to climate change, because residence times will also decrease simply as a result of increased productivity, e.g. due to elevated CO2, even if no carbon is released from permafrost -- see e.g. Koven et al 2015 https://doi.org/10.5194/bg-12-5211-2015. It is therefore not an unambiguous metric of permafrost carbon release.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
28305	57	1	58	18	Compared to Sect. 5.4.1 about increasing plant uptake, Sect. 5.4.33 remains quite cryptic about the possible net effects of soil carbon emissions. While it is important to note that research on this complex topic has moved forward from simpler assumptions as mentioned at p57L5, readers need to know whether according to our current knowledge this positive feedback is still a valid first-order assumption that has been refined since, or whether it has been overthrown altogether. The undisputed progressive dependence of soil respiration to temperature, as well as the continuous decrease of the global mean airborne fraction of emitted CO2 determined by independent methods, seem to suggest to me that the former rather than the latter is the case. The same seems to be suggested by Fig. 5.10 (where increase of the net land sink seems to slowly flatten out although NPP is linearly increasing) and Fig. 5.12 (where there is an athropogenic positive component to "total respiration and fire"). Such information (or the contrary if it is the authors' conclusion) is hard to find in the current text. One factor contributing to the somewhat confusing impression is talking in the middle of this section about increasing soil carbon stocks as a (trivial) result of the higher plant uptake already discussed in Sect. 5.4.1. Maybe it would be helpful to draw a clearer line (e.g. by subsectioning or moving things to other sections) between the temporal development of soil carbon stocks, which are a net result of net fluxes like plant uptake and soil respiration, and on the other hand soil emissions, which may increase even though (and partly because) stocks increase. [Alexander Graf, Germany]	Taken into account. The separation of changes into productivity versus turnover is quite difficult in practice, (e.g. Koven et al., Biogeoscinces, 2015), and we already note this point in the text.
83013	57	1	58	19	Needs to include peat soils and their carbon storage. Plenty of research on this topic in the literature. [Dan Zwartz, New Zealand]	accepted -- added some discussion of peatlands in section 5.4.3.3
16065	57	3	57	4	Is really all soil carbon eventually cycled back to the atmosphere? No conversion to the geological reservoir at all? [Gerhard Krinner, France]	Rejected. We don't have good estimates of this rate.
99681	57	3	57	12	No reference to the lack of representation in the models associated to carbon losses linked to increased coastal erosion in the Arctic, as shown in various studies, eg. in the Beaufort Sea Coast? [Goncalo Vieira, Portugal]	Rejected. This is greater detail than we had space for in this section.
89475	57	5	57	5	This sentence should say AR5 and SOCCR since the latter special report covers the latest material on high latitude soils in detail. There could also be a reference to Schuur et al. 2015 (already in reference list) since the other topics in this paragraph have citations (Cox, Schmidt, Luo) [Edward Schuur, United States of America]	Accepted. See extensive edits and addition of permafrost box.
26909	57	8	57	12	2) is really well written, but there is still some work to be done to consider the soil as a whole. It's not just the first 30 centimetres that interact with the atmosphere. So we suggest to add a point : 3) an effort to consider the soil as a whole and not for its first 30 centimetres has been initiated but still needs to be intensified (Balesdent et al. 2018) Balesdent J., Basile-Doelsch I., Chadoeuf J., Cornu S., Derrien D., Fekiacova Z., Hatté C. (2018) Atmosphere-soil carbon transfer as a function of soil depth. Nature 559, 599-602. [Eric Brun, France]	accepted, added a point to this effect
58929	57	9	57	14	Very difficult to read, particularly the (2) point. I suggest breaking into two sentences. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Partially Accepted. Changed "to" to "and towards" in point 2.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
26911	57	12	57	12	Kléber et al. 2007 that so nicely described organo-mineral association is here missing : Kleber M., Sollins P., Sutton R. (2007) A conceptual model of organo-mineral interactions in soils: self-assembly of organic molecular fragments into zonal structures on mineral surfaces. Biogeochemistry 85, 9-24. [Eric Brun, France]	Accepted
15965	57	14	57	32	The paragraph claims that the feedback sensitivity of soil carbon release to rising temperatures may not be as strong as previously believed. However, it does not seem that the assessment of fire related feedback includes the effects of peat fires which can be underground and persistent and liable to early flare ups in the spring time. Given the huge quantity of carbon that is stored in peat, and the wide areas covered by the peat in the Northern hemisphere, clarity on this is needed. [Kevin Lister, United Kingdom (of Great Britain and Northern Ireland)]	partially accepted -- added discussion pf peatland carbon feedbacks in 5.4.3.3
89477	57	14	57	32	This final sentence of the paragraph could clarify whether the numbers in that sentence is CMIP5 analysis as implied by the content of the rest of the paragraph. Not sure by the fact that the last sentence references a newly submitted paper; maybe a re-analysis? Also noted that none of this paragraph has uncertainty language [Edward Schuur, United States of America]	Accepted -- added CMIP6 to this to be clear what is being used.
58857	57	32	57	32	There is a modelled carbon loss estimation also in Burke, E. J., Zhang, Y., and Krinner, G.: Evaluating permafrost physics in the CMIP6 models and their sensitivity to climate change, The Cryosphere Discuss., https://doi.org/10.5194/tc-2019-309, in review, 2020. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected. Final version of this paper does not include carbon estimates.
99679	57	34	57	34	correct: "two ESMs includes permafrost" to "two ESMs include permafrost" [Goncalo Vieira, Portugal]	Accepted - change was made.
78505	57	34	57	34	which two ESMs? Table 5.4 only lists one. [Chris Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted -- text is clarified in permafrost box.
73167	57	34	57	34	Change 'includes' to 'include'. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change was made.
8845	57	34	57	37	includes should be include. It is not clear if all these references refer to the finding from the two ESMs. [Vaishali Naik, United States of America]	Accepted -- text is clarified in permafrost box.
52791	57	34	57	39	It is unclear if the CMIP6 models discussed in this text assumed that the soil carbon stock in their permafrost models was at equilibrium at the beginning of the simulations, and if not, then how it was initialized. Indeeed, if the overturning time scale of carbon in the permafrost stock is on the order of millenia, it is unlikely that the carbon stock in nature is in equilibrium (since climate changes naturally on this time scale, e.g. LGM ended only around 15kyr ago). It is not clear how the models (or other estimates) take that uncertainty into account. [Sergey Malyshev, United States of America]	Rejected. Too much detail to get into for this, especially as permafrost carbon is not included in most ESMs and permafrost text has been moved to separate box.
127835	57	34	57	39	It is unclear if the CMIP6 models discussed in this text assumed that the soil carbon stock in their permafrost models was at equilibrium at the beginning of the simulations, and, if not, then how it was initialized. Indeed, if the overturning time scale of carbon in the permafrost stock is on the order of millennia, it is unlikely that the carbon stock in nature is in equilibrium (since climate changes naturally on this time scale, e.g., LGM ended only around 15kyr ago). It is not clear how the models (or other estimates) take that uncertainty into account. [Trigg Talley, United States of America]	Rejected. Too much detail to get into for this, especially as permafrost carbon is not included in most ESMs and permafrost text has been moved to separate box.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
16071	57	34	57	52	The "permafrost carbon" feedback actually also refers to soil at depths that is frozen during the largest part of the year, but that is technically not permafrost, and it can be carbon-rich. These parts of the active layer in permafrost regions will also be unfrozen during larger parts of the year in a warming climate, and thus be sources of CO2 and/or CH4 emissions. So it's not only the permafrost thaw in the strict sense that leads to this feedback. [Gerhard Krinner, France]	Accepted -- text is clarified in permafrost box.
54991	57	34	57	53	Due to the clear acknowledgement of missing processes in model representations of permafrost carbon thaw and emission, the numerical value reported with low confidence (20±13 PgC/°C) is certainly too low. Could this estimate be supplemented with the statement from the Nature review paper Schuur et al. (2015, doi:10.1038/nature14338) which states that "5-15% of permafrost carbon is vulnerable to thaw", "with 10% being equivalent to ~130-160 PgC". [Nancy Hamzawi, Canada]	Partially accepted. We are reconciling estimates of Schuur et al 2015, and SROCC, to better account for this uncertainty.
58891	57	34	58	8	Much of projected future permafrost carbon emissions is predicted to be absorbed by high-affinity methanotrophic bacteria (see Oh et al. 2020, Nat Clim Change, https://doi.org/10.1038/s41558-020-0734-z). In addtion, these bacteria will become more active with a warming climate. Do the CMIP6 ESMs that account for permafrost carbon cycle dynamics also account for bacterial carbon uptake in thawing soils? If not, it may be worth including that more specific disclaimer here, in the paragraph (page 58, lines 1-8) on soil microbial dynamics, because this is a specific "complex longer-term trophic effect" that is relatively well-studied. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected. This is greater detail than we had space for in this section.
617	57	34			include, not includes [Vicca Sara, Belgium]	Accepted - change was made.
96589	57	38	57	38	The SROCC stated in its SPM A1.3"Arctic and boreal permafrost contain 1460–1600 Gt organic carbon, almost twice the carbon in the atmosphere (medium confidence)." What the reason for reducing this figure to 1300 PgC in the AR6? [Nicole Wilke, Germany]	Accepted, reusing SROCC statements in permafrost box, with updates.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
89479	57	38	57	38	This stock number has been updated. It is partly based on Hugelius 2014, which is still a good reference to include but others shold be added. This reference has been reconciled with some other references such that the new stock number has been reported in a several newer publications (as well as SROCC) and so should probably be updated to be consistent. The citations with the newest stock numbers report a range for the mean value of 1440 to 1600 Pg C. The full uncertainty of the top 3 m reported in Hugelius is plus/minus 15%. New stock number citation: Meredith, M., M. Sommerkorn, S. Cassota, C. Derksen, A. Ekaykin, A. Hollowed, G. Kofinas, A. Mackintosh, J. Melbourne-Thomas, M.M.C. Muelbert, G Ottersen, H. Pritchard, E.A.G. Schuur, P. Boyd, W. Hobbs. 2019. Polar Regions. In: IPCC Special Report on the Ocean and Cryosphere in a Changing Climate [H.-O. Pörtner, D.C. Roberts, V. Masson-Delmotte, P. Zhai, M. Tignor, E. Poloczanska, K. Mintenbeck, A. Alegría, M. Nicolai, A. Okem, J. Petzold, B. Rama, N.M. Weyer (eds.)]; and/or Schuur, E. A. G., A. D. McGuire, V. Romanovsky, C. Schädel, and M. Mack, 2018: Chapter 11: Arctic and boreal carbon. In Second State of the Carbon Cycle Report (SOCCR2): A Sustained Assessment Report [Cavallaro, N., G. Shrestha, R. Birdsey, M. A. Mayes, R. G. Najjar, S. C. Reed, P. Romero-Lankao, and Z. Zhu (eds.)]. U.S. Global Change Research Program, Washington, DC, USA, pp. 428-468, https://doi.org/10.7930/ SOCCR2.2018.Ch11; with the peer reviewed reconciliation in Schuur E.A.G., A.D. McGuire, G. Grosse, J.W. Harden, D.J. Hayes, G. Hugelius, C.D, Koven, P. Kuhry, D.M. Lawrence, S.M. Natali, D. Olefeldt, V.E. Romanovsky, C. Schädel, K. Schaefer, M. Turetsky, C. Treat, and J.E. Vonk. 2015. Climate change and the permafrost carbon feedback. Nature 520, 171–179. (the full reconciliation appear for the first time finalized in the 2018 citation). [Edward Schuur, United States of America]	Accepted, reusing SROCC statements in permafrost box, with updates.
41655	57	39	57	40	Moreover, more recent literature should be added, e.g. Voigt et al. 2017 GCB 23:3132-3138, doi: 10.1111/gcb.13563. This reference also includes information about all three important greenhouse gases - -CO2, CH4, and N2O [Katharina Meurer, Sweden]	Partially accepted. We added one sentence on N2O emisisons in permafrost box. "Since AR5, there have been new studies showing that permafrost thaw also leads to N2O release from soil (Abbott and Jones, 2015; Karelin et al., 2017; Wilkerson et al., 2019), a previously unaccounted source. However, this release is unquantified at the pan-Arctic scale."
89481	57	40	57	40	This citation specifically helps support the text of this sentence in addition to the one listed: Mauritz, M., Bracho, R.G., Salmon, V.G., Webb, E., Hutchings, J.A., Natali, S., Crummer, K.G., E.A.G. Schuur, Schaedel, C. 2017. Nonlinear CO2 flux response to 7 years of experimentally induced permafrost thaw. Global Change Biology 2017. 23:3646–3666. https://doi.org/10.1111/gcb.13661 [Edward Schuur, United States of America]	rejected -- reorganized test into permafrost box
89483	57	42	57	42	This sentence should also cite McGuire 2018 PNAS since the 2016 paper is historical modeling and the 2018 is the future modeling [Edward Schuur, United States of America]	Accepted, reusing SROCC statements in permafrost box, with updates.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
89485	57	42	57	44	It's worth looking closely at the ES statement from Chapter 3 SROCC on this to see how we assigned confidence to this: Widespread disappearance of Arctic near-surface permafrost is projected to occur this century as a result of warming (very high confidence), with important consequences for global climate. By 2100, near-surface permafrost area will decrease by 2–66% for RCP2.6 and 30–99% for RCP8.5. This is projected to release 10s to 100s of billions of tons (Gt C), up to as much as 240 Gt C, of permafrost carbon as carbon dioxide and methane to the atmosphere with the potential to accelerate climate change. Methane will contribute a small proportion of these additional carbon emissions, on the order of 0.01–0.06 Gt CH4 yr–1, but could contribute 40–70% of the total permafrost-affected radiative forcing because of its higher warming potential. There is medium evidence but with low agreement whether the level and timing of increased plant growth and replenishment of soil will compensate these permafrost carbon losses. {3.4.2; 3.4.3}. This is especially important in light of Page 57, Line 50 statement on emissions (and associated confidence) [Edward Schuur, United States of America]	Accepted, reusing SROCC statements in permafrost box, with updates.
26913	57	46	57	46	is not section 5.6 but section 5.5 [Eric Brun, France]	Accepted - change was made.
88193	57	46	57	48	Isn't it the permafrost dynamics that are not well represented in these models (i.e. warming and thaw of permafrost)? Ice-wedge degradation is a thermokarst process. What you appear to be getting at here is that there are landscape changes that are associated with permafrost thaw (or thermokarst processes) that may enhance thawing of the ground with subsequent impact on carbon release (note amount of carbon release will depend on subsurface conditions and carbon content of underlying materials). [Sharon Smith, Canada]	Accepted -- more details on ice-wedge and other thermokarst processes included in permafrost box.
4241	57	46	57	49	I propose to add here that recent observations indicate widespread occurrerence of ice wedge thaw and other abrupt thaw processes such as landslides: see Liljedahl AK, Boike J, Daanen RP, Fedorov AN, Frost GV, Grosse G, Hinzman LD, Iijma Y, Jorgenson JC, Matveyeva N, Necsoiu M, Raynolds MK, Romanovsky VE, Schulla J, Tape KD, Walker DA, Wilson CJ, Yabuki H, Zona D (2016) Pan-Arctic ice-wedge degradation in warming permafrost and its influence on tundra hydrology. Nature Geoscience 9 (4):312-318. doi:10.1038/ngeo2674 and Lewkowicz AG, Way RG (2019) Extremes of summer climate trigger thousands of thermokarst landslides in a High Arctic environment. Nature Communications 10 (1):1329. doi:10.1038/s41467-019-09314-7 [Jacobus (Ko) van Huissteden, Netherlands]	accepted -- added citation to this in permafrost box
16067	57	46	57	49	Chapter 9(section 9.5) could be referred to here [Gerhard Krinner, France]	accepted -- reference made in permafrost box

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58863	57	46	57	49	According to this paragraph, only two ESMs include permafrost carbon cycle dynamics (pg 57, line 34). If this is true, it seems odd to say "most models that do include permafrost carbon dynamics still do not consider..." and "those that do…". If there are only two models, there cannot be a "most," and it would make to more sense to say that "only one of the two models considered fine-scale processes...". If this statement refers to models outside of ESMs, this broader reference could be made more clear. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted -- text clarified in permafrost box.
18225	57	47	57	47	Perhaps some further but brief explanations of thermokarst and ice-wedge polygon degradation would be useful? These processes are not common knowledge and as the IPCC is for a wide audience either more explanation or removing the terms would be advised. [Chelsey Baker, United Kingdom (of Great Britain and Northern Ireland)]	Accepted -- more details on ice-wedge and other thermokarst processes included in permafrost box.
98487	57	47	57	47	It may be useful to mention here that models also do not include estimates for ground subsidence, or for lateral loss of DOC and POC carried to headwater stream and offgassed there. I do see discussion of POC a few pages later (related to oceans), but it's particularly important in headwater streams draining permafrost landscapes. [Emily Romano, United States of America]	Accepted -- addition of this to permafrost box.
74271	57	48	57	48	maybe it would be a good idea to include a statement over which timescales this happens [Christoph Völker, Germany]	Accepted -- these processes now separated into pre-2100 and post-2100 in permafrost box.
73169	57	48	57	48	Date of Nitzbon reference missing. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. This reference was deleted in final version for space reasons.
89487	57	50	57	50	It is not clear to me where this number is coming from since there are no citations here; hard to tell which of the above work is being referenced. It is CMIP6? Or one/some of the cited literature above? Also - the use of uncertainly language in this sentence is confusing. I'm not sure if you need a low confidence if you present a standard error, in that case you can use likely with a range of values. If you are saying the low confidence comes out of the model structure, you have sort of already said that in the preceding sentence. [Edward Schuur, United States of America]	Accepted, coordinating feedback estimates with SROCC assessment and tracing text throughout the assessment via permafrost box.
89489	57	55	58	18	Noted that these two paragraphs have no uncertainty language in contrast to the preceding ones. [Edward Schuur, United States of America]	Taken into account. Rewriting much of this into permafrost box.
33345	58	3			Erase space at the end of the line. [Guiomar Rotllant, Spain]	Rejected. Between "separate" and "decomposition" there is only one space, which is expected. The fact that "decomposition" goes to the next line is because of the layout of the Word document.
40717	58	21	61	13	section 5.4.4: this section is a bit hard to follow (I got lost). Could you emphasize more what the key-message of the section is? [TSU WGI, France]	Taken into account - this section has been largely rewritten so is hopefully clearer
38477	58	25	58	26	This sentence is a bit confusing. The decrease of 20-44 PgC ocean uptake is relative to the scenario without consideration of climate change. [LONG CAO, China]	Not applicable - this section has been extensively rewritten and these sentences removed as a result

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787	58	25	58	50	The topic was discussed in SROCC. Cite the major outcomes from SROCC and add newly published literature (2019-2020) or aspects not discussed in SROCC. [Baruch Rinkevich, Israel]	Taken into account - the text has been updated with CMIP6 results. SROCC didn't directly address climate-carbon cycle feedback in the ocean (as far as I can find)
131543	58	29	58	29	define "buoyancy fluxes" [Hans Poertner and WGII TSU, Germany]	Not applicable - sentence has been rewritten
52337	58	31	58	46	Suggest adding information on storm events and effect on air-sea CO2 exchange in the Arctic: In the Arctic Ocean, storm events in winter and spring cause open leads in the sea-ice cover, which have shown to increase the potential for ocean-atmosphere CO2 exchange, with mainly ocean uptake of CO2 in the Nansen Basin, Eurasian Arctic, even though storms induce increased mixing of CO2-rich sub-surface water to the surface (Fransson et al., 2017). [Agneta Fransson, Norway]	Taken into account - sentence has been added
36377	58	35	58	36	Others predict that intensification of winds that accelerate the overturning circulation would also increase outgassing of CO2, reducing the net CO2 sink in the Southern Ocean (Le Quéré et al., 2007). Over the next century models also predict reductions in sea-ice cover and surface ocean warming, freshening, and stratification will also impact the CO2 sink. How these processes impact the overall balance of CO2 outgassing and uptake in the Southern Ocean is highly uncertain. Reference: Le Quéré, C., Rödenbeck, C., Buitenhuis, E.T., Conway, T.J., Langenfelds, R., Gomez, A., Labuschagne, C., Ramonet, M., Nakazawa, T., Metzl, N., Gillett, N., Heimann, M. (2007) Saturation of the Southern Ocean CO2 Sink Due to Recent Climate Change. 316, 1735-1738. [Adrienne Sutton, United States of America]	Taken into account - sentence has been added
58893	58	36	58	41	The summary of results from Yamamoto et al. (2018) misses a key conclusion of that study: the decreased CO2 uptake in high latitudes and increased CO2 uptake in low latitudes cancel each other out, leaving only downstream effects of ocean circulation (e.g. its effects on nutrient transport and biological process) to affect oceanic CO2 uptake. I'd encourage modifying the last sentence here that acknowledges this (important) clarification: "the warming of the ocean, alongside changes in oceanic nutrient transport and biological processes due to changes in oceanic circulation in a high CO2 world, also explain...." [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account - sentence has been modified
87693	58	43	58	50	I believe this paragraph does not consider the potential effect of changing seas-ice on the air-sea equilibration of CO2. Should it not be included? [Ivy Frenger, Germany]	Taken into account - sentence has been added
69091	58	44	58	46	Brine rejection during sea ice formation, not sea ice growth, would increase DIC and TA. [Hidetaka Kobayashi, Japan]	Accepted - change has been made
88975	58	45	58	45	Not "sea-ice melt" but "sea-ice formation"? [AKIHIKO MURATA, Japan]	Accepted - change has been made
52251	58	45	58	46	change "Søren et al. (2011)" to "Rysgaard et al. (2007; 2009; 2012)" also add "Fransson et al. (2011; 2013) [Agneta Fransson, Norway]	Taken into account - the Rysgaard reference has been changed and one of the Fransson references added
42813	58	46	58	46	Is this refereing to "Rysgaard et al., 2011"? Double check! [Melissa Chierici, Norway]	Accepted - text revised

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42815	58	46	58	48	Add" The formation of ikaite in the brine, releaases CO2 ot the brine, which may be transported upward and outgass CO2 to the atmosphere or be transported by gravity to underlying water. At onset of sea ice melt part of the ikaite particles may be transpoorted to the water column and dissolve. During dissolution CO2 is consumed, hence promoting CO2 uptake from the environment (inlc the atmosphere) Fransson et al., 2013; 2015." [Melissa Chierici, Norway]	Taken into account - a reference to the inorganic brine pump has been added
52253	58	48	58	48	suggest adding "so-called inorganic carbon brine pump" (Rysgaard et al., 2007; 2009; Fransson et al. 2011; 2013)" [Agneta Fransson, Norway]	Noted - but could not include due to lack of space
52339	58	49	58	50	In the driver section, suggest adding information on "increased meltwater from sea ice and glaciers will increase the potential for ocean CO2 uptake (Fransson et al. 2009; 2015). Moreover, the bedrock-derived source of carbonate minerals in the meltwater will add to the buffering capcity and increase the potential for ocean CO2 uptake (Fransson et al. 2015) " [Agneta Fransson, Norway]	Taken into account - sentence added
87703	58	53	58	53	I suggest to state somewhere clearly that increased stratification slows circulation/increases water mass ages, hence respired carbon stays longer in the ocean before it outgasses, allowing respired carbon to accumulate and to counteract (to some extent) a potentially decreasing PP; I am not sure if you want to attribute this to biology 5.4.4.2 or physics 5.4.4.1. (or do you mention this point and I have missed it? In this case, please make it clearer as I think it is an important point) [Ivy Frenger, Germany]	Taken into account - this section has been extensively rewritten and this point is now included
18227	58	53	61	13	Section 5.4.4.2 - there is no mention of zooplankton in this section. Changes in PP, temperature and phytoplankton community structure will affect zooplankton which may alter the effiency of the BCP i.e. through changes in fecal pellet production, grazing, fragmentation etc. Some discussion of zooplankton is warranted even if, for now, the conclusions have very low confidence or confounding results. [Chelsey Baker, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - information added
83827	58	53	61	13	The title of this section is “biological drivers of ocean carbon uptake”, however, it only refers the biological carbon pump and the particulate organic carbon. There is also a well-known carbon sequestration mechanism, termed as the microbial carbon pump (Jiao et al., 2010, Nature reviews microbiology), regarding the dissolved phase of organic carbon, which accounts for ~95% of carbon in the oceanic organic carbon pool. Increasing filed and laboratory studies emphasize the roles of microbial carbon pump in the origin and maintenance of the tremendous dissolved organic carbon pool (~700 Pg Carbon). Global flux of the sequestrated carbon via the microbial carbon pump (180~242 Tg C yr-1) has also been modelled and calculated (Legendre et al, 2015, Progress in Oceanography; Walker et al, 2016, Nature Geoscience), which is on the same order of magnitude of the biological carbon pump (300~720 Tg C yr-1 at the depth of 2000 m, Guidi et al., 2015, Global Biogeochemical Cycles; Boyd et al., 2019, Nature). Therefore, assessing the microbial carbon pump and relative flux of dissolved organic carbon is strongly recommended. [Nianzhi Jiao, China]	Accepted - this section has been extensively rewritten and reference to DOC and the microbial carbon pump is now included

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
87695	58	53	61	13	Section 5.4.4.2: This section appears very long to me (also compared to section 5.4.4.1 before that discusses physical/circulation effects), given that biological changes are not expected to play a big role for atmospheric CO2 in the 21 century. It may be misleading for a general audience. At least from my perspective I suggest a shortening. [Ivy Frenger, Germany]	Taken into account - this section has been extensively rewritten. Section 5.4.4.2 remains longer however as the biological feedbacks to climate is where the majority of the uncertainty lies
58895	58	55	59	2	It is unclear if the "net ocean uptake of atmospheric CO2" is a predicted future condition, based on increasing oceanic primary production driving decreased surface ocean pCO2, or a current statement of year-to-year variation depending on the amount of oceanic productivity. This could be clarified by adding a statement about timescales. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable - this section has been extensively rewritten and these sentences removed as a result
100545	58	55	59	7	Phytoplankton blooms of the coccolithophore Emiliania huxleyi are known to produce CO2, causing less uptake of atmospheric CO2 by the ocean, but a global assessment of this phenomenon has so far not been quantified. A quantification of the increase in CO2 partial pressure (ΔpCO2) at the ocean surface within E. huxleyi blooms for polar and subpolar seas has been made in [Kondrik, D. V., Pozdnyakov, D. V., and Johannessen, O. M., 2018. Satellite evidence that E. huxleyi phytoplankton blooms weaken marine carbon sinks, Geophysical Research Letters, 5, 846–854. doi: 10.1002/2017GL076240] using an 18 year ocean color time series (1998–2015) [Kondrik, D., Kazakov, E., and Pozdnyakov, D., 2019. A synthetic satellite dataset of the spatio-temporal distributions of Emiliania huxleyi blooms and their impacts on Arctic and sub-Arctic marine environments (1998–2016), Earth Syst. Sci. Data, 11, 119–128. doi: 10.5194/essd-11-119-2019]. An ensemble of climate models that most accurately reproduces the state of environmental variables that influence the coccolithophore E. huxleyi bloom over the historical period was selected in [Gnatiuk, N., Radchenko, I., Davy, R., Morozov, E., & Bobylev, L., 2020. Simulation of factors affecting Emiliania huxleyi blooms in Arctic and sub-Arctic seas by CMIP5 climate models: model validation and selection. Biogeosciences, 17(4), 1199-1212, https://doi.org/10.5194/bg-17-1199-2020] by validation of CMIP5 models versus reanalysis data. [Dmitry Kovalevsky, Germany]	Noted - unfortunately space constraints meant we couldn't discuss in this section, but see section 5.4.2
109661	58	55	59	8	In recent years there has been a rising discussion to not only include soil microbial dynamics in carbon models, but also soil fauna dynamics. I think, this aspect should be added to this paragraph. See e.g. Filser, J., Faber, J. H., Tiunov, A. V., Brussaard, L., Frouz, J., De Deyn, G., Uvarov, A. V., Berg, M. P., Lavelle, P., Loreau, M., Wall, D. H., Querner, P., Eijsackers, H., and Jiménez, J. J.: Soil fauna: key to new carbon models, SOIL, 2, 565–582, https://doi.org/10.5194/soil-2-565-2016, 2016. [Carolyn-Monika Görres, Germany]	Accepted - a sentence has been added to section 5.4.3 citing this paper
789	58	55	61	13	The topic was discussed in SROCC. Cite the major outcomes from SROCC and add newly published literature (2019-2020) or aspects not discussed in SROCC. Much of the content has been discussed recently in SROCC. [Baruch Rinkevich, Israel]	Rejected - the feedbacks between climate and biological and physical factors was not covered in SROCC
63615	58	55	61	13	Paragraphs in this section are far too long [Galen McKinley, United States of America]	Taken into account - this section has been extensively rewritten

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
116449	58		58		Low confidence is related to model skills for Antarctic sea ice and apsects related to the southern ocean (ch 3, ch 4), please integrate this in the assessment of physical drivers of ocean carbon uptake. [Valerie Masson-Delmotte, France]	Accepted. Added a sentence to section 5.4.4.1 on the model uncertainty, with reference to the relevant sections of Chapters 3 and 9
30575	59	1	59	1	An updated estimate of of the soft tissue and hard tissue biological pump has been submitted for publication in GBC. The reference is: Carter, B. R., Feely, R. A., Lauvset, S. K., Olsen, A., DeVries, T, Sonnerup, R., (submitted) Preformed properties for organic matter and carbonate mineral cycling quantification, Global Biogeochemical Cycles. [nina bednarsek, United States of America]	Rejected - this section has been extensively rewritten and this manuscript is no longer relevant
36389	59	4	59	7	An updated estimate of of the soft tissue and hard tissue biological pump has been submitted for publication in GBC. The reference is: Carter, B. R., Feely, R. A., Lauvset, S. K., Olsen, A., DeVries, T, Sonnerup, R., (submitted) Preformed properties for organic matter and carbonate mineral cycling quantification, Global Biogeochemical Cycles. [Adrienne Sutton, United States of America]	Rejected - this section has been extensively rewritten and this manuscript is no longer relevant
74273	59	6	59	7	I have been trying to find a good source for the number of 3000 PgC, which is also quoted in Galbraith and Skinner 2020; they cite DeVries and Weber (2017) for it, but I haven't found the number in that paper. [Christoph Völker, Germany]	Not applicable - this section has been extensively rewritten
83829	59	6	59	7	3000 PgC is a huge number, I am quite confused what is the time scale related to this number? Giving such an ambiguous number is less confident. In addition, the citation here, i.e. Parekh et al., 2006, may not represent the magnitude of carbon sequestration via the biological carbon pump. [Nianzhi Jiao, China]	Not applicable - this section has been extensively rewritten
87697	59	6	59	7	"The biological carbon pump currently stores on the order of 3000 PgC, lowering atmospheric CO2 by hundreds of ppm (Parekh et al., 2006)." -> As far as I know these numbers are not that clear; could you provide a range and some more references? [Ivy Frenger, Germany]	Not applicable - this section has been extensively rewritten
7335	59	6	59	7	specify: atmospheric CO2 would be almost 200 ppm higher without the BCP. Cite Knox and MacElroy 1984, Sarmiento and Toggweiler 1984 and Toggweiler 1999 from Parekh et al 2006 [Svenja Halfter, Australia]	Not applicable - this section has been extensively rewritten
37941	59	6			In chapter 5.4.4, the author introduces two main drivers of ocean carbon uptake. Could you add the information about the amount of physical ocean carbon uptake to compare with the amount of biological uptake? [Junhee Lee, Republic of Korea]	Noted - the full carbon cycle is shown in Figure 5.12
87699	59	9	59	9	"Efficiency" -> please define bcp efficiency before using it, to be clear about what definition you are using. [Ivy Frenger, Germany]	Noted - unfortunately due to space constraints this information has not been added

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11633	59	9	59	22	This paragraph addresses our understanding of the sensitivity of the biological carbon pump (BCP) to climate, as evinced by paleoclimate records from the last glacial maximum (LGM) and deglacial period. This paragraph appears to be the only mention of LGM carbon sequestration in the chapter. This topic was given a more comprehensive treatment in AR5; for example, AR5 WGI Figure 6.5 shows mechanisms contributing to the change in carbon dioxide concentrations bewteen the LGM (glacial) and Holocene (interglacial) periods. For the ocean, five mechanisms are shown, two of which have high confidence levels, and three of which have medium confidence levels. A nice addition to this section in AR6 might be to revisit the three medium confidence estimates for the contribution of ocean circulation, Fe fertilization, and coral reef/carbonate compensation to changes in LGM carbon dioxide concentrations, as these are mechanisms discussed in the text regarding changes to the BCP during the LGM. Is there new literature published since AR5 that might change these confidence levels from medium to high, or not? [Ellie Broadman, United States of America]	Accepted - the paragraph has been modified
87701	59	11	59	11	"Nutrient inventories…" -> Nutrients show up a bit out of the blue here. [Ivy Frenger, Germany]	Rejected - covering changes to nutrients are out of the scope of this chapter
69095	59	11	59	13	I would recommend to clarifying the processes that may increase the inventory nutrients. [Hidetaka Kobayashi, Japan]	Accepted - the sentence has been modified
7337	59	13	59	16	Can you specify how much lower the temperatures were during the LGM? [Svenja Halfter, Australia]	Accepted - the sentence has been modified
69093	59	13	59	16	The effects of low seawater temperatures on biological activities are discussed not only in terms of bacterial degradation but also in terms of biological production (Chikamoto et al., 2012, Geophys. Research. Lett., https://doi.org/10.1029/2012GL053828). [Hidetaka Kobayashi, Japan]	Accepted - the sentence has been modified
74275	59	15	59	15	remove brackets between citations of Anderson et al and of Galbraith and Skinner [Christoph Völker, Germany]	Accepted - change made
3923	59	15	59	15	"2019)(Galbraith" => "2019; Galbraith" [Makio Honda, Japan]	Accepted - change made
73171	59	15	59	15	Replace)(with ; [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change is made.
33347	59	15			Change: "…(Galbraith and Jaccard, 2015; Anderson et al., 2019)(Galbraith and Skinner, 2020)…" by "…(Galbraith and Jaccard, 2015; Anderson et al., 2019; Galbraith and Skinner, 2020)…" [Guiomar Rotllant, Spain]	Accepted - change made
73173	59	16	59	16	Replace 'colder' with 'lower' [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable - this section has been extensively rewritten
2503	59	16			There is some evidence however, that a reduced AMOC might decrease the efficiency of the biological pump by increasing the overall fraction of preformed nutrients in the global ocean. Compared to the glacial, this was probably even more pronounced during HS1 and the YD. (Hertzberg et al., 2016, GRL 43, 070723) [Thomas Ronge, Germany]	Rejected - There is little evidence to support such a statement.
7339	59	17	59	18	Isolated deep ocean due to a different ocean conveyor belt? Specify! [Svenja Halfter, Australia]	Noted. The deep ocean was generally more poorly ventilated and more stratified (i.e. increased density contrast between GNAIW and AABW) during the LGM, yet the ocean circulation scheme did not change fundamentally.

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2499	59	17			As mentioned above a recent study now indicates a more isolated southern Indian Ocean for the first time (Ronge et al., 2020, Paleoceanography & Paleoclimatology 35, PA003733). [Thomas Ronge, Germany]	Accepted - reference has now been taken into consideration
83511	59	19	59	19	same comment as before regarding the use of "termination" (mid-point of transition) vs. last deglacial transition (like in Chapter 2) or last deglaciation, i.e. my recommendation is to amend the phrasing used in the current text. [Antje H. L. Voelker, Portugal]	Accepted
98343	59	24	59	30	I suggest to add (e.g., in l. 27 and l. 29) the following reference that highlights the role of phytoplankton growth rates and zooplankton grazing rates for the projected phytoplankton decline: Olonscheck, D. et al. (2013), "Decomposing the effects of ocean warming on chlorophyll a concentrations into physically and biologically driven contributions", Environmental Research Letters 8, 014043, doi:10.1088/1748-9326/8/1/014043 [Dirk Olonscheck, Germany]	Not applicable - this section has been extensively rewritten
21839	59	24	60	4	This is a very long paragraph and mixes several things. Could it be split into several smaller paragraphs to make for an easier read? [Peter Thorne, Ireland]	Not applicable - this section has been extensively rewritten
69097	59	26	59	33	It may be useful to refer to studies that discuss the limiting factors for future changes in net primary production in the individual ocean basins (Nakamura and Oka, 2018, J. Oceanogr., https://doi.org/10.1007/s10872-019-00513-w). They show that the nutrient limitation in the Southern Ocean is expected to weaken (this is a unique feature of the Southern Ocean). The increase in iron supply due to warming-induced wind changes is the most plausible reason for this. [Hidetaka Kobayashi, Japan]	Rejected - this section has been extensively rewritten and focuses on the global scale feedback between climate and PP
73175	59	28	59	28	Replace 'warmer' with 'higher'. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - 'warmer' is clearer than 'higher' when referring to temperature
73177	59	29	59	29	Quantify 'high temperatuares'. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable - this section has been extensively rewritten
37943	59	30	59	32	Could you add more explanation the mechanism how increasing stratification alleviate seasonal light limitation in high latitudes? [Junhee Lee, Republic of Korea]	Noted - unfortunately space constraints meant we couldn't discuss in this section
102067	59	32	59	33	"However, increased stratification also reduces nutrient supply to the upper ocean (Section 9.2.3.3) ..." - It is likely that this cross reference is not correct, as this information does not seem to be mentioned section in Chapter 9. Perhaps change to 9.2.1.4 where stratification and ocean mixing layer are discussed??? See also comment for chapter 9, p17 lines 8-10. [IAPSO ECS group review, United States of America]	Accepted - section 9.2.1.4 is referenced
102069	59	32	59	35	Chapter 9 discusses increased thermocline stratification (rather than decreased mixed layer depth) and a modeled increased strengthening of the subtropical gyres (low confidence) in sections 9.2.3.4 and 9.2.1.4. This section of chapter 5 could discuss the biogeochemical implications of strengthening of the subtropical gyres (expansion of the oligotrophic subtropical biome) in particular due to the potential of reduced nutrient supply and changing community composition. Some possible citations are Dutkiewicz, S., Hickman, A. E., Jahn, O., Henson, S., Beaulieu, C., & Monier, E. (2019). Ocean colour signature of climate change. Nature communications, 10(1), 1-13. Cabré, A., Marinov, I., & Leung, S. (2015). Consistent global responses of marine ecosystems to future climate change across the IPCC AR5 earth system models. Climate Dynamics, 45(5-6), 1253-1280. [IAPSO ECS group review, United States of America]	Rejected - the feedbacks between climate and changing community composition are already discussed. The gyre expansion is already discussed in chapter 9. The 2 papers suggested don't discuss phytoplankton community composition.

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58897	59	34	59	35	I think this statement could be revised to say that there is low confidence in the direction oceanic PP will respond to ongoing climate change over different timelines. Decreases in oceanic PP have already observed on current timescales (e.g. Roxy et al. 2015, Geophys Res Let). One of the two studies that's mentioned here (Agusti et al. 2019) makes more claims about the species distribution of marine photoautotrophs than about the actual net changes in productivity. Incorporating a statement about timescales will better reflect the uncertainy outlined in the subsequent paragraphs. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable - this section has been extensively rewritten and these sentences removed as a result
26915	59	43	59	43	The correct section is 5.2.1.3 [Eric Brun, France]	Accepted - change is made.
73179	59	48	59	49	Remove split of numbers and units across line break. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial - this kind of issues will be taken care of during the production phase, which includes professional copy-editing.
73181	59	54	59	54	Capital 'T' required for 'tropics'. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change is made.
36417	59	55	59	55	Several recent papers have shown evidence for ocean acidfication impacts on foraminifera, pteropods, and dungeness crab larvae (Osborne et al., 2019; Bednarsek et al., 2014, 2017, 2019, 2020). [Adrienne Sutton, United States of America]	Noted - unfortunately space constraints meant we couldn't discuss in this section
42817	59	55	59	55	Require more information on effects of calcification and more references. [Melissa Chierici, Norway]	Noted - unfortunately space constraints meant we couldn't discuss in this section
30603	59	55	59	55	Several recent papers have shown evidence for ocean acidfication impacts on foraminifera, pteropods, and dungeness crab larvae (Osborne et al., 2019; Bednarsek et al., 2014, 2017, 2019, 2020). [nina bednarsek, United States of America]	Noted - unfortunately space constraints meant we couldn't discuss in this section
45451	60	1	60	1	Krumhardt et al. (2019, Journal of Advances in Modeling Earth Systems, 11) found that increasing CO2 stimulates coccolithophore growth in certain regions, at the cost of calcification, but future CO2 conditions drive a decrease in global marine CaCO3 export. This study should be mentioned here as it complements the points made in this paragraph. [Olivier Sulpis, Netherlands]	Taken into account - reference added
127837	60	1	60	2	This sentence needs a citation. There is no clear evidence that OA will reduce PP. [Trigg Talley, United States of America]	Not applicable - this section has been extensively rewritten and these sentences removed as a result
42819	60	2	60	3	Add" However, some studies show that increased CO2 may enhance spring bloom production in the Eurasian Arctic Ocean (Holding et al., 2015; Sanz-Martin et al., 2018)" [Melissa Chierici, Norway]	Not applicable - this section has been extensively rewritten and these sentences removed as a result
41753	60	6	60	14	As written the median trend in primary production in CMIP6 is an increase, compared to a decrease in CMIP5. This is a problematic comparison given that not all CMIP6 output are available. It's unclear whether this is a real trend over the CMIP6 ensemble or an artefact of incomplete reporting given the wide spread in CMIP5 trends. I think this needs to be clarified more, either if there is some consistency between CMIP5 and CMIP6 models (e.g., if the model was included in CMIP5, is the trend consistent?), or making this caveat explicit in the text. [Jamie Wilson, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable - since the SOD review, new literature has been published which includes an increased number of CMIP6 models

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
73183	60	7	60	7	Remove split of numbers and units across line break. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial - this kind of issues will be taken care of during the production phase, which includes professional copy-editing.
102071	60	14	60	15	"Observations provide little direct constraint on the modelled esponses of PP to climate change, partly due to insufficiently long records (Henson et al., 2016)." - there is a number of recostructed changes in PP based on marine sediment archives from different regions and across different timesscales (e.g. Emeis et al., 2009; Filipsson et al., 2011; Jaccard et al., 2013; Müller et al., 2018; Polovodova Asteman et al., 2018) - those stretch much further back in time than time series several decades-long, which according to Henson et al. are required to distinguish climate change signal from natural variability. Most of these existing paleostudies suggest higher PP during colder climate periods. Full references: Emeis, K. C., Struck, U., Leipe, T., & Ferdelman, T. G. (2009). Variability in upwelling intensity and nutrient regime in the coastal upwelling system offshore Namibia: results from sediment archives. International Journal of Earth Sciences, 98(2), 309-326; Jaccard, S. L., Hayes, C. T., Martinez-Garcia, A., Hodell, D. A., Anderson, R. F., Sigman, D. M., & Haug, G. H. (2013). Two modes of change in Southern Ocean productivity over the past million years. Science, 339(6126), 1419-1423; Müller, J., Romero, O., Cowan, E. A., McClymont, E. L., Forwick, M., Asahi, H., ... & Stoner, J. (2018). Cordilleran ice-sheet growth fueled primary productivity in the Gulf of Alaska, northeast Pacific Ocean. Geology, 46(4), 307-310; Filipsson, H. L., Romero, O. E., Stuut, J. B. W., & Donner, B. (2011). Relationships between primary productivity and bottom-water oxygenation off northwest Africa during the last deglaciation. Journal of Quaternary Science, 26(4), 448-456.; Polovodova Asteman, I., Risebrobakken, B., Moros, M., Binczewska, A., Dobosz, S., Jansen, E., ... & Bąk, M. (2018). Late Holocene palaeoproductivity changes: a multi-proxy study in the Norwegian Trench and the Skagerrak, North Sea. Boreas, 47(1), 238-255. [IAPSO ECS group review, United States of America]	Taken into account - sentence has been modified
26917	60	16	60	16	This information is important and should be added at the conclusion. We consider as a missing point : to monitoring the PP changes (oceanographic cruises and buoy with autonomous sensor ...) [Eric Brun, France]	Noted - the point about requiring better observational constraints is returned to in the concluding sentences of this section
73185	60	16	60	16	Change 'based-on' to 'based on'. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change is made.
83831	60	30	60	31	Assessing the biological carbon pump and relative POC flux variation is very important, it is very interesting some publications report that the DOC flux will be increased under the increasing warming scenario (for example, Roshan and Devries 2017, Nature Communications) [Nianzhi Jiao, China]	Taken into account - this section has been extensively rewritten

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
41763	60	30	61	13	There is no mention of changes in dissolved organic carbon (the microbial carbon pump) and how this may also impact on the ~600 Pg C of carbon stored as DOC in the deep ocean. The mechanisms behind the persistence of DOC in the ocean are not well known and therefore it's response to a changing climate is very uncertain (Legrendre et al., 2015, Progress in Oceanography (http://dx.doi.org/10.1016/j.pocean.2015.01.008)). But, it seems likely there would be some change given DOC production has been linked to ecosystem factors such as nutrient availability (Polimene et al., 2017, http://dx.doi.org/10.1093/plankt/fbw091) and cell size (Roshan and DeVries 2017, Nature Communications, https://doi.org/10.1038/s41467-017-02227-3). [Jamie Wilson, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - sentence added
21841	60	30	61	13	This is another very long paragraph that would probably be more accessible if it could be split into several smaller paragraphs. [Peter Thorne, Ireland]	Not applicable - this section has been extensively rewritten
7341	60	30	61	13	It's interesting that in the whole paragraph focuses predominantly on the impact of climate change on phytoplankton and hence, on POC export. However, we know that zooplankton can affect the BCP in various ways: by grazing on phytoplankton, fragmenting and repacking particles into faecal pellets, and by actively transporting carbon below the thermocline and deeper (DVM and lipid pump in higher latitudes). The impact of zooplankton community structure is mentioned once, but there is nothing about the active carbon transport in their diel vertical migration or other contributions to passive fluxes. While I understand that there might not be enough global models to predict future developments, this could be a good opportunity to talk about gaps of knowledge and future research. Maybe it's worth to summarize these and other knowledge gaps in an own section. [Svenja Halfter, Australia]	Taken into account - the section (and whole chapter) focuses on potential feedback mechanisms between changing processes of carbon cycling and climate/CO2. As yet there have been no studies on the possible feedbacks between zooplankton and climate - likely because the climate models used in CMIPs do not have an adequate representation of zooplankton community structure or behaviours. However, a couple of sentences have been added on potential response of zooplankton in the context of particle respiration.
41759	60	38	60	39	"The mechanisms underlying the remineralisation of POC…" is unclear as to whether this relates to the specific remineralisation rate of POC or the attuenuation of POC flux with depth as a function of remineralisation rate and sinking rate. [Jamie Wilson, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - the latter was intended. Sentence has been changed.
38479	60	39	60	46	Many of these mechansims described here have been mentioned in the above paragraphs. It seems a redundacy to repeat them. [LONG CAO, China]	Not applicable - this section has been extensively rewritten
41761	60	40	60	40	"efficiency and functioning of the biological pump" is not consistent with language used later in the paragraph ("magnitude and efficiency"). I think "magnitude and efficiency" are clearer and more informative terms. [Jamie Wilson, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable - this section has been extensively rewritten

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
102073	60	46	60	48	"Other climate effects such as de-oxygenation and warming could also result in alterations to the magnitude and efficiency of the biological carbon pump.." - for coastal regions subject to a precipitation increase there will be potentially also an effect of increased land-runoff bringing along nutrients from land (e.g. Paerl, 2006; Paerl et al., 2010, 2014; Polovodova Asteman et al., 2018) and, hence, counteracting the effect of a decreased nutrient supply to the primary producers due to increased ocean stratification. Also modulations in wind fields may cause major shifts on coastal PP (Daewel and Schrum, 2017). If added, this information on PP in coastal regions should be cross-referenced to Chapter 9, section 9.2.3.5.2 Coastal Systems and Marginal Seas. Full references: Paerl, H. W. (2006). Assessing and managing nutrient-enhanced eutrophication in estuarine and coastal waters: Interactive effects of human and climatic perturbations. Ecological Engineering, 26(1), 40-54.; Paerl, H.W., Rossignol, K.L., Hall, S.N. et al. Phytoplankton Community Indicators of Short- and Long-term Ecological Change in the Anthropogenically and Climatically Impacted Neuse River Estuary, North Carolina, USA. Estuaries and Coasts 33, 485–497 (2010). https://doi.org/10.1007/s12237-009-9137-0; Paerl, H.W., Hall, N.S., Peierls, B.L. et al. Evolving Paradigms and Challenges in Estuarine and Coastal Eutrophication Dynamics in a Culturally and Climatically Stressed World. Estuaries and Coasts 37, 243–258 (2014). https://doi.org/10.1007/s12237-014-9773-x; Polovodova Asteman, I., Risebrobakken, B., Moros, M., Binczewska, A., Dobosz, S., Jansen, E., ... & Bąk, M. (2018). Late Holocene palaeoproductivity changes: a multi-proxy study in the Norwegian Trench and the Skagerrak, North Sea. Boreas, 47(1), 238-255; Daewel, U., & Schrum, C. (2017). Low-frequency variability in North Sea and Baltic Sea identified through simulations with the 3-D coupled physical–biogeochemical model ECOSMO. Earth System Dynamics, 8(3), 801. [IAPSO ECS group review, United States of America]	Rejected - these references do not consider the effects on the biological carbon pump and feedbacks to climate (the theme of this section)
58899	60	50	60	54	The results of Matear and Lenton (2014) are not clearly conveyed here. In their models, POC accounts for a 43 ppm decrease in projected atmospheric CO2, and remineralization/dissolution account for a potential 18 ppm increase. The net concentration of atmospheric CO2 still increases dramatically, to over 1000 ppm. However, the statement "atmospheric CO2 drops by about 43 ppm by 2100" suggests that the actual concentration will drop. This distinction between specific effect accounted for by one model component and overall net effect from all components could be more clear using language like "accounts for..." or "is associated with..." [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable - this section has been extensively rewritten and these sentences removed as a result

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
41755	60	50	60	54	There is a big focus on the findings of Matear and Lenton (2014). The changes in POC fluxes imposed in this study are idealised and are intentionally tested over a large range to capture the potential for impacts. However, the change in POC flux attentuation, as measured by the Martin curve value 'b', varies by approx. 2.25 (a change in e-folding depth of 138m). In comparison, a model simulation with an explicit temperature-dependent remineralisation rate (Laufkotter et al., 2017, Global Biogeochemical Cycles, 10.1002/2017GB005643) found only a change of approx. 0.25 by 2100 (an efolding depth change of 56m) (values calculated by Wilson et al., 2019, Biogeosciences, 10.5194/bg-16-2923-2019). Therefore, the CO2 changes reported by Matear and Lenton (2014) are potentially an over-estimate of the realised impact of the biological pump on CO2. I think this should be reflected in the text at the very least. [Jamie Wilson, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable - this section has been extensively rewritten and these sentences removed as a result
58871	60	54	55	60	Add referecence Brewer 2019: Warming can increase the oxygen utilization rates, increase the remineralization and ultimately reduce the export. Brewer, P. G. (2019). The molecular basis for understanding the impacts of ocean warming. Reviews of Geophysics, 57 , 1112–1123. https://doi.org/10.1029/ 2018RG000620 of Ocean Warming, [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - reference added
41757	61	2	61	4	Suggested rewording: "Enhanced remineralisation also implies a shoaling in the average depth at which organic carbon penetrates the deep ocean, resulting in a reduction in the sequestration time of carbon and creating a positive feedback between remineralisation and atmospheric CO2". [Jamie Wilson, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - sentence has been altered

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
102075	61	9	61	11	"Improved model representation (which will require better observational constraints) of the biological carbon pump is required, as the contribution of biological processes to CO2 uptake is expected to become more significant with continued climate change" - see my comment regarding existing paleorecords aiming to reconstruct primary productivity (p60, line 14-15). It is essential to show that such studies do exist but indeed more evidence is needed to constrain the models, especially aiming to project effects of climate change on biological pump in coastal regions (e.g. Holt et al 2016), which are hotspots of productivity due to either coastal upwelling or presence of fjords and estuaries representing large carbon sinks (Smith et al., 2015; Cui et al., 2016; Smeaton et al., 2016). Full references: Smith, R. W., Bianchi, T. S., Allison, M., Savage, C., & Galy, V. (2015). High rates of organic carbon burial in fjord sediments globally. Nature Geoscience, 8(6), 450-453; Cui, X., Bianchi, T. S., Savage, C., & Smith, R. W. (2016). Organic carbon burial in fjords: Terrestrial versus marine inputs. Earth and Planetary Science Letters, 451, 41-50; Smeaton, C., Austin, W., Davies, A., Baltzar, A., Abell, R. E., & Howe, J. A. (2016). Substantial stores of sedimentary carbon held in mid-latitude fjords. Biogeosciences.13, 5771–5787; Holt, J., Schrum, C., Cannaby, H., Daewel, U., Allen, I., Artioli, Y., ... & Pushpadas, D. (2016). Potential impacts of climate change on the primary production of regional seas: A comparative analysis of five European seas. Progress in Oceanography, 140, 91-115. [IAPSO ECS group review, United States of America]	Taken into account - the reference to paleo records has been added.
40779	61	16	68	20	sections 5.4.5 and 5.4.6: the structure of the sections is very well outlined and guiding the reader really helps! [TSU WGI, France]	Noted - thank you!
41657	61	18	61	19	It is interesting that "ESM" is explained here, even though it has been used in the previous subchapters already. I suggest to explain in before - and "CMIP" too [Katharina Meurer, Sweden]	Noted - but we prefer to repeat the definition of acronyms here so that the section is reasonably self-contained.
89491	61	18	61	47	The focus on the CMIP6 advances seems the most important part of this section, while perhaps condensing the history would save space [Edward Schuur, United States of America]	Noted - however we feel that this background is useful as it sets the context for the advance of the CMIP6 models.
21843	61	19	61	19	Add a reference to chapter 4 after projections otherwise the inference is that chapter 3 looks at projections and D&A which is incorrect. [Peter Thorne, Ireland]	Accepted - change made.
26919	61	23	61	23	We suggest to change the order Ocean-atmosphere and land atmosphere carbon fluxes (because the section is written in this order) [Eric Brun, France]	Accepted - order changed.
19985	61	23	61	24	Why mention 5.4.2 and 5.4.3, whereas the thematic units for land and ocean are 5.4.(1 and 2) and 5.4.(3 and4) respectively? [philippe waldteufel, France]	Accepted - correction made: "..for the reasons outlined in subsections 5.4.1 to 5.4.4"
19311	61	25	61	25	change "GCM climate models" to GCMs [Benjamin Lamptey, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - changed to "General circulation Models (GCMs) of the climate"
41659	61	25	61	25	what does "GCM" mean? [Katharina Meurer, Sweden]	Taken into account - "General Circulation Models (GCMs)" now defined.
19313	61	27	61	27	change "six GCM modls" to "six GCMs" [Benjamin Lamptey, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change made.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
78507	61	35	61	35	only one (shared) land model included N-cycle in CMIP5 – instead of a “key difference between them” might be better to say “a key deficiency of almost all…” [Chris Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change made.
78509	61	39	61	39	should say “some of CMIP6 models include N cycle…” [Chris Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change made.
17883	61	39	61	47	There are many more CMIP6 models on ESGF. Is there a reason why you've limited your assessment to a single paper? [Katherine Calvin, United States of America]	Taken into account - these are the coupled climate-carbon cycle models included in the C4MIP project (i.e. models that include both interactive land and ocean carbon sinks). We make that clearer by adding the sentence: "In this subsection we consider the 11 fully coupled climate-carbon cycle models involved in C4MIP (i.e. models that include both interactive land and ocean carbon uptake)".
105957	61	39	62	3	While the E3SM model has been somewhat late to the game, it may be valuable to include the E3SM Land Model (ELM) in the discussion and model table presented here. This land model incorporates both N- and P-limitation and resolved soil carbon by depth, and thus is likely to show somewhat different results from the CLM4.5 model from which it was derived. This nutrient limitation has an impact on the feedback parameters, discussed later in the report and in a separate comment from me. [Forrest Hoffman, United States of America]	Noted, but this would require repeating the Araro at al. (2020) analysis for this model. Time-constraints prohibits this now.
619	61	39		47	How about phosphorus effects on the land C sink? In how many of the models is P cycling on land included? [Vicca Sara, Belgium]	Taken into account - "include nutrient limitations" changed to "include nitrogen limitations",
26921	61	41	61	41	The referenced paper has been accepted [Eric Brun, France]	Accepted - update to reference made.
86757	61	44	61	44	Forest fire in carbon cycle models. Forest fires cause a temporal increase in atmospheric (and ocean) CO2 levels, but as forests re-grow its carbon stocks will be filled up again. Does the carbon cycle models handle such temporal fluxes? Is this the ILAMB mentioned in line 1-2 page 62? Please consider explaining further how forest fires are dealt with in the models. [Oyvind Christophersen, Norway]	Noted - some models include forest fire (see Table 5.4), and all land carbon cycle models attempt to simulate carbon uptake after disturbance.
58901	61	44	61	45	It states here that only 1 of 11 CMIP6 models considered permafrost carbon, but earlier (pg 57, line 34) it states that two models include permafrost carbon cycle dynamics. This presentation should be consistent. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - consistently stated now as "2 of CMIP6 ESMs". Table 5.4 has been updated to have Yes in the Permafrost row for NorESM.
115379	61	44		47	Check for consistency across the chapter - I believe earlier in the chapter it indicated that more than one assessed CMIP6 model included permafrost processes. [Gillett Nathan, Canada]	Taken into account - see response to comment 58901.
30621	61	45	61	45	Furthermore, no CMIP6 ESMs have included biogeochemical feedbacks of ozone and aerosols to land carbon cycle, which have been found influential on the future projections of regional carbon balance and ecosystem functions (Yue, X. et al., 2020. Pathway dependence of ecosystem responses in China to 1.5°C global warming. Atmospheric Chemistry and Physics 20, 2353-2366). [Hong Liao, China]	Noted.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
90093	61	46	61	47	This statement uploaded by Ted Schuur on behalf of a graduate class that reviewed the chapter (this was communicated with IPCC TSU in advance): The message in the statement seems out of place with the rest of the paragraph. How can they be confident in the representation of processes in the models despite half of models not including major processes laid out in the paragraph? I think they should clarify that they are confident in the improvement of the model, it currently sounds like the new model could be an absolute representation. [Edward Schuur, United States of America]	Taken into account - we have modified the last sentence of this paragraph to : "Despite these remaining limitations, the carbon cycle components of CMIP6 represent an advance on those in CMIP5 as they represent additional important processes (e.g. N-limitations on the land carbon sink, Fe-limitations on ocean ecosystems)". Thanks to Ted's graduate class for pointing this out..:-)
19987	61	50	62	3	Table 5.4: the acronym PFTS is not to be found anywhere else in the SOD; What does it mean? According to Wikipedia, PFTS is an Ukrainian index for the local Stock Exchange. [philippe waldteufel, France]	Taken into account - "Plant Functional Types (PFTs)" now included in caption.
96591	61	50	62	3	Please introduce acronyms (model names and modelling groups). [Nicole Wilke, Germany]	Noted - all model names and modelling groups are defined int he Model Annex of this report.
41661	61	50	62	3	This table needs more explanation regarding the content. It is understandable that the numbers written behind e.g. "Veg C pools" refer to the number of pools that are represented in the model. However, it is not clear to me, what "PFTS" means [Katharina Meurer, Sweden]	Accepted - more descriptive figure caption now included.
78567	61	52			table can be updated to include other models, not just those in Arora 2019 (e.g. GISS). Data cut off will be in Jan 2021 so may have more models to go into analysis by then. [Chris Jones, United Kingdom (of Great Britain and Northern Ireland)]	Rejected, table was used for models only from Arora as these formed the basis of feedback assessments.
58903	62	1	62	1	Table 5.4: Why is the "permafrost C" row blank for all but three models? According to page 61, line 45, only one model represented permafrost carbon. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted
21845	62	1	62	1	I would spell out in longhand all the entries in the first column. In particular PFTS is completely non-intuitive. Were the table to be used standalone this would yield an issue. The meaning of the numbers in 5 of the rows are also unclear from a combination of the present table and caption. Again, this could be catered for by being explict. I assume these are number of vegetation carbon pools for example rather than Veg C pools. Efforts to improve labelling in the first column would thus yield enormous potential benefits with regards to clarity here. [Peter Thorne, Ireland]	Taken into account - more descriptive table caption now included.
111029	62	1	62	1	MPI model has 12 not 13 PFTs [Julia Nabel, Germany]	Accepted
68561	62	1	62	1	Table 5.4 describes the GFDL COBALTv2 model as having 2 Phytoplankton groups. There are actually 3 phytoplankton groups: diazotrophs plus other small and large phytoplankton. Please change the entry in the Phytoplankton row of COBALTv2 from 2 to 3. [Robert Hallberg, United States of America]	Accepted
17885	62	1	62	1	Do any of the land models have P represented? Can that be added to the table? And why are there blanks for permafrost? Does blank = No? [Katherine Calvin, United States of America]	Rejected - d to space constraints and very few models with P, we don't get into this in this table.
52255	62	1			Table head: change "JAMSETC" to "JAMSTEC" [Agneta Fransson, Norway]	Accepted
73187	62	9	62	9	Change 'feedback' to 'feed back'. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change made.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
28285	62	14	62	15	Clarify where in Chapter 4 are emission-driven projections discussed. From what I saw there is not much about emission-driven projections in Chapter 4, so maybe just delete this sentece. [Ryan Padrón, Switzerland]	Taken into account - sentence referring to Chapter 4 deleted.
78511	62	18			this section is nice, and really important. Figure 5.22 is key – worth tidying up a bit given it will become very widely used/cited – e.g. how does “net carbon balance” differ from “NEE”, or what does the permafrost line represent given most models don’t include it? [Chris Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - figure tidied-up, definition of net carbon balance and NEE clarified.
23733	62		62		In Table 5.4: Please change from "JAMSETC" to "JAMSTEC" [Tomohiro Hajima, Japan]	Accepted
115381	62				Table 5.4. There is a second version of CanESM5, labelled CanESM5-CanOE, which uses the CanOE ocean biogeochem model which has more nutrient species than CMOC. [Gillett Nathan, Canada]	Rejected, table was used for models only from Arora as these formed the basis of feedback assessments.
87705	63	1	63	1	Section title "5.4.5.3 Evaluation of latitudinal distribution of simulated carbon in emissions-driven runs" appears somewhat out of place"; also, p62 L14/15 says that emission driven runs are discussed in Chapter 4. [Ivy Frenger, Germany]	Accepted - subsection title change to "Evaluation of latitudinal distribution of simulated carbon sinks"
58905	63	1	63	2	It could be worth mentioning that a similar benchmarking standard exists for oceanic data (International Ocean Model Benchmark, IOMB); otherwise the reader asks how are benchmarking scores obtained for the oceanic model parameters. Both ILAMB and IOMB were published/released in 2018, so it's unclear why both aren't mentioned. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - IOMB now given equal billing to ILAMB
33349	63	3	63	4	Re-write: “Figure 5.22 shows an overview of an overview set of (Figure 5.23a) land and (Figure 5.23b) ocean benchmarks applied to both the CMIP5 and CMIP6 models from the historical simulation.” [Guiomar Rotllant, Spain]	Accepted - text edited to: "Figure 5.22 shows an overview set of land (Figure 5.22a) and ocean (Figure 5.22b) benchmarks applied to both the CMIP5 and CMIP6 historical simulations".
29991	63	4	63	4	Is "an overview of an overview" an intended expression or typo? Also, "an overview set" doesn't make much sense to me. The figure caption at line 19 says "Overview scores", which is more readlily understandable. [Michio Kawamiya, Japan]	Taken into account - see response to 33349
41663	63	4	63	4	Fig. 5.23 = Fig.5.22 [Katharina Meurer, Sweden]	Taken into account - see response to 33349
2805	63	4	63	4	repeat "overview of" "overview set of" [Stephen Wilkinson, United Arab Emirates]	Taken into account - see response to 33349
21847	63	4	63	5	This sentence is clearly incorrect and needs fixing. It is 5.22 and there is double use of 'an overview' [Peter Thorne, Ireland]	Taken into account - see response to 33349
41665	63	8	63	8	remove "is" [Katharina Meurer, Sweden]	Accepted - change was made.
2807	63	8	63	8	delete "is" [Stephen Wilkinson, United Arab Emirates]	Accepted - change was made.
58907	63	19	63	23	Figure 5.22: In this figure, land models are presented on the left and ocean models on the right. The next three figures all reverse this order (oceans on the left, land on the right). I recommend rearranging Figure 5.22 to be consistent with the approach of the following figures. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. We may update this as we iterate the figure but for now we are keeping land on the left here,
13455	63	19	63	23	It's recommended to improve the font size of the legend in figure 5. 22, as it is diff icult to distinguish the different models and the name of the datasets. [Maria Amparo Martinez Arroyo, Mexico]	Accepted. We have increased the font size on the figure legends

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17887	63	19	63	23	For Figure 5.22, how were the CMIP6 models chosen? There are a different number of models here than in the table and both are less than the number available on ESGF. Also, can you label CMIP5 vs CMIP6 on the figure directly (or do something to make this more noticeable)? [Katherine Calvin, United States of America]	Partially accepted. We chose only models that participated in both the CMIP5/ AR5 and CMIP6/AR6 C4MIP assessments, so as to document the generational change from one assessment to the next. Thus 9 model families were chosen for each group.
29993	63	31	63	31	The abbreviation "GCP" should be declared here. In the current form, it is used without spelling out at line 36 and elsewhere. [Michio Kawamiya, Japan]	Accepted - GCP now defined at first use in subsection 5.4.5
73189	63	34	63	34	Poor expresion ''global mean, annual mean' do you mean 'global annual mean'? [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change made.
44647	63	35	63	36	Please make sure that you compare apples with apples. The GCB estimate is for the anthropogenically perturbed flux,i.e. without river fluxes. Is that the same in the historical CMIP6 simulations? I would guess these numbers are the 'mean contemporary' CO2 sink, i.e. need to be adjusted for the riverine outgassing (e.g. 0.78 PgC/yr from Resplandy et al., 2018 https://doi.org/10.1038/s41561-018-0151-3) to be comparable to the GCB number. [Judith Hauck, Germany]	Noted - however most ESMs do not include riverine carbon fluxes, and it is these models that we compare with the GCP data.
29995	63	36	63	36	Figure 5.23 is drawn with integrated numbers in PgC. It is more reader-friendly if the text uses numbers in this unit rather than those in PgC yr-1. [Michio Kawamiya, Japan]	Noted - however we want to provide the reader here with information about both fluxes and changes in stores.
41667	63	36	63	36	what is meant by "GCP"? [Katharina Meurer, Sweden]	Taken into account - see response to 29993
41167	63	36			What is GCP? [TSU WGI, France]	Taken into account - see response to 29993
58909	63	37	63	38	What implications does this observation (lower model mean oceanic carbon storage than observed) have for the evaluation of CMIP6 models into the future? The next paragraph (page 44-46) offers a nice assessment of how the uncertainty in model estimates for land carbon storage reflects our ongoing uncertainty for this question, and it would be valuable to have even a simple, similar statement here. For example, "This benchmarking result suggestions that climate models may mildly underestimate oceanic carbon storage..." [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - additional sentence added on the underestimate of the ocean carbon sink in CMIP6 versus GCP.
21849	63	38	63	42	Do the GCP estimates not come with a range? Earlier sections suggested they did and that it was non-negligible. If so this range should be given to enable these two values to be more directly comparable? [Peter Thorne, Ireland]	Accepted - uncertainty in GCP estimate now included.
29997	63	40	63	40	Please consider removing the word "much". The difference between max. and min. is ~50 PgC for ocean and ~80 PgC for land. I would say they are comparable. [Michio Kawamiya, Japan]	Accepted - "much" removed.
78513	63	40	63	46	can you compare the spread across CMIP6 models to CMIP5 [Chris Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - CMIP5 and CMIP6 ranges now compared.
23735	63	42	63	44	This sentence can be supported by a work, which is based on simulations by a CMIP6-era ESM (Fig.8 of Hajima et al. (2020), https://doi.org/10.5194/gmd-13-2197-2020) [Tomohiro Hajima, Japan]	Accepted - reference added.
115383	63	44			This is a statement about the sources of uncertainty in your assessment. You don't need a confidence qualifier here. [Gillett Nathan, Canada]	Noted - but we would like to make it clear that this is a firm conclusion.

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44649	63	51	63	54	It is unclear which estimate is used for the "observation-based estimate from the GCP". Probably you mean the 'two diagnostic ocean models' used in the GCB, i.e. Khatiwala et al 2013 and DeVries et al 2014. Please cite them directly, and don't call them observation-based estimates. Also these are ANTHROPOGENIC CO2 estimates. Please be specific here, it is so often mixed up. The anthropogenic signal is of course much larger than natural, so this is okay to use here, also for the CONTEMPORARY carbon storage change, which I assume you actually mean here. Khatiwala, S., Tanhua, T., Mikaloff Fletcher, S., Gerber, M., Doney, S. C., Graven, H. D., Gruber, N., McKinley, G. A., Murata, A., Ríos, A. F., and Sabine, C. L.: Global ocean stor- age of anthropogenic carbon, Biogeosciences, 10, 2169–2191, https://doi.org/10.5194/bg-10-2169-2013, 2013. DeVries, T.: The oceanic anthropogenic CO2 sink: Stor- age, air-sea fluxes, and transports over the indus- trial era, Global Biogeochem. Cy., 28, 631–647, https://doi.org/10.1002/2013gb004739, 2014. [Judith Hauck, Germany]	Accepted - GCP estimate now explained more fully.
44651	64	5	64	5	Figure 24 doesn't show any coloured lines. Also please specify in Figure caption, whether this is contemporary or anthropogenic carbon sink. [Judith Hauck, Germany]	Accepted - Figure caption corrected to specify "contemporary" carbon sink. Also see response to 41071.
41071	64	5	64	12	These informaiton can not be seen in current Figure 5.24 since the red/blue wedges are not shown now. [TSU WGI, France]	Taken into account - Figure formatting error during the SOD compilation. Format error removed for the FGD.
78515	64	8	64	8	figure 5.24 lacks a red wedge? [Chris Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - Figure formatting error during the SOD compilation. Format error removed for the FGD.
115385	64	14			A wider range than the ocean or a wider range than CMIP5? [Gillett Nathan, Canada]	Accepted - text clarified to : "Compared to the ocean, the land components of ESMs produce a much wider range..."
73191	64	17	64	17	I would delete the hyphen, it looks like a misplaced exponential expression and the hyphen is not really required here anyway. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - hyphens deleted between "nitrogen" and "limitations".
4461	64	26	64	36	The figure does not seem complete (only black shades) [Ana Bastos, Germany]	Taken into account - Figure formatting error during the SOD compilation. Format error removed for the FGD.
58911	64	28	64	34	Figure 5.24 does not currently contain blue or red bands showing the results of CMIP5 and CMIP6 ESMs against inversion models. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account - Figure formatting error during the SOD compilation. Format error removed for the FGD.
40211	64	28			Figure 5.24: the caption does not correspond to what we see in the image [TSU WGI, France]	Taken into account - Figure formatting error during the SOD compilation. Format error removed for the FGD.
37945	64	31			Observation-based estimates from the GCP also have error range. How about adding the range in Figure 5.23 and comparing with the ensemble model spread? [Junhee Lee, Republic of Korea]	See response to 86637

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28287	64	39			Section 5.4.5.4 seems too concise. The coupled climate-carbon cycle projections are of high relevance. If possible I would suggest to expand the results from this section. Maps of the spatial distribution of the surface carbon sink/source (similar to Fig. 5.26) would be helpful, as well as information about the different flux components like GPP, respiration and disturbances. [Ryan Padrón, Switzerland]	Taken into account - space limitations and time constraints prevent us from extending the analysis this far. However, we have introduced a new figure showing maps of projected changes in land carbon storage for 4 ssp scenarios, and also added an additional frame to Figure 5.26 showing the change in cumulative land and ocean uptake at 2xCO2 in 1% per year runs.
30003	64	50	64	51	Many of the CMIP6 ESMs include river transport of carbon, meaning that some portion of carbon absorbed into land is ultimately stored in the ocean. The term "carbon storage" should be replaced with something like "cummulative carbon uptake". [Michio Kawamiya, Japan]	Accepted - we replace "carbon storage" with "cumulative carbon uptake" throughout.
73193	64	53	64	53	Capital 'C' for 'century'. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - TSU style guide requests "21st century" without capital C.
54993	65	1	65	1	Replace “sensitivity of land carbon storage to climate change” with “sensitivity of land carbon storage to temperature change”. [Nancy Hamzawi, Canada]	Accepted - change made.
23709	65	3	65	3	The ensemble [Massimo Lupascu, Singapore]	Accepted - change was made.
41671	65	3	65	3	"The" [Katharina Meurer, Sweden]	Accepted - change was made.
73195	65	3	65	3	Change 'Th' to 'The'. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change was made.
17895	65	3	65	11	How comparable are the CMIP5 and CMIP6 carbon uptake estimates? I would assume differences in LULCC between the scenarios would have implications for terrestiral carbon storage. Please state the potential reasons for differences, including updates to the models, changes in the level of warming, and changes in LULCC [Katherine Calvin, United States of America]	Taken into account - we compare NEP=NPP-Rh in both CMIP5 and CMIP6 models. Text added to explain the possible contributions of differences in process representation and climate sensitivity.
78569	65	4			true land models have greater spread in global totals. Ocean models agree less closely though in regional (basin) scale – e.g. see Hewitt et al (2016: J. Clim, 29, p7203-7213, DOI: 10.1175/JCLI-D-16-0161.1.) [Chris Jones, United Kingdom (of Great Britain and Northern Ireland)]	Noted, but ocean models still validate surprisingly well against inversion estimates (see Figure 5.24).
114711	65	16	65	26	Check consistency with figure 4.5 and consider adding uncerianty ranges. [Jan Fuglestvedt, Norway]	Noted - Uncertainty ranges added
78571	65	29			this section could also explain that alpha, beta, gamma can be used to reconstruct quantities such as AF and TCRE (Jones and Friedlingstein, ERL, 2020) [Chris Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text and reference added.
58913	65	31	66	34	It would help the reader if the discussion of the two parameters (beta and gamma, the CO2 effect and warming effect) were consisently presented in the same order. For example Lines 34-35 (pg 65) say give the explanation for gamma before beta, but then the equations (Lines 36-38) are reversed and lines 43-50 similarly discuss beta before gamma - but then they're reversed again at the end of the page (65, l. 54-55) and the top of the next (66, l. 3-4) and the Table 5.5. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - consistent order now of beta and then gamma. Table 5.5 columns also switched.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
21141	65	41	65	41	Consider adding at the end of this paragraph: "This feedback formalism is one of several that have been proposed for analysing climate-carbon cycle feedbacks (Lade et al. 2018)." Reference: Lade et al. 2018, https://www.earth-syst-dynam.net/9/507/2018/ . This paper reviewed and compared three commonly used feedback metrics, finding that they measured the same climate-carbon cycle processes. The paper also constructed a climate-carbon cycle emulator that approximately reproduced CMIP5 results and was used to analytically study how different climate-carbon cycle processes affect the measured feedbacks. [Steven Lade, Sweden]	Accepted - sentence and reference added.
19989	65	44	65	44	Sentence to be corrected [philippe waldteufel, France]	Taken into account - see response to 73197
73197	65	44	65	44	Delete 'we'. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change was made.
17899	65	52	66	7	There is a paper indicating that human system feedbacks can alter beta & gamma: https://doi.org/10.1029/2018GL079350 [Katherine Calvin, United States of America]	Noted.
105959	65	52	67	5	The effect of nutrient limitation on the magnitude of the feedback parameters is an important point from this analysis. Including results from the new E3SM model, which includes both N- and P-limitation on land, would be valuable to the discussion. [Forrest Hoffman, United States of America]	See Response to 105957
35109	65	53	65	53	Arora et al paper is now accepted. [Richard Williams, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - reference updated.
37947	66	1	66	7	In addition to no significant change in climate effects on global ocean carbon storage (gamma) between CMIP5 and CMIP6, the standard deviation of the feedback parameter between models also increase (only the parameter). Could you add the possible reasons for the results? [Junhee Lee, Republic of Korea]	Noted - but this would just be conjecture at this stage.
30005	66	3	66	7	It is perhaps safe to say that gamma_L is significantly reduced from CMIP5 to CMIP6. For other values for gamma_O and beta_L,O, there does not seem to be statistically significant differences for other values for gamma_O and beta_L,O between CMIP5 and CMIP6. Wording should be modified accordingly. (In the current text, for example, it is said that there is evidence in reduction for beta_L, but no evidence for gamma_O. I simply do not understand the difference.) [Michio Kawamiya, Japan]	Accepted - text about changes in beta and gamma factors from CMIP5 to CMIP6 has been rewritten.
78517	66	10			is table 5.5 needed? If it simply recreates a published table in Arora et al then it is not needed here – IPCC chapters are great for synthesising info from a range of sources, but no need to replicate a single item from 1 paper. [Chris Jones, United Kingdom (of Great Britain and Northern Ireland)]	Noted - but we feel this Table is needed for completeness. The table has now been updated beyond Arora et al.
17897	66	12	66	16	Can you indicate which models have N limits in this table? Do any have P limits? If so, please indicate that as well. [Katherine Calvin, United States of America]	Noted - the number models with limits are the models with "N cycle". Very few models have P limits on land, so we have decided not to introduce an add additional row for that.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
58915	66	24	66	25	If beta is consistently positive, does that not mean that as CO2 increases, the amount of carbon stored in either the ocean or land increases? It's not clear why it would be called a negative feedback, or at least what the negative feedback is on. If the authors are trying to suggest that increasing carbon storage in turn is a negative feedback on climate change, that could be more clear. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account - - all feedbacks are relative to climate change. We clarify this by writing "negative feedback on CO2-induced climate change".
41673	66	24	66	25	It is not clear what "negative feedback" means in this context. What is the result for simulated C uptake/emissions? [Katharina Meurer, Sweden]	Taken into account - see response to 58915
58849	66	24	66	25	Figure 5.26 does not have A and B panels that are referenced here. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - panel labels added.
41675	66	25	66	34	The same applies here: what do negative and positive values of "gamma" mean? [Katharina Meurer, Sweden]	Noted - negative values of gamma imply that carbon is releasee to the atmosphere as a result of warming alone.
58917	66	28	66	29	Could be more clear to say "the ensemble mean shows a negative gamma, but the individual models lack widespread agreement in the sign" or "without widespread model agreement in the sign." [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account - text reworded
73199	66	31	66	31	Change 'arctic' to 'Arctic'. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - correction made.
16069	66	31	66	33	"so there is not model unanimity of the sign of in this region": OK but there are good reasons to think that gamma should be negative in this region because of the permafrost carbon, so why not say that more clearly? [Gerhard Krinner, France]	Noted - but we are talking about model results here. Missing processes are discussed later in the Section 5.7.
116451	66		66		Is it possible to discuss changes between CMIP5 and CMIP6 and reasons for differences if possible? [Valerie Masson-Delmotte, France]	Taken into account - comparison between CMIP5 and CMIP6 is now a more significant part of the section.
115387	67	8		50	This section is written as a review, but it lacks on overall assessment on what emergent constraints tell us about the carbon cycle - about carbon climate feedbacks etc. Add assessment here. The text implies that they can help constrain carbon cycle properties, but this isn't carried through into an assessment. [Gillett Nathan, Canada]	Accepted - assessment added at conclusion of the subsection.
66669	67	10	67	10	space between "based on". [Dave Frame, New Zealand]	Accepted - change was made.
73201	67	10	67	10	Change 'based-on' to 'based on'. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change was made.
18231	67	12	67	12	The - before relationships makes the sentence flow awkwardly. Remove the dash. [Chelsey Baker, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - now made into a separate sentence
37961	67	17	67	17	“~ published since (Cox, 2019; Hall et al., 2019).” [Junhee Lee, Republic of Korea]	Accepted - space removed.
21851	67	17	67	17	2 /= many so either use a more accurate term or add citations to justification the use of many or make clear at the very least that these are review papers summarising many more studies (if indeed they are). [Peter Thorne, Ireland]	Accepted - have added: "see for example summaries in.."
19991	67	34	67	35	These statements concerning ENSO variability driving CO2 land uptake are quite affirmative. Is this link a definitely established fact? Then this report should issue a likelihood statement. [philippe waldteufel, France]	Taken into account - this has indeed been well-known for a long-time. Have added a reference to the carbon cycle in the IPCC AR5

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
73203	67	46	67	46	See previous comment: delete hyphen (which is in a different style to that used previously). [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change made.
73205	67	46	67	46	Change 'on mid and high-latitides' to 'in mid- and high latitides'. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change made.
44655	67	49	68	15	Figure 5.27 - present day Southern Ocean carbon uptake appears small. Is it contemporary or anthropogenic CO2 uptake? What is it based on? What is the latitudinal boundary of the Southern Ocean? Grey bars not visible in Fig. 5.27 [Judith Hauck, Germany]	Taken into account - there was an error in the formatting of Figure 5.27 that led to bars not showing up. This has now been corrected. The details on each of the studies are given in the cited papers.
8847	68	6	68	11	It would be useful to give the range of predicted increases in methane wetland emissions from various studies. For example, Koffi et al note that the range of wetland emissions predicted is 50-150% from LSMs. Please clarify the part of sentence of beginning from "but that the corresponding effect..." If wetland response to future cliamte change is to enahnce atmospheric CH4 then how are the emissions limited. Koffi et al provide observational constraints on the feedback from wetland emissions https://advances.sciencemag.org/content/6/15/eaay4444.full [Vaishali Naik, United States of America]	Accepted - reference taken info account
19993	68	14	68	15	What is meant by "thickness" in this figure 5.27? [philippe waldteufel, France]	Taken into account - Figure formatting error during the SOD compilation. Format error removed for the FGD.
8849	68	17	68	19	Methane has a feedback on its own lifetime - increases in methane reduce its primary sink the hydroxyl radical, which would then further increase CH4 abundance inducing a positive feedback on climate. Furthermore, perturbations in species affecting OH (e.g., BVOCs, lightning NOx) also affect methane lifetime. I dont think it has been conclusively shown that the negative feedback on methane lifetime from physical climate change offsets the positive feedback from biogenic methane emission increases or other indirect effects (BVOCs, LNOx). See also, Thornhill et al. 2019 https://www.atmos-chem-phys-discuss.net/acp-2019-1207/. The discussion in section 6.3.6 will be revised to better capture these nuances. I am happy to connect with you to produce a consistent assessment. [Vaishali Naik, United States of America]	Accepted. The FGD version of the Figure makes use of the FGD assessment of Chapter 6

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
17047	68	20	68	20	Marine methane reservoirs constitutes a significantly large carbon pool and ocean margins are characterized by the widespread subsurface methane transport towards the seafloor and the water column (Reeburgh, 2007). Efficient microbial oxidation prevents the marine methane from directly interacting with the climate system (Knittel and Boetius, 2009; Boetius and Wenzhöfer, 2013). However, the microbial oxidation of methane produce dissolved inorganic carbon, that can significantly contribute ocean carbon budgets as well as sediment carbon burial through authigenic carbonate precipitation (Wallmann etal., 2008; Dickens 2011). There is an important need to quantify the global subusrface methane transport. Recent compilation suggested 2.8 to 3.8 Tmol year-1 CH4 is transported via diffusion controlled settings globally (Egger et al., 2018). A complete oxidation of this diffuisve methane flux (Luff et al., 2004), along with upward DIC input from deep sediments and organic matter degradation coupled to sulfate reduction can result in a DIC flux to the water colum averaging 6.5 Tmol year-1 DIC (Akam et al., 2020). Future studies need to include advective settings, where methane enters the water column and undergoes aerobic oxidation, contributing to oxygen consumption and ocean acidification (Biastoch et al., 2011; Boetius and Wenzhöfer, 2013; Boudreau et al., 2015). Further, our current estimates of marine methane dynamics is largely confined to data from continental margins and methane dynamics in deep sea is largely uncontrained (Akam et al., 2020; Boetius and Wenzhöfer, 2013). Citations: Boudreau, B. P., Luo, Y., Meysman, F. J., Middelburg, J. J., and Dickens, G. R. (2015). Gas hydrate dissociation prolongs acidification of the Anthropocene oceans. Geophys. Res. Lett. 42, 9337A–9344A. Boetius, A., and Wenzhöfer, F. (2013). Seafloor oxygen consumption fuelled by methane from cold seeps. Nat. Geosci. 6, 725–734. doi: 10.1038/ngeo1926 Biastoch, A., Treude, T., Rüpke, L. H., Riebesell, U., Roth, C., Burwicz, E. B., et al. (2011). Rising Arctic Ocean temperatures cause gas hydrate destabilization and ocean acidification. Geophys.	Rejected. All release of CH4 is already accounted in the estimates of this Section
23435	68	20	69	45	This section on non-co2 feedbacks should be made consistent with other similar sections: (a) In Chapter 6 (Section 6.3.6, "Non-CO2 biogeochemical feedbacks"). (b) In Chapter 7 (Section 7.4.2.5, "Biophysical and non-CO2 biogeochemical feedbacks"). For example, the title of this section would more consistently be "Non-CO2 biogeochemical feedbacks". But also, consistent language and symbols (e.g. alpha for feedback parameter) should be used. [Daniel Lunt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The section title is extended by word 'biogeochemical'. .Parameter α is explicitly listed in the section header with reference to Section 7 and its notation is explicitly introduced in caption to Fig. 5.28. The section body and this figures are insured to be consistent with indicated Sections. In particular, the CH4 lifetime-climate feedback is listed with explicit statement that it is assessed in Section 6. We note that Fig. 5.28 already contains material from Sect. 6 with appropriate reference

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
23443	68	20	69	45	Figure 5.28 shows that there is an assessed range for alpha (the feedback parameter), for each of "permafrost CH4 response to climate", "wetlands CH4 response to climate", "land N2O response to climate", and "ocean N2O response to climate". However, in the underlying text (Section 5.4.7), the only statement is "the combined additional radiative forcing arising from climate-CH4 and climate-N2O feedbacks of 0.05 ± 0.02 W m-2 °C-1 ...". It would be better if the individual values for each of these feedbacks was given in the text, as well as the combined total of 0.05, otherwise it is not possible to check that the total of 0.05 is consistent with the assessment of each feedback individually. [Daniel Lunt, 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
15967	68	22	68	28	There should be some reference to the potential of non-CO2 and CO2 feedback mechanisms to interact and thereby self-amplify. It is also established that the rate of change during the transition from one condition to another (i.e. Holocene to future Hothouse) is a function of the number of feedback mechanisms and the correlation between them, rather than the characteristics of each feedback mechanism. Even now, we do not have a full set of all the feedback mechanisms, much less the correlations between them. The paragraph is also contradictory - it first states that "the strength of many feedbacks can be estimated in a similar linear framework as for CO2," but the next sentence says that "non linearities may make these estimates state or scenario dependent." So, surely there is an extreme risk in assuming that a linear framework will exist, and as a minimum the paragraph should be clear about this rather than suggesting the default response is linear, when there is nothing to support this, and thereby implying that the situation in someway can be controllable. The paragraph should also make clear that by excluding these feedbacks then future predictions from ESM, when subjected to higher radiative forcing, are likely to be increasingly unreliable both in terms of the final equilibrium temperature rise and the time constants of any change. [Kevin Lister, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - the note is added
23437	68	22	68	28	It would be worth referencing Chapter 7; in particular, reference Section 7.4.1.1, and Section 7.4.2.5, where feedbacks are defined. It should be clearly stated here that the aim of Section 5.4.7 is to assess a feedback parameter, alpha, for CH4 (wetland emssion and permafrost thaw) and N2O related feedbacks. In the intro to this section it would be worth telling the reader that the definition of feedback parameter is given in Chapter 7 (Section 7.4.1.1, and Section 7.4.2.5). [Daniel Lunt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Parameter α is explicitly listed in the section header with reference to Section 7.
73207	68	25	68	25	Delete hyphen. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change was made.
127839	68	30	68	32	Table 5.7 changes remaining carbon budget units to CO2 whereas previous text is in units of C. This is not clear unless the reader goes to the footnotes to find units. Can C be used throughout? Why switch to CO2? [Trigg Talley, United States of America]	Noted - Units are consistent as much as possible . However, Table 5.7 uses units of C.
19995	68	42	68	42	"by with" to be corrected [philippe waldteufel, France]	Accepted - ""by with" was removed.
73209	68	47	68	47	Change 'as' to 'have' [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change was made.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
23439	68	57	68	57	In my understanding, rice farming is not a feedback, so I think that "(including rice farming)" should be removed. Changes in rice farming leading to changes in CH4 are an anthropogenic forcing on climate, not a feedback. [Daniel Lunt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. It is indicated that only CH4 production dependence on climate state gives rise to a feedback.
78519	68	57	68	57	Be careful what you define as a feedback – rice farming is not a feedback, it's an anthropogenic activity which can act as a forcing [Chris Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - see comment id 23439
88195	68	57	68	58	What is meant by "new" with respect to permafrost thaw - do you mean this has never happened before or hasn't happened in a particular place or do you mean it has not been considered in assessments. [Sharon Smith, Canada]	Accepted - it is indicated that 'new' means 'not exhibited for historical time interval'
127841	68	57	69	19	Could mention importance of nutrient loading to methane emissions as well. Methane scales with chlorophyll a in wetlands, lakes, and reservoirs. [Trigg Talley, United States of America]	Accepted - the sentence is added with appropriate references
4367	68	57	73	30	This is an important section in my view given the substantial uncertainties that still remain in terms of non-CO2 feedbacks and interactions. As highlighted, positive CH4-feedbacks may be considerable for a range of systems. I failed to find mention of wildfire CH4 emissions in this section - suggest this is at least mentioned as a source of uncertainty in terms of feedback processes (e.g. Reay et al 2018 Methane and global env. change). Similarly for freshwater systems, incl. reservoirs (though guess many of those constitute 'anthropgenic sources'). Finally, there is an interesting question here about sea ice retreat in the Arctic and a 'feedback' being increased exploitation of FFs (incl. clathrates potentially). Not sure where in AR6 such phyiscal-societal feedbacks should sit (if at all) but would be good for the authors to consider these and how this chapter integrates wiht others (incl WGII and WGIII). [David Reay, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. This is very important comment. Indeed, these two feedbacks were overlooked in the previous drafts. In FGD, the fire-CH4 feedback is quantified by an order-of-magnitude, and some note on CH4 release from freshwater reservoirs and from methane hydrates are added.
81025	69	1	69	1	Is reference to the role of waste lanfill deposits and increased CH4 emissions with temperature relevant here? Maybe a mention of their significance in realtion to natural sources here or elsewhere would be useful and worthwhile. [Jeffrey Philip OBBARD, Singapore]	Accepted - a note on landfills is added
96593	69	1	69	2	Could there another word be used instead of "positive"? The sentence taken for itself could be misunderstood written this way. [Nicole Wilke, Germany]	Rejected - word 'positive' is used here as a part of the commonly accepted term 'positive feedback'

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
31989	69	1	69	14	This discussion of wetlands has a rather elderly flavour (many cited papers written about 10 years ago) Maybe also look at Lunt, Mark F.; Palmer, Paul I.; Feng, Liang; Taylor, Christopher M.; Boesch, Hartmut; Parker, Robert J.. 2019 An increase in methane emissions from tropical Africa between 2010 and 2016 inferred from satellite data. Atmospheric Chemistry and Physics, 19 (23). 14721-14740. https://doi.org/10.5194/acp-19-14721-2019 and at the wider discussion of whether the warming will feed the warming in Nisbet et al 2016 (already cited in ref list). Also: Gedney, N., C. Huntingford, E. Comyn-Platt, and A. Wiltshire. "Significant feedbacks of wetland methane release on climate change and the causes of their uncertainty." Environmental Research Letters 14, no. 8 (2019): 084027. Zhang, Xiaoyan, Xiyan Xu, Gensuo Jia, Benjamin Poulter, and Zhen Zhang. "Hiatus of wetland methane emissions associated with recent La Niña episodes in the Asian monsoon region." Climate Dynamics (2020): 1-13. [Euan G. Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - some newer references are added
33351	69	4			Change: “(e.g., during El Niños)” by “(e.g., during El Niño episodes)”. [Guiomar Rotllant, Spain]	Accepted - change was made.
71711	69	6	69	14	The text here implies that CH4 from wetlands has a different effect on climate forcing than other forms of CH4. There is no difference between the RF due to CH4 from wetlands or from cows. Questions about how wetland sources may change still need to consider the effects of changes in the seasonal cycles in precipitation and the well recognised trend towards heavier but less frequent rainfall that can have structural effects on an ecosystem leading to differences for methanogenic and methanotrophic processes. These structural uncertainties are reflected in Melton et al (2013) where the intercomparison of wetland models showed that substantial disagreements are still there. [Martin Manning, New Zealand]	Rejected - no distinction between the RF of CH4 from wetlands and from other sources is made in the indicated lines
12687	69	10	69	11	Omission. Add reference: Gedney et al 2019 Env. Res Letts, vol 14, issue 8, article no 084027 Significant feedbacks of wetland methane release on climate change and the causes of their uncertainty [Nicola Gedney, United Kingdom (of Great Britain and Northern Ireland)]	Accepted
78521	69	11	69	14	the CH4 emissions response to CO2 is key. Is there really no literature update on this since AR5? Would be good to update figure 6.37 from last time with the multitude of drivers of CH4 wetland emissions. [Chris Jones, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - there is no important updates for this figure since AR5
88197	69	14	69	15	suggested revision: "Methane emissions from thermokarst ponds and wetlands resulting from permafrost thaw, may further…" I think this better reflects what is meant as permafrost thaw results in the formation of thermokarst ponds and wetlands. [Sharon Smith, Canada]	Accepted

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
89493	69	14	69	17	I paste here a section from SROCC Chapter 3 page 253 because while there are limitations to methane estimates, there is in fact a fair bit known and assessed in SROCC. It might be worth taking the range of numbers presented in SROCC and use them here, citing the SROCC chapter that put them together. It migh provide a bit more inforation that the sentence as currently written in your report. Text from SROCC: "In many of the dynamical model projections previously discussed, methane release is not explicitly represented because fluxes are small even though higher global warming potential of methane makes these emissions relatively more important than on a mass basis alone. Global models that do include methane show that emissions may already (from 2000 to 2012) be increasing at a rate of 1.2 Tg CH4 yr–1 in the northern region as a direct response to temperature (Riley et al., 2011; Gao et al., 2013; Poulter et al., 2017). A model intercomparison study forecast northern methane emissions to increase from 18 Tg CH4 yr–1 to 42 Tg CH4 yr–1 under RCP8.5 by 2100 largely as a result of an increase in wetland extent (Zhang et al., 2017). However, projected methane emissions are sensitive to changes in surface hydrology (Lawrence et al., 2015) and a suite of models that were thought to perform well in high-latitude ecosystems showed a general soil drying trend even as the overall water cycle intensified (McGuire et al., 2018). Furthermore, most models described above do not include many of the abrupt thaw processes that can result in lake expansion, wetland formation, and massive erosion and exposure to decomposition of previously frozen carbon-rich permafrost, leading to medium confidence in future model projections of methane. Recent studies that addressed some of these landscape controls over future emissions projected increases in methane above the current levels on the order 10–60 Tg CH4 yr–1	Accepted - a reference on SROCC Ch. 3 is included in the text. We note that SROCC assessment is basically based on information, which is already included in Fig. 5.28
16317	69	14	69	17	Glacial and permafrost melt and CH4 emissions from lakes, rivers and wetlands in high-latitude and altitude cryosphere may further contribute to a positive climate feedback (Lamarche-Gagnon et al., 2019; Wik et al., 2016; Zhang et al., 2020), hence causing a vicious spiral—an increase in CH4 emissions would lead to more warming, more thawing and even more emissions. Lamarche-Gagnon, G., Wadham, J.L., Sherwood Lollar, B., Arndt, S., Fietzek, P., Beaton, A.D., Tedstone, A.J., Telling, J., Bagshaw, E.A., Hawkings, J.R., Kohler, T.J., Zarsky, J.D., Mowlem, M.C., Anesio, A.M. and Stibal, M., 2019. Greenland melt drives continuous export of methane from the ice-sheet bed. Nature, 565(7737): 73-77. Wik, M., Varner, R.K., Anthony, K.W., MacIntyre, S. and Bastviken, D., 2016. Climate-sensitive northern lakes and ponds are critical components of methane release. Nature Geoscience, 9(2): 99-105. Zhang, L., Xia, X., Liu, S., Zhang, S., Li, S., Wang, J., Wang, G., Gao, H., Zhang, Z., Wang, Q., Wen, W., Liu, R., Yang, Z., Stanley, E.H. and Raymond, P.A., 2020. Significant methane ebullition from alpine permafrost rivers on the East Qinghai-Tibet Plateau. Nature Geoscience, 13(5): 349-354. [Xinghui Xia, China]	Rejected - unclear what is to be changed

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
58919	69	14	69	19	It could be worth acknowledging here that biological activity in the warming arctic (e.g. bacterial nutrient cycles) also makes these estimates of permafrost methane release uncertain and/or could potentially provide a further negative feedback. Schneider von Deimling et al. don't model this, but several studies address the potential for permafrost thaw to not only release stored methane, but also activate methanogens and methanotrophs. One example is Singleton et al. 2018 ISME https://doi.org/10.1038/s41396-018-0065-5, but there are others. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. A note on nutrient modification for this feedback is introduced.
31991	69	18	69	19	Pretty complex topic for this throwaway remark. Warmer tropical mid troposphere will have more water and thus the zone of high OH creation will be more intense. I skimmed Section 6.3 and also Chapter 6.3 but didn't find the comment. Maybe I was too quick but it might help to put a specific pointer to this evidence. [Euan G. Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - unclear what is to be changed
74315	69	21	69	45	Please add a discussion about how acidification is affecting N2O production and emissions. see Rees, A. P., Brown, I. J., Jayakumar, A., and Ward, B. B.: The inhibition of N2O production by ocean acidification in cold temperate and polar waters, Deep-Sea Research Part II-Topical Studies in Oceanography, 127, 93-101, 2016 and Breider, F., Yoshikawa, C., Makabe, A., Toyoda, S., Wakita, M., Matsui, Y., Kawagucci, S., Fujiki, T., Harada, N., and Yoshida, N.: Response of N2O production rate to ocean acidification in the western North Pacific, Nature Climate Change, 9, 954-958, 2019. [Hermann Bange, Germany]	Accepted
23441	69	22	69	23	"limited evidence, high agreement". This doesn't sound like the usual IPCC calibrated language for confidence or likelihood. [Daniel Lunt, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - this is one of possibilities figured in Fig. 1 in "Guidance Note … on Consistent Treatment of Uncertainties"
4243	69	23	69	25	N2O emission in the generally strongly nutrient-limited Arctic ecosystems is related to severe erosion by permafrost thaw, which leads to fast mineralization of permafrost carbon while the vegetation is strongly reduced by the erosion process and cannot take up the liberated nitrogen. There is growing evidence that N2O emission by abrupt permafrost thaw is a much more common feature of Arctic ecosystems with permafrost thaw than previously has been assumed: Abbott BW, Jones JB (2015) Permafrost collapse alters soil carbon stocks, respiration, CH4, and N2O in upland tundra. Global change biology 21 (12):4570-4587; Wilkerson J, Dobosy R, Sayres DS, Healy C, Dumas E, Baker B, Anderson JG (2019) Permafrost nitrous oxide emissions observed on a landscape scale using the airborne eddy-covariance method. Atmospheric Chemistry and Physics 19 (7):4257-4268 I suggest to add after the period in line 25: There is growing evidence that N2O emissions are related to disturbance of organic-rich permafrost soils by abrupt thaw processes and erosion, and are a more common feature in of generally nitrogen-limited Arctic ecosystems than previously thought (Abbott and Jones, 2015; Voigt et al., 2017; Wilkerson et al., 2019). [Jacobus (Ko) van Huissteden, Netherlands]	Rejected - already assessed in the existing text
73211	69	24	69	24	Change 'boreal and arctic' to 'Boreal and Arctic'. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change was made.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
58921	69	34	69	34	Change "recover" to "return to baseline." 'Recover' has a positive connotation that isn't intended here. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. However. The wording is put in 'partly recover' because it is unclear how complete this return would be
41681	69	37	69	37	good to mention/repeat the atmospheric lifetime of N2O at this point [Katharina Meurer, Sweden]	Accepted
12689	69	43	69	43	Addition of results from Gedney et al 2019 (above) may change the number quoted. See next comment [Nicola Gedney, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - the reference to the paper is added to the text
31993	69	43			This dismisses methane and N2O "warming feeding warming" feedbacks as trivial? Having just run two large aircraft campaigns in Africa (upper Congo) and S America (Upper Amazon) and found huge fluxes, I'd be very scared about making such a confident assertion. Moreover it is imposisble in tropical settings to split anthropogenic and 'natural' feedbacks: more natural growth = more cows (African wetlands are full of cows) and also more dry season crop waste and grass/leaf litter fires. [Euan G. Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - local scale measurements can not be readily extrapolated on global scale
66671	69	48	70	15	I don't find this section at all convincing. (1) The section is asymmetrically written, so the table writes of timescales over which the systems in question are irreversible, while neglecting to say whether they are subsequently reversible, or whether it is the biological, chemical, or physical climate that is permanently affected. (I had the same gripe with a couple of items in table 12.4 of AR5.) This seems to confuse things - I'm not sure what "irreversible at multidecadal scales" means - does it mean that boreal forest dieback is reversible on centennial timescales? If so, it should say so. Also, clathrate release may be chemically irreversible, but most the radiative forcing would be short-lived because of methane's short residence time: i.e. the system is biochemically irreversible, but is a transient climate phenomenon. Plus, the fact that virtually all these things have such a small effect on concentrations (with the possible exception of permafrost) seems to sit awkwardly beside the hype around tipping points. If the most alarming irreversible phenomena in the carbon cycle amount to a few ppm CO2 per annum, it suggests the text really ought to reflect the striking linearity of the response, rather than to emphasise, wherever it can, nonlinearity. [Dave Frame, New Zealand]	Accepted - I note is introduced in the Sect. 5.4.8. header that all cases which are figured as irreversible at particular timescale, may or may not be reversible at a larger timescale depending on forcing trajectory and system stability properties.
83015	69	48	73	32	Needs to include a section on 'Drying / burning of peat wetlands' loss of carbon. Plenty of research on this topic in the literature. [Dan Zwartz, New Zealand]	Rejected - this is not a global scale tipping point
30007	69	50	69	50	Section 5.4.4.1 -> 5.4.5.5? [Michio Kawamiya, Japan]	Accepted
28243	69	50	70	9	This is just one of several paragraphs in different Chapters of the report, where abrupt change, tipping points, irreversibility, bifurcations etc are defined (again). These are many concepts at once and the definitions sometimes don't match (for example, Table 4.10 distinguishes irreversibility from tipping points). Why not combine Table 5.6 with Table 4.10? The caption of Table 5.10 reads much more general than the content (i.e. cases in Table 4.10 do not reappear here). One could reduce the number of concepts mentioned by dropping the word bifurcation. A bifurcation is typically just a metaphore more than a real result from complex models, so maybe a more phenomenological vocabulary (abrupt, tipping) is sufficient. [Sebastian Bathiany, Germany]	Rejected - Table 5.6 is specific for this Chapter and includes columns not applicable to Table 4.10. The merged Table would look awkward.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
73213	69	55	69	55	Change 'tropical and boreal' to 'Tropical and Boreal' [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted
89495	69		69		Table 5.6. It is better to list both carbon dioxide and methane as a consequence of permafrost thaw; estimates could be refined more with what was reported in SROCC Chapter 3, CO2 was reported as up to 240 Gt C release; see main text and ES statements from SROCC [Edward Schuur, United States of America]	Accepted - the estimate is refined
36419	70	1	70	1	Table 5.6 Key regions should be expanded to inclue the Arctic Ocean and eastern Boundary Current upwelling regions. [Adrienne Sutton, United States of America]	Accepted
30605	70	1	70	1	Table 5.6 Key regions should be expanded to inclue Arctic Ocean and eastern Boundary Current upwelling regions. [nina bednarsek, United States of America]	Taken into account - see the reply to comment id 36419
41683	70	1	70	1	It is not only the release of carbon from permafrost, but as recent research has shown, N2O and CH4 are also being released when permafrost thaws (see my comments above). Moreober, Table 5.6 states "Biogenic emissions from permafrost", which I think is much more precise [Katharina Meurer, Sweden]	Accepted - word 'carbon' is replaced by 'greenhouse gases'
106173	70	3	70	3	but note that the definition of a tipping point in the foundational Lenton et al., 2008 does not require irreversibility (see supplement of that paper) [Wolfgang Lucht, Germany]	Rejected - it is unclear what is to be changed
21133	70	3	70	4	This statement confuses irreversibility with hysteresis -- and also confuses the definition of a bifurcation point. Suggested amendment: "These cases are described as displaying hysteresis when the bifurcation leads to a qualitatively new state that is not destabilised by the forcing or other parameter passing back through the level that triggered the bifurcation. The cases are described as displaying irreversibility when there is no plausible value of the forcing that will trigger a return to the previous state or if the transition back to the previous state would take much longer than the timescale of interest." [Steven Lade, Sweden]	Accepted
73215	70	5	70	5	Insert 'Century' after 21st. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change was made, with "century" (not capital) to comply with TSU style guidelines.
90097	70	12	70	17	This statement uploaded by Ted Schuur on behalf of a graduate class that reviewed the chapter (this was communicated with IPCC TSU in advance): Table 5.6 column 5 compares a rate of change in CO2 or CH4 between tipping points. However, the useful nature of this column for making a comparison seems limited since three of the entries are unquantified and the only two with quantities measure different carbon compounds. If the goal of this column is a comparison between tipping points it does not seem helpful. [Edward Schuur, United States of America]	Rejected - unclear what is to be changed
78523	70	12			table 5.6 is good, but overlaps to some extent with synthesis in chapter 4. Please liaise with chapter 4 (Chris Jones, John Dunne LAs) as to what content is covered where. [Chris Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - the contact with the Chap.4 Las is set, and the consistency between Chap. 4 and 5 on this respect is insured
26923	70	14	70	14	Greening of subsaharan africa is listed as an example in the text but doesn't appear in table 5.6. Would it be possible to add it? [Eric Brun, France]	Rejected - this is not a global scale tipping point

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
52239	70	14	70	15	Suggest adding : "Moreover, new time-series are established in the Arctic such as in the Arctic outflow water in the East Greenland Current (Chierici et al. submitted). Yasunaka et al 2018 summerize air-sea CO2 fluxes in the Arctic Ocean using a combination of data and self-organizing maps. " [Agneta Fransson, Norway]	Taken into account - see the reply to comment id 36419
52257	70	14	70	15	perhaps add in Table tipping points: add increased freshwater due to glacial meltwater. Region: Arctic and Antarctica/Southern Ocean, [Agneta Fransson, Norway]	Rejected - this is not a global scale tipping point
88199	70	14			Table 5.6 "Clathrates" is used in the table but throughout the text "hydrate" is used. Consistency in terminology might be good. [Sharon Smith, Canada]	Accepted - term 'hydrates' is replaced by 'clathrates'
15969	70	15	70	15	Table 5.6: The table entry, Methane release from clathrates should be further clarified. The comment about the "ocean floor" should be more specific and state "ocean floor and shallow waters of the Arctic Ocean." In effect, the existing choice of words excludes the vast shallow waters of the Arctic ocean. This has a direct bearing on the next column which incorrectly states the risk is "unquantified, but very likely small," when the reality is that the large methane releases are inevitable from these regions and are already evident in the measurements from the greenhouse gas monitoring network and the NOAA satellite measurements. Furthermore, as heat flows into the Arctic Ocean primarily in the ocean surfaces then it puts the shallow subsea permafrost directly in the contact with an ongoing heat source and will lead to its rapid destablisation, see papers from Shakova, and Kennett. This also contradicts the statements following in section 5.5 which acknowledge the risk of methane related feedbacks. [Kevin Lister, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - the existing wording is 'ocean shelf' (which is accurate for the delivered assessment) rather than 'ocean floor'
78525	70	15	70	15	in table 5.6 why does the permafrost emissions line focus on CH4 emissions when fig 5.28 implies the CO2 term is more important? [Chris Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - table 5.6 is extended by the respective estimate for CO2 emissions
78527	70	15	70	15	in table 5.6 clathrates line says “unquantified” but 5.4.8.3 says <5 TgCH4 yr-1. [Chris Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - the number is added into the Table, as well as the respective explanation is added to the text body
106175	70	15	70	15	the text states boreal die-back C loss could be up to 100 PgC (cf. page 71, line 22). Is that "likely small", as stated in the table? See comment on page 71 line 18-22 for why the magnitude may be underestimated [Wolfgang Lucht, Germany]	Accepted - the sentence in the text is clarified: the figured number mostly reflects the tropical forest dieback
54995	70	15	70	15	On table 5.6, permafrost carbon emissions are reported only for CH4, while for other tipping points both CH4 and CO2 are specified. The majority of permafrost carbon would be released as CO2. The accompanying text on page 71 does give a more complete description. [Nancy Hamzawi, Canada]	Taken into account - see reply to comment id 58861
21853	70	15	70	16	Table would be clearer if first row and first column text were bolded. [Peter Thorne, Ireland]	Editorial - this kind of issues will be taken care of during the production phase, which includes professional copy-editing.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
54997	70	15	70	18	Row 4 of table. While I understand affected areas are/will be within the pan-Arctic, thaw and development of thermokarst lakes can only occur where there is ice-rich permafrost and the topography is such that water cannot drain away as ice-rich permafrost thaws. So, I'd suggest just "regions of ice-rich permafrost", as there is such permafrost on Qinghai-Tibet Plateau, but the plateau is not in the pan-Arctic. [Nancy Hamzawi, Canada]	Rejected - thermokarst lakes formation is widespread in the pan-Arctic. For instance, its widely observed in the Russian north
90095	70	20	73	30	This statement uploaded by Ted Schuur on behalf of a graduate class that reviewed the chapter (this was communicated with IPCC TSU in advance): Within section 5.4.8 there are 6 subsections. 4 of these subsections (5.4.8.1-4) describe certain tipping points represented in Table 5.6 and discussed throughout the section. The other two subsections do not describe specific tipping points. It would be useful to further separate the tipping point subsections so that, especially section 5.4.8.5, is not confused with being yet another tipping point. [Edward Schuur, United States of America]	Accepted - the former Sects. 5.4.8.1-5.4.8.4 is gathered as subsections of an arching section. The former Sects. 5.4.8.5 and 5.4.8.6 are renumbered accordingly
106515	70	20			WGII ch2 "Terrestial and freshwater ecosystems" has sections on observed and projected changes in tree mortality that overlap with section 5.4.8.1. Care should be taken to ensure consistency between WGs in messages and uncertainty assessments of those messages. [camille parmesan, France]	Not Applicable - WGII drafts are not available for WGI members
132095	70	22	70	31	In line with tropical rain forest tip. Point identification, it would be beneficial to mention: Thomas E. Lovejoy and Carlos Nobre, Amazon Tipping Point, Science Advances 21 Feb 2018: Vol. 4, no. 2, eaat2340 DOI: 10.1126/sciadv.aat2340. This is especially important because of the newest tresholds in reaching tipping point in Amazon by reaching just less than 20% of the overall canopy. (previous treshold was 40%: Sampaio, G., Nobre, C., Costa, M. H., Satyamurty, P., Soares-Filho, B. S., and Cardoso, M. (2007), Regional climate change over eastern Amazonia caused by pasture and soybean cropland expansion, Geophys. Res. Lett., 34, L17709, doi:10.1029/2007GL030612.) [Mikulas Cernota, Slovakia]	Rejected - this paper is an editorial one, and the cited threshold is obtained in a very heuristic manner
29189	70	22	71	16	his paragraph also ignores the growing high risk of climate-induced fire. See Brando et al. 2020. Sci. Adv. 2020; 6 : eaay1632 10 January 2020 (it was submitted and accepted in 2019). While it may be true that "the real world has a much greater plant trait diversity which conferes additonal resilience" to heat and water stress, these traints confer little protection to increased risk of fire in habitats where the plants have not been exposed to fire in the past and therefore have not evolved resilience to fire, which is the case for many of the regions of the Amazon where fire has spread due to a combination of drier conditions and land use change. [Eric Davidson, United States of America]	Accepted - to make the statements more clear, word 'enhancement' is added to the former l. 29 in p. 70. The suggested paper is added to the chapter reference list
115391	70	22			Insert 'in models' after 'feedbacks'. [Gillett Nathan, Canada]	Accepted
13441	70	24	70	24	Eliminate the extra space between parenthesis and comma. [Maria Amparo Martinez Arroyo, Mexico]	Accepted - change was made.
23711	70	24	70	24	2017) ,and…should be 2017), and [Massimo Lupascu, Singapore]	Accepted - change was made.
51153	70	24	70	24	"2017) ,and temperate..." should be "2017), and temperate..." [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change was made.
33353	70	24			Change: "…2017) ,and…" by"… 2017), and…". [Guiomar Rotllant, Spain]	Accepted - change was made.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
81551	70	25	70	32	Page 5-70: section 5.4.8.1 Forest dieback: The suggested drivers causing transition may not complete here. Transition can also relate to vegetation–atmosphere feedback. One of the possible mechanisms was proposed by Zemp et al. 2017 (DOI: 10.1038/ncomms14681), who illustrated that self-amplified Amazon forest loss could be intensified through atmospheric circulation cascading effects with dry-season intensification. But due to the complexity of the related processes, the question of how to quantify the impact of vegetation feedback on the tipping point, in particular when considering the impacts from elevated CO2 on stomatal conductance, or/and land use changes, may still be largely unanswered. ESMs may show some potentials to answer this question, but may be constrained by the uncertainties in simulating land-atmosphere interaction and vegetation dynamics in their land surface scheme. I would suggest to review the advancement since AR5 for this aspect, or add this point to the knowledge gaps (section 5.4.8.6). [Wu Minchao, Sweden]	Accepted
13439	70		70		Unify table 5.6. It is recommended to write the first word in capital letter. [Maria Amparo Martinez Arroyo, Mexico]	Editorial - this kind of issues will be taken care of during the production phase, which includes professional copy-editing.
116453	70		70		For possible tipping points and abrupt change, is it possible to be explicit on unerlying lines of confidence, knowledge gaps, and potential conditions for occurrence of abrupt change (and confidence) (all in the table?) This could also buildon SRCCL, SROCC, SR15. It is hard to understand what is new here compared to AR5 and previous assessments. [Valerie Masson-Delmotte, France]	Accepted - the sentences are clarified and the information sources are made traceable
58861	70		70		Table 5.6. Shouldn´t the biogenic emissions from permafrost thaw and development of thermokarst lakes also include CO2 emissions? In page 71 row 29 CO2 emissions due to permafrost thaw by 2100 are estimated to be up to 199 PgC (under RCP8.5). [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted
28247	70				Table 5.6: Why is boreal forest loss listed as "likely irreversible"? This row seems to have no corresponding paragraph in the text. [Sebastian Bathiany, Germany]	Rejected - in the existing text there is sentence 'Tropical and boreal dieback tipping trajectories are slow, associated with a multi-decadal development timescale'
115389	70				Table 5.6. If using likelihood language in the phases 'likely small', 'very likely small' etc. you should somewhere quantify what you mean by 'small'. [Gillett Nathan, Canada]	Accepted - in the Table caption it is explicitly indicated that word ‘small’ in the third and fifth columns means ‘smaller than quantified items in the same column’.
58923	71	4	71	16	Many of the studies cited here do not address the potential increase in wildfires that is associated with climate change (mentioned earlier in this paragraph and earlier in the chapter). Consider mentioning that the estimated amount of carbon released by die-back is therefore underestimated (based on the absence of fire activity) as much as it is overestimated (based on the lack of accounting for carbon absorbed by new savanna grass, see pg 71, lines 15-16). [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account - see reply to comment id 29189

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
30009	71	6	71	16	It is somewhat strange to use the concept of emergent constraint for estimating carbon emission following a tipping event. Tipping elements are abrupt, non-linear phenomena that do not follow such patterns indicated by emergent constraints established based on those data measured during the period of gradual, linear changes. Cox et al. (2013) do mention that Amazonian dieback is unlikely, though only conditionally. In this Section 5.4.8.1, aren't we trying to estimate carbon emission when Amazonian dieback ever happens? This part (lines 6-16) is confusing so I would suggest omitting it. [Michio Kawamiya, Japan]	Accepted - the sentence on emergent constraints in put in a more tentative fashion. However, it is kept in the text because it still may be instructive
115393	71	6		8	Can an emergent constraint approach be applied to a variable which exhibits a tipping point (noting that this discussion is in a section labelled 'Possible abrupt changes and tipping points'? I would think not. In this case I think that tropical land carbon change is not really behaving as a tipping point, in which case flag this in the discussion. [Gillett Nathan, Canada]	Taken into account - see reply to comment id 30009
73217	71	10	71	10	See previous comments: delete hyphen. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted
54999	71	11	71	15	This paragraph provides an estimate of carbon emissions associated with tropical forest dieback. The estimate is based on values from the Amazon basin that are extrapolated to the entire tropics. This seems to be an unnecessary crude approach, given the wealth of literature on the subject, including the publications cited in the same section (e.g. Cox, Peter M., David Pearson, Ben B. Booth, Pierre Friedlingstein, Chris Huntingford, Chris D. Jones, and Catherine M. Luke. 2013. “Sensitivity of Tropical Carbon to Climate Change Constrained by Carbon Dioxide Variability.” Nature 494 (7437): 341–44.) [Nancy Hamzawi, Canada]	Rejected - we did not find any particular references. The Cox et al.'s (2013) paper estimates the carbon release from the tropical forest dieback employing emergent constraints framework, which also has a number of assumptions (see also comments with ids 30009 and 115393). It is already stated that this paper likely overestimates such release. Thus, the estimate figured out here is an important additional line of evidence. It is clearly stated in the text this this estimate is an upper bound as well (despite it is smaller than the Cox et al's (2013) estimate).
22057	71	13			This seems entirely the domain of WG3. Why is this included here? What is the physical science basis insight that it brings? This is very unclear presently. If retained the section needs to be substantively rewritten to orientate to the WG1 charge. [Peter Thorne, Ireland]	Rejected - this material is included to assess possible release of greenhouse gases from the biogeochemical tipping points. It is not meant to assess the impact of climate changes on terrestrial ecosystems.
41685	71	15	71	16	Is there a number for this? As the previous sentence seems to present a value that is known to being too high, it would be good to mention the "real" value here [Katharina Meurer, Sweden]	Rejected - the 'real' value is unknown
106177	71	18	71	22	I would suggest being less confident that the C release from potential boreal forest is likely small; this is an artifact of models not having any temperature stress mortality of leaf tissues or sapling survival limitations (a driver of some forest decline in Canada: failure to reproduce due to top-soil drought), not to speak of changes in fire frequency; models tend to "like" warmer temperatures as a stimulant of vegetation growth - mortality and disturbance could lead to large C releases; but the uncertainty is high, both in modelling and in ecological understanding [Wolfgang Lucht, Germany]	Accepted - relevant reference on Reyer et al.'s (2015) review is added

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
41687	71	18	71	22	The last sentence of the paragraph is confusing and I suggest to re-phrase it. It would be easier to understand if "not more" could simply be replaced by "less" - though I am not sure if the content of the sentence is still correct with that [Katharina Meurer, Sweden]	Rejected - the sentence would change the meaning if accepted.
621	71	18		22	Does this take into account the loss of soil C? If not, the 100 PgC by 2100 should be revised. See this recent paper: Walker, X. J., Baltzer, J. L., Cumming, S. G., Day, N. J., Ebert, C., Goetz, S., . . . Mack, M. C. (2019). Increasing wildfires threaten historic carbon sink of boreal forest soils. Nature, 572(7770), 520-523. doi:10.1038/s41586-019-1474-y [Vicca Sara, Belgium]	Accepted - a note on carbon release from soil due to wildfires is added
132097	71	20	71	22	Regarding the discussion of uncertainties in changing CO 2 concentration and quantifying tree mortality as examples – pls consider study of Reyer, C., Brouwers, N., Rammig, A., Brook, B., Epila, J., Grant, R. et al. (2015) Forest resilience and tipping points at different spatio-temporal scales: approaches and challenges. Journal of Ecology, 103, 5–15. [Mikulas Cernota, Slovakia]	Accepted - this very informative paper is added to the citation list
78529	71	21	71	21	on what do you base a statement that dieback would be slow? A mechanism involving increased fire could cause a rapid loss of forest [Chris Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - the reference on Reyer et al. (2015) is added
21855	71	27	72	3	This is a very long and involved paragraph and may be clearer if it could be broken into several paragraphs instead [Peter Thorne, Ireland]	Accepted - the Section is subdivided into paragraphs
4245	71	27	72	3	Given the remark on N2O emission from permafrost on page 69, it would be logical to mention N2O emissions here as well next to CO2 and CH4. See also my previous remark [Jacobus (Ko) van Huissteden, Netherlands]	Accepted - note on N2O release is added
81175	71	28	71	32	We estimated CO2 emission due to permafrost degradation, 34-86 PgC in RCP8.5 at 2100, and 20-46 PgC in RCP2.6 at 2100. This estimation is based on the permafrost thaw calculated by state-of-the-art global land surface model (a component of MIROC) with different GCM forcings, combined with a simple GHG emission model. Yokohata T, Saito K, Ito A, Ohno H, Tanaka K, Hajima T, Iwahana G (2020) Future projection of climate change due to permafrost degradation with a simple numerical scheme, under review in in Earth and Planetary Science. [Tokuta Yokohata, Japan]	Accepted - this estimate is added to the text
38515	71	28	71	32	unclear sentence [Siv K Lauvset, Norway]	Accepted - the sentence is clarified
73219	71	29	71	29	Move (to start of following line. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted
58841	71	29	71	29	For clarification, I think it would be good to name the RCP8.5 when mentioning the high warming scenario. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted
90101	71	30	71	30	This statement uploaded by Ted Schuur on behalf of a graduate class that reviewed the chapter (this was communicated with IPCC TSU in advance): The citation for Koven et al. 2015 is incorrect. Koven et al. 2015 has 3 different citations in the references page. This citation should be: Koven, C. D., Schuur, E. A. G., Schädel, C., Bohn, T. J., Burke, E. J., Chen, G., et al. (2015c). A simplified, data- constrained approach to estimate the permafrost carbon–climate feedback. Philos. Trans. R. Soc. A Math. Phys. Eng. Sci. 373, 20140423. doi:10.1098/rsta.2014.0423. [Edward Schuur, United States of America]	Accepted

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
106179	71	31	71	31	Citing has to be selective, but a very systematic evaluation of longer-term effects was Schaphoff et al., Environm. Res. Lett., 8, 014026,doi: 10.1088/1748-9326/8/1/014026, 2013. [Wolfgang Lucht, Germany]	Accepted
90099	71	31	71	31	This statement uploaded by Ted Schuur on behalf of a graduate class that reviewed the chapter (this was communicated with IPCC TSU in advance): The citation for MacDougall and Knutti, 2016 is incorrect. MacDougall and Knutti, 2016 have 3 different citations in the references page. This citation should be: MacDougall, A. H., and Knutti, R. (2016b). Projecting the release of carbon from permafrost soils using a perturbed parameter ensemble modelling approach. Biogeosciences 13, 2123–2136. doi:10.5194/bg-13-2123-2016. [Edward Schuur, United States of America]	Accepted
58925	71	34	71	35	"These emissions also depend on CO2 pathways." Kleinen and Brovkin (2018) are specifically referring to the model of CO2 accumulation in the atmosphere (for example, RCP4.5 vs. RCP8.5), and how atmospheric CO2 accumulation can affect permafrost carbon release independent of climate. This could be more clear here, because the sentence makes it sound like these "CO2 pathways" are in the permafrost. Consider: "These emissions also depend on the predicted amount of CO2 in the atmosphere, based on the emissions model." [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted
89497	71	35	71	35	Does 'CO2 pathway' here refer to SSP scenario or something else? [Edward Schuur, United States of America]	Taken into account - see reply to comment id 58925
89499	71	35	71	36	Natali 2019 Nature Climate Change is a new updated reference here. The sentence structure is a bit clunky, maybe reword 'The above estimates might be underestimated…" [Edward Schuur, United States of America]	Accepted
58843	71	35	71	36	I suggest to add that emissions might be underestimated also because they do not include emissions from abrupt permafrost thaw processes. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected - emissions from abrupt thaw processes is a goal of this section
38517	71	36	71	38	Clarify that only one CMIP6 model includes permafrost carbon at the time of writing. Without that information the sentence is unclear. [Siv K Lauvset, Norway]	Accepted - the sentence is revised
58855	71	36	71	38	Is there are reference for the estimate of 29PgC released from permafrost soils? [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected - no reference is available. All calculations are made by the responsible contributing author (S.N. Denisov) and are not published yet
88201	71	37	71	37	Permafrost-containing soil - poor terminology. Permafrost is soil (or rock) at a temperature below 0C - it is a thermal condition of the soil. Suggested revision "….estimates losses up to 27 PgC from permafrost affected soils…." [Sharon Smith, Canada]	Accepted

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
89501	71	40	71	40	This is a repeated comment to Table 5.6. The new Turetsky article is the only one that has come since SROCC and so is new here (wasn't accepted by SROCC). I paste here a section from SROCC Chapter 3 page 253 because while there are limitations to methane estimates, there is in fact a fair bit known and assessed in SROCC. It might be worth taking the range of numbers presented in SROCC and use them here, citing the SROCC chapter that put them together. It migh provide a bit more inforation that the sentence as currently written in your report. Text from SROCC: "In many of the dynamical model projections previously discussed, methane release is not explicitly represented because fluxes are small even though higher global warming potential of methane makes these emissions relatively more important than on a mass basis alone. Global models that do include methane show that emissions may already (from 2000 to 2012) be increasing at a rate of 1.2 Tg CH4 yr–1 in the northern region as a direct response to temperature (Riley et al., 2011; Gao et al., 2013; Poulter et al., 2017). A model intercomparison study forecast northern methane emissions to increase from 18 Tg CH4 yr–1 to 42 Tg CH4 yr–1 under RCP8.5 by 2100 largely as a result of an increase in wetland extent (Zhang et al., 2017). However, projected methane emissions are sensitive to changes in surface hydrology (Lawrence et al., 2015) and a suite of models that were thought to perform well in high-latitude ecosystems showed a general soil drying trend even as the overall water cycle intensified (McGuire et al., 2018). Furthermore, most models described above do not include many of the abrupt thaw processes that can result in lake expansion, wetland formation, and massive erosion and exposure to decomposition of previously frozen carbon-rich permafrost, leading to medium confidence in future model projections of methane. Recent studies that addressed some	Rejected - unclear what is to be changed
73221	71	40	71	41	Remove split of numbers and units across line break. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial - this kind of issues will be taken care of during the production phase, which includes professional copy-editing.
81177	71	40	71	42	We estimated CH4 emission due to permafrost degradation, 2300-7560 TgCH4 in RCP8.5 at 2100, and 218-921 TgCH4 in RCP2.6 at 2100. This estimation is based on the permafrost thaw calculated by state-of-the-art global land surface model (a component of MIROC) with different GCM forcings, combined with a simple GHG emission model. Yokohata T, Saito K, Ito A, Ohno H, Tanaka K, Hajima T, Iwahana G (2020) Future projection of climate change due to permafrost degradation with a simple numerical scheme, under review in in Earth and Planetary Science. [Tokuta Yokohata, Japan]	Accepted - this reference has been added
88203	71	41	71	41	Why not say "all permafrost thaw" rather than "aburupt thaw processes" Thermokarst appears to have been associated with abrupt thaw but this includes a wide range of processes which can be gradual. It is usually the response to thaw (e.g. slope failure) that is abrupt rather than the thaw itself. [Sharon Smith, Canada]	Accepted

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
73223	71	41	71	42	Remove split of numbers and units across line break. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial - this kind of issues will be taken care of during the production phase, which includes professional copy-editing.
55001	71	42	71	42	Suggest caution in interpretation of results from Turetsky et al. here. The modelling in that paper relies on a number of assumptions, and on previous modelling that relies on the accuracy of the circum-Arctic representation of ground ice. This draft IPCC report itself cites "low confidence because of limited evidence" in the amount of ground ice in permafrost globally (p. 74 Ch. 9), and the ground ice dataset is only one input in the Turetsky modelling. Issues with accuracy of the representation of ground ice conditions in some areas of Canada have recently been highlighted in a paper by O'Neill et al. 2019 (https://www.the-cryosphere.net/13/753/2019/). It is not possible to assess the accuracy of quantitative estimates of carbon loss following "abrupt thaw" from this paper, due to the lack of compiled volumetric ground ice observations around the world. [Nancy Hamzawi, Canada]	Accepted - a note on assumptions in (Turetsky et al., 2020) is added
73225	71	43	71	44	Remove split of numbers and units across line break. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial - this kind of issues will be taken care of during the production phase, which includes professional copy-editing.
8851	71	43	71	45	Please use the the value of methane-lifetime feedback factor assessed in Chapter 6 for consistency. [Vaishali Naik, United States of America]	Accepted - the methane-lifetime feedback factor is set to 1.34±0.04
31995	71	44			This permafrost methane discussion is prejudiced and needs to be better grounded in observations. Berchet et al, France et al. and Fisher et al (cited above) showed Arctic emissions are currently small. In most cases soil methanotrophy has a strong impact and takes up methane released by underlying thawing permafrost. In most years since 2008, despite strong climate warming, the Arctic has lagged the global methane growth, not led it. Even in 2007, when Arctic CH4 rose fast, this was probably due to strong summer winds with methane likely blown north from neighbouring boreal wetlands. For methanotrophy and thaw see: Oh, Youmi, Qianlai Zhuang, Licheng Liu, Lisa R. Welp, Maggie CY Lau, Tullis C. Onstott, David Medvigy et al. "Reduced net methane emissions due to microbial methane oxidation in a warmer Arctic." Nature Climate Change 10, no. 4 (2020): 317-321. See also the NOAA sine lat map (reproduced in Nisbet et al, 2019.) [Euan G. Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - the discussions is not prejudiced. Moreover, we show that even the assessed upper limits do not lead to catastrophic consequences. It is clearly stated in the end of the new sect 5.4.8.2 former sect. 5.4.8.5)
88205	71	46	71	49	Note that permafrost aggradation is also a slow process. [Sharon Smith, Canada]	Not applicable - this sentence has nothing to do with the existing text
89503	71	48	71	48	There is a switch in this sentence in using the 'likely' confidence language, which has a precise meaning to IPCC. Not sure if that was indended here? [Edward Schuur, United States of America]	Rejected - the statement is intentional
89505	71	51	71	51	It should be made clear that modeling evidence doesn't rule this out, but no reliable field evidence actually exists. Also - its probably better to get rid of the term compost-bomb; jargon term and kind of inflammatory [Edward Schuur, United States of America]	Accepted - term 'compost-bomb' is explained in plain words and it is stated that this effect is not exhibited in experiments
115395	71	51			Can you add some more text to explain the compost-bomb instability? Is it potentially important? [Gillett Nathan, Canada]	Taken into account - see reply to comment id 89505

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
88207	71	55	71	55	hydrates could also be below permafrost depending on temperature-pressure conditions [Sharon Smith, Canada]	Accepted - the sentence is revised
115397	72	5		6	Will this give rise to an abrupt change and/or tipping point? Assess this here too. [Gillett Nathan, Canada]	Accepted - the assessment is made
17049	72	9	72	45	The above comments could be incorporated as appropriate to sections 5.4.8.3 Methane release from clathrates and 5.4.8.4 Ocean acidification and de-oxygenation [Sajjad Abdullajintakam, United States of America]	Rejected - unclear what is to be changed
10255	72	11	72	11	Beaudoin et al (2014) 'Frozen heat: A UNEP global outlook on methane gas hydrates', Vol. 1 quotes a range of 1-10,000 Gt C with a mid-range estimate of 5,000 Gt C. I suggest this estimate should be included. See https://www.unenvironment.org/resources/report/frozen-heat-global-outlook-methane-gas-hydrates-volume-1. [Chris Vivian, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - this estimate is traced to the publications from 1980's and even older. Now these estimates are superseded by newer ones, and are of historical importance
15971	72	11	72	24	The assertions made in this section that "Abrupt change is very unlikely for the permafrost-embedded subsea clathrates" and "This makes the 'clathrate gun' hypothesis extremely unlikely to be relevant on the timescales we are considering in this report" are not fully justified and are in conflict with section 7.4.4.1.1 which warns "Because many factors contribute to polar amplification, projections of polar warming are inherently more uncertain that global mean warming." Thus, the assertions made do not consider that the ancient subsea permafrost structure of the shallow East Siberian Arctic Sea and the Laptav Sea are uniquely at risk from warming caused by polar amplification which as stated futher in the report ranges in magnitude from a factor of two to four. Furthermore, much of the energy build up associated with this amplification will be focused in the upper surface waters of the Arctic Sea, making shallow subsea permafrost structures uniquely vulnerable. The effects from these physical properties are already being observed in multiple independent measurements, these being (1) observations of methane bubbling to the ocean surface in quantities never before seen, https://www.mdpi.com/2076-3263/9/6/251/htm, (2) increased frequencey of extremely high methane measurements in the Barrow, Alaska Global Greenhouse Network measuring station, and (3) NOAA satellite measurements immediately above shallow seas of the ESAS and the Laptov sea, see https://www.ospo.noaa.gov/Products/atmosphere/soundings/iasi/m2/rp/mrm_t1_D2.html [Kevin Lister, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - while the overall statement is kept it was before it is added the phrase 'despite of polar amplification of warming'
39789	72	12			"Methane release from shelf clathrates is <10 TgCH4 yr-1" over what time frame? today? Please clarify [TSU WGI, France]	Accepted - this a present-day estimate
16073	72	23	72	23	medium confidence that something is unlikely: Not sure this is the correct usage of the calibrated uncertainy language. "Unlikely", implying a <33% probability, should in principle be used when there is high confidence (only in exceptional cases, medium confidence). And in that case, when the word "unlikely" is used, then the expression "XXX confidence" should not be used anymore. I think. [Gerhard Krinner, France]	Taken into account - see reply to comment id 66673

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
66673	72	23	72	24	Why is this only medium confidence? This section seems clear and emphatic, and consistent with AR5. Why not make it high confidence? [Dave Frame, New Zealand]	Accepted - 'medium' is replaced by 'high'
115399	72	23		24	Could you make a stronger statement about the low likelihood of substantial warming from methane clathrates? Re-phrasing your assessment, it says that there is up to a 33% change that this tipping point could substantially warm the climate system over the next few centuries with medium confidence. [Gillett Nathan, Canada]	Taken into account - see reply to comment id 66673
31997	72	26			Mention the possibility of overturn of Lake Kivu (huge methane store)(volcanic as well as climate overturn drivers) and other African meromictic lakes with dozens of Tg of methane in them? Pasche, Natacha, Martin Schmid, Francisco Vazquez, Carsten J. Schubert, Alfred Wüest, John D. Kessler, Mary A. Pack, William S. Reeburgh, and Helmut Bürgmann. "Methane sources and sinks in Lake Kivu." Journal of Geophysical Research: Biogeosciences 116, no. G3 (2011). [Euan G. Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - this is not a global-scale phenomenon
30577	72	28	72	36	This paragraph makes no mention of the potential impacts of the changing CaCO3 production and/or dissolution on organic matter remineralization in the water column. These processes occur close to the surface in the Pacific and Indian Oceans and are deepr in the Atlanic.Changes in the CaCO3 cycle may have a significant impact on the time scales of organic matter recycling in the oceans (a positive feedback). [nina bednarsek, United States of America]	Not applicable - drivers of the ocean acidification are discussed in Sect. 5.3. There is a lack of space to extend this discussion here
36391	72	29	72	36	This paragraph makes no mention of the potential impacts of the changing CaCO3 production and/or dissolution on organic matter remineralization in the water column. These processes occur close to the surface in the Pacific and Indian Oceans and are deepr in the Atlantic. Changes in the CaCO3 cycle may have a significant impact on the time scales of organic matter recycling in the oceans (a positive feedback). [Adrienne Sutton, United States of America]	Taken into account - see reply to comment id 30577
10257	72	30	72	33	However, this does not take into account the potential use of ocean alkalinisation (dealt with below in section 5.6.2.2.3) that "...can increase surface total alkalinity and thus increase CO2 uptake and storage". I suggest inserting the following new text after this sentence "However, ocean alkalinisation, via the deposition of alkaline minerals or their dissociation products at the ocean surface, can increase surface total alkalinity and help reverse the effects of surface ocean acidification (section 5.6.2.2.3)." [Chris Vivian, United Kingdom (of Great Britain and Northern Ireland)]	Accepted
38481	72	32	72	32	It would be clearer to say 'reverse ocean acidification in the ocean interior' [LONG CAO, China]	Accepted
10259	72	38	72	38	The reference to section 5.4.3 is incorrect.Possibly the reference should be to section 2.3.4.2. [Chris Vivian, United Kingdom (of Great Britain and Northern Ireland)]	Accepted
26925	72	38	72	38	It is Section 5.3.3 [Eric Brun, France]	Taken into account - see reply to comment id 10259

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
10261	72	38	72	45	Again, I suggest that this section should take into account the IUCN report on deoxygenation referred to in the comments above to page 74. [Chris Vivian, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - this section is devoted on ocean acidification and deoxygenation as possible tipping points. Their mechanisms and drivers are figured out in Sect. 5.3.3 and 5.3.4.
58927	72	38	72	45	Although it is true that carbon release due to deoxygenation has not been quantified, it could be worth adding 1-2 sentences here addressing the qualitative effects of ocean deoxygenation on carbon release. This information was provided earlier for nitrous oxide (which we have better quantitative data on) but could still be provided qualitatively here. (For example, an acknowledgement of how oxygen deprivation affects marine productivity and therefore decreases biological carbon uptake, potentially decreasing the ocean's "sink" capacity). [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected - because the release is unquantified, such statements would be non-informative
93455	72	40	72	41	The proper ref here should be Battaglia and Joos, 2018a and not 2018b [Carles Pelejero, Spain]	Accepted
115401	72	45			As written this implies that this process could release an unknown amount of CO2. Can you put an upper bound on this? [Gillett Nathan, Canada]	Rejected - the possible release is still unquantified
40719	72	48	73	16	section 5.4.8.5: the assessment lacks traceability [TSU WGI, France]	Taken into account - see reply to comment id 116453
28245	73	6	73	7	Besides Drijfhout et al. 2015, there is now a more comprehensive scan for abrupt shifts in CMIP5: Bathiany, Hidding and Scheffer, 2020: Edge Detection Reveals Abrupt and Extreme Climate Events (doi: 10.1175/JCLI-D-19-0449.1) [Sebastian Bathiany, Germany]	Accepted - the reference to Bathiany et al. (2020) is added
39857	73	7		8	"The most commonly detected abrupt changes in the CMIP5 archive relate to sea-ice changes," be careful: sea ice is not presented as a tipping point in ch9. [TSU WGI, France]	Accepted - the respective reference is added
115403	73	7		10	Elsewhere in the chapter it says that no CMIP5 models represented permafrost, but here it says that some simulated abrupt change associated with permafrost thaw. [Gillett Nathan, Canada]	Accepted - 'permafrost thaw' is replaced by 'physical permafrost thaw'
41689	73	8	73	8	…there "are"… [Katharina Meurer, Sweden]	Accepted
16075	73	11	73	11	"limited confidence" -> low confidence [Gerhard Krinner, France]	Accepted
37921	73	14	73	16	I wonder if there is any evidence to support this statement or not. [Junhee Lee, Republic of Korea]	Accepted - the clarifying sentence is added
90833	73	14			Past report shows that there is an uncertainty propagates through the chain and results in a spread of ESM projections. This spread is only one way of assessing uncertainty in projections. Alternative methods, which combine information from simple and complex models and observations through statistical models or expert judgement, are also used to quantify that uncertainty. [Vivien How, Malaysia]	Rejected - unclear what is to be changed
78531	73	19			this section should be part of 5.7 at the end of chapter [Chris Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - the former Sect. 5.4.8.6 is moved to Sect. 5.7
90835	73	19			Author may refer to working paper "Nature Based Solutions for Climate Change Adaptation – Knowledge Gaps" to elaborate further in this context [Vivien How, Malaysia]	Accepted - the text is checked to be consistent with the indicated paper
21857	73	21	73	30	For stylistic consistency with other chapters consideration should be given to collating all such knowledge gaps and placing them at the end of the chapter. [Peter Thorne, Ireland]	Taken into account - see reply to comment id 78531

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29191	73	23	73	24	This statement or a new bullet point should acknowledge large gaps in knowledge about the potential effect of climate change on forest fires [Eric Davidson, United States of America]	Taken into account - see reply to comment id 106181.
21135	73	23	73	24	Recommending changing this statement to: "For tropical and boreal feedbacks, there is insufficient knowledge on plant resilience, the effects of plant biodiversity, and acclimation to future climate changes (Lade et al. 2019)." Lade et al. 2019 assesses the state of knowledge including potential mechanisms by which these factors could influence climate-carbon cycle feedbacks. Reference: Lade et al. (2019), https://doi.org/10.1017/sus.2019.18 [Steven Lade, Sweden]	Accepted
106181	73	23	73	24	"For tropical and boreal feedbacks, there is insufficient knowledge on plant resilience and acclimation to future climate changes;" Add: "and on the future development of ecological disturbance regimes such as fire, mortalities due to extremes and others. [Wolfgang Lucht, Germany]	Accepted
58931	73	23	73	30	It would be very helpful to summarize in a table all the model limitations mentioned throughout the text. That would help researchers around the world to more easily identify the research needs and contribute to it. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected - such tables would be incomplete and would not deliver new information with respect to the bulleted list
31999	73	24			I would add the tipping point of catastrophic tropical forest and woodland fire and replacement by savanna grass. We still know very little about this process, which is now inextricably interwoven with human deforestation. [Euan G. Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - see reply to comment id 106181.
17051	73	26	73	26	Need to incorporate methane-induced CO2 cycling and it's impact to ocean-atmosphere carbon dynamics (e.g., Akam et al., 2020). A significant amount of CH4 is being converted to the inorganic carbon in the marine sediments and water column. However, the constraints on the impact of this inorganic carbon towards ocean carbon budgets is limited. Along with better quantification of marine CH4 fluxes, there needs attention to the fate of methane carbon after oxidation. [Sajjad Abdullajintakam, United States of America]	Rejected - this item is already assessed in sect. 5.4.8.1.3 (the former sect. 5.4.8.3)
21137	73	29	73	30	Lenton et al. (2019) also speculate on interactions between tipping elements. Reference: https://www.nature.com/articles/d41586-019-03595-0 [Steven Lade, Sweden]	Rejected - there is enough references
89507	73	29	73	30	The last key point here is not specfic enough to be meaningful to this reader. Could elaborate a bit. [Edward Schuur, United States of America]	Accepted
96595	73	35	73	39	Additional references would be helpful. Please add references. [Nicole Wilke, Germany]	accepted -- added more references
115405	73	35		40	Tokarska et al. (2016 - already in the reference list) show and discuss the climate and carbon cycle response of four CMIP5 ESMs over the period 1850-2300 in response to RCP8.5-E. This could be discussed here. [Gillett Nathan, Canada]	accepted -- added discussion of this paper
23737	73	37	73	40	Hajima et al. (in review) peformed 1000/2000 years simulations using CMIP5- and CMIP6-era ESMs and thus can be a support for this sentence; see Fig. 1 (on carbon cycle feedbacks) and Fig. 2 (on diagnosed compatible fossil fuel emission) of Hajima et al. (in review) Progress in Earth and Planetary Science (the draft has already sent to TSU) [Tomohiro Hajima, Japan]	accepted -- added discussion of this paper

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18237	73	42	73	43	The phrase ' leading to an overall strengthening of the carbon cycle gain from one century to the next' is unclear, specifically 'carbon cycle gain'. Is there another way to phrase this to improve the clarity for the reader? [Chelsey Baker, United Kingdom (of Great Britain and Northern Ireland)]	accepted --reworded to increase clarity
89509	73	43	73	48	Please make sure these numbers are checked with SROCC chapter 3. We used the same citations, although I don't think we specifically broke out emissions from 2100 to 2300. There may be additional citations in SROCC that do have those estimates; not sure. I do think that the full range of C emisisons estimates by 2300 are larger than those shown in the text here, so its not clear what the ranges that are shown. [Edward Schuur, United States of America]	Partially accepted: Moving this to permafrost box
10045	73	44	73	48	MacDougall & Knutti 2016 (10.5194/bg-13-2123-2016) examined 2100 to 2300 for all 4 RCPs. For RCP 8.5 release of 132 to 387 PgC (5th to 95th percentile). RCP 2.6 release of 19 to 57 (5th to 95th percentile). Same paper also examed RCP 4.5 and 8.5, extended to year 10,000 CE. [Andrew MacDougall, Canada]	rejected -- moving permafrost discussion out of this section and to permafrost box
73227	73	47	73	47	Replace apostrophe with comma. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change was made.
37963	73	47	73	47	“under the RCP8.5 scenario’ leading to up to a 0.23oC increase in global temperature.” [Junhee Lee, Republic of Korea]	Accepted - change was made.
39847	73	55	74	1	"terrestrial carbon-climate feedbacks to strengthen beyond the carbon-concentration feedbacks," this phrasing is a bit convoluted, can't you express things more simply? (e.g. by saying that carbon-climate feedback overtakes the effect of carbon-concentration feedback.? [TSU WGI, France]	accepted --reworded to increase clarity
115407	74	1		5	I wonder if a stronger assessment on the land source to sink transition can be made here. Tokarska et al. (2016 - alredy in ref list), Fig 1b shows that all four CMIP5 ESMs used show such a transition under this scenario. [Gillett Nathan, Canada]	accepted -- added discussion of this result
73229	74	10	74	10	Capital 'S' for 'section'. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change was made.
127843	74	20	74	33	The entire section on natural climate solutions, particularly the role of BECCS and biochar, and the trade-offs on food security, biodiversity, non-CO2 emissions, only weakly highlights the challenges that have been noted for full-scale implementation. SR1.5, the IPBES report, and SRCCL go into a lot of detail on these challenges, i.e., land area requirements. The message from this section is that BECCS and natural climate solutions only moderately affect ecosystem services and doesn't appear to cover the breadth of recent literature on this topic. [Trigg Talley, United States of America]	Noted - AR6 builds on the Special Reports
40713	74	28	75	49	section 5.4.10: Please check the use of this IPCC uncertainty language term, I suspect some misuse. The IPCC guidance note on uncertainty: https://wg1.ipcc.ch/SR/documents/ar5_uncertainty-guidance-note.pdf [TSU WGI, France]	Edited for conciseness - new area with single expert assessment
40715	74	28	75	49	section 5.4.10: this section still needs to be turned into an assessment [TSU WGI, France]	Edited for conciseness - new area with single expert assessment

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41209	74	30		31	you mention the topic assessed in AR5 but what was their conclusion? [TSU WGI, France]	Noted - however the point of this and the next sentences was that AR5 only assessed the predictability of the physical climate components based on ESM prediction systems, whereas this sections makes a step forward to assess predictions of the carbon cycle.
41165	74	45			what is a perfect model study? (can a study be perfect?) do you mean idealised? [TSU WGI, France]	Accepted - clarification added: "The predictive horizon of the globally integrated air-sea CO2 fluxes has been assessed in perfect-model frameworks that are based on an idealized ensemble simulations in which each ensemble member serves as a verification, while no observations are assessed. Perfect-model studies provide an estimate of the upper range of potential predictability...."
41693	74	50	74	50	the variables "DIC" and "TA" should be explained [Katharina Meurer, Sweden]	Accepted - now "Dissolved Inorganic Carbon (DOC)" and "Total Alkalinity (TA)"
19999	75	12	75	15	Using "but" to introduce the second part of the sentence seems here totally inappropriate. Something like "eventhough" would make sense. [philippe waldteufel, France]	Accepted - sentence split the sentence in two. First sentence now ends with 'assessed.' Second sentence starts 'The seem to be associated ...'
13443	75	17	75	17	Eliminate the extra space between parenthesis and semicolon. [Maria Amparo Martinez Arroyo, Mexico]	Accepted - change made.
33355	75	17			Change: "…(Ilyina et al., submitted; Lovenduski et al., 2019a) ; this…" by "…(Ilyina et al., submitted; Lovenduski et al., 2019a); this…". [Guiomar Rotllant, Spain]	Accepted - references updated.
19997	75	21	75	23	The meaning of this sentence is particularly mysterious [philippe waldteufel, France]	Accepted - sentence rewritten : "Initialized simulations suggest that observed variability in the land carbon sink is improved through initialization of prediction systems with the observed state of the physical climate".
33357	75	27		28	Change: "…(Ilyina et al., submitted ; Figure 5.30…" by "…(Ilyina et al., submitted; Figure 5.30…". [Guiomar Rotllant, Spain]	Accepted - references updated.
40165	75	34			Fig 5.3 the y axis is not very clear to me [TSU WGI, France]	Not applicable. Figure removed
73231	75	42	75	42	Change 'time period' to 'periods'. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. Figure removed
86759	76	1	78	3	Please consider adding som text on cummulative emissions versus fluctuations in the land sink. [Oyvind Christophersen, Norway]	Rejected - This has not been included in this subsection, as this level of integration would typically be considered in the ES or TS only.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
18479	76	1	88	24	There is an inconsistency between the glossary definitions of "remaining carbon budget ("[Estimated] cumulative [net] global [anthropogenic] CO2 emissions from the start of 2018 to the time that [anthropogenic] CO2 emissions reach net zero that would result, at some probability, in limiting global warming to a given level, accounting for the impact of other anthropogenic emissions") and "carbon budget" ("the estimated cumulative amount of global carbon dioxide emissions that that is estimated to limit global surface temperature to a given level above a reference period, taking into account global surface temperature contributions of other greenhouse gases (GHGs) and climate forcers"). Which definition is followed would affect the drafting of Section 5.5 [Jim Skea, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - Applicable glossary entries have been finalised and this section consistently applies the definition of remaining carbon budget given therein.
34403	76	1			In the section it would be useful to include an explanation for the shift in remaining carbon budget between the SR15 and the AR6. For example the 50% 2C value from SR15 is 1500 Gt whereas the AR6 value is 1140. The start year and 135 Gt from feedbacks accounts for some but not all. Adding detail about the shift would be helpful to users of budget information. [Haroon Kheshgi, United States of America]	Accepted - the update in carbon budget assessment between AR5 and AR6 is clarified in Box 5.1
18481	76	4	76	4	The term "global average surface temperature" is ambiguous given the discussion of GMST v GSAT in Chapter 2. While both metrics are in play, it would be better to say which is being discussed, or make a statement right at the beginning that "global average surface temperature" means [whatever it means]+I29 [Jim Skea, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - at this stage the distinction between GSAT and GMST cannot be made (the relationship applies to both and Chapter 2's assessment came to the conclusion that currently no statement can be made on their difference). Where specific TCRE estimates are being discussed, this difference is highlighted explicitly.
18483	76	8	76	9	The glossary definition of "carbon budget" is used. [Jim Skea, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - The subsection presenting the remaining carbon budget assessment (5.5.2) makes this link explicitly.
33359	76	12			Change: “(Chapter 7, Figure 7.12 [[Placeholder: check reference]]),” by “(Chapter 7, Figure 7.12 [Placeholder: check reference]),”. [Guiomar Rotllant, Spain]	Rejected - All placeholders are removed for the final government draft. Meanwhile, placeholders have a specific format so they are machine-searchable in the drafts.
52673	76	13	76	13	It is unclear what this phras 'depends on more than the cumulative emissions of CO2 only) is referring to. Also, is this sentence about human-induced warming or observed warming (natural + internal variability)? Please clarify. [Katarzyna Tokarska, Switzerland]	Accepted - The sentence has been edited for clarity
18485	76	15	76	16	Inconsistent with "remaining carbon budget" definition. [Jim Skea, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - The statement is consistent with the cross-working group glossary definition of the remaining carbon budget. Details about how precisely it is calculated are outside the scope of this very first introductory paragraph.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
58811	76	15	76	17	Remaining carbon budget' is taken from the literature, defined here and used throughout the chapter. However, whilst individual budgets have been completed for different temperature scenarios, XXXGt C is not a budget it is a quantity, in this case 'remaining emissions to stay within the budget'. I understand a shorter turn of phrase is being used to reduce sentence size and this arguement could be described as symantic but this terminology is not accurate and that matters. .Even referring to this as 'remaining carbon/remaining carbon emissions' would be a suitable fix as you are not redefining the meaning of what a budget is. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected - The term carbon budget is a well-established term in both the literature and earlier IPCC reports. Its meaning is defined unambiguously in the cross-working group glossary. The two suggested fixes ("remaining carbon" or "remaining carbon emissions") do not convince for being better terms.
131545	76	15	76	17	sentence unclear, specify the relationship between transient climate response to cumulative emissions of carbon dioxide (TCRE). [Hans Poertner and WGII TSU, Germany]	Accepted - The sentence has been amended to more explicitly highlight the relationship with TCRE.
52675	76	23	76	23	It would be helpfu if Section 5.5.1. (and especially section 5.5.1.2.) included a paragraph about the role of internal variability, that is currently not mentioned explicitly, but is a contributing factor to the divergences in TCRE and remanining budget estimates if based on ESMs (that include internal variability). While, by definiton, TCRE refers only to forced response, and is not subject to itnernal variabilty, estimating TCRE from a single simulation of ESM run in CMIP5 and CMIP6 models is subject to interal variability, even in p1ctCO2 simulations. See also a recent paper in review that expands on this point. (Reference: Tokarska et al., Uncertainty in carbon budget estimates due to internal climate variability (in review at ERL)). [Katarzyna Tokarska, Switzerland]	Taken into account - given space constraints a full paragraph on this issue is not possible, but a sentence highlighting this uncertainty has been included.
90105	76	23	81	19	This statement uploaded by Ted Schuur on behalf of a graduate class that reviewed the chapter (this was communicated with IPCC TSU in advance): Although table 5.7 displays an overview of TCRE estimates, a having a figure associated with section 5.5.1 would be useful to visually display the linear relationship between CO2 emissions and temperature increase. Right now, there is no figure references in this entire section. [Edward Schuur, United States of America]	Taken into account - This relationship is shown in the left panel of the figure in the Section 5.5.2
90103	76	23	83	9	This statement uploaded by Ted Schuur on behalf of a graduate class that reviewed the chapter (this was communicated with IPCC TSU in advance): This section would benefit from a TCRE and ZEC diagram sooner. The concept is introduced on page 76, but the concept was not illustrated until page 83. The right panel of figure 5.31 would be much more effecticve if presented sooner [Edward Schuur, United States of America]	Taken into account - while space for figures is limited and the exact location of visuals will be determined at the time of production of the report, a cross-reference to the visual was included in this section to more easily make a connection between the various concepts.
35111	76	29	76	29	The sentence is broadly correct, but by only refering to Section 5.4 is ignoring non-biogeochemical contributions to the TCRE. In particular, the TCRE is also affected by physical climate feedbacks, as well as radiative forcing, heat uptake and carbon cycling as listed. So needs to also link to Chapter 7 as well as Section 5.4. [Richard Williams, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - References to the assessment of ocean heat uptake (Sections 3.5, 4.3, 4.5), ocean and land carbon uptake (Sections 4.3, 4.5, 5.4) and radiative forcing of CO2 (Section 7.3).

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
52677	76	29	76	29	Please note that some of these mechanisms were proposed before AR5 [Katarzyna Tokarska, Switzerland]	Accepted - Pre-AR5 studies like Matthews et al. (2009) also already proposed physical mechanisms for TCRE. The sentence has been changed to reflect this.
40891	76	29		32	TCRE already defined in the intro of 5.5 [TSU WGI, France]	Noted - This sentence does not provide a definition of TCRE (which stands for "Transient Climate Response to Cumulative Emissions of CO2"). The inclusion of TCRE between brackets serves as a cross-reference to an earlier defined quantity.
39821	76	29			"since AR5" was in introduced in AR5 for the first time? After AR5? Ambiguous phrasing. [TSU WGI, France]	Accepted - This statement was edited to clarify that AR5 provided an initial explanation and more studies followed since.
17065	76	32	76	32	extra space before new paragraph [Sergio Aquino, Canada]	Accepted - All text will be edited and layouted before publication.
13445	76	32	76	33	Add line spacing between paragraphs [Maria Amparo Martinez Arroyo, Mexico]	Accepted - All text will be edited and layouted before publication.
33361	76	32			Erase space at the end of the line. [Guiomar Rotllant, Spain]	Accepted - All text will be edited and layouted before publication.
18491	76	35	76	36	is it possible to define pathway independence? When is the boundary crossed? Are there criteria? [Jim Skea, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - It has been clarified that pathway independence refers to "The degree to which the value of TCRE is independent on the historical rate of CO2 emissions"
115409	76	44			Insert 'in part' after 'arises'. This mechanism does not explain why, for example, GSAT remains approximately constant after a cessation of emissions. [Gillett Nathan, Canada]	Taken into account - In this section we are describing the mechanisms that make the relationship between cumulative emissions of CO2 and GSAT nearly linear. By definition this linearity can only be defined while emissions continue. After emissions cease the mechanisms that control Zero Emission Commitment take over and could lead to either cooling or continued warming (MacDougall et al. 2020). Early work on TCRE did not clearly separate it from ZEC (e.g. Allen et al. 2009) leading to conflation of the two concepts. Section 5.5.1 covers the assessment of TCRE, while ZEC is assessed separately in Section 4.7.2 of Chapter 4.
73233	76	49	76	49	Change reference to 'Matthews et al. (2009) [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - References will be formatted consistently before final publication
58299	76	49	76	49	Placement of (should be before 2009 [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - change was made.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
51155	76	49	76	49	"The approach of (Matthews et al., 2009),..."should instead be "The approach of Matthews et al. (2009),... " [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - References will be formatted consistently before final publication
35113	76	49	76	53	May not need to alter the text, but just to note that the Williams et al. (2016) approach is taken further in Williams et al. (2020) ERL to understand the TCRE for 9 CMIP6 models. Their framework provides insight into how the TCRE may be interpreted in terms of the product of the temperature change/radiative forcing, radiative forcing/atmospheric CO2 and the airborne fraction. See doi:10.1088/1748-9326/ab97c9 [Richard Williams, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - The decision was made to only reference to two papers that introduced this approach here, while the others are reference further down.
33363	76	49			Change: "…radiative forcing [[Placeholder: check reference]]). The approach of (Matthews et al., 2009), which has later" By "…radiative forcing [Placeholder: check reference]). The approach of Matthews et al. (2009), which has later". [Guiomar Rotllant, Spain]	Taken into account - All placeholders are removed for the final government draft. Meanwhile, placeholders have a specific format so they are machine-searchable in the drafts. References will be formatted consistently before publication.
58813	76	55	76	55	path independence' is a niche term that is not commonly seen, readers are unlikely to know the meaning of this without looking it up . It is relevant and suitable to define the meaning of this term here. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - This sentence was edited to clarify the meaning of the term pathway independence.
116455	76		76		The very clear description of the mechanisms underlying the near constancy of TCRE should be reported in the ES and reflected in the TS and SPM. [Valerie Masson-Delmotte, France]	Taken into account - The inclusion has been considered in consultation with all chapter authors.
20001	77	1	77	2	What is meant here by "cumulative land fraction"? [philippe waldteufel, France]	Taken into account - The phrase 'cumulative land fraction' has been replaced with the more descriptive: "cumulative fraction of carbon taken up by the terrestrial biosphere"
52679	77	2	77	2	It is unclear what the 'cumulative land fraction is constant' is this referring to, fraction of land carbon uptake? Please clarify. [Katarzyna Tokarska, Switzerland]	Taken into account - The phrase 'cumulative land fraction' has been replaced with the more descriptive: "cumulative fraction of carbon taken up by the terrestrial biosphere"
35115	77	6	77	12	The two additional assumptions also require that physical climate feedbacks are unchanging in time, which might not always hold. See supplementary figure 1 for Williams et al. (2017) GRL https://doi.org/10.1002/2017GL075080, which shows a decrease in climate feedback parameter in time or equivalently an increase in the efficacy in time. [Richard Williams, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - The assumptions about the climate feedback parameter and efficacy of ocean heat uptake have now been made clear.
52681	77	18	77	19	It would be helpful to briefly summarize how the ocean ventilation affects the TCRE value (rather than only introduce the study). [Katarzyna Tokarska, Switzerland]	Accepted - The sentence describing the study has been re-written to better capture the study's conclusions.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
90107	77	18	77	19	This statement uploaded by Ted Schuur on behalf of a graduate class that reviewed the chapter (this was communicated with IPCC TSU in advance): This paragraph focuses on terrestrial carbon and the sentence involving ocean ventilation seems out of place. May make more sense in previous paragraph to with other ocean carbon pool info [Edward Schuur, United States of America]	Accepted - This sentence was moved to the end of this section in order to not interrupt the discussion of land-carbon effects.
58815	77	31	77	31	It was previously state the TCRE does not hold for very high and very low emissions. So by 'Going beyond these upper limits' is is notclear whether beyond in this context only means above when it could also mean below. If >200-3000 is meant than just say 'above/greater than these upper limits' [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected - According to the Merriam-Webster dictionary "beyond" as used in this sentence means "1: on or to the farther side of : at a greater distance than". This indicates that there is no reason to change this word.
52683	77	35	77	35	It is unclear why the window where TCRE is constant ends at 1560 PgC, as the first sentence in this paragraph says that it is well founded that TCRE is linear up to at least 2000PgC and well above 3000 PgC. [Katarzyna Tokarska, Switzerland]	Accepted - This section was re-ordered to make clear that at least up to a limit of 3000 PgC the linearity would still hold, but beyond that the evidence is weaker.
58817	77	36	77	37	remain constant for considerably larger quantities of cumulative emissions' - why not simply quantify this with a number from these references? [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - This section was re-ordered to make clear that at least up to a limit of 3000 PgC the linearity would still hold, but beyond that the evidence is weaker.
74657	77	37	77	37	Please check if Franks et al. (2013) is a correct citation. [Kaoru Tachiiri, Japan]	Accepted - Citation has been removed
58821	77	38	78	1	This paragraph discusses several processes that could alter the TCRE linearity. They are not immeditely linked and this does make the flow of the paragraph feel t jump around. Numbering the different processes would be helpful [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected - Unfortunately it is in the nature of having to condense a lot of information in a very few words that little space remains available to consider improvements to the flow of the text that would involve adding more words.
58819	77	42	77	44	No explanation is offered for why some models disagree on this. My guess would be that it has something to do with how the models paramaterise radiative forcing but this should be explained here without breaking the flow of the text to interrogate the literature. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account - The disagreement is more likely on the side of how quickly carbon sinks weaken
40445	77	53	55	53	incorrect use of IPCC confidence language [TSU WGI, France]	Taken into account - The use of the IPCC confidence language was double-checked
52685	77	54	77	55	It is unclear why this sentence claims that TCRE is linear only up to 1500 PgC - this section slightly contradicts the introductory paragraph in this section that states that TCRE is linear up to at least 2000 PgC and above it. In the current form, this paragraph does not provide enough evidence to bring that limit down to 1500 PgC. [Katarzyna Tokarska, Switzerland]	Accepted - we now clarify in this statement that we have high confidence of TCRE's linearity up to 1500 PgC and with medium confidence up to 3000 PgC.
18487	78	2	78	2	To note that WG III will always use GtCO2, never Pg and never C. [Jim Skea, United Kingdom (of Great Britain and Northern Ireland)]	Noted - Units of C are used typically in the geoscientific literature, while units of CO2 are typically used in the policy literature. They are equivalent by a constant factor 3.66. At the level of the chapter units of PgC are preferable.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
9841	78	4	78	31	The National Academies of Sciences (2016, doi:10.17226/21898) introduced another policy-relevant metric, the Initial Pulse Adjustment Timescale, defined as the time to peak warming after the injection of a small pulse of CO2 on top of a baseline scenario. IT would be useful to have an IPCC assessment of this metric. [Robert Kopp, United States of America]	Rejected - This metric, although policy relevant, is not included in approved AR6 outline for Chapter 6. Given the stage the report is currently at, including an assessment of an additional climate response quantity that was not requested in the approved outline is not possible.
51157	78	5	78	5	"10 year" should be "10 years" [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change was made.
58823	78	8	78	11	Does initial mean 10 years in this instance? Not clear, sentence could simply be 'Temperature responses to cumulative emissions from large emission pulses.also shows a deviation from a linear temperature responsen in the initial 10 year following the release of the pulse.' [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - the sentence has been reworded for clarity
23739	78	8	78	12	Hajima et al. (in review) also shows such initial deviation in abrupt CO2 doubling experiment; they also show another type of deviation – the deviation in millenium time-scale simulations. The latter deviation might depend on the CO2 emission rate, but the authors suggest a decoupled mechanism (behavior) of ocean heat and C uptake beyond centuries. (see Figs. 3 of Hajima et al. (in review) Progress in Earth and Planetary Science (the draft has already sent to TSU)). [Tomohiro Hajima, Japan]	Accepted - This study has been cited as additional line of evidence
115411	78	30		31	Isn't a non-zero ZEC a deviation from constant TCRE? I would re-frame as small non-zero ZEC represents a departure from a constant TCRE. [Gillett Nathan, Canada]	Taken into account - This statement has been edited for clarity. ZEC is considered separately in the assessment. This statement now solely speaks to whether TCRE remains a good predictor for CO2-induced warming.
15973	78	34	78	47	The opening sentence discusses that "relatively few studies that have assessed how the TCRE is expected to change in scenarios of declining emissions." However the predicted temperature rise in response to an increase in greenhouse gases will have an error band. As the temperature response to a reduction in greenhouse gases will depend on where the starting point is and for how long the planet has been in this warm condition, then the return path cannot be accurately predicted irrespective of how many studies are conducted. [Kevin Lister, United Kingdom (of Great Britain and Northern Ireland)]	Noted - It is unclear which correction, or clarification the reviewer wishes to be included. No references to additional literature have been provided. No statements have been disputed.
78533	78	34			the discussion on feedbacks assumes that a process is either “included” or “not included” in ESMs. How can we treat processes which are in some, but not all, ESMs? The framework does not cater for this, but N-cycle for example makes a leading order change to TCRE for models which include it, but the multi-model ensembles has approximately half and half inclusion/exclusion of (land) N-cycle. [Chris Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - The sentence was made more specific on the various ways these processes can be included to a varying degree.
41695	78	39	78	39	"overshoot" should be explained in this context [Katharina Meurer, Sweden]	Accepted - The sentence was edited for clarity.
58301	78	43	78	44	Should read Tachiiri et al. (2019) also reported an increase in TCRE [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - References will be formatted consistently before final publication

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
73235	78	44	78	44	Delete hyphen. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change was made.
74659	78	44	78	44	delete "in" of "in increase" [Kaoru Tachiiri, Japan]	Accepted - "in" was replaced by "an".
51159	78	44	78	44	"also reported in increase in TCRE" should be "also reported an increase in TCRE" [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change was made.
41697	78	44	78	44	"in increase" = "an increase" (?) [Katharina Meurer, Sweden]	Accepted - change was made.
45819	78	44	78	44	"in increase" should be "an increase". [Twan van Noije, Netherlands]	Accepted - change was made.
52687	78	46	78	46	Please note that this sentence should clearly include the caveat that this holds only for low levels of overshoot (up to ~300 PgC) (Tokarska et al. 2019 b). For much higher levels of overshoot (e.g. MacDougall et al. 2016), TCRE is no longer fully reversible. [Katarzyna Tokarska, Switzerland]	Accepted - this has been clarified
18489	78	51	78	51	ZEC is unexplained at this point. And surely ZEC as applied later is not a fundamental physical property that drives warming. [Jim Skea, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - ZEC has now been defined immediately after its first mention in this section, consistent with its definition in Chapter 4. It is unclear which clarification the reviewer seeks with their comment that ZEC is not a fundamental physical property that drives warming. The text makes no such assertion.
87707	78	54	78	55	"These additional changes in global mean temperature increase at various timescales are known as the ZEC" -> I suggest to move this explanatory sentence to where ZEC previously are mentioned p78 L25. [Ivy Frenger, Germany]	Accepted- ZEC has now been defined immediately after its first mention in this section, consistent with its definition in Chapter 4.
89511	79	15	79	17	Ouch. Is there a way to highlight what has been learned here? This concluding sentence kind of takes the breath away from the paragraph. For confidence language, I'm not sure if you have to factor in the multi century forecasting since as a baseline that is a future that is not known. [Edward Schuur, United States of America]	Accepted - We have included a concluding paragraph that present an assessment and level of confidence.
41173	79	16			what is the reversibility assessment? Is it reversible or not? We only have the level of confidence [TSU WGI, France]	Accepted - We have included a concluding paragraph that present an assessment and level of confidence.
78573	79	20			a key development since AR5 is not just the ability to decompose the TCRE into component uncertainty, but the fact that this has changed remarkably since CMIP5. In CMIP5 climate and carbon cycle terms accounted for approximately half of the uncertainty each (Gregory, Huntingford, Booth, Williams references). But now in CMIP6 this has changed and the climate term (TCR) carries a much greater fraction of variance than the carbon cycle term (AF). Jones & Friedlingstein and Arora et al point this out. It seems that both a reduction in spread of land carbon response to CO2 due to including N-cycle, and an increase in spread in TCR has caused this. Total uncertainty in TCRE has not changed much, but it's constituents have. This marks quite a big change to our understanding. [Chris Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - The fact that the latest CMIP6 ensemble analysis supports TCR as the most important contribution to TCRE uncertainty is now noted. However, given the nature of the CMIP ensembles (being an ensemble of opportunity and the distributions across these ensembles thus not being representative of a formal uncertainty range) this does not provide robust evidence to assert that our understanding on this issue has changed.
73237	79	22	79	22	Move 'likely' to after 'TCRE'. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted
58303	79	22	79	23	Confidence statement needed? [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected - No, a likelihood statement is a quantified expression of confidence.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
74661	79	24	79	24	Mattews et al. (2009) used an EMIC and ESMs. [Kaoru Tachiiri, Japan]	Accepted - We edited the sentence to indicate that these are Earth system models of varying complexity
41171	79	24	79	24	what is the difference between carbon-cycle models and ESMs? [TSU WGI, France]	Accepted - We edited the sentence to indicate that these are Earth system models of varying complexity, as there is insufficient space to explain the differences between the various types of models
44301	79	29	79	34	Jenkins et al (submitted 2020) offers an estiamte of the TCRE through observational constraints and use of CO2-forcing-equivalent emissions which is highlighted in the table but not referred to in the text. [Stuart Jenkins, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - Table 5.7 provides an overview of all studies, with notes on their approach taken. Due to space constraints the table is the main place where all studies are listed, although forcing equivalent literature is also highlighted in the text for how the non-CO2 contribution can be taken into account.
52689	79	29	79	34	There are two additional studies that also provide observation-based estimates of TCRE that could be listed here: Matthews et al. (in review) "A new framework for understanding and quantifying uncertainties in the remaining carbon budget", and Spafford and MacDougall (2020) ERL "Quantifying the probability distribution function of the transient climate response to cumulative CO2 emissions" [Katarzyna Tokarska, Switzerland]	Accepted - both studies have been included

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
35117	79	36	79	46	The TCRE discussion is now much more substantive than the previous draft, so well done. The text could further address the reasons for intermodel differences in the TCRE; see Williams, R.G., P. Ceppi and A. Katavouta (2020) Controls of the Transient Climate Response to Emissions by physical feedbacks, heat uptake and carbon cycling. Environmental Research Letters, doi:10.1088/1748-9326/ab97c9. This work shows that intermodel differences in the TCRE for a subset of 9 CMIP6 models are mainly due to intermodel differences in the physical climate feedback parameter, rather than differences in the carbon cycling. This effect of physical climate feedback appears more prominent in the subset of CMIP6 models analysed, as opposed to the subset of CMIp5 models. See Figure 10 of that study. In more detail: (i) Standard view of the TCRE Following the view that the TCRE is controlled by the product of the TCR and the airborne fraction, the coefficient of variation for the TCRE is 0.25 for a subset of 9 CMIP6 models, the coefficient of variation for the TCR is 0.23 and the coefficient of variation for the airborne fraction is 0.08; the coefficient of variation is provided by the ratio of the standard deviation divided by the model mean. Hence for CMIP6, the intermodel differences in the TCR provide the dominant contribution to intermodal differences in the TCRE, being nearly a factor of 3 larger than for the airborne fraction. In comparison, for a subset of 7 CMIP5 models, the coefficient of variation in the TCR is 0.09 and for the airborne fraction is 0.10, so that there were comparable contributions to the intermodel differences in the TCRE from the TCR and the airborne fraction. See Table 2 of Williams et al. (2020), ERL, doi:10.1088/1748-9326/ab97c9.	Taken into account - the dominant contribution of climate sensitivity (be it expressed as ECS, TCR, or warming/radiative forcing increase) has been highlighted with appropriate references. The inter-model differences in CMIP6 models have not been highlighted due to space constraints.
78575	79	36	79	46	this paragraph could more clearly spell out that TCRE is driven by both climate and carbon cycle processes and can be nicely split into a climate component (TCR) multiplied by a carbon cycle component (AF). These are not independent but do nicely demarcate research areas. Studies such as Williams et al (2017) then dig deeper into the climate response, further splitting the TCR term, while Jones & Friedlingstein (2020) dig into the AF term and decompose the carbon cycle uncertainty contributions. [Chris Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - This split up of TCRE in its contributing factors is now explicitly highlighted.
73239	79	37	79	38	References should be in chronological order, with the submitted one at the end of the list. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial - this kind of issues will be taken care of during the production phase, which includes professional copy-editing.
115413	79	37		38	Gillett et al. (2013) did this too. [Gillett Nathan, Canada]	Accepted - has been added

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
52691	79	41	79	46	It is unclear how equilibrium climate sensitivity is related to TCRE. TCR only mentioned later in this paragraph seems to be more relevant, since both TCR and TCRE are transient metrics. Also, how do the potential non-linearities from present day to equilibrium are taken into account in such studies that use ECS? (including the pattern effect). Please note that the transition from TCR to ECS is not a straightforward one. Please clarify. [Katarzyna Tokarska, Switzerland]	Taken into account - In the various theories used to describe the TCRE relationship the climate feedback parameter is often direct part of the expression. See MacDougall and Friedlingstein 2015, Goodwin et al. 2015, MacDougall 2017, Seshadri, 2017. Thus when such theories are used to assess uncertainty in TCRE the climate feedback parameter and hence ECS are taken to be sources of uncertainty. This sentence now refers more broadly to "climate sensitivity", covering both TCR and ECS.
73241	79	43	79	44	References should be in chronological order, with the submitted one at the end of the list. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial - this kind of issues will be taken care of during the production phase, which includes professional copy-editing.
66675	79	44	72	46	This point was made explicitly in Allen et al., 2009, which was published before Matthews et al 2009. It seems strange to omit this paper. [Dave Frame, New Zealand]	Taken into account - The sentence was edited.
78535	79	49			I'm torn over this table. In one way it is a really nice collection and synthesis of information and extremely useful and interesting. But it's not clear then how you reach the final assessed range of TCRE. The penultimate line implies you take TCR from chapter 7 and AF from CMIP models. In which case where do all the studies in this table feature? More clarity needed on whether or not each entry is included in your final assessment of TCRE [Chris Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - the final assessment of TCRE has now been more clearly spelled out in Section 5.5.1.4
78537	79	49			2nd Last line of table – you use AF from C4MIP and CMIP5. Update this with CMIP6 values. Jones & Friedlingstein (2020, table 2) list these for available models and calculate AF from CMIP6 is 53.2 +- 3.1%. This represents quite a big reduction in spread compared with CMIP5. [Chris Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - this updated spread is now used in the combined assessment of TCRE
52695	79	51	79	51	It is unclear what the diffference between GSAT and SAT is? [Katarzyna Tokarska, Switzerland]	Accepted - a clarification was added to the caption
52693	79	51	79	52	Table 5.7. caption: It should be clarified that these studies are not like-for like comparison, since some studies use GMST temperature definition, and thus, may be biasaed low compared to studies based on GSAT [Katarzyna Tokarska, Switzerland]	Taken into account - The caption highlights the various acronyms used on the table.
7425	79	51	81	1	Table 5.7 : The 1.0–2.2 range retained in the overall assessment is said to be based on combination of cross-AR6 lines of evidence. Why only the « Transient Climate Response (TCR) and Airborne Fraction (AF) » range is used ? And the « Carbon-cycle feedback parameters » range is not reflected in the overall assessment ? If any modification occur there, please correct in the executive summary of this chapter. [Geremy PANTHOU, France]	Taken into account - the carbon-cycle feedback parameter range uses less lines of evidence, as it merely samples the variation across CMIP6 models, while the TCR-AF approach uses all lines of evidence integrated in Chapter 7 and the AF range across CMIP6 models.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
18493	79	51	81	2	Flag that we have had recent examples of individuals taking ranges with very skewed distributions from IPCC (carbon prices) and in the absence of a median/mean estimate taking a misleading middle of the range to perform calculations. I'd be asking what is the "best estimate" in the final row of Table 5.7, but I'm sure there is a solid scientific reason why you won't give it.... [Jim Skea, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - Given that a symmetric normal distribution is assumed, the mean can also serve as a best estimate.
40885	79	51			Table 5.7: wouldn't it be easier to get the message across if that was turned into a figure? [TSU WGI, France]	Rejected - A figure would not be able to report on the details and differences between the various estimates.
35119	80	0	81	0	For 10 CIMP5 models, TCRE ranges from 1.20 to 2.45 K EgC-1 by Williams et al. (2017) J.Climate, doi: 10.1175/JCLI-D-16-0468.1. In addition, from 9 CMIP5 models, the effective TCRE is diagnosed at 2095 for RCP8.5 as 1.82 to 3.59 K EgC-1. [Richard Williams, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - the first range was included, while the second, based on RCP8.5 is not applicable for the assessment of TCRE as presented in the section.
35121	80	0	81	0	Range in TCRE for 9 CMIP6 models: 1.10 to 2.35 K EgC-1 In Williams, R.G., P. Ceppi and A. Katavouta (2020) Controls of the Transient Climate Response to Emissions by physical feedbacks, heat uptake and carbon cycling. Environmental Research Letters, doi:10.1088/1748-9326/ab97c9 [Richard Williams, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - has been included
35123	80	0	81	0	Range in TCRE for 11 CMIP6 models: 1.30 to 2.38 K EgC-1 In Table A4 in Arora et al. (2019) Biogeosciences, doi:10.5194/bg-2019-473 [Richard Williams, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - has been included
10047	80	1	80	70	Add Spafford and MacDougall (2020) to the table. Also check Table 1 in that paper. Spafford conducted an extensive literature review of TCRE values which seems more complete than the table given here. [Andrew MacDougall, Canada]	Accepted - all references in Spafford and MacDougall (2020) have been considered and appropriate references were included in the table.
41699	80	49	82	2	Table 5.7: please explain "TCRE" in the table caption. Moreover, it is not clear to me why TCRE is given in Kelvin, even though it has been Celsius in the previous subchapters [Katharina Meurer, Sweden]	Accepted - has been clarified and consistent units of °C per 1000 PgC have been used
21861	80	51	81	1	Given that a number of the studies being synthesised here use earlier versions of temperature products which, as chapter 2 shows, have been shown to be low-ball estimates what attempts have been made to account for this in this assessment and should that be made more explicit in revisions because it really wasn't clear to me as presently discussed. [Peter Thorne, Ireland]	Accepted - In each case the notes highlight the temperature definitions applied in each study.
33365	80				Table 5.7. No need to put reference between brackets. [Guiomar Rotllant, Spain]	Editorial - this kind of issues will be taken care of during the production phase, which includes professional copy-editing.
20003	81	5	81	19	When reading through the TCRE subsection, one notes how often the specific effect of permafrost melting is mentioned. One might consider providing 2 TCRE brackets estimates, depending whether the impact of permafrost evolution is included or not. [philippe waldteufel, France]	Rejected - To our knowledge MacDougall & Friedlingstein 2015 and Gasser et al. 2018 are the only two studies to explicitly consider the effect of the permafrost carbon feedback on TCRE. Given how slow the permafrost carbon feedback is expected to be (e.g. Schuur et al 2015) evaluating its effect is more a concern for ZEC than TCRE.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
45821	81	7	81	8	Please clarify that the quoted TCRE range only describes the slope of the quasi-linear relation, and does not account for non-linear feedbacks or effects, such as the Zero Emissions Commitment (ZEC). [Twan van Noije, Netherlands]	Accepted - it has been clarified that TCRE should be considered in combination with ZEC
21859	81	8	81	8	Given that Pg has been the unit all the way up to present use of /1000PgC may be better here than adding a new prefix in terms of readability? [Peter Thorne, Ireland]	Accepted - units have been harmonized
114677	81	8	81	8	This unit could need some explanation (also used in table5.7) [Jan Fuglestvedt, Norway]	Taken into account - Units have been harmonized
52697	81	8	81	9	It would be helpful if this sentence specified that this is referring to GSAT? [Katarzyna Tokarska, Switzerland]	Accepted - the next sentence makes this point explicitly
52699	81	8	81	11	It would be helpful to clearly indicate here that by defition, TCRE refers to the anthropogenic only component of warming (free of internal variability). However, Individual model simulations are subject to internal variability of the climate system (even in 1pctCO2-only simulations). (Reference: Tokarska et al., Uncertainty in carbon budget estimates due to internal climate variability (in review at ERL)). [Katarzyna Tokarska, Switzerland]	Accepted - this has been clarified. A reference to the study was included earlier in the Section.
45823	81	9	81	11	ECS and TCR estimates from CMIP6 models are not used to constrain the ECS and TCR estimates of Chapter 7. Please explain if this has any implications for the consistency with the TCRE estimates presented in this chapter. [Twan van Noije, Netherlands]	Taken into account - the TCRE assessment is based on a combination of the Chapter 7 TCR estimate and an assessment of the Airborne fraction. CMIP6 models are one line of evidence that support the robustness of this range.
114697	81	22	88	24	This is a well written section with high policy relevance. The coordination with WGIII on the definitions and applications will help in the further development of the section. [Jan Fuglestvedt, Norway]	Taken into account - The carbon budget definitions have been coordinated across working groups.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
86621	81	22			Entire section 5.5.2 reads like a summary of Rogelj et al 2018, It should be an assessemnt of ALL lines of evidences of the remaining budget. I understand that Joeri is the lead author in charge of this section and is naturally inclined to present his method here, but this could easily be seen as a severe conflict of interest... Many other estimates have been published and none are reported here (the papers are only briefly mentioned in the introduction of the section). The Rogelj et al estimate is only one estimate amongts several. This chapter should assess all lines of evidences and then provide a best estimate of the remaining C budget. This is how it's always done in IPCC, especially for key numbers. They should not be based on one single method (if they are the confidence should be low, as lack of multiple lines of evidences). I'm sorry but this entire section needs to be fundzmentally rewritten to become a robust assessment of the remaining carbon budget. . [Pierre Friedlingstein, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - All sections of the chapter (thus including Section 5.5.2) are supported by the entire author team. The assessment approach builds on the approach developed in Chapter 2 of the IPCC Special Report on Global Warming of 1.5°C (SR1.5), with reference Rogelj et al (2018). The reviewer is thus correct that this section strongly builds on the very recent IPCC SR1.5 assessment work on the remaining carbon budget. However, the section is neither a summary of a previous study, nor is the method of a specific author. The estimates that use the IPCC SR1.5 approach use multiple lines of evidence to estimate TCRE, ZEC, human-induced warming to date, and all other components. The numbers thus represent a consolidated assessment of all these lines of evidence. This relationship and assessment approach has now been more explicitly clarified in the chapter.
18495	81	24	81	24	Contradiction between RCB statement and glossary definition. I actually prefer this sentence to the glossary. [Jim Skea, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - the glossary definitions have been coordinated across working groups.
15975	81	24	81	33	The introductory paragraph on remaining carbon budgets is incomplete without a statement on what is considered to be a safe temperature rise above the pre-industrial baseline. Our assessment is that any temperature rise over 0.5degC is ultimately dangerous due the risk on interacting feedback mechanisms, see https://cop23.unfccc.int/documents/65014. For example it is already clear at the current radiative forcing imbalance that the long term prognosis for the Greenland and Antarctic Ice Sheets are very poor and many critical ecosystems, such as the coral reefs, are already critically threatened. As such talk about "carbon budgets" is misleading by implying further emissions or rises in radiative forcing are possible without seriously detrimental effects. [Kevin Lister, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - The assessment of remaining carbon budgets takes no position on which level of warming is could be considered safe. This would be a value judgment which the IPCC cannot make.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
44303	81	25	81	31	The non-CO2 forcing contribution to remaining carbon budgets is explored extensively in Jenkins et al (submitted 2020). It is argued the sampling of IAM scenario database for a percentile likely contribution from non-CO2 sources is dangerous and doesn't represent the 2D nature of the problem. CO2-fe emissions can help highlight the 2D (CO2 and non-CO2 CO2-fe cumulative budgets) nature of the problem, and show how an obeservationally-consistent best estimate of the TCRE over history doesn't appear consistent with the IAM scnearios labelling of scenario catagories if GMST is used as temperature metric. [Stuart Jenkins, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - the study has been included in the overview of studies, as well as in the assessment of the non-CO2 contribution. The AR6 assessment of remaining carbon budgets does not sample the IAM database for a percentile likely contribution as the study presenting the database (Huppmann et al, 2018) already explicitly highlighted in its good practice for analysing ensembles of opportunity of IAM scenarios to not "interpret the scenario ensemble as a statistical sample or in terms of likelihood/agreement in the literature". Following this well-established feature, this assessment therefore does not use percentiles to quantify a likely contribution. AR6 WG1 has decided to take GSAT as its central temperature metric.
73243	81	27	81	27	Replace 'like' with 'such as' (better English). [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted
90837	81	31			Other factors shall be included, such as, the extra emissions from Earth system processes or feedbacks that are typically not included in the models used to make these estimates, such as thawing permafrost; the amount of warming still in the pileline even emission are brought back to zero, and etc. [Vivien How, Malaysia]	Accepted - These factors are included in the assessment of the remaining carbon budget
18499	81	36	83	22	A small box that concisely explained the different definitions in use, and that have been used, with their advantages and disadvantages would be really helpful. [Jim Skea, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. Space limitations prevented us from including a box however all definitions are given in section 5.5.2.1.
114679	82	5	82	8	No mention of net zero here. Please update definitions in accordane with coorination activties going on. [Jan Fuglestvedt, Norway]	Taken into account - The carbon budget definitions have been coordinated across working groups.
45825	82	6	82	6	"starting today". Please indicate the reference year used in the definition. [Twan van Noije, Netherlands]	Taken into account - This has been made consistent with the AR6 glossary
18497	82	6	82	8	As far as I can see this is NOT consistent with the SR15 usage as in the report glossary. [Jim Skea, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - The carbon budget definitions have been coordinated across working groups.
40911	82	6	82	9	The current glossary definition for remaining carbon budget says "...from the start of 2018...". Should it be updated to '...starting today (or at a point in the recent past...'? Also, the SR1.5 definition was more specific than this one, saying it is the net global anthropogenic CO2 emissions...' and 'to the time that antrhopogenic CO2 emissions reach net zero'. Please advise on how to reconcile the chapter definition with the glossary definition. [TSU WGI, France]	Taken into account - The carbon budget definitions have been coordinated across working groups.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
52701	82	7	82	8	It would be helpful to clarify that this definition makes it irrelevant to calculate carbon budgets from most of SSP or RCP scenarios (that allow only for calculation of TEBs) [Katarzyna Tokarska, Switzerland]	Taken into account - This was included when discussing the drawbacks of TEBs
18503	82	11	82	14	though not necessarily within the 21st century - see examples in the SR15 database [Jim Skea, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - the statement doesn't refer to a specific time frame.
33367	82	27			Avoid starring sentences with TABs. [Guiomar Rotllant, Spain]	Rejected - TABs is an acronym defined in the previous sentence.
58791	82	35	82	35	Would read better "…thus TEBS also do not provide a precise estimate of the remaining…" [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account - the statement was edited
51213	82	40	82	40	Temperature levels is potentially misleading when applied to 1.5C, 2C etc as it could be misinterpreted as indicating that these are the absolute temperature of the Earth. The SPM uses "Global Warming Levels" and this term is preferable since it makes clear it is about a change in temperature not absolute temperature. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - "temperature" was removed in this sentence.
18505	82	44	82	55	no mention in the text of ZEC of net zero, although it does appear in Figure 5-31. It really needs explained in the text as pat of the framework as it bridges the net zero /T limit definitions of CB. [Jim Skea, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - these factors have been introduced explicitly
51215	82	46	82	46	The use of "global warming levels" as here in line 46 is preferable to "temperature levels" as used in line 40 above. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Accepted
78539	82	50	82	50	“four” contributing factors? Include ZEC – then it’s five. [Chris Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - ZEC is explicitly highlighted as a fifth factor
52703	82	51	82	51	It would be good to clarify that historical warming is referring to human-induced warming (free from internal variability)? [Katarzyna Tokarska, Switzerland]	Taken into account - this is highlighted in section 5.5.2.2.2
112589	82	54	82	55	CO2-fe emissions (Jenkins et al, 2018) merit more than a passing mention as "another way of doing it": for example "The remaining CO2-fe budget is (unllike the CO2 budget) constrained by physics, scenario-independent, and determined by the current level of warming and the TCRE (Leach et al, 2018; Mengis and Matthews, 2020; Jenkins et al, 2020). Hence a complementary method of determining remaining carbon budgets to that taken in this Assessment is to determine the CO2-fe budget and subtract the contribution due to non-CO2 forcing. Over multi-decade timescales, this contribution can be approximated by DeltaF/alpha, where DeltaF is the change in net total non-CO2 forcing and alpha = AGWP_H/H, AGWP_H being the absolute global warming potential of CO2 over a time-horizon H similar to that for which the budget is calculated. This expression follows from the definition of AGWP and the assumption of a constant TCRE." [Myles Allen, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - These new contributions are highlighted in more detail in the subsection dealing with non-CO2 warming.
114681	82	54	82	55	Some more attention and assessment of these approaches could be useful. [Jan Fuglestvedt, Norway]	Accepted - These new contributions are highlighted in more detail in the subsection dealing with non-CO2 warming.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
127845	83	1	83	23	For Figure 5.31, the numeric uncertainty in the CO2-only contribution is noted, but not the mean (so as to implicitly determine the type of distribution involved). The numeric uncertainty for the non-CO2 contribution in never specified or referenced. Given its size/importance, this should be noted somewhere and referenced. [Trigg Talley, United States of America]	Taken into account - This is now explicitly noted in Section 5.5.2.2.3 on the non-CO2 warming contribution
29999	83	2	83	4	Having a short subsection on the estimate of "total carbon budget" would be disirable. Although it seems redundant to add such a subsection when historial and remaining carbon budgdet are both estimated, the pieces of information are currently scattered in the text (section 5.2.1 and 5.5.2). Total carbon budget often becomes an issue at IPCC plenaries so it would be nice to put together the pieces of information. [Michio Kawamiya, Japan]	Accepted - a concluding sentence has been included to highlight this information.
20005	83	9	83	23	Figure 5.31 (rhs), while providing a clear description, raises a question: is it a general rule that the ZEC constraint is more severe than the global warming limit of interest as displayed here? Please comment. [philippe waldteufel, France]	Taken into account - this has been explained in the caption
73245	83	19	83	20	Remove split of numbers and units across line break. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Noted - Authors are not responsible for final type-setting of the report
32001	83	33			TCRE Few people will read this chapter through form start to finish. Many may enter here. Thus extremely important terms like TCRE should be spelled out in full, especially when section headings, and repeated reminders of what they mean should be subtly given, even if there is an acronym guide somewhere else in the monster download. [Euan G. Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - TCRE is introduced at the start of Section 5.5. and is defined in the Glossary. Spelling out terms every time is not practical.
18509	83	34	83	38	I infer that you subsequently assume this is normally disitributed round a mean of 1.6. Good to say so. [Jim Skea, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - This has been explicitly spelled out in the combined assessment section for TCRE (5.5.1.4)
52705	83	37	83	38	A short justification of why this choice of distribution remains valid would be helpful. Please note that a aecent study (e.g. Spafford and MacDougall 2020) suggests support of the log-normal distribution. [Katarzyna Tokarska, Switzerland]	Accepted - This has been explicitly spelled out in the combined assessment section for TCRE (5.5.1.4)
52707	83	41	83	41	This section 5.5.2.2.2. contains very useful information, but it does not mention the role of internal variability. It would be helpful to clarify that this section is specifically about human-induced warming, and that internal variability should be removed or accounted for (since the human-induced anthropogenic warming is not subject to it) [Katarzyna Tokarska, Switzerland]	Accepted - this aspect has been highlighted explicitly
116459	83	42	83	56	please refer to the cross chapter box in chapter 1 here. [Valerie Masson-Delmotte, France]	Taken into account - A cross-reference to Cross-chapter Box 1.2 was included here.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
52709	83	46	83	47	It would be helpful to clearly state here what temperature definition should be used for the remaining carbon budgets (i.e. GSAT), and why using blended temperature definition causes issues for future levels of warming). Box 2.3 does list different choices, but it does not explicitly recommends which temperature metric should be used for carbon budgets. This should be clearly stated either here or in that box. Also, the issues with using GMST for future warming levels should be clearly stated either here or in Box 2.3 - currently, it is unclear at first glance why GMST in the future is not a good idea (i.e. mention the divergence in sea ice coverage among the models that would introduce further inconsistencies to the blending method). [Katarzyna Tokarska, Switzerland]	Taken into account - This is explicitly highlighted with reference to Cross-Chapter Box 1.2 and 2.3.
52711	83	49	83	50	It would be helpful to clarify here that this is a metric at full coverage, and not blended (If I understand it correctly) [Katarzyna Tokarska, Switzerland]	Taken into account - This has been clarified.
93477	83	49	83	52	Chapter 3 Executive Summary states "The likely range of human-induced warming in global-mean surface air temperature (GSAT) in 2010-2019 relative to 1850-1900 of 0.8-1.4°C encompasses the observed warming of 1.0-1.3°. But here we have: "We here apply an assessed historical warming estimate expressed in global average surface air temperatures (GSAT) of 1.1°C (0.9–1.3°C, likely range) between the 1850–1900 and 2010–2019 periods, based on the assessment of human-induced global warming by Chapter 3 (Section 3.3)". So .... is it +/-0.3C or +/- 0.2C? [David Clarke, Canada]	Accepted - Historical warming estimates have been made consistent with the Chapter 3 assessment.
30001	83	49	83	52	Using GSAT is consistent with AR5, but seemingly inconsistent with SR1.5, in which GMST is adopted as the main indicator for temperature rise on global scale. There should be an explanation on why consistency with AR5 is prioritized compared to that with SR1.5. [Michio Kawamiya, Japan]	Taken into account - The carbon budget assessment in SR1.5 uses GSAT as its default temperature metric, while also reporting how numbers would change under GMST. This has been made explicit, including a reference to the Cross-Chapter Box 2.3.
3567	83	49	83	52	The text describes that an assessed historical warming estimate expressed in global average surface air temperatures (GSAT) of 1.1°C (0.9–1.3°C, likely range) between the 1850–1900 and 2010–2019 periods, whereas in FOD (p. 66 lines 32-34) historical warming expressed in global average surface air temperatures (SAT) was 0.97°C between the 1850–1900 and 2006–2015 periods. This means temperature increased by 0.13°C between 2006-2015 and 2010-2019.Is this understanding correct? If yes, this is so drastic and need additional explanation. [Mitsutsune Yamaguchi, Japan]	Taken into account - the updates in estimates of historical warming are discussed in detail in Cross-Chapter Box 2.3, which is now referenced explicitly.
73247	83	51	83	51	Delete 'the' and 'periods' [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted
3569	83	52	83	53	The text describes that the application of GSAT temperature metrics is fully consistent with AR5. On the other hand in FOD (p. 66 lines 34-35), temperature increase is calculated based on IPCC SR 1.5. Generally speaking, the most recent report (SR1,5) reflect the most recent literatures at that time compared to the old one (AR5). The authors need to explain why they refer to AR5 instead of SR1.5. Was there any flaw in SR1.5? [Mitsutsune Yamaguchi, Japan]	Taken into account - The carbon budget assessment in SR1.5 uses GSAT as its default temperature metric, while also reporting how numbers would change under GMST. This has been made explicit, including a reference to the Cross-Chapter Box 2.3.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
44305	83	52	83	56	Could it be noted that reporting GMST and GSAT remaining carbon budgets would be the way to be most transparent and consistent between previous reports and the AR6 document? [Stuart Jenkins, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - the aspects of GMST and GSAT are now covered with reference to Cross-Chapter Box 2.3. The AR6 WG1 contribution applies GSAT as its central temperature metric. There is no difference between assessed historical warming in GSAT or GMST.
52713	83	55	83	56	Perhaps it would be useful to also mention that other periods may be subject to stronger decadal variability, thus, introducing further inconsistencies [Katarzyna Tokarska, Switzerland]	Rejected - There is insufficient space to go into the details of this aspect, particularly because it has already been clarified that the human-induced component should be considered.
114683	83	55	83	56	It woudl be useful with some quantification of this; as well as references to CCB 2.3 [Jan Fuglestvedt, Norway]	Taken into account - a cross-reference to Cross-chapter Box 2.3 was included here.
114689	84	1	84	30	non-CO2 forcers play a critcial role for the low temp levels, and the treatment of this group in the context of remaining carbon budgets is important to make clear. You refer to internally conistent evolutions used. I think you could make it even more clear how the non-CO2 ranges are extracted from the scenario classes and assigned to the levels of cumulative CO2 [Jan Fuglestvedt, Norway]	Accepted - We have made clearer the role of non-CO2 gases in changing the carbon budget provided as uncertianty in the text and table 5.8
32003	84	1	84	30	This paragraph is important and yet hard to read. It's long and without a break. It is important because the Paris Agreement scenarios essentially relied on convincing politicians there is hope that methane gas leaks and landfills etc can be cut to give a quick hit on warming while we tackle the essential but much tougher and politically more sensitive task of cutting CO2. [Euan G. Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - the paragraph has been edited for readability.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
44307	84	1	84	30	Jenkins et al (submitted 2020) also centres on estiamting non-CO2 contribution, shows that the range of non-CO2 contributions implied from IAM database of scenarios used in SR15 in terms of CO2-fe emissions. This makes them totally equivalent to the carbon budgets of those scenarios, and we can given a range of reported non-CO2 emissions budgets remaining in ambitious mitigation scenarios. By not including calculation of temperature response wee reduce uncertqainty in the thermal response characteristics of the non-CO2 pollutants, and argue we can't simply use IAM scnearios to characterise non-CO2 warming contribution because distribution is heavily biased by the IAM groups modelling decisions exogenously set before model is run. [Stuart Jenkins, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - This study is now highlighted as an alternative approach. The observation that IAM scenarios can't be used as a probabilistic distributions is consistent with the user guidance provided with the scenarios database. The AR6 assessment of remaining carbon budgets therefore does not sample the IAM database for a percentile likely contribution as the study presenting the database (Huppmann et al, 2018) already explicitly highlighted in its good practice for analysing ensembles of opportunity of IAM scenarios to not "interpret the scenario ensemble as a statistical sample or in terms of likelihood/agreement in the literature". Following this well-established feature, this assessment therefore does not use percentiles to quantify a likely contribution.
127847	84	1	84	30	As currently worded, this is difficult to understand (and seems to contrast with statements elsewhere about the importance of non-CO2 gases). [Trigg Talley, United States of America]	Taken into account - the revised section now highlights the magnitude of estimated non-CO2 warming contribution and therewith clarifies their importance.
13457	84	1	84	30	It's recommended to explain in this section that other processes o gaseous cycles are directly related with the non-CO2 emissions. [Maria Amparo Martinez Arroyo, Mexico]	Rejected - there is insufficient space in this section to discuss this, although the section references Section 7.6 in which this is discussed in detail.
40721	84	1		34	section 5.5.2.2.3: the assessed contribution from non-co2 warming (i.e. the conclusion of this section) is not that clear [TSU WGI, France]	Accepted - this has been clarified.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
66181	84	1			The role of N2O in cumulative carbon emissions is understated in this report. The sections where CCE and N2O are/could be discussed include 1.6.3 (Fig1.26), 5.5.2.2.3 (Fig 5.31), 7.1, SPM Box 2 (Table 3). For example, Ch. 7 has a key statement: "Therefore, the impacts of CO2, N2O and other long-lived gases are usually functions of cumulative emissions.(P 7-113 / L34)". The discussion about the linearity of the CCE vs T response across scenarios and the conclusion is a bit optimistic, especially when looking at 1.5C or 2C, where CCE ramps down and may reverse. For these, I question the utility of TCRE/CCE without including N2O. For example, the CCE for the for the two lowest warming SSPs is 578 & 1279, while the equiv CCE-N2O over the same period (2015-2090) ranges from 190 to 350 GTCO2e, a large fraction of the CCE. The problem with ignoring N2O is that the path to carbon neutrality is unlikely to reduce N2O: for CO2 it is CCS/BECCS and renewable energy, while for N2O, it is based on feeding people. The ability to control N2O emissions from fixed-N is not well studied and has no obvious strategy (at least as I can find here). N2O emissions look harder to control than any other SLCFs like CH4. There is an odd note (Ch 5-88 L14) that says something about "used to estimate the non-CO2 contribution across a wide variety of stringent mitigation scenarios (Huppmann et al., 2018)" - I looked up the Huppmann commentary, but could find little on non-CO2 or N2O. Maybe putting the SLCF & N2O equiv CCE in SPM Box 2 Table 3 would add a useful perspective. [Michael PRATHER, United States of America]	Taken into account - The reference to Huppmann et al (2018) is included because this refers to the scenario database of stringent mitigation that was compiled as part of the IPCC SR1.5. This has been clarified. The other aspects do not seem to apply to this section on remaining carbon budgets.
90839	84	1			Refer study "CO2 and non-CO2 radiative forcings in climate projections for 21 century mitigation scenarios" for non-CO2 radiative force and model [Vivien How, Malaysia]	Rejected - this study discusses evolutions in scenarios that do not cap cumulative emissions and thus not applicable in the context of remaining carbon budgets.
66677	84	2	84	4	Suggest a rewrite of this sentence: "Because non-CO2 forcings contribute either cumulatively (N2O) or in proportion to emissions (methane, short-lived forcings) to global mean temperatures, emissions of non-CO2 emissions also affect estimates of remaining carbon budgets." That would bring out the stock/flow distinction, shorten the sentence, and describe the relationship between other emissions, temperatures, and RCBs. [Dave Frame, New Zealand]	Accepted - the suggestion was adopted with small edits as the introductory sentence to this section.
90111	84	2	84	5	This statement uploaded by Ted Schuur on behalf of a graduate class that reviewed the chapter (this was communicated with IPCC TSU in advance): sentence could be shortened without removing substance: "Projected global average warming of non-CO2 emissions affects estimates of remaining carbon budgets consistent with limiting warming to specific temperature thresholds" [Edward Schuur, United States of America]	Taken into account - The entire section was edited for readability.
58793	84	2	84	6	This sentence is long and doesn't make sense- maybe it should read "are consistent" but I am not sure. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account - The entire section was edited for readability.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
115415	84	2		30	Jenkins, Cain, Friedlingstein, Gillett and Allen (2020) - Quantifying non-CO2 contributions to remaining carbon budgets) is relevant here too. I can provide a copy if needed. [Gillett Nathan, Canada]	Accepted - this study has been included
52715	84	6	84	9	Please note that "Tokarska and Gillett 2018" is an incorrect citation in this paragraph. The relevant citation is: Tokarska et al. 2018 The influence of non-CO2 forcings on cumulative carbon emissions budgets (ERL). [Katarzyna Tokarska, Switzerland]	Accepted - has been corrected
66679	84	10	84	13	Sentence doesn't scan, or make sense. Actually the whole paragraph could do with a rewrite - it looks hastily put together and isn't very clear. [Dave Frame, New Zealand]	Taken into account - The entire section was edited for readability.
90109	84	10	84	13	This statement uploaded by Ted Schuur on behalf of a graduate class that reviewed the chapter (this was communicated with IPCC TSU in advance): It is unclear how the non-CO2 warming contributions can "reflect societal developments". Is this supposed to be referring to the need to adjust non-CO2 budgets based on the CO2 emission trajectory? Additionally, I feel that everything following the citations and dash could be removed without adversely affecting the content or clarity of the sentence. [Edward Schuur, United States of America]	Taken into account - The entire section was edited for readability.
13447	84	21	84	21	Add space before the word “Cross”. [Maria Amparo Martinez Arroyo, Mexico]	Accepted
33369	84	21			Change: “…2018a).Cross…” by “2018a). Cross…”. [Guiomar Rotllant, Spain]	Accepted
112587	84	26	84	27	Hence the current level and rate of warming determine remaining carbon budgets under ambitious mitigation. A reference to Leach et al (2018) would make sense here. And for the treatment of non-CO2 climate forcing, a complementary approach to those listed here is that of Mengis and Matthews (2020) and Jenkins et al (2020) both of which include non-CO2 forcers as CO2-forcing-equivalent emissions (Jenkins et al, 2018). The CO2-fe literature should be assessed because the remaining CO2-fe budget is constrained directly by the TCRE. [Myles Allen, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - the current level and rate of warming only determines remaining carbon budgets if the fractional contribution of non-CO2 drivers to warming remains unchanged. This, however, is not the case, see SI Leach et al (2018).
66681	84	27	84	30	Sentence unclear and doesn't really bridge to what we are doing in Chapter 7. Suggest this instead: "Additional methods that can estimate the climate (or warming) "equivalence" of non-CO2 forcings have recently been suggested and are assessed in Chapter 7 Section 7.6." The point about uncertainties and the literature are peremptory here - we can discuss that in chapter 7. [Dave Frame, New Zealand]	Taken into account - The sentence has been included with minor edits. However, because a reason should be given as to why these methods are not applied here, the second part cannot be removed. To our knowledge, it is an accurate reflection of the state of the literature on this topic.
114687	84	27	84	30	It would strengthen the assessment if you could try and apply these additional methods. [Jan Fuglestvedt, Norway]	Taken into account - these methods require the change in non-CO2 forcing to be estimated and can then be applied.
66683	84	37	84	37	Suggest rewriting this title. It doesn't scan well in English. How about just "Potential limitations of TCRE" [Dave Frame, New Zealand]	Taken into account - This section was split up into the ZEC and other Earth system contributions and edited for readability.
18507	84	37	85	14	I think a separate section on ZEC would be good. Basically a subsection for each element of table 5.8. [Jim Skea, United Kingdom (of Great Britain and Northern Ireland)]	Accepted

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
40723	84	37	86	2	section 5.5.2.2.4: I think the introduction to this section could be shortened. [TSU WGI, France]	Taken into account - This section was split up into the ZEC and other Earth system contributions and edited for readability.
78541	84	37			Split this section into two: ZEC and “other feedbacks” are separate issues. First paragraph can be dropped as it duplicates statements in chapter 4 (4.7.2) – you take the assessment from chapter 4, so no need to replicate text. [Chris Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - the first part of this discussion was removed
98581	84	39	84	42	The wording of the glossary definition of ZEC suggests that it is always positive: "climate change commitment that would result from setting carbon dioxide (CO2) emissions to zero". But Chapter 5 and Chapter 4 point out that there is low confidence about the sign. This not well explained to people who dont know the full story already. Can the glossary definition be refined? And presumably the sign will be affected by the rate of change of non-CO2 forcers as well as geophysical factors. [Jim Skea, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - the glossary definition has been refined.
114685	84	47	84	47	I find "do not contribute to substantial warming" a bit vague; can you be more quantitative? [Jan Fuglestvedt, Norway]	Does not apply anymore as this section was removed because of duplication with Chapter 4
45827	84	49	84	49	Change "range cumulative" to "range of cumulative". [Twan van Noije, Netherlands]	Accepted - change was made.
35125	84	55	85	1	For the ZEC, should add MacDougall et al. (2020) .Is there warming in the pipeline? A multi-model analysis of the zero emission commitment from CO2, Biogeosciences Discuss., https://doi.org/10.5194/bg-2019-492 [Richard Williams, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - the main ZEC assessment is in Section 4.7.2, where this is covered.
35127	84	55	85	14	There is uncertainty in the estimates of the ZEC from not knowing the time evolution of climate feedback parameter or the efficacy. This comment applies to the comprehensive study by MacDougall et al. (2020) .Is there warming in the pipeline? A multi-model analysis of the zero emission commitment from CO2, Biogeosciences Discuss., https://doi.org/10.5194/bg-2019-492 [Richard Williams, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - the main ZEC assessment is in Section 4.7.2, where this is covered.
51161	85	1	85	7	Beside the time frame of half a century and beyond discussed here, it would also be useful to discuss ZEC on shorter timescales to help the reader understand whether the targets in table 5.8 should be percieved as achievable without overshoot (or not). [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - It is now highlighted that the ZEC on shorter time scales does not differ much.
52717	85	5	85	6	Please clarify if this ZEC value is relevant for the 1.5C budget only, or also for for higher target warming levels too? It would be helpful to clarify when the +/-0.18C value for ZEC can be used. [Katarzyna Tokarska, Switzerland]	Accepted - this has been clarified.
73249	85	7	85	7	Incorrect English! 'Change to either increase or decrease' [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted
16535	85	8	85	8	Since it is expected to be around zero, rather than saying there is low confidence in the sign, It might be more useful to assess the confidence that magnitude of ZEC is less than a specific value (according to 4.7.2.2.2.1 it is likely less than 0.18 deg) . [William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - the section was shortened to avoid overlap with Section 4.7.2

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
58827	85	10	85	11	Is the 1%increasing trajectory in reference to 1%emissions increase per year? If so say per year. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Does not apply anymore as this section was shortened because of duplication with Section 4.7.2
4255	85	21	85	22	Logically, this sentence should start with mentioning "...additional carbon emissions to the atmosphere..." and end with "...increase the value of TCRE". Below is our suggested revision: "… this process is anticipated to both add additional carbon emissions to the atmosphere over timescales of centuries to millennia and increase the value of TCRE". [Claude-Michel Nzotungicimpaye, Canada]	Taken into account - the sentence was edited
10323	85	22	85	24	CDR implementation at a large scale need not only be used to compensate for overshoot of long-term climate stabilization targets. In the absence of any overshoot, it could be used to reduce atmospheric CO2 concentrations. [Chris Vivian, United Kingdom (of Great Britain and Northern Ireland)]	Noted - It is unclear which correction, or clarification the reviewer wishes to be included. This section does not refer to CDR.
73251	85	26	85	26	Change to Gregory et al. (2009) [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - References have been double-checked for layout.
33371	85	26			Change: "…by applying the reverse method by (Gregory et al., 2009)." By "…by applying the reverse method by Gregory et al. (2009).". [Guiomar Rotllant, Spain]	Accepted - References have been double-checked for layout.
32005	85	28	85	29	I'm not sure there is high agreement here. Rapid vegetation growth picks up CO2 and enhanced methanotrophy oxidises CH4 thus the net emissions may not be great, or even negative. My own (admittedly local) field work in areas of discontinous permafrost would suggest both processes are very active as boreal wetlands extend northwards. [Euan G. Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - The growth of plants is already accounted for in the ESMs, thus does not constitute part of the set of unrepresented feedbacks. There is no evidence that methanotrophy results in an increase in ecosystem carbon stocks — rather methanotrophs feed off of the oxidation of CH4 to CO2 and thus still result in net carbon loss from ecosystems.
89513	85	28	85	55	This material and confidence language needs to be carefully checked against section 5.4 of this chapter and then in particular with the matching section in Chapter 3 SROCC. While I agree that confidence must be assessed, there has been much progress since AR5 (largely reported in SROCC and here) about permafrost carbon emissions. These sections contain many qualifiers downplaying the knowledge gain. It is important that you are estimating a reduction in remaining carbon, even as more work will be done. That message gets a bit lost in the qualifiers here. [Edward Schuur, United States of America]	Accepted - The text in this section builds on Section 5.4 and has been made fully internally consistent in its confidence language.
16077	85	29	85	29	"unrepresented": maybe worth clarifying that this is about feedbacks not represented in ESMs (although the preceding paragraphs already explicitly talk about ESMs)? [Gerhard Krinner, France]	Taken into account - the first sentence of the rewritten paragraph now clarifies this.
41701	85	29	85	29	carbon in the form of CO2 and CH4, as well as nitrogen in the form of N2O will be released [Katharina Meurer, Sweden]	Taken into account - Assessed in the permafrost box on which the discussion and assessment in this section builds

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
90113	85	43	86	2	This statement uploaded by Ted Schuur on behalf of a graduate class that reviewed the chapter (this was communicated with IPCC TSU in advance): When all of the additional Earth system feedbacks are added up, they add up to 210 Gt CO2 per degree C, not 135 Gt CO2 per degree C, which is reported to be the total effect. The reason for this discrepancy should be explained, or any inaccuracies should be fixed. [Edward Schuur, United States of America]	Rejected - The sum of the rounded values reported in the text adds up to 75 - 35 + 100 = 140, which is very close to the 135 GtCO2 that was computed from unrounded numbers.
73253	85	49	85	49	Delete hyphen. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change was made.
73255	85	49	85	50	Remove split of numbers and units across line break. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial - this kind of issues will be taken care of during the production phase, which includes professional copy-editing.
78543	85	52	85	52	Sudden switch of units from PgC to GtCO2. As a physical science chapter, I favour PgC. But I understand need for carbon budgets expressed to policy makers to also be in GtCO2. But I am nervous that mixing them in the text can cause confusion (TCRE is expressed in PgC earlier but modifications to it in GtCO2). One solution is to use PgC throughout the text but translate it into GtCO2 in exec summary and SPM statements. Any figures can have dual axes as per AR5 figure SPM.10. [Chris Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - the units have been changed to PgC, while Table 5.8 is given in GtCO2.
114691	86	5	87	1	It woudl be useful if it can be more clear what temperature metric is used. I suggest you also add budget numbers based on alternative - according to what has been discussed in WGI [Jan Fuglestvedt, Norway]	Accepted - has been clarified in the footnote
114693	86	5	87	1	It would also be useful if you could make a clearer separation about what is due to assumptions (e.g. scenarios for non-CO2) and what is due te geophysical uncertainties [Jan Fuglestvedt, Norway]	Taken into account - The column labels make a distinction between "uncertainties" and "variations". The latter are further clarified in the footnote.
111855	86	5	88	22	The section the remaining carbon budget should include a rough quantitative estimate how the adjusted method of calculating carbon budgets compares to AR5 and or SR1.5, probably for two temperature levels (probably referring to the LTTG range - but then how to represent "well below 2C"?). Not sure why the last sentence in Box 5.1 gives a comparison between AR5 and SR1.5 [Oliver Geden, Germany]	Accepted - This is included in Box 5.1
52719	86	12	86	12	Is this "likely range" referring to 17-83% of TCRE range or the 67th percentile as in Table 5.8 below? (I find it confusing that the phrase 'likely range' may differ to different intervals than the commonly used 17-83% in climate sensitivty assessments. [Katarzyna Tokarska, Switzerland]	Accepted - the range has been specified explicitly.
15405	86	12	86	13	The likely range of TCRE uncertainty corresponds to 17th to 83rd percentiles of TCRE. This is different from what is shown in Table 5.8, 33rd and 67th percentiles, and for this reason the +50% to -25% variation described here is confusing. [Junichi Tsutsui, Japan]	Accepted - the range has been specified explicitly.

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52721	86	13	86	15	This is a very strong sentence, which may not necessarily be correct. Even if the quantities are correlated among each other in the Earth system, their uncertainties do not need to be correlated, if they are measured using different methodologies. See Matthews et al. paper: "A new framework for understanding and quantifying uncertainties in the remaining carbon budget" (in review) that aims to combine different sources of uncertainties. [Katarzyna Tokarska, Switzerland]	Taken into account - while some dimensions could be combined, others will never be combined because they represent uncertainty dimensions reflecting choices.
41703	86	25	87	3	Table 5.8: the "remaining C budget" has to be explained in more detail. It actually becomes clear in the FAQ 5.4, but should be explained much earlier in order to really understand this table [Katharina Meurer, Sweden]	Taken into account - The preceding section 5.5.2 provides a detailed explanation of the remaining carbon budget.
52723	86	27	86	27	Table 5.8. While it is useful that this table has similar layout to SR1.5 Table 2.2., some readers may not find the immediate connection between the uncertainties on the right, and the 33rd, 50th, and 67th percentiles of the remaining budget. It would be helpful to clearly state at least the headings in third column: Remaining carbon budget "subject to uncertainties in columns 4-10". [Katarzyna Tokarska, Switzerland]	Accepted
52725	86	27	86	27	Table 5.8. it may be good to clarify that the level of accuracy in the remaining budgets cannot be higher than +/- 50GtCO2 or similar. (See the limitations of accuracy in estimating remaining budgets in Tokarska et al (in review): "Uncertainty in carbon budget estimates due to internal climate variability" in review at ERL. [Katarzyna Tokarska, Switzerland]	Rejected - This limitation is taken into account in the assessment of TCRE. Additionally highlighting it here would be double-counting of this aspect.
52727	86	27	86	27	It is unclear if the 'Key uncertainties and variations are relevant to all warming levels, or only to the 1.6C warming (since they are aligned with that row) [Katarzyna Tokarska, Switzerland]	Accepted - has been clarified
18511	86	27	86	30	the median RCBs move by 150 GtCO2 for every 0.1 deg C. Ths is not the strict inverse of the TCRE (1.6 deg C/EgC). Presumably this is because eof earth system feedbacks and correlations between CO2 and non-CO2 emission. Or amI wrong. An explanation would be good. [Jim Skea, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - The subsection presenting the non-CO2 contributions now clarifies that the non-CO2 warming correction increases as a function of additional warming.
18513	86	27	86	30	Why use 33/67 percentiles when the range of TCRE in the text is 5/95?the median RCBs move by 150 GtCO2 for every 0.1 deg C. Ths is not the strict inverse of the TCRE (1.6 deg C/EgC). Presumably this is because of earth system feedbacks and correlations between CO2 and non-CO2 emission. Or am I wrong. An explanation would be good. [Jim Skea, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - The subsection presenting the non-CO2 contributions now clarifies that the non-CO2 warming correction increases as a function of additional warming. The 33-67 percentiles are because of the established practice in WG3 to look at the "likely" (>66%) probability of limiting warming to a given temperature level.
34405	86	27			Table 5.8 is ambiguous as to the start date of the remaining carbon budget. I assume that it is January 1, 2020 but this is not stated in the table. Since the avg temperature is used for the 2010's one might aslo choose a start date of 2015. This should be explicit in the table so as to avoid multiple interpretations and it would be helpful to explain in the text why the difference between the temperature period and remaining budget start date and how this is accomodated in the estimated budget. [Haroon Kheshgi, United States of America]	Accepted - has been included

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
78545	86	27			State clearly from which date “remaining” is taken in this table. From 2020? [Chris Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - has been included
73257	86	29	86	29	Delete 'the' and 'period' [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change was made.
111857	86	30	86	30	Third column should explicity state the starting year of the remaining carbon budget, in this case 2020 [Oliver Geden, Germany]	Accepted - has been included
116461	86		86		please provide clarity on the effect of each change compared to the methods, data sets, and time intervals used in AR5, SR15 and here. [Valerie Masson-Delmotte, France]	Taken into account - Box 5.1 speaks explicitly to this point.
52729	87	8	87	8	I found this Box 5.1. quite confusing to read, while it contains very important information regarding the methodological improvements. I would suggest to structure it in a way that makes it clear what problems were identified and how the new framework addressees it. (Currently, the solutions are unclear in some cases). In addition, it would be very useful for Box 5.1 to include information on the two following topics that are central to the remaining carbon budgets methodology: -the use of GSAT metric for future levels of warming (this is only briefly mentioned in one sentence, but it would be helpful to provide reasons and justification behind it - i.e. not subject to changing future sea ice coverage issues, etc.). Please note that Box 2.3. does not clearly or explicity explain these reasons either, which may undermine justification of this choice. -the role of internal variability (that Paris Agreement is anthropogenic warming only, and how to account for internal variability) [Katarzyna Tokarska, Switzerland]	Taken into account - The box has been restructured and focusses on explaining how methodological improvements between AR5 and AR6 impact the remaining carbon budget assessment.
18515	87	8	88	22	This box helps to explain some of the earlier text. Good if it were referred to. [Jim Skea, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - The box is now highlighted explicitly at the start of the subsection
73259	87	26	87	26	Delete 'time'. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change was made.
114695	87	30	87	31	Please check terminology in light of ongoing revisions and coodriations of definitions and usages of concepts [Jan Fuglestvedt, Norway]	Accepted - terminology has been cross-checked with glossary definitions
18239	87	31	87	31	A word needs to be added after pre-industrial to improve the flow of the sentence, or the sentence needs to be rephrased. [Chelsey Baker, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - the box has been edited for flow and readability.
20007	87	32	87	32	"in" is missing after "results" [philippe waldteufel, France]	Accepted - change was made.
15407	87	32	87	33	The 300 GtCO2 upward revision is limited to 1.5C warming. The amount of upward revision depends on warming levels. [Junichi Tsutsui, Japan]	Taken into account - because the adjustment of the historical estimated cumulative emissions and warming is an absolute adjustment and applicable to all warming levels, this adjustment is the same for any level of additional future warming.
41161	87	32		33	what do you mean by upward revision? This phrasing is unclear. [TSU WGI, France]	Taken into account - an upward revision means that the value is revised towards higher values. Other words have been used.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
39913	87	32		33	300 GtCO2 figure repeated at the end of the box. [TSU WGI, France]	Taken into account - the box has been edited for flow and readability.
37699	87	33	87	33	The 300 GtCO2 increase is the sum of all the methodological improvements combined? (cf. page5-88, l.21) It is desired that estimates of individual contributions from AR5 and from SR1.5 are clearly stated. [Masahide Kimoto, Japan]	Taken into account - The box has been restructured and focusses on explaining how methodological improvements between AR5 and AR6 impact the remaining carbon budget assessment.
66685	87	35	87	38	This material could be expressed better. For one thing, "limited evidence was available informing the specific contributions of CO2 and non- CO2 forcers to future warming" is still true because we don't know the future. For another, treating the warming from non-CO2 as fixed, and then deducting that from the available CO2 warming is not obviously a preferred approach (though the text makes it sound that way). I think it would be better to say something like: "Most warming now and in the future is from cumulative CO2 emissions. Other contributions to warming from cumulative emissions of LLCFs like N2O, and from the levels of emissions of SLCFS such as methane, also contribute to warming. Assumptions regarding the future mix of warming from non-CO2 LLCFs and SLCFs determine the remaining warming space available within, and hence the RCB consistent with, a given temperature target." [Dave Frame, New Zealand]	Rejected - While the suggested text is not incorrect, this box intends to clarify the differences between estimates at the time of AR5 and AR6. This intention was not clear in the SOD. The box has been restructured and focusses now solely on explaining how methodological improvements between AR5 and AR6 impact the remaining carbon budget assessment.
73261	87	38	87	38	Capital 'P' for 'phase' [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted
52731	87	44	87	45	I am afraid this sentence is incorrect, or can be easily misunderstood. Actually, in RCP and SSP scenarios the effective TCRE curves are co-linear for most scenarios, and there is no statistically significant difference between carbon budgets derived from different RCPs. For this sentence to be correct, I would suggest rather emphasizing that RCPs and SSPs represent only a few scenarios, none of which were specifically designed to be compatible with the Paris Agreement goal. Non-CO2 forcing influences the slope of the effective TCRE, that's why the new framework seperates the effects of non-CO2 forcing from TCRE, and the scenario uncertainty due to non-CO2 forcing can be accounted for seperately. [Katarzyna Tokarska, Switzerland]	Taken into account - This statement does not speak to RCPs and SSPs specifically, but highlights the range of scenarios that WG3 assesses. The solution is presented in the subsequent paragraph.
116463	87		88		There is overlap between the box and the chapter text here, I do not fully understand the added value of the box. I would suggest considering an infographic representation of methodological changes. [Valerie Masson-Delmotte, France]	Rejected - Several reviewers asked explicitly for a comparison of AR5, SR1.5 and AR6 budget numbers, which this box provides. An infographic was unfortunately not possible due to the tight timeline of delivering the revised sections.
52733	88	1	88	2	It is unclear how these two limitations are actually solved in this new framework, without looking up the literature. I would suggest to explicitly list the solutions or clearly describe them: i.e. by changing the baseline to the present-day period, and by accounting for non-CO2 component separately in the framework. [Katarzyna Tokarska, Switzerland]	Accepted - This has now been highlighted.
18517	88	4	88	4	I know what an "ensemble of opportunity" but I'd defy a first time reader to understand this insiders term. Explain! [Jim Skea, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - has been explained.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
73263	88	6	88	7	Remove split of numbers and units across line break. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial - this kind of issues will be taken care of during the production phase, which includes professional copy-editing.
16079	88	7	88	7	Here and on a few other pages, the unit EgC is used, while in the rest of the chapter, PgC is used. You might consider using PgC throughout. [Gerhard Krinner, France]	Accepted - everything to PgC
52735	88	7	88	8	Please note that it may be confusing providing TCRE in units per EgC but uncertainties in GtCO2. Perhaphs listing the alternative TCRE units in brackets (per 1000 GtCO2) or the uncertainties in PgC and GtCO2 would be helpful to the reader [Katarzyna Tokarska, Switzerland]	Accepted - everything to PgC, with alternative GtCO2 units in brackets
115417	88	8			Give the sign of this change. [Gillett Nathan, Canada]	Accepted
52737	88	11	88	11	Perhaps explicit accounting for ZEC (or other feedbacks) could also be mentioned here, as it is also part of this framework) [Katarzyna Tokarska, Switzerland]	Accepted - this has also been highlighted as a further advancement
44309	88	15	88	15	Jenkins et al (submitted 2020) disputes the usefullness of the IAM scnearios if sampled for a given percentile non-CO2 contribution. [Stuart Jenkins, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - Jenkins is not in dispute with the original study (Huppmann et al, 2018) which states: "Don't interpret the scenario ensemble as a statistical sample or in terms of likelihood/agreement in the literature"
52739	88	18	88	19	This is a very important methodological choice of using GSAT for carbon budgets, and it would be very helpful to provide more justification behind it (e.g. does not run into issues with varying sea ice coverage for future levels of warming). Please note that Box 2.3 (on different temperautre metrics) does not explicitly explain this either, nor makes explicit recommendations for carbon budgets. Therefore, the readers may be left wondering that this was just an IPCC choice but unaware excaly why it is a lot more problematic to use GMST for carbon budgets. A short justification of the reasons for choosing GSAT would be very helpful. [Katarzyna Tokarska, Switzerland]	Accepted - this has been included.
21863	88	18	88	22	Should this passage not mention the delta in the GSAT estimates from AR5 which, arguably have in turn reduced the remaining budgets by a similar amount? [Peter Thorne, Ireland]	Accepted - this is now highlighted explicitly.
52741	88	22	88	22	Since this box is about different methodological choices, it would be helpful to also mention the role of internal climate variability and its relevance to carbon budgets. Even if Paris Agreement refers to the human-induced anthropogenic warming only, the observed warming is subject to internal variability, and how to account for it (or remove it) is another important technicality. [Katarzyna Tokarska, Switzerland]	Rejected - The box is refocussed on explaining the difference between AR5, SR1.5 and AR6. Internal variability is highlighted elsewhere in the section.
111859	88	27	88	56	Why is CDR and SRM in one section (5.6.)? Wouldn't it be better to have two different ones, more clearly distinguishing between the two approaches, like in SR.15, when IPCC started to abandon the category "geoengineering"? You explicitly say that CDR and SRM are treated seperately (line 39f.) in the report, but you could highlight this by dissolving 5.6.1 and seperating CDR as 5.6 and SRM as 5.7 [Oliver Geden, Germany]	Rejected. We made it clear in the introduction that we consider SRM and CDR as distinct response options, and do not see the need to have a separate section for each (the section on SRM would be relatively short). The Ch 5 structure is consistent with that of Ch 4, which assesses the response to mitigation, CDR and SRM in one section.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
93639	88	27	99	32	This assessment of the efficiency of CDR and the symmetry of emissions and removals could be improved by a reference to removals of GHG that are ongoing at a wide scale already, and to ongoing discussions over metrics that appropriately capture the intermittency of certain gases (i.e. methane) in the atmosphere. [Jon Magnar Haugen, Norway]	Noted. It is not clear what this comment is asking exactly. We added a sentence in section 5.6.2.2 clarifying that the status of CDR methods is discussed in detail in WG III Ch 12
86761	88	27	101	36	These discussions on effects of particular response options (CDR and SRM) discusses the implications of such options for various forcing agents/processes, including biophysical factors such as albedo. However, we invite further description of the baseline. After all, CDR with associated albedo changes are already taking place on wide scale, even before they became a part of climate policy. Discussions on CDR and SRM in chapter 5 seem to jump right into interventions, before describing the baseline. Some description is given in chapter 2 p 27, chapter 4 page 80-82 and in chapter 7, page 67-69. However, to understand the baseline correctly, we propose to account for gross amounts under various land use/land cover, and not only net amounts, as any CDR/albedo contribution is relevant even if changes elsewhere work in the opposite direction. [Oyvind Christophersen, Norway]	Noted. We interpret this comment to suggest that land-use changes and their associated biophysical effects should be assesses in the chapter. Unfortunately we do not have room for a comprehensive assessment of land-use change, and we limit our assessment to greenhouse gas emissions from land-use change.
51163	88	31	88	40	Definition of SRM: as in Ch 4, SRM is defined as a modification of only Earth's shortwave radiative budget and then talks about how cirrus cloud thinning is included in the definition (long wave radiation modification). We recommend using and referring to the AR6 glossary definition (it's the same as the SR1.5 definition) - where SRM refers only to short wavelength radiation modification, and, when categorising methods in section 4, say that you are referring to both to SRM and Cirrus Cloud Thinning. (We have made a similar comment on ch.4 p.81 rows 15-27) [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We clarified that cirrus cloud thinning is not included in the formal SRM definition, but is included in the portfolio of SRM measures for convenience
51165	88	34	88	40	This discussion covers very similar material to that on p.81 of ch.4, rows 15-30. It would be better to have just one explanation of what the technologies are, which can be referenced from other chapters as needed. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. We prefer to include brief explanations for the chapter to be self-consistent.
96597	88	49	99	34	Does the literature provide any information on the pros and cons of CDR approaches that build on existing ecosystems (protect, restore, increase) rather than creating new and artificial systems? Such information could be very useful for decision making. If such an assessment will be provided in WGII or WGIII, please note in the text. [Nicole Wilke, Germany]	Accepted. It was clarified that a comprehensive assessment of the co-benefits and trade-offs of CDR methods (including natural climate solutions) is provided in WGII and WGIII.
96599	88	54	89	2	"Potentials" is used in a unspecific way, making it unclear why WGIII and why SRCCL deal with it. It should be distinguished at least between biogeochemical/physical potentials, economic potentials, technological potentials. [Nicole Wilke, Germany]	Accepted. It was clarified that the chapter briefly discusses technical potentials.
39695	88	54			". Effects of CDR methods on climate are also assessed" doesn't it contradict p88, L44? [TSU WGI, France]	Accepted. Sentence was deleted.
17901	89	1	89	1	Note that WG3 Ch3 will discuss use of CDR in scenarios [Katherine Calvin, United States of America]	Noted

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
77753	89	1	89	12	Could used of novel crops to increase production and remove carbon, be included in Table 5.9? [Emer Griffin, Ireland]	Rejected. Because of space limitations, Table 5.9 only lists the most commonly discussed land-based CDR methods. However, novel crops to increase production and remove carbon are included in agricultural management practices to improve soil carbon storage. See also the footnote of Table 5.9 and the discussion in Section 5.6.2.2.1.
58307	89	4	89	4	1.5 degrees --> stay precise and specify celsius! Better use unit like "°C" as done before, to stay consistent [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - change was made.
111861	89	7	89	21	This categorisation differs from the one used in WG3, ch12. There's maybe a cross-WG consistency issue, and it would be good to hedge this. There might be good reasons why categorisations remain (slightly) different, but maybe then it's useful to give an explanation. Preferable would be using the same one, since that needs to be done in the SYR anyway [Oliver Geden, Germany]	Taken into account. For consistency with the WGIII Ch 12 categorization (Fig. 12.3), we introduced an additional mixed biological/technological category for BECCS. The "biological production and storage on land" category is then consistent with the "Natural" or "Land-based" category in WGIII, the "combined" category is consistent with the "combined" category in WGIII , and the ocean biological, geochemical and technological categories distinguish between methods that are all classified as "technological" in the WGIII report.
82087	89	13	89	13	Concerning insufficient literature for assessment, many of the approaches that have not been assessed are ocean based ones. The recent GESAMP report would be good to cite to back up the statement on insufficient literature as they provided the most recent review of most of the listed methods. GESAMP (2019). “High level review of a wide range of proposed marine geoengineering techniques”. (Boyd, P.W. and Vivian, C.M.G., eds.). (IMO/FAO/UNESCO-IOC/UNIDO/WMO/IAEA/UN/UN Environment/ UNDP/ISA Joint Group of Experts on the Scientific Aspects of Marine Environmental Protection). Rep. Stud. GESAMP No. 98, 144 p. [David Keller, Germany]	Accepted. Reference to the GESAMP report was included.
10263	89	13	89	15	This comment is out of date as the report by GESAMP reviewed some 18 marine-based CDR techniques (including variations of approaches) as well as reviewing methane capture and destruction/degradation - see Table 4.2 and section 5 of the report. Reference is GESAMP (2019). “High level review of a wide range of proposed marine geoengineering techniques”. (Boyd, P.W. and Vivian, C.M.G., eds.). (IMO/FAO/UNESCO-IOC/UNIDO/WMO/IAEA/UN/UN Environment/ UNDP/ISA Joint Group of Experts on the Scientific Aspects of Marine Environmental Protection). Rep. Stud. GESAMP No. 98, 144 p. http://www.gesamp.org/site/assets/files/1996/rs98e-1.pdf [Chris Vivian, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The statement is consistent with the GESAMP report, which characterizes the knowledge base for "Depositing crop waste or biochar in deep ocean", ocean downwelling, and CO2 removal from seawater as low (Table 4.2).

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
130407	89	13	89	15	for recent literature on ocean carbon removal see SROCC and SR 1.5C and IDDRI Brief 02/19 Gattuso J-P et al [Alberto Pedace, Argentina]	Accepted. References to more recent literature were included.
58345	89	15	89	15	The citation given (Keller et al., 2018b) only briefly mentions seawater & CCS, and not the other mentioned CDR options (biomass burial, ocean downwelling, cloud alkalinisation). However, Keller et al. (2018a) does mention these techniques as examples of knowledge gaps, so updated this citation from 2018b to 2018a should be sufficient. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. The citation was updated.
127849	89	18	89	21	Regarding carbon dioxide removal (CDR) methods summarized in Table 5.9 and discussed in more depth on pages 90-166, enhanced biological production and storage on land offers the safest, most practical, technologically accessible, and ecologically responsible CDR strategy, for the simple reason that the living plant is the most direct and powerful means to remove excess CO2 from the atmosphere and sequester the carbon in a stable combined form -- stable soil organic matter. This is how the natural carbon cycle has operated ever since land plants evolved 450 million years ago. Lal et al. (2018) have estimated that, over the next 60 years, optimized biological production and land management can potentially store an additional 333 Pg carbon in soil and biomass, which would lower end-of-century atmospheric CO2 concentration by 156 ppm. Afforestation, reforestation, and wetland restoration can sequester more than 2 Mg C (7.33 Mg CO2) per hectare annually as soil organic carbon (SOC) and perennial plant biomass. Best agricultural soil management systems for annual crop rotations, especially organic farming systems that integrate cover crops, diversified rotations, compost and other organic amendments, and judicious tillage; or conservation agriculture that integrates cover crops, diverse rotations, organic amendments, no-till, and judicious use of agrochemical inputs can sequester 0.45-0.67 Mg C/ha-year (1.6-2.5 Mg CO2/ha-yr) (based on multiple studies reviewed in NSAC, 2019, and Schonbeck et al., 2018). While avoiding tillage protects the soil biota from physical disturbance, the herbicides needed to effect no-till in annual crop production can also damage mycorrhizal fungi and other key components of the soil microbiome (Klein, 2019). Advanced grazing management systems such as management intensive rotational grazing (MIG), and agroforestry practices like alley cropping, permaculture, multistory cropping, forest gardening, silvopasture, and woody perennial conservation buffers can sequester >2.25 Mg C/ha-yr (8.2 Mg/ha-yr CO2) (Feliciano et al., 2018; Machmuller et al., 2015; Teague et al., 2016; Wang et al., 2015). In addition, organic farming systems protect the	Noted. The revised text and table more clearly distinguished between natural climate solutions (NCS) and artificial/technological methods. We included some of the suggested references, but leave a comprehensive assessment of the sequestration potential of NCS to the WGIII report.
58347	89	20	89	20	In Table 5.9 the first CDR option is referred to as "Enhanced biological production…" but in the main text it is generally referred to as "enhanced biological uptake". A minor point, but it's perhaps clearer to make the table terminology consistent with the wider text. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. The terminology in the table and in the text were made consistent.
10265	89	20	89	21	Consequent on the comment immediately above, a number of additional marine-based CDR techniques could be included in Table 5-9. Notable techniques omitted from Table 5.9 are those that store carbon in the ocean and some of the ocean alkalinity techniques. [Chris Vivian, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. Because of space limitations, we limited the table to methods with sufficient knowledge to allow for an assessment. This is clarified in a statement in the text.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
96601	89	20	90	2	We would appreciate an assessment of positive and negative side effects of CDR approaches, possibly adding such information to Table 5.9 or 5.10. [Nicole Wilke, Germany]	Taken into account. An assessment of positive and negative side effects of CDR methods is provided in the text and in Fig. 5.36. We excluded them from the table because of space limitations.
83833	89	21	89	21	in the Category of "Enhanced biological production and storage in ocean", the Description of "Artificial ocean upwelling" says "pump nutrient rich deep ocean water to the surface", this description seems too abstract and could confuse audiences. It could be: using green energy to pump nutrient rich deep water and control the water flow to just meet the reqirement of photosynthesis but avoid leading alge bloom, [Nianzhi Jiao, China]	Rejected. We tried to keep description in the table as brief as possible. Further details are provided in the text.
82089	89	21	89	21	In the table of CDR methods it would be good to better describe artificial ocean upwelling. Currently the description section reads, "Pump nutrient rich deep ocean water to the surface". I would suggest adding the phrase, "for a fertilizing effect" to the end of the existing description to be clearer about how this method works. Note that the ferilizing effect must be strong enough to enhance CO2 uptake even though CO2 rich seawater is pumped with the nutrients. If this is not the case then CO2 outgassing will actually occur and atmospheric CO2 will increase. [David Keller, Germany]	Accepted. The description was revised.
58309	89	21	89	21	"Wetland restoration": Shouldn't it be Peatland restoration?? Wetlands are not necessarily taking up CO2, instead they have enhanced methane emissions. Carbon will be stored more effectively in accumulating peat/peatlands in the end. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. Wetlands include peatlands. We now talk about wetlands and peatlands.
21865	89	21	90	1	Table should have top row and first column bolded for clarity? [Peter Thorne, Ireland]	Editorial - this kind of issues will be taken care of during the production phase, which includes professional copy-editing.
90115	89		89		This statement uploaded by Ted Schuur on behalf of a graduate class that reviewed the chapter (this was communicated with IPCC TSU in advance): Has there been an assessment of the carbon cost to implement CDR methods? There appears to be a high carbon cost associated with inputting fertilizers into the ocean. [Edward Schuur, United States of America]	Rejected. Assessment of economic costs is beyond the scope of the WGI report.
18911	89		89		Table 5.9: A column with some key references for each CDR option would be useful to readers. [Govindasamy Bala, India]	Rejected. Because of space limitations we decided to include the references in the technical appendix .
116465	89		89		It is a pity that table 5.9 does not include an outcome of the assessment, for instance related to the level of scientific understanding [Valerie Masson-Delmotte, France]	Accepted. A column indicating the level of scientific understanding was added.
116467	89		89		Please also use SR15, SRCCL as starting points, not just AR5. [Valerie Masson-Delmotte, France]	Accepted. We clarified that AR5 was used as the starting point for the assessment of biogeochemical effects of CDR, and SR1.5, SRCCL, SROCC as starting point for the assessment of potentials and side effects.
111865	90	5	90	5	This subsection is mainly on "net negative emissions" not on (gross) CDR "as such". This should be reflected in this title, or in some of the following sub-subsection titles [Oliver Geden, Germany]	Accepted - change was made.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
40727	90	5		56	section 5.6.2.1: the title doesn't really reflect the content of the section and I don't understand the idea behind having both a mini section introduction and a box it doesn't seem justified to me [TSU WGI, France]	Accepted. The section title was changed.
10267	90	7	92	4	This entire section is based on the direct removal of CO2 from the atmosphere and does not take into account ocean-based CDR techniques that directly remove CO2 from surface waters e.g. ocean fertilisation, ocean alkalinisation and artificial upwelling. While there has been a limited amount of modelling of these ocean-based CDR techniques on the carbon cycle - see Keller et al (2018a) - that could have been referred to, more needs to be done. I suggest that some text recognising this need should be included at an appropriate point in this section. [Chris Vivian, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. The carbon cycle response to specific ocean-based CO2 methods is assessed in section 5.6.2.2.2
96603	90	12	90	12	Consider replacing "a limited number of" by "only few" [Nicole Wilke, Germany]	No longer applicable - sentence was deleted.
96605	90	12	90	14	It is right that there are a limited number of idealized studies on CDR and climate-carbon cycle response, but there are many more on specific methods that also include the climate-carbon cycle response (they are discussed further down). This should be rewritten to clarify that on top of studies investigating specific CDR methods there are idealized studies that provide an isolated view on feedbacks, and only those are now discussed (but the feedbacks are included in many more studies, e.g. Keller et al 2014, Sonntag et al 2016). This should be rewritten in a more neutral way not emphasizing any limitation but also mentioning that there idealized simulations in addition to such on plausible scenarios. [Nicole Wilke, Germany]	Noted. The sentence in question was deleted.
87801	90	12	90	22	This paragraph may give a false impression that there are only a few studies that looked at the climate-carbon response to negative emissions. Actually, there is a number of other studies that also look into the climate-carbon response to negative emissions, and this paragraph should recognize a substantial body of literature regarding this point. For example: Schwinger and Tjiputra, 2019. Ocean Carbon Cycle Feedbacks Under Negative Emissions. GRL. Krause et al. 2018. Large uncertainty in carbon uptake potential of land-based climate-change mitigation efforts. Global Change Biology Zickfeld et al. 2016. On the proportionality between global temperature change and cumulative CO2 emissions during periods of net negative CO2 emissions. ERL MacDougall et al. 2013. Reversing climate warming by artificial atmospheric carbon-dioxide removal: Can a Holocene-like climate be restored? ERL. [Katarzyna Tokarska, Switzerland]	Noted. The sentence in question was deleted. Most of the suggested studies explore the climate-carbon cycle response in simulations with prescribed CO2, not negative emissions, and are assessed in section 4.6.3.2.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
18913	90	14	90	19	The CDRMIP experiments ramp down the CO2 concentration by 1% per annum. Can you call this "pulse removal"? They are certainly idealized but not "pulse removal" [Govindasamy Bala, India]	Rejected. The CDRMIP experiments referred to here are not the 1% CO2 ramp-down simulations assessed in Chapter 4. We refer to additional CDRMIP experiments that involve instantaneous removal and emission of 100 PgC (see CDRMIP protocol; Keller et al., 2018, GMD).
58349	90	19	90	19	The citation (O'Neill et al., 2018) is mislabelled as 2018 - it should be 2016 as per the References section. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - change was made.
78547	90	25			This box is very short – it's good, but is it needed as a box? Could just be an opening paragraph of the section? [Chris Jones, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. We considered the reviewer's suggestion but decided to keep the box as it is self-contained and distinct form the rest of teh subsection due to its of pedagogic nature.
39981	90	27		55	Box 5.2: structure is a bit odd, it's hard to follow the flow [TSU WGI, France]	Noted. Unclear what exactly is "odd". The figure was designed with TSU support.
18915	90	29	90	30	"redistributed between atmosphere, land, ocean and geological carbon reservoirs" This is not right. It is redistributed between atmosphere, land and oceans. [Govindasamy Bala, India]	Accepted - change was made.
84799	90	30	90	30	Remove the term "geological". [Martin Heimann, Germany]	Accepted - change was made.
58351	90	31	90	33	I think there may be a slight mismatch between the quoted reservoir %s and the citation given. For the 2018 global carbon budget, the figures should be 45% atmosphere, 23% ocean, 32% terrestrial, whereas the given figures (44/24/29% respectively) are closer to the 2019 GCB (45/24/29%, Friedlingstein et al., 2019, ESSD, 10.5194/essd-11-1783-2019). I suggest shifting to this latter citation and its numbers for the most up to date figures covering 1959-2018. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected. Numbers consistent with the CO2 budget assessment in section 5.2.1.5 are used.
111863	90	31	90	33	The numbers don't add up tp 100%, and it can't be because of rounding effects alone [Oliver Geden, Germany]	Noted. The imbalance is discussed in section 5.2.1.5 where the CO2 budget is assessed.
96607	90	32			Here and elsewhere: update numbers and reference to LeQuere 2018b to Friedlingstein 2019 (global carbon budget 2019)? [Nicole Wilke, Germany]	Noted. Numbers consistent with the CO2 budget assessment in section 5.2.1.5 are used.
39911	90	32			24% it is indicated 23% in section 5.2.1.5 [TSU WGI, France]	Accepted. Numbers were made consistent with section 5.2.1.5
40961	90	33	90	36	The glossary definitions distinguish between negative emissions for CO2 alone vs. for multiple greenhouse gases. The term 'negative greenhouse gas emissions' is used to refer to removal of multiple GHGs, while CDR only refers to removal of CO2. 'Net negative CO2 emissions' is the equivalent of 'Net negative greenhouse gas emissions' but only for CO2. So you should call the terms here 'net positive CO2 emissions' and 'net negative CO2 emissions'. [TSU WGI, France]	Accepted - change was made.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
90117	90	33	90	40	This statement uploaded by Ted Schuur on behalf of a graduate class that reviewed the chapter (this was communicated with IPCC TSU in advance): Pulse CDR removal seems to increase C02 released from the environment. Should we try not to overshoot CDR to minimize natural C02 release. [Edward Schuur, United States of America]	Noted. Pulse removal are idealized scenarios to probe the climate-carbon cycle response to CDR, not scenarios to be translated into reality.
10269	90	36	90	38	An explanation of the reasons for this would be helpful. [Chris Vivian, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Explanation is given.
58353	90	38	90	41	Is it worth adding a brief explanation of why land/ocean carbon reservoirs begin to release CO2 for the unfamiliar reader? Perhaps something along the lines of "This release is the result of the enriched land and ocean carbon reservoirs remaining in quasi-equilibrium with the now-depleting atmosphere reservoir". Or alternatively explain this later on p.81 l.12-13 [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Explanation is given in subsequent section.
18919	90	39	90	39	"by the magnitude of negative emissions" should be changed to "by the magnitude and rate of change of negative emissions"? [Govindasamy Bala, India]	Accepted - change was made.
58355	90	46	90	53	Consider adding "with CDR" to the panel c/d subtitles after "Net positive/negative CO2 emissions" to make Box5.2Fig1 as clear as possible and differentiate from panel b. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Panel titles were revised
58357	90	52	90	53	I can't find a version of this figure in Keller et al. (2018b) that this figure might be adapted from, but there is one very similar in Keller et al. (2018a) (as with comment #7, I think the two citations have been mixed up here). [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Citation was revised.
26927	90		90		Table 5.10 / Raw 4 : We suggest toaAdd the complete definition of Biochar : Burning biomass at high teperature under anoxic conditions [Eric Brun, France]	Accepted. The description was revised.
26929	90		90		Table 5.10 / Raw 5 : We suggest to replace BECCS with "BECCS / Bioufuels from Bioenergy crops, combined with Carbon capturage and storage" [Eric Brun, France]	Accepted. The description was revised.
127851	91	5	91	23	Regarding carbon cycle response over time in scenarios with CDR, it is not at all clear why land-stored carbon (the land CO2 sink) would behave similarly to the ocean CO2 sink. Excess CO2 is stored in the oceans as dissolved CO2 in equilibrium with bicarbonate (HCO3-) and carbonate (CO3--), so when the vapor pressure of atmospheric CO2 declines, outgassing of CO2 from the ocean (with concomitant shift in the CO2-HCO3-CO3 equilibrium) is expected. However, CO2 is stored on land as plant biomass C and soil organic C, whose decomposition back into CO2 is driven primarily by temperature, moisture, soil disturbance, and forest clearing -- and not (or at most to a very slight degree) by atmospheric CO2. Thus, land-sequestered C would not necessarily convert back to CO2 in response to biological, geochemical, or technological CDR -- or at least would not do so with the rapidity and predictability of ocean-dissolved inorganic carbon. [Trigg Talley, United States of America]	Accepted. These processes were described in more detail.
40725	91	5	92	23	section 5.6.2.1.1: the title is confusing as you open the section with "pulse simulations" and not scenario ones [TSU WGI, France]	Accepted. We divided the section in two and now distinguish between pulse and scenario simulations.
73265	91	6	91	6	Replace 'to' with 'for'. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change was made.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
38519	91	6	91	6	word missing, probably "response" (after carbon cycle) [Siv K Lauvset, Norway]	Accepted - change was made.
82091	91	6	91	6	Shouldn't the word "response" be after "carbon cycle" [David Keller, Germany]	Accepted - change was made.
51167	91	6	91	6	is there a word missing? Should "response of the" be inserted before "carbon cycle"? [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change was made.
13449	91	8	91	8	Add colon after the word “from” [Maria Amparo Martinez Arroyo, Mexico]	Accepted - change was made.
58359	91	12	91	13	I think it might be worth adding somewhere a brief explanation of why land/ocean carbon reservoirs begin to release CO2 in response to declining atmospheric CO2 for the unfamiliar reader. For example, Tokarsa & Zickfield (2015) could be cited when stating something like "Some of the carbon sequestered in the ocean sink is eventually re-released as the ocean becomes disquilibrated with the atmosphere, and carbon from the land reservoir is released as the CO2 fertilisation effect declines while soil respiration rates remain elevated due to continued warmth." [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. These processes were described in more detail.
58361	91	28	91	36	The model used for Fig5.32 is cited, but it is not immediately clear which study these results are from (or if it's new for AR6). [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. A citation is now provided.
111971	91	31			to be consistent in GtCO2 unit [Tomas Halenka, Czech Republic]	Rejected. GtC is used consistently in this chapter
58363	91	35	91	36	The purpose of the dashed lines is not directly stated in the caption or figure - presumably they relate to this final statement on relative calculations and the transient run, and so are dashed to diminish the relevance of pre-pulse behaviour. Either way, their purpose should be stated in the caption for clarity. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. The meaning of the dashed lines was clarified (historical simulation).
18929	91	41	91	56	Here, I believe the sink response for RCP 2.6 scenario is assessed. The rate of net emission increase and decrease is rather gentle in this scenario? How do the land and ocean sinks respond for high emission scenarios such as RCP8.5? I am not sure this question has been addressed for RCP8.5 but it would be interesting to explore the carbon cycle response in the CDRMIP experiments. These are concentration driven experiments but fluxes could be diagnosed. The question I have is will these conclusions hold in the rapidly increasing and decreasing emission scenario. [Govindasamy Bala, India]	Taken into account. To our knowledge there is no CDRMIP SSP5-8.5 simulation, but in addition to SSP1-2.6 we assessed the response to SSP5-3.4OS, which has higher CO2 emissions and stronger CO2 removals.
63619	91	44	91	47	Figure 5.33 shows CO2 in ppm. This is proportional to cumulative CO2 emissions (PgC). However, the text at the cited lines says that the ocean sink is weakening over time In fact, if the lower emission trajectory is taken, the ocean is net emitting carbon, which reduces the cumulative accumulation. At 2050, it has 55 PgC while at 2100 only 27 PgC, so the ocean has not weakening in its sink (implying a non-zero rate of flux still into the ocean), but instead, it has outgassed significant carbon (i.e. a net flux of carbon to the atmosphere over the period 2050 to 2100). The text here needs to be more precise to properly represent what is happening to the ocean carbon sink. [Galen McKinley, United States of America]	Rejected. The numbers in the coloured boxes indicate CO2 fluxes (positive: into atmosphere; negative: out of atmosphere) integrated over the indicated 50-year period, not the entire simulation. In the models examined by Jones et al. 2016 the ocean remains a carbon sink throughout the duration of the simulation.
96609	91	51	91	52	The explanation of the processes why land and ocean sinks respond (also still) to prior, not actual CO2 concentration should be given shortly here. [Nicole Wilke, Germany]	Accepted. An explanation was provided.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
38521	91	51	91	53	Unclear what the mechanisms are. What does it mean that the sinks "respond much later to the prior atmospheric CO2"? What constitutes "prior"? [Siv K Lauvset, Norway]	Accepted. The text was revised.
73267	91	52	91	52	Move (to start of following line. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial - this kind of issues will be taken care of during the production phase, which includes professional copy-editing.
40783	91	53	91	55	Should add the confidence level for this assessment. [TSU WGI, France]	Accepted. A confidence statement was added.
29161	92	6	92	10	How will these new texts reviewed? In my view, quality control is required for all newly inserted findings and assessments. [Helmut Haberl, Austria]	Noted. The analysis of Jones et al. 2016 was repeated for CMIP6 SSP1-2.6 simulations. The analayis based on CMIP6 simulations supported the findings of earlier studies and no change was made to the assessment.
87803	92	6	92	10	Please note that it would be helpful to provide a comparison with (or at least cite) other studies that looked at the Earth System response to overshoot scenarios, using different scenarios and different models. For example: Tachiiri et al., 2019. Effect on the Earth system of realizing a 1.5 °C warming climate target after overshooting to the 2 °C level. ERL. Tokarska et al. 2019. Path Independence of Carbon Budgets When Meeting a Stringent Global Mean Temperature Target After an Overshoot. AGU Earth's Future. [Katarzyna Tokarska, Switzerland]	Accepted. Citation of these studies was added.
58365	92	6	92	10	This is not a direct comment, but it would be interesting to use this work to quantify how much CDR effectively 'takes the pressure off' natural carbon sinks (i.e. by contrasting these results with SSP1/SSP5 without any CDR to compare how much bigger natural carbon sinks are in the same scenarios without CDR). [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. CMIP6 does not include an SSP1 scenario without CDR. For SSP5, there are SSP5-8.5 and SSP5-3.4OS. Differences between the two are discussed in section 5.4.9.
58367	92	15	92	21	It would be useful to explicitly label a/b/c/d directly on the figure alongside the subtitles to make clearly match the caption. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Labels were added.
73269	92	21	92	21	Change to Jones et al. (2016a) [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Citation was revised.
58369	92	32	92	44	I think this section could do with an extra sentence to clearly explain to unfamilar readers how to interpret the AF & PAF here and in figures 5.34 & 5.35 - for example, "…a higher AF or PAF value for Negative Emission scenarios means that more carbon succesfully stays out of the atmosphere, with a value of 1 indicating all the removed carbon remains out of the atmosphere and a value of 0 indicating none of the removed carbon remains out of the atmosphere". All this becomes clear when studying the figures, but it'd be useful to clearly reiterate this in the main text as well. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Suggested explanation was included.
20009	92	38	92	38	"airborne fraction of the perturbation" is a strange expression. Is one to guess that "perturbation" (undefined) is the magnitude of the CO2 removed? Possibly, what is actually meant is "perturbation of the airborne fraction"? In any case, the text should indicate whether or not this definition is adopted from now on for the effectiveness [philippe waldteufel, France]	Taken into account. Airborne fraction of the perturbation is correct. The meaning was clarified in the text (i.e. how much of the removed CO2 stays out of the atmosphere).

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
90119	92	38	92	40	This statement uploaded by Ted Schuur on behalf of a graduate class that reviewed the chapter (this was communicated with IPCC TSU in advance): Measuring effectiveness of CDR should be officially standardized so assessments are equal across studies. [Edward Schuur, United States of America]	Noted. No edit required.
58371	92	49	92	51	These state dependencies and feedbacks may need further explanation. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. A brief explanation was provided.
5657	92	49	92	51	Please check: how can the effectiveness of CO2 removal be higher if the response to this removal is weaker? [Joachim Rock, Germany]	Accepted. An explanation was provided.
38523	92	49	92	51	The sentence is difficult to understand since it first states that effectiveness is larger and then that overall response is weaker. I suggest to add "of the natural carbon sinks" after "weaker overall response" [Siv K Lauvset, Norway]	Accepted. An explanation was provided.
51169	93	1	93	2	"the 'cooling effectiveness' of CDR is approximately independent of the background level of CO2" seems to contradict this statement in ch.4 p.80 rows 31-32 "The cooling effectiveness of negative CO2 emissions is also found to be less if applied at higher CO2 concentration". Please could you check for consistency with Ch 4 authors. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. This statement was made consistent with that in Ch 4.
58373	93	1	93	4	High confidence for this statement implies at least medium evidence (if assuming high agreement), which may be a bit borderline given that only a few studies have been cited here. However, if borne out by the forthcoming CDRMIP results then this could stand. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Confidence statement was revised to medium confidence.
96611	93	6	93	6	More detailed results from CDR-MIP are welcomed. Their presentation and interpretation should be guided by their usability in WG III, in particular the question: What would be a scientifically justifiable incentive price for a unit of CDR? Which metric of "effectiveness of CDR" is most suitable for this thinking? The results so far seem to indicate that (leaving aside any additional side effects, permanence and risks of leakage) a reduced price (p in $/t) should be granted to a unit of CDR compared to an unit of CO2 emission, but what would be the most suitable metric, based on PAF or based on the difference from positive and negative pulse responses (asymmetry Ch. 5.6.2.1.3)? It is very important that some guidance is given here, as current practice in IAMs is to set p(CDR) = p(Emission)! [Nicole Wilke, Germany]	Taken into account. The implications for WGIII were clarified.
39589	93	9	93	24	The calculated airborne fractions are not validated by observed values in Fig. 5.5 showing again the poor predictive skill of models on which are mainly based the conclusions of AR6. [François Gervais, France]	Noted. Unclear what change is requested.
40169	93	11			Fig 5.34: it is hard to get the message conveyed by the figure. [TSU WGI, France]	Accepted - change was made.
58375	93	21	93	22	There's no Zickfield et al. (2019) in the reference section - I suspect it should be (2016) (or maybe it's the submitted one?). [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - change was made.
86763	93	27	93	50	CO2 emissions will partly be absorbed by land -and ocean sink, and only the residual amount of CO2 will reach the atmospheare. CDR removal from the athmospheare will in the other hand be in absolute numbers? [Oyvind Christophersen, Norway]	Noted. CO2 emissions and removals are treated in the same way.
20011	93	30	93	32	As this sentence implies that the brain of the reader has little ability to think by himself, it might be wiser to delete it. [philippe waldteufel, France]	Rejected. We prefer to provide too much rather than too little information.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
58377	93	42	93	44	I think it'd be useful to briefly state what these state-dependencies entail - is it for example due to the logarithmic relationship between CO2 and temperature? I assume once Zickfield et al. (sub) is accepted this may be expanded upon. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. A brief explanation was provided.
96613	93	46			Please provide peer-reviewed evidence for this statement and the associated confidence level on +/- CO2 emissions. [Nicole Wilke, Germany]	Accepted. Statement was revised in light of latest scientific evidence and peer-reviewed literature.
29163	93	48	93	50	How will these new texts reviewed? In my view, quality control is required for all newly inserted findings and assessments. [Helmut Haberl, Austria]	Noted. We did not use results from CDRMIP simulations to support the assessment of CDR effectiveness. One data point from CDRMIP pulse simulations was included in Fig. 5.34 to allow for comaprison of results of simpler models to those of ESMs.
58379	93	55	94	5	This figure is important but was hard to take in initially (it took me a while to get that for removals the atmospheric fraction was the CO2 remaining succesfully out of the atmosphere or in the land/ocean reservoirs respectively, and vice versa for emissions). Some extra labels - for example indicating Removals/Emissions above each side, and indicating reservoir outgassing/uptake within panels b & c - in the final updated figure version would help the reader grasp the results more quickly. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - change was made.
116469	93		93		There is a potential to nicely integrate insights from ch 5 and from ch 4 on responses to increased and decreased emissions, with x chapter coordination to develop insights / TS. [Valerie Masson-Delmotte, France]	Noted

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
112125	94	10	99	34	On the whole this section does not live up to established scientific standards, and in this form would do a disservice to the community doing research on this topic. Most importantly, numbers (especially for maximum potentials) are "picked out of the air" from the literature and then reported as if they were the most authoritative values, often without being clear exactly what they mean (i.e., are they the maximum achievable by 2100, or the average sustainable values, or the values just for the next decade, etc.). The values are also often inconsistent with those in the most authoritative reviews on the topic, in the companion papers by Minx et al., Fuss et al. and Nemet et al. (Environ. Res. Lett., 2018) and the review by Lawrence et al. (Nature Communications, 2018). It is especially noteworthy that Fuss et al. lists the kinds of values in Table 5.10 in their abstract, and that Fuss et al. is actually cited for a few values in Table 5.10, but then there are several other values that are completely inconsistent with those in Fuss et al. (with no justification), and even a few values that cite Fuss et al. don't match the values in their table. The IPCC authors could make life much easier on themselves if they would start from these reviews as a basis for the assessment (since they are the most scientifically robust overviews available and are only 2 years old), and then augment these with other values where they see fit. If not, then for scientific robustness, a comparison of the selected values with those in these reviews should be given, with explanations for where there are differences (as was done in Lawrence et al., 2018, in comparison to the other three reviews which had been published earlier in the same year). [Mark Lawrence, Germany]	Not applicable. This is similar to comment 112127. Table 5.10 has been merged with Table 5.9 in Section 5.6.2.1. CDR potentials are not longer presented and discussed in Chapter 5, as they overlap with WGIII Chapters 7 and 12. As WGIII will be published later, they will present updated results on CDR potentials.
104707	94	12	96	13	"Among the land-based CDR methods, I would also cite Agroforestry, Agroecology and Climate-Smart Agriculture. Maybe it is not the goal of this chapter to deepen these topics but they are acknowledged by scientific literature and the SRCCL as important practices for soil conservation, restoration and carbon sequestration." [Andrea Bianchi, Italy]	Taken into account. This is not the goal of this chapter to deepen these topics. We add a footnote to Table 5.9 (which has been merged with Table 5.10) referring to AR6 WGIII Section 7.5 and SRCCL Section 5.6, where these methods have been discussed under different, or more detailed, perspectives. .
40729	94	12	97	3	section 5.6.2.2.1: there is nothing said about the previous reports (AR5/SRCCL) [TSU WGI, France]	Statements were added in specific points of the text, in particular when the state of knowledge has changed substantially (e.g. BECCS)
5659	94	13	94	15	Please rephrase this paragraph. "Deforestation" is not sufficient to emit carbon. Deforestation primarily changes the vitalty status of trees from "alive" to "dead" and dislocates at least part of them. The release of carbon to the atmosphere is mainly by fire or heterotropic respiration, including decay of wood products. [Joachim Rock, Germany]	Accepted. The term "deforestation" was indeed misused here. Sentence was rewritten according to comments 5659, 71739, 96615, 69193.
71739	94	13	94	15	Therea are other human interventions than only deforestation. Please add forest use and management to the list because due to it ecosystem carbon storages are much lower than what is potential of them (Erb et al. 2017). Erb et al. 2017, Unexpectedly large impact of forest management and grazing on global vegetation biomass, Nature 553, doi:10.1038/nature25138 [Tuomo Kalliokoski, Finland]	Accepted. The term "deforestation" was indeed misused here. Sentence was rewritten according to comments 5659, 71739, 96615, 69193.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
96615	94	14	94	16	This sentence does not use clear terminology -- deforestation is one specific land use transition among many, and it comprises a multitude of processes (decay of biomass, residues and wood products by heterotrophic respiration or fire). Aren't the broad 4 categories GPP, autotrophic respiration, heterotrophic respiration, and non-respiratory fluxes such as fire? [Nicole Wilke, Germany]	Accepted. The term "deforestation" was indeed misused here. Sentence was rewritten according to comments 5659, 71739, 96615, 69193.
69193	94	15	94	15	"By deforestation" seems to be unsuitable here. As deforestation is an activity of land conversion from forest to non-forest land (not covering harvesting as a part of forest management) and not a direct source of emissions to atmosphere; and therefore should not be used in the same context as autotrophic and heterotrophic respiration. Perhaps rewriting to text like "final use of harvested wood" would be better. [Kaoru Magosaki, Japan]	Accepted. The term "deforestation" was indeed misused here. Sentence was rewritten according to comments 5659, 71739, 96615, 69193.
20013	94	17	94	17	A comma following "sources" would be useful [philippe waldteufel, France]	Accepted - change was made.
58381	94	20	94	22	No reference is given for this sentence, and in particular for sequestration being higher for younger rather than mature forests. Perhaps SRCCL could be a useful general reference here before going into the details in the rest of the paragraph. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. A reference is added, and the text was slightly modified, replacing "forest age" by "forest demography". These sentences were modified according to comments 90123, 58381.
68279	94	20	94	22	Primary forests left alone sequester more carbon over time: they increase carbon stocks at a rate of 0.47-1.3 PgC per year (1.7 - 4.8 GtCO2e per year), roughly equivalent to emissions resulting from deforestation. Thus, “primary tropical forests are a potentially significant sink for near-term additional carbon dioxide removal.” Mackey et al. (2020), Understanding the importance of primary tropical forest protection as a mitigation strategy, Mitigation and Adaptation Strategies for Global Change (“Contrary to the widely held view that carbon stocks in primary forests reach a fixed equilibrium amount (Xu et al. 2017), these stocks appear to be increasing monotonically throughout the tropics at a rate of 0.47–1.3 PgC yr−1 (Grace et al. 2014; Lewis et al. 2009; Mitchard 2018; Pan et al. 2011), equivalent to 5–13% of annual global anthropogenic emissions (IPCC 2018). The rate of sequestration in primary tropical forests is estimated to be approximately equivalent to the emissions resulting from deforestation, based on comparisons of atmospheric inverse models (Gaubert et al. 2019).”). [Durwood Zaelke, United States of America]	Not applicable. The paragraph was rewritten and the sentence on primary forests sequestration potential was eliminated.
90123	94	20	94	23	This statement uploaded by Ted Schuur on behalf of a graduate class that reviewed the chapter (this was communicated with IPCC TSU in advance): Careful with this argument as it may be used to argue for the removal of old growth forests to increase CDR. [Edward Schuur, United States of America]	Accepted. The argument was removed to avoid possible misinterpretations of it. These sentences were modified according to comments 90123, 58381.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
55003	94	20	94	24	Suggest modifying statement on lines 23-24. The statement that forests may be considered a temporary solution until fossil fuels are phased out is a misunderstanding of the role of sustainable forest management which allows for the continuous maintenance of forest sinks through harvest, transfer of carbon to wood products and their use to store carbon (Churkina et al 2020 in Nature Sustainability) and to substitute emissions intensive materials. Thus sustainably managed forests can be a long-term and sustained C sink. For example see also the discussions on Chapter 4 of the IPCC SRCCL where SFM is also discussed. References: Churkina, G., Organschi, A., Reyer, C. P., Ruff, A., Vinke, K., Liu, Z., ... & Schellnhuber, H. J. (2020). Buildings as a global carbon sink. Nature Sustainability, 1-8. [Nancy Hamzawi, Canada]	Accepted. The role of managed forests to produce wood products and constructions materials was included in the sentence.
90121	94	20	94	55	This statement uploaded by Ted Schuur on behalf of a graduate class that reviewed the chapter (this was communicated with IPCC TSU in advance): CO2 sequestration via restoration, reforestation, and afforestation recieveives more attention and explantion than other CDR methods in the section 5.6. Consider reducing to save space. [Edward Schuur, United States of America]	Rejected. Most of the comments received on this section asked for more details on restoration, reforestation, afforestation, and mainly, forest management, in particular from government.
5661	94	20	94	55	This paragraph needs a very thorough make-over, because it lacks understanding of forest growth and of what forest management is. You concentrate on carbon stock increases and completely neglect substitution effects of wood use. "Management of forests for carbon sequestration" is not "plant and forget", it means ensuring a state of the forest where the current annual increment is maximized over the landscape (see e.g. Pretzsch, H. (2010). Forest Dynamics, Growth and Yield. Springer, 664 pp. for the basics on forest growth and its management). This way, the uptake of carbon from the atmosphere is maximized and the decrease observed over time which you correctly mention for an even-aged forest stand, is avoided. The papers of Kurz et al. and Yousefpour et al. which you cite consider parts of the whole context, Naudts et al. and Griscom et al. do not and should be deleted here. [Joachim Rock, Germany]	Accepted. We recognize the importance of forest management, and expanded the discussion on forest management specifically. The revised paragraph addresses this method in many ways.
21867	94	20	94	55	This very long paragraph would be more digestible if split up into two or more shorter paragraphs. [Peter Thorne, Ireland]	Accepted. The paragraph was divided into three smaller paragraphs.
96617	94	20	95	17	The potential of forests to sequester carbon depends on forest areas as much as on forestry, i.e. how much carbon is taken out and regrows and how the wood products are used. Yousefpour et al 2019 show the high future forestry yield potentials, but there is more forestry literature showing that the sink in re/afforestation can be much enhanced by forest management. Naudts et al 2016 10.1126/science.aac9976 and Luyssaert et al 2018 doi.org/10.1038/s41586-018-0577-1 are good references for trade-offs between carbon and biogeophysical effects for forest management. Forest management is in Table 5.10, but not reflected in the text. Please modify accordingly. [Nicole Wilke, Germany]	Accepted. We recognize the importance of forest management, and expanded the discussion on forest management specifically. The revised paragraph addresses this method in many ways.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
29167	94	20	95	50	Growing biomass for bioenergy to be used for BECCS and afforestation/reforestation/"natural succession" compete with each other for land, in particular when implemented at the large scales foreseen in many 1.5-2.0 degree scenarios. Hence the discussion of C sequestration in biota and soils (p94) and that of bioenergy/BECCS (p95) need to be discussed together, respectively these systemic interactions between these two options need to be highlighted. Indeed analyses of possible alternative uses of land outside forests not required for the food system (e.g. Kalt et al.,2019 GCB Bioenergy, 11, 1283–1297) indicate that it depends on a host of factors whether bioenergy or C sequestration associated with "natural succession" save more GHG, above all the efficiency of the biomass-utilization pathway of bioenergy use. Moreover, C sequestration foregone due to the area demand of bioenergy plantations may be quite substantial and rises per Joule of bioenergy with the amount of bioenergy to be produced ("GHG cost curve of bioenergy"; see Kalt et al, 2020, Env Res Lett, Environmental Research Letters 15(3), 034066). Such C balance effects resulting from systemic interrelations in the land system need to be considered when assessing the possible GHG benefits of BECCS. Among those effects are also GHG effects of increased fertilization associated with high-yield scenarios (Theurl et al., 2020, Science of the Total Environment, 735, 139353). In my view, the assessment presented here should explicitly discuss these systemic interrelations, not least because they are highly policy-relevant. Policy-makers need to be aware of such systemic effects in order to plan robust policies that optimize the contribution of land to climate-change mitigation, which means that it would not be wise to maximize any of these options at the expense of the other - rather, analyses (see Kalt et al 2019, op cit) indicate that the best option may be highly variable between locations, but also between technological choices. These three papers are also relevant for the statements made in the lines 46-50 on possible detrimental effects of BECCS (which are often related to the systemic	Taken into account. We understand the reviewer´s point of view. As the discussion on afforestation, reforestation and forest management is already too long, some of the points raised were included in the BECCS paragraph.
605	94	22			I think it is worth mentioning here that old growth forests do remain a net C sink. See for example Luyssaert, S., Schulze, E. D., Borner, A., Knohl, A., Hessenmoller, D., Law, B. E., . . . Grace, J. (2008). Old-growth forests as global carbon sinks. Nature, 455(7210), 213-215. doi:10.1038/nature07276 [Vicca Sara, Belgium]	Not applicable. The paragraph was rewritten and the sentence on primary forests sequestration potential was eliminated.
98489	94	23	94	23	I would add erosion to this list, even though other disturbances finishes the list [Emily Romano, United States of America]	Accepted. Erosion was added to the list, although implicit in "other disturbances"
71741	94	23	94	24	There is no ecosystem based physical reason why ecosystem carbon storages could not be increased from the current level permanently. Thus, please consider to revise the statement that forest management for increasing carbon storage of forests should be seen only as temporary solution. [Tuomo Kalliokoski, Finland]	Accepted. Sentence was revised, and the role of proper forest management was added.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
84001	94	23	94	25	This sentence is very problematic. Forest management should indeed be considered carefully, to achieve sustainable land use, hence, contributing to the overall efforts to control emissions and increase resilience. However, considering any sort of efforts as a temporary solution while fossil fuels are phased out it is unacceptable, considering the urgency of the needed action. While it is appreciated the complexity of phasing out fossil fuels, we recall, as mentioned in chapter 1 and others, that the CO2 emissions from fossils are at the core problem of climate change and should be drastically and effectively addressed. Without this action, all other actions will be only temporary, reversing to further problems to all natural ecosystems. [Marco Tulio Cabral, Brazil]	Accepted. The sentence was rephrased, separating the role of unmanaged forests as temporary from the role of managed forests to permanent carbon sinks. Fossil fuels are highlighted as the main source of emissions.
69195	94	23	94	25	In "the management of forests for carbon sequestration may be considered a temporary solution while fossils fuels are phased out", the word "temporary" may not be appropriate here. It should be noted that forest management may not dramatically enhance carbon removals but maintains sequestration potential as well as prevents becoming a source of emissions. Thus, it is essential to continue to implement forest management to seek a zero emissions world. [Kaoru Magosaki, Japan]	Accepted. Sentence was revised, and the role of proper forest management was added.
86765	94	23	94	25	Quotation: "Thus, the management of forests for carbon sequestration may be considered a temporary solution while fossils fuels are phased out (Houghton et al., 2015). Comment: When stated as a temporary solution this can easily be misinterpreted. Especially if carbon stocks are maintained and kept intact. Please consider rewriting to better reflect this. Please also consider to use another word than sequatration, since we are more and more using about removals by sinks or uptake. [Oyvind Christophersen, Norway]	Accepted. The sentence was rephrased, separating the role of unmanaged forests as temporary from the role of managed forests to permanent carbon sinks.
5663	94	25	94	27	This topic is too broad to be assessed just from one paper. Please have a look at Messier et al. (doi.org/10.1186/s40663-019-0166-2) and the literature cited therein for a broader view. [Joachim Rock, Germany]	Accepted. New references were added to the assessment.
86767	94	25	94	27	Quotation: "Primary forests are generally more resilient as carbon stocks than modified natural forests or plantations, as their higher biodiversity, the genetic variability within species and the large size of natural forest ecosystems make them less susceptible to pests and fires (Thompson et al., 2009)." Comment: Please consider if tree breeding for climate adaptation may change this conclution? If there are relevant literature please include it in your assessment. [Oyvind Christophersen, Norway]	Accepted. A sentence was added on forest genomics in general, and their potential to accelerate breeding for tree health and productivity.
90125	94	27	94	29	This statement uploaded by Ted Schuur on behalf of a graduate class that reviewed the chapter (this was communicated with IPCC TSU in advance): Some verbage should be included about the reason tropical have been degraded. Getting at the root cause of this degredation will reduce degredation and allow for more focus on remediation. [Edward Schuur, United States of America]	Accepted. A sentence was added explaining the causes of tropical forest degradation.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
96619	94	27	94	29	Tropical forests are important C sinks mostly because of their high biomass density, the higher resilience is just another, less well understood, aspect. This should be clarified. Also, please make clear that stopping tropical deforestation is more efficient than restoring degraded tropical forests (but that it is just not treated in this chapter because it is about CDR and not about avoided emissions, although the atmosphere really does not care how emissions are brought down, by reducing gross emissions or by creating negative emissions). A short clarification that stopping deforestation has side benefits restoration will not immediately achieve, such as keeping biodiversity up, should be added. We consider it highly important to avoid the misunderstanding that larger CO2 reduction potentials lie in reforestation and restoration and not in reducing deforestation and degradation. Please modify. [Nicole Wilke, Germany]	Accepted. These points were included before the discussion on restoration of degraded tropical forests.
58383	94	27	94	30	Some of this sentence's references are mostly not very specific to tropical forests. Kurz et al. (2016) is mostly about promoting longlife wood products and forestry management and is not linked to tropical forests in particular. Naudts et al. (2016) is tangentially related in that it shows how European afforestation has led to a slight carbon release. Yousefpour et al. (2018) should be (2017), and predicts largest increase in harvest potential in the tropics but doesn't specifically mentioned degraded forests or restoration. And Maxwell et al. (2019) is not listed in the references (but Maxwell et al., 2019, Science Advances, 10.1126/sciadv.aax2546, would be an excellent citation to support this sentence). Possible additional/alternative reference would be Pan et al. (2011, 10.1126/science.1201609) for the importance of both tropical deforestation and regrowth for global forest carbon stocks, and Tyukavina et al. (2015, 10.1088/1748-9326/10/7/074002) for data on carbon loss in both primary and managed tropical forests (and is one of the key sources for Griscom et al., 2017). [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. References were modified to focus on tropical forests only.
84009	94	27	94	30	While restoring degraded forest, tropical and others, is an essential line of action, relying all efforts upon tropical forest to sustain CO2 sequestration rates, it is an extremely unbalanced approach, that dismisses the urgency, and related responsibility of mitigating CO2 emissions from fossil fuels. [Marco Tulio Cabral, Brazil]	Taken into account. Fossil fuels are highlighted as the main source of emissions a few lines above, but we do not want to state repeatedly here, as it is a paragraph on forests.
112641	94	29	94	30	Please check references: Maxwell is not in the reference list, Yousefpour and Naudts do not write about restoration of tropical forests [Julia Nabel, Germany]	Accepted. References were deleted and refence list was updated.
96621	94	29			Naudts et al investigate European forests and make no statement about tropical potentials, remove here. [Nicole Wilke, Germany]	Accepted. Reference was deleted.
96623	94	29			Yousefpour et al 2018 should be 2019. [Nicole Wilke, Germany]	Taken into account. The reference was deleted, as it was not specific to the context in which it was cited.
5667	94	30	94	30	"Maxwell et al. 2019" is missing in the references. [Joachim Rock, Germany]	Taken into account. Reference was added to the list.
96625	94	30	94	32	The sentence is unclear. Does this include the rate of deforestation during 1981-2010? Please rephrase. [Nicole Wilke, Germany]	Taken into account. Sentence was clarified.
115419	94	30		32	Does this include deforestation over the 1981-2010 period? I think not, but would be good to clarify. [Gillett Nathan, Canada]	Taken into account. Sentence was clarified.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
96627	94	30			Reference Maxwell missing in bibliography. [Nicole Wilke, Germany]	Taken into account. Reference was added to the list.
58385	94	32	94	32	Should this read "reforestation" rather than just "forestation" (to differentiate from afforestation)? [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. We indeed mean forestation, as the sum of reforestation and afforestation. We modified the rest of sentence, hoping it is more clear now.
96629	94	32			The carbon uptake potential of re/afforestation is not that much "uncertain", but really dependent on the assumptions concerning the scenario of available land and of background climate; these scenario assumptions make studies differ a lot. It is also complex to consider everything (pre-existing vegetation, biomass and soil, climate-C cycle feedbacks) -- but many studies include this all. Therefore we think "uncertain" is the wrong term here. Please modify. [Nicole Wilke, Germany]	Taken into account. We agree with the reviewer and modified the sentence.
17067	94	33	94	33	Bastin et al. (2019) [Sergio Aquino, Canada]	Not applicable. Given that WGIII is deeply assessing the range and uncertainties of sequestration potentials of forestation methods, a decision was made to avoid an overlapping assessment, and point to WGIII. These sentences have thus been erased.
71743	94	33	94	35	The study of Bastin et al. (2019) has errors, please remove it from here and refer to other studies. [Tuomo Kalliokoski, Finland]	Not applicable. Given that WGIII is deeply assessing the range and uncertainties of sequestration potentials of forestation methods, a decision was made to avoid an overlapping assessment, and point to WGIII. These sentences have thus been erased.
96633	94	33	94	35	The study by Bustin et al (2019) should be cited more precisely: Just calling 9 Mkm2 "available" seems too simplistic as the cited study mainly analysed the aspect of suitability and only employs very simple assumptions regarding availability. "Availability" assumes that the usage for reforestation does not compete with any previous "usage". The original wording reads "9 Mkm2 [of land that could potentially support trees] are found outside croplands and urban regions and may represent regions for potential [tree] restoration". We suggest to use language closer to the original source. [Nicole Wilke, Germany]	Not applicable. Given that WGIII is deeply assessing the range and uncertainties of sequestration potentials of forestation methods, a decision was made to avoid an overlapping assessment, and point to WGIII. These sentences have thus been erased.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
96631	94	33	94	36	The Bastin et al study has been heavily criticized, see the comments, Letters and other replies. Most importantly the large number of 205 PgC depends (1) on pastures and grazing land being assumed as convertible to forests, ignoring that growing food demand will expand cropland areas and/or drive up food prices and makes it impossible to restore all pasture to forest; see comment by Delzeit et al Letter, "Forest restoration: Expanding agriculture", Science 2019: if these effects are accounted for the Bastin et al estimate is reduced by 19 to 57%. (2) The Bastin et al number assumes 0 carbon in the pre-existing ecosystems (also soils) in the original publication (see Lewis et al Technical Comment, Science 2019) -- the authors provide additional calculations that includes pre-existing carbon in reply to the criticism, and come to the same estimate of 205 PgC, but this has not gone through peer-review and should thus not be cited here in my view. However, the study by Sonntag et al, GRL, 2016 finds potentials in reforestation of 215 PgC by 2100 in a plausible scenario (reforestation of RCP4.5, which assumes a high carbon price and thus agricultural intensification and area available for reforestation without interfering with food demand) compared to the deforestation projected without mitigation efforts, when driven under RCP8.5 climate and CO2 concentrations. So there are studies that find such high potentials under plausible (but specific: high carbon price, high CO2) scenarios and accounting for all feedbacks (ESM study) to the best of knowledge 5 years ago. Please revise the text. [Nicole Wilke, Germany]	Not applicable. Given that WGIII is deeply assessing the range and uncertainties of sequestration potentials of forestation methods, a decision was made to avoid an overlapping assessment, and point to WGIII. These sentences have thus been erased.
5665	94	33	94	39	The study from Bastin et al. has many scientific shortcomings, has received much disagreement from the scientific community, and should not be cited here. [Joachim Rock, Germany]	Not applicable. Given that WGIII is deeply assessing the range and uncertainties of sequestration potentials of forestation methods, a decision was made to avoid an overlapping assessment, and point to WGIII. These sentences have thus been erased.
98491	94	35	94	36	Could this sentence include a brief reason other authors suggest the figures are overestimated? e.g. "these figures are overestimated by a factor of five, due to insert what they say has not been considered". [Emily Romano, United States of America]	Not applicable. Given that WGIII is deeply assessing the range and uncertainties of sequestration potentials of forestation methods, a decision was made to avoid an overlapping assessment, and point to WGIII. These sentences have thus been erased.
58387	94	35	94	39	The first citation (l.36) should be Veldman et al. (2019), as this is the paper directly critiquing Bastin et al. (2019) as overestimating by 5x. Also relevant here is Friedlingstein et al. (2019) which also critiqued Bastin et al. (2019) for similar reasons but also included additional issues with the assumed airborne fraction of CO2. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. The paper was indeed misquoted. However, given that WGIII is deeply assessing the range and uncertainties of sequestration potentials of forestation methods, a decision was made to avoid an overlapping assessment, and point to WGIII. These sentences have thus been erased.
607	94	35			"protected" areas? [Vicca Sara, Belgium]	Not applicable. This sentence has been eliminated from the text, as it overlaps with WGIII assessment.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
96637	94	36	94	36	The reference Veldman et al. 2015, should be Veldman et al., 2019. [Nicole Wilke, Germany]	Not applicable. This sentence has been eliminated from the text, as it overlaps with WGIII assessment.
70833	94	36	94	36	One could add the commentary by Lewis 10.1126/science.aaz0388 and add "by a factor 2-5 (lewis et al., Veldman et al.) [Karlheinz Erb, Austria]	Not applicable. This sentence has been eliminated from the text, as it overlaps with WGIII assessment.
96635	94	36	94	39	The authors may wish to consider inserting some elements (or all aspects) of the following message which has emerged from IPBES after the sentence which ends with "(...; Bastin et al., 2019)": "Inappropriate restoration techniques can further exacerbate land degradation. An example is the planting of trees where they did not historically occur (afforestation), which can have a similar impact as deforestation, including the reduction of biodiversity and disruption of water, energy and nutrient cycles." Rationale: Implementing measures should preferably not lead to new ecological and socio-economic challenges. Source: For further reading check: IPBES (2018: 31): Summary for policymakers of the assessment report on land degradation and restoration of the Intergovernmental Science-Policy Platform on Biodiversity and Ecosystem Services. R. Scholes, L. Monanarella, ... at https://ipbes.net/assessment-reports/ldr [Nicole Wilke, Germany]	Accepted. New information and references were added.
18933	94	36	94	39	How would one get water for afforestation of grasslands? Thus afforestation could be limited by the availability of water. Even when water is available, afforestation could alter the regional hydrological cycle. [Govindasamy Bala, India]	Taken into account. The limitation of water for the growth of forests was included, and the possible effects of afforestation on evapotranspiration and runoff was already discussed in the previous draft (below in the paragraph).
96639	94	39			Bastin et al does not investigate biodiversity, and the loss of carbon storage of the pre-existing vegetation is exactly what their original submission does not take into account (see the comments, Letters and other replies), partly explaining the large potential of 205 PgC. [Nicole Wilke, Germany]	Accepted. The reference has been deleted.
58389	94	42	94	42	The listed reference for Sonntag et al. (2015) is missing a journal, which appears to be because it refers to an EGU conference abstract (ideally this would be replaced by a citation to a full article - I believe this might be Sonntag et al, 2016, GRL, https://doi.org/10.1002/2016GL068824). [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Reference corrected.
58391	94	42	94	43	Boysen et al. (2017) doesn't specify whether it means 2017a or 2017b in the reference list and it's hard to tell which is meant (both involve DVGMs, but seem to only breifly mention the specific mechanisms mentioned). [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Both references are cited now.
58393	94	43	94	46	Other potential supporting citations here could include Naudts et al (2016) and Veldman et al (2019) from earlier in the paragraph, as well as Bathiany et al. (2010, doi:10.5194/bg-7-1383-2010). [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected. We already have four citations supporting this sentence, and the text actually needs shortening.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
96641	94	43	94	46	On the biogeophysical side-effects of forest cover changes: It is right that boreal forests may warm the climate due to snow masking and generally low canopy albedo and the proximity to the Arctic oceans, such that snow-ice-albedo feedbacks act more strongly. But the two cited studies refer mostly to natural vegetation dynamics in high latitudes, not specifically CDR. There is more detailed literature out closer related to CDR, in particular since it relates directly to land-use changes: observation-based studies (e.g. Bright et al. 2017, DOI: 10.1038/NCLIMATE3250, and Li et al 2016 https://doi.org/10.1002/2016JD024969) show a biogeophysical warming, but mostly only in very northern latitudes. This signal is in discrepancy with previous studies, which were based on models. In models, the warming effect from forest reaches substantially further south than the observation-based studies suggest. The IPCC SRCCL attributed this to model deficiencies. However, Winckler et al 2018 (10.1029/2018GL080211) resolved this discrepancy between models and observations about whether or not large regions would show a warming signal upon re/afforestation: They shows that the discrepancy stems from observations capturing only the local effects of forest cover changes, while models include also the non-local effects via advection and changes in atmospheric circulation, and it is mostly the non-local effects that impose the warming for afforestation (cooling for deforestation) (because the albedo effect, which is the strongest effect on cooling, acts foremost non-locally; Winckler et al., 2018 10.1029/2018JD030127). So I think there is better than "low confidence" given the discrepancy between models and observations has been resolved. Both local and total (local+non-local) effects are important to discuss in terms of side-effects of CDR measures: Mitigation policies need to consider local and non-local effects since they aim at the entire global signal, while adaptation strategies need to consider only local effects, since they target local living conditions (Winckler et al., 2019 https://doi.org/10.5194/esd-10-473-2019). [Nicole Wilke, Germany]	Rejected. The low confidence statement refers to the overall impact of afforestation on global mean temperature, not only to the biophysical impact. Also, there is no space to add discussion on local vs non-local impacts.
71745	94	43	94	51	The Biogenic Volatice Organic Compunds and their effect on the aerosol and cloud formation should be accounted for when discussing feedbacks and the net climate impacts of different actions. It should be more clearly aknowledged that the studies modeling the mitigation through land-use are still missing this effect, although the BVOC-aerosol-CCN phenomena has been described well enough as a process (please see references in SRCCL chapter 2.5.2 and footnotes 27 and 28 of SRCCL SPM A4.5). Increased cloud albedo due to BVOC-SOA-CCN likely reduces the adverse surface albedo effect of forests in high latitudes. [Tuomo Kalliokoski, Finland]	Accepted. Added a short sentence on that. There is unfortunately no space to discuss the deficiencies in the models.
58311	94	44	94	44	...lead to changes in the albedo... [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - change was made.
41705	94	44	94	45	changes "in" (?) [Katharina Meurer, Sweden]	Accepted - change was made.
41707	94	46	94	46	the difference between "afforestation" and "reforestation" might not be clear at this point [Katharina Meurer, Sweden]	Taken into account. We slightly modified the sentence to make it clear that we mean both processes.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
58395	94	46	94	48	The citation Mengis et al. (2019) suggests possibly more surface runoff in RCP8p5 due to forestation (leading to the diltuion of surface ocean in their results) rather than less, which perhaps makes this statement a little less confident. However, an additional reference supporting less runoff would be Krause et al. (2017) cited two sentences later, and overall less runoff from forestation seems more likely. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Thank you for noticing this.
41709	94	48	94	50	it might help the reader to get an explanation for why N2O fluxes could either decrease or increase due to afforestation/reforestation [Katharina Meurer, Sweden]	Accepted. Explanations added.
96643	94	50	94	55	The authors may wish to consider inserting some elements (or all aspects) of the following message which has emerged from IPBES at the end of this para: "Avoiding, reducing and reversing land degradation can contribute substantially to adaptation to and mitigation of climate change, but land-based climate adaptation and mitigation strategies must be implemented with care if unintended negative impacts on biodiversity and ecosystem services are to be avoided (well established)." For further reading see: IPBES (2018: 27): Summary for policymakers of the assessment report on land degradation and restoration of the Intergovernmental Science-Policy Platform on Biodiversity and Ecosystem Services. R. Scholes, L. Monanarella, ... at https://ipbes.net/assessment-reports/ldr Rationale: Whilst looking for solutions, it is important to ensure that no new ecological or socio-economic challenges are created. Source: IPBES (2018: 27): Summary for policymakers of the assessment report on land degradation and restoration of the Intergovernmental Science-Policy Platform on Biodiversity and Ecosystem Services. R. Scholes, L. Monanarella, ... at https://ipbes.net/assessment-reports/ldr. [Nicole Wilke, Germany]	Rejected. Mitigation, adaptation as well as biodiversity will be thoroughly addressed by WGII. WGI's focus is in the physical science basis of carbon cycle and biogeochemical aspects.
96645	95	1	95	2	Please consider to point out restoration or marginal/degraded land and agricultural practices separately. [Nicole Wilke, Germany]	Taken into account. It was separated in the beginning of the paragraph, but many of the methods cited can be used both for restoration of degraded land and applied to traditional agricultural lands.
58397	95	1	95	8	This sentence is very long and repeats itself several times, including mentioning cover crops three separate times, root mass & depth two times, and crop residue two times as well. Avoiding repetition and possibly breaking the sentence at the comma on line 2 ("... and improved agricultural practices. These include selection of...") and moving Paustian et al. (2016) to the end with Fargione et al. (2018) (as Paustian et al. covers many of the listed techniques, and so is better cited at the end of the list) would improve readability and save a lot of space. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. The sentence has been restructured to reduce repetition and improve readability, following comments 16985 and 58397.
16985	95	1	95	8	This sentence seems to contain repeated information about cover crops, residues and root depth/mass. [Andreas Krause, Germany]	Accepted. The sentence has been restructured to reduce repetition and improve readability, following comments 16985 and 58397.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
68281	95	1	95	17	Carbon losses from human agriculture accounted for about 113 Gt C (~415 Gt CO2). With best management practices, two thirds of losses may be recoverable, setting a theoretical maximum of 276 Gt CO2 that can be sequestered in soils. See Sanderman et al., Soil carbon debt of 12,000 years of human land use, PNAS (2017) and Correction for Sanderman et al., Soil carbon debt of 12,000 years of human land use. [Durwood Zaelke, United States of America]	Accepted. A modified version was included as an introduction to the paragraph.
127853	95	1	95	50	The paragraph on agricultural soil management for CDR covers the many practices that can sequester C, and accurately cautions that striving for enhanced plant biomass through increased N fertilization can boost N2O emissions. Enhancing plant biomass production through N fertilization has also been shown to yield little or no additional soil C sequestration, and can even result in net soil C losses (Khan et al., 2007). Agroforestry practices that integrate reforestation with food or fodder production merit greater emphasis in this paragraph. These include alley cropping, permaculture, forest gardening, multistory cropping, silvopasture, and buffer plantings such as hedgerow that include tree fruits or nuts. In addition, management intensive rotational grazing methods adapted to locale should be mentioned explicitly. As noted and referenced earlier (comments on page 89 of Chapter 5) agroforestry and advanced grazing management have been shown capable of sequestering >2.25 Mg C/ha-yr (8.2 Mg/ha-yr CO2), compared to 0.45-0.67 Mg C/ha-year (1.6-2.5 Mg CO2/ha-yr) for best integrated management of annual crop rotations (cover crops, tight diverse rotation, reduced till, best nutrient management). In addition, managed plant communities that include a diversity of food-bearing trees can play key roles in food security, especially in developing nations -- thus accomplishing CDR without difficult tradeoffs in terms of food production, biodiversity, or other vital services. Regarding biochar, Lal et al. (2018) estimates that production and land application of biochar can sequester 0.5-0.9 Pg C annually (global total), accounting for 22 ppm of the 156 ppm reduction in end-of-century atmospheric CO2 concentration from land-based biological CDR. However, feedstocks for biochar production must be selected and harvested with care. Removal of plant biomass from native plant communities or agricultural fields for biofuel, biochar, or compost production can have severe adverse impacts on biodiversity, water supplies, food security, cultural values, and resilience of rural communities to climate change. Conversely, conversion of organic ""wastes"" (manure from	Rejected. Detailed description of different agroforestry practises is given in WGIII. WGI's focus is in the physical science basis of carbon cycle and biogeochemical aspects.
58399	95	7	95	7	Wilhelm et al. (2004) has a wrong doi in the reference list - it should end in ".1000a" [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Reference corrected.
104709	95	7			"in addition to low-tillage I would put no-tillage" [Andrea Bianchi, Italy]	Taken into account. Both techniques are included, as both contribute to increase soil carbon.
20015	95	9	95	9	"The carbon sequestration potential of soil carbon sequestration methods" leaves speechless [philippe waldteufel, France]	Accepted. The redundancy was eliminated.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
115437	95	9	95	9	the statement that "the carbon sequstration potential of soils carbon sequestration methods is substantial", referred to technolgical means is not sustained by litterature and it is at best is a controversial and contsted area, as the carbon balance could be zero, positive or negative, but can´t be said it would be substantial, referred ti to technological CDR (geoengineering) methods such as biochar and others. Natural / indingenous conservation of soils -without geoengineering- could be potentially very important, but in this sentence it gives a worng idea, as if all could have the same potential [SILVIA RIBEIRO, Mexico]	Rejected. While we agree that, in general, soil "carbon balance could be zero, positive or negative", the text argues that well-managed soils can increase soil carbon content significantly.
58401	95	10	95	11	It would be useful to explicitly indicate what the sign and magnitude of grassland sequestration actually is, for example by referring the reader to Figure 5.36. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. Reference is made to Figure 5.36. A decision was made to remove all sequestration potentials values from this chapter, to avoid overlapping with WGIII.
41177	95	10			what is this high confidence magnitude? Nothing is said about the actual values [TSU WGI, France]	Not applicable. A decision was made to remove all sequestration potentials values from this chapter, to avoid overlapping with WGIII.
58403	95	11	95	12	The given reference & doi for Sanderman et al. (2018) is for the correction, but it might be better to cite the original article (which by default includes the correction online): doi:10.1073/pnas.1706103114 [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Reference corrected.
18243	95	12	95	12	The sentence is missing a word as it doesn't make sense. Please amend. [Chelsey Baker, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Sentence was fixed following suggestion of comments 609, 16987 and 16989.
16987	95	12	95	12	"to (Yang et al., 2018)" should be (re)moved. [Andreas Krause, Germany]	Taken into account. Sentence was fixed following suggestion of comments 609, 16987 and 16989.
51171	95	12	95	12	This sentence is a little confusing - should it read "Several of these methods would not only go to increase carbon input and storage in soil,"? [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Sentence was fixed following suggestion of comments 609, 16987 and 16989.
51173	95	12	95	12	I think there's a word missing; "several of these methods would serve not only to"? Suggest addition of 'serve' [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Sentence was fixed following suggestion of comments 609, 16987 and 16989.
58405	95	12	95	15	Yang et al. (2018) is about permafrost and nitrous oxides rather than agricultural soil management. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Reference has been deleted.
58407	95	12	95	15	Fornora et al. (2011) is primarily about the impacts of liming, which hasn't previously been discussed in this section. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Reference has been deleted.
73271	95	12	95	15	This sentence does not make sense, and I cannot work out what it should say! [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Sentence was fixed following suggestion of comments 609, 16987 and 16989.
609	95	12			remove "to (Yang et al., 2018)" [Vicca Sara, Belgium]	Taken into account. Sentence was fixed following suggestion of comments 609, 16987 and 16989.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
16989	95	13	95	13	"to" should read "also" or "additionally". [Andreas Krause, Germany]	Accepted - The sentence was rephrased for clarity. "Several of these methods would not only to (Yang et al., 2018) increase carbon input and storage in soil, but to hamper environmental..." was changed to: "Several of these methods would not only (Yang et al., 2018) increase carbon input and storage in soil, but also hamper environmental..."
58409	95	13	95	15	David & Drinkwater (2006) should read Tonitto et al/ (2006) (Tonitto seems to have jumped to the end of the parentheses), and there are two "to"s ("would not only to" l.12, & "but to hamper" l.13) which seem to be surplus to requirement. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Corrected.
58411	95	16	95	17	Smith et al/ (2018b) specifically discusses how cover crops and crop diversity can increase biodiversity, but does not cover all carbon sequestration methods - either an additional reference to cover other methods or revising the sentence to specifically target cover crops & diversity would be useful to justify medium confidence through medium evidence. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. The sentence was rewritten, restricting it to the methods analysed by Smith et al. (2018b).
106183	95	19	95	19	An excellent recent general-purpose summary of the available forms of biomass pyrolysis is: Schmidt, H.-P., et al., Pyrogenic Carbon Capture and Storage, Global Change Biology Bioenergy, 11:573-591, 2019. [Wolfgang Lucht, Germany]	Taken into account. The reference was cited.
106185	95	19	95	19	Here and in table 5.10 the wording "biochar" is used when speaking about products of biomass pyrolysis, but these techniques lead to more than char - e.g. the resulting oils also are an important factor. It has therefore been proposed (see Schmidt et al., GCB Bioenergy 11, 573, 2019) to call this pathway "PyCCS" as a parallel to "BECCS" [Wolfgang Lucht, Germany]	Taken into account. We mention the term PyCCS, but keep our discussion to the carbon sequestration purpose of the method.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
51175	95	19	95	27	this section seems to overstates the advantages of biochar and understates the risks, and places the focus on carbon benefits over soil carbon sequestration benefits. Suggest adding: "Biochar application can also improve soil porosity (De Gryze, 2010, Oguntunde et al., 2008), increase pH (high confidence) (De Gryze et al., 2010), reduce nutrient losses (medium confidence) (Woolf et al., 2010), decrease fertiliser need (medium confidence) (Yeboah et al., 2009), adsorb and immobilise harmful environmental contaminants (medium confidence) (Stefaniuk et al., 2017; Beesley et al., 2011) and ultimately increase plant yield (low confidence)(Ye et al., 2020) - although benefits are often noted in already degraded, acidic soils, with less evidence for soil benefits in healthy soils with moderate pH (Jeffery et al., 2017). Risks are noted in the potential to introduce environmental contaminants to the soil, such as heavy metals and polycyclic aromatic hydrocarbons (medium confidence) (Helsen et al., 1997; Zhurnish et al., 2005). Ingestion of crops grown in biochar amended soil is modelled to have low human health risk overall (low confidence)(Wang et al., 2019). " or, if authors want to focus purely on soil benefits/disbenefits - add "Biochar application can improve many soil qualities (medium confidence) and increase crop yield (low confidence), particularly in already degraded, acid-rich soils (Woolf et al., 2010; Jeffery et al., 2017; De Gryze et al., 2010). There are risks of introducing harmful contaminants into the soil environment however (medium confidence) (Helsen et al., 1997; Zhurnish et al., 2005)." [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Part of the suggested text + references was added to the paragraph.
58415	95	22	95	27	If there is low confidence in biochar impacts on nitrogen use efficiency and phosphorus bioavaliability on l.26/27 (which seems to be the right confidence rating), can there be medium confidence in reduced nutrient losses on l.23 (given that nutrient losses are partly linked to impacts on nutrient use efficiency and bioavailability?). [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Confidence statement was changed.
58413	95	24	95	24	The title of Kammann et al. (2017) is misspelled in the reference list ("knows, unknows…" rather than "knowns, unknowns…"). [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Reference corrected.
58417	95	27	95	27	Liu et al. (2017) should be Liu et al/ (2017b). [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Reference corrected.
29165	95	29	95	29	I propose to replace "biofuels" in this line with "bioenergy". Many experts equate bio"fuels" with liquid fuels, but for BECCS it will be most likely more useful to burn solid biomass without accepting the substantial conversion losses resulting from turning biomass into liquids with highly defined chemical properties, as required for vehicle motors (which are difficult to couple with CCS anyway) [Helmut Haberl, Austria]	Taken into account. Word has been replaced.
71747	95	29	95	31	Reference you give here is not proper for the content. Anderson and Peters criticize BECCS and the IAM dependence of them to reach mitigation targets. Please change the content of sentence or reference. [Tuomo Kalliokoski, Finland]	Accepted. Although Anderson and Peters did say what we quoted, we updated the reference to a more recent review.
115439	95	29	95	31	BECCS has been erroneusly considered too much as an option by former IPCC reports. The cited article from Anderson and Peters are extremely critical of this consideration. Tjerefore, please delete the expresion m "one fo the most important" to say that is was abundalty considered. [SILVIA RIBEIRO, Mexico]	Taken into account. More recent literature, published since the SOD was finalized, has been added to the assessment.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
15977	95	29	95	50	Again, as is common elsewhere in the report, the discussion of BECCS is incomplete without a statement on the thermal efficiency of a BECCS power plant. Our estimation is that this can be no more than 8% once the energy needed for compression of the CO2 exhaust gas is included. [Kevin Lister, United Kingdom (of Great Britain and Northern Ireland)]	Rejected – no scientific evidence/publication provided to support changes suggested by the reviewer
68283	95	29	95	50	While an improvement on previous IPCC discussions of BECCS, this section should include that BECCS is not carbon negative in the near-term because bioenergy leaves a carbon deficit for several decades to a century—far longer than the window of a decade or two available for slowing feedbacks and avoiding crashing through the 1.5C guardrail. See Sterman J. D., et al. (2018) Does replacing coal with wood lower CO2 emissions? Dynamic lifecycle analysis of wood bioenergy, Envtl. Research Letters 13(015007):1–10, 1 ("We simulate substitution of wood for coal in power generation, estimating the parameters governing NPP and other fluxes using data for forests in the eastern US and using published estimates for supply chain emissions. Because combustion and processing efficiencies for wood are less than coal, the immediate impact of substituting wood for coal is an increase in atmospheric CO2 relative to coal. The payback time for this carbon debt ranges from 44–104 years after clear-cut, depending on forest type—assuming the land remains forest. Surprisingly, replanting hardwood forests with fast-growing pine plantations raises the CO2 impact of wood because the equilibrium carbon density of plantations is lower than natural forests. Further, projected growth in wood harvest for bioenergy would increase atmospheric CO2 for at least a century because new carbon debt continuously exceeds NPP. Assuming biofuels are carbon neutral may worsen irreversible impacts of climate change before benefits accrue. Instead, explicit dynamic models should be used to assess the climate impacts of biofuels."). [Durwood Zaelke, United States of America]	Taken into account. The Sterman et al. Paper was cited in the SOD and continues to be in the FGD, although now we make it more specific that their results are for wood-based biofuels.
111867	95	29	95	50	I think this para on BECCS could be a bit more 'belanced', by highlighted why it play a dominant role in IAMs (https://www.springerprofessional.de/the-value-of-beccs-in-iams-a-review/17461088), and that the assessment vof BECCS is very feedstock (https://pubs.rsc.org/en/content/articlelanding/2020/se/c9se00609e#!divAbstract) and process depependent (https://www.sciencedirect.com/science/article/pii/S2352484719301829) [Oliver Geden, Germany]	Taken into account. More recent literature, published since the SOD was finalized, has been added to the assessment.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
58419	95	33	95	35	As far as I can tell, some of the citations given here don't state the specific impact of replacing marginal land with woody biomass plantations on carbon uptake or soil carbon. For example, Don et al (2012) does mention non-woody miscanthus and woody short-rotation coppice increasing carbon uptake but discusses them together, Kraxner et al (2013) doesn't mention SOC or marginal land, and Smith et al (2012b) doesn't appear to mention SOC or woody biomass. Furthermore, Boysen (2017) could be either 2017a or 2017b (although both are relevant). Widening marginal to include degraded land would make some of these citations more relevant, but otherwise it seems Heck et al (2016) is the key citation here with the others being rather more general. Unless I'm missing something I suggest either narrowing the citations to be about this specific case or broadening/clarifying the statement to match the citations. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. The sentence was modified, references were revised and some were changed.
16991	95	33	95	38	Does not seem like a fair comparison to assume marginal land for woody bioenergy plants while carbon-rich ecosystems for herbaceous bioenergy plants. Maybe just use "bioenergy plants"? [Andreas Krause, Germany]	Accepted. Text was changed.
5669	95	35	95	38	Please check and correct: if ecosystems are replaced by herbaceous plants, there is no standing forest any more whose sink capacity could be reduced. [Joachim Rock, Germany]	Accepted. Text was corrected.
58421	95	35	95	38	It again seems that several of the citations are not specific to the particular case of carbon-rich ecosystems being replaced by herby plantations leading to soil or forest carbon loss. Boysen et al. 2017a & 2017b don't specifically mention SOC and imply the primary issue with forest conversion is biodiversity loss, Richards et al. (2017) isn't listed in the references, Heck et al. (2018) doesn't specifically discuss forest conversion, and Vaughan et al. (2018) assumes no forest conversion in their calculations. Only Harper et al. (2018) unambigiously looks at this particular case, while Don et al. (2012) could also be a useful addition here. With both this and the previous sentence and if I've not missed something I suggest narrowing down the references to be more specific, or reusing the other references to make a more general statement beforehand. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. The sentence was modified, references were revised and some were changed.
58831	95	39	95	39	biomass transport, conversion and capture chain' Please define. Are these a series of processes? [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. Sentence of clarified by introducing "chain of processes".
58423	95	39	95	41	These numbers don't quite match those from the citations - Humpenöder et al. (2014) and Heck et al. (2018) both quote 90% capture rate & 55% conversion for one BECCS technology and another technology giving 48% & 41% respectively, Fuss et al. (2018) mention a 10-30% conversion loss, while Creutzig et al. (2015), Vaughan & Gough (2016), and Krause et al. (2017) don't appear to have specific figures for this. This would seem to give a range of more like 40-90>% rather than 50-90<% efficiency. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. Numbers and references were updated.
16993	95	40	95	40	"to 50 to >90%" is not quite clear, maybe replace by "up to 50%"? [Andreas Krause, Germany]	Taken into account. Numbers and references were updated.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
84003	95	42	95	43	First, the sentence should specify that the conclusions reffer specifically to bioenergy originated for wood. The sources for bioenergy are very different, involving different pathways, with different outcomes. Generalizing a study on wood, is problematic. It is important to remember that CO2 emission from bioenergy crops comes from the active carbon pool, different from CO2 from fossil fuel emissions. Further, the reference for this affirmation, was extracted from a paper that received several critical responses. These should be considered carefully. [Marco Tulio Cabral, Brazil]	Accepted. The sentence was rewritten, making it specific for wood-based bioenergy.
90127	95	43	95	46	This statement uploaded by Ted Schuur on behalf of a graduate class that reviewed the chapter (this was communicated with IPCC TSU in advance): What steps should be taken to commercially scale CCS. [Edward Schuur, United States of America]	Rejected. Outside the scope of this chapter. This is covered in AR6 Chapter 12.
58425	95	46	95	46	CCS leakage or permanence doesn't appear to be mentioned in Vaughan et al. (2018). [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. This reference has been eliminated.
115441	95	46	95	47	There is wide convergence that BECCS would have serious side effects on biodiversity, please change the "low confidence" category. [SILVIA RIBEIRO, Mexico]	Rejected. The sentence was modified to be valid for really big scales of BECCS deployment
16995	95	47	95	47	C should probably read "carbon". Also e.g. line 15. [Andreas Krause, Germany]	Accepted.
96647	95	47	99	16	Please provide information on the permanence of CCS. Currently there is inconsistent information in 05-95-47 "while leakage and uncertain permanence of CCS could substantially diminish the anticipated CDR goal" vs. " 05-99-15 "Storage is potentially permanent in both pressurised gas form and mineral form." Such information is highly policy relevant. Please add also information regarding the implementation of CCS, or refer to WGIII if appropriate. [Nicole Wilke, Germany]	Accepted. Assessment of CCS has been rewritten and is now given in the DACCS section.
58427	95	48	95	48	Farley et al. (2005) focuses on afforestation impacts and not BECCS (admittedly BECCS somewhat overlaps, but in this report they are being discussed as separate techniques). [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. The reference was changed to Krause et al. 2017
106187	95	48	95	50	Since BECCS is a land-based NE method for climate stabilisation it would be good to put it not just into a climate contaxt but an Earth system context. The paper by Heck et al., Nature Climate Change 2018 - already cited above, but not its main message - shows that BECCS can help maintain the climate planetary boundary (Rockström et al., 2009; Steffen et al., 2015) but implies transgressing further or putting more pressure on other land-related planetary boundaries; if all planetary boundaries of the planetary boundaries are respected, there is no or only a small remaining potential for BECCS. Reference to this would put the climate discussion of this chapter into the context of an Earth system discussion (cf. Anthropocene). [Wolfgang Lucht, Germany]	Noted. Reference Heck et al. 2018 was added in the sentence discussing water and nutrient deficiency. Planetary boundary concept was not introduced due to lack of space.
58429	95	52	95	53	Is a reference required here to back up having a very high carbon stock per unit area? It's mentioned in Griscom et al. (2017) cited in the next sentence, so perhaps that citation can be brought up to this sentence to plug the gap. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
5047	95	52	96	2	Comment/Addition: There is still a lot of discussion surrounding emissions from re-wetted peatland sites, and the subject is a complex one (not least because there are inter-annual differences in peatland emissions relating to differing weather which are unrelated to management). However, a recent article by Günther et al. (2020) suggests that over the long term the reduction in CO2 emissions which results from re-wetting more than compensates for the initial increase in CH4 emissions when viewed in terms of the overall radiative forcing. As they say "...postponing rewetting increases the long-term warming effect through continued CO2 emissions." Some expansion of this paragraph I think is required as it has important management implications for peatland areas; in particular it should at least be mentioned that there is a growing body of evidence that, in the long term, peatland rewetting is beneficial in terms of reducing the warming potential of the different GHGs emitted by degraded peatlands. Günther et al. (2020) Prompt rewetting of drained peatlands reduces climate warming despite methane emissions Nature Communications 11:1644 https://doi.org/10.1038/s41467-020-15499-z [Thomas Kelly, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Information was added to the text.
58313	95	52	96	6	I think you should really specify that the high carbon stock is actually hold in PEATLANDS within the accumulated peat layers, which can make up >10m of peat. These peat layers are massive carbon stores, but not so much the "wetlands" in gerenal, which is defined having peat layers only <30-40 cm (depending on the country you are in and how they differentiate between peatlands and wetlands). [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Information was added to the text.
41711	95	52	96	6	this is certainly true, but very simplified. Actually, the high CH4 emissions that CAN occur after rewetting of drained peatlands, is very much dependent on the nutrient status of the peatland (PAPER SABINE) [Katharina Meurer, Sweden]	Noted. Unfortunately there is no space to go into details what comes to the rewetting of peatlands.
98493	95	53	95	53	"Wetland restoration relies on back-conversion or buid of high-carbon density soils" - should it be "building high-carbon-density soils"? Awkward phrasing. [Emily Romano, United States of America]	Accepted.
109663	95	54	96	2	A recent study using global scenarios for future peatland management showed that these CH4 emissions do not undermine the climate change mitigation potential of rewetting (Günther, A., Barthelmes, A., Huth, V. et al. Prompt rewetting of drained peatlands reduces climate warming despite methane emissions. Nat Commun 11, 1644 (2020). https://doi.org/10.1038/s41467-020-15499-z). [Carolyn-Monika Görres, Germany]	Accepted. A sentence discussing the Gunther et al paper was added.
58431	95	55	96	2	While it's true that it is fairly likely that rewetting boosts methane emissions, it might be worth explicitly noting for context that these papers state the overall effect is still a net GHG emission reduction - a point also well shown by the recent work of Günther et al. (2020, doi.org/10.1038/s41467-020-15499-z) which would make a useful extra citation here. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. A sentence discussing the Gunther et al paper was added.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
58833	96	3	96	3	what is a 'nutrient retention function'?Please define, what is the nutrient retenetion a fucntion of? [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. We removed "function", as it was redundant.
58433	96	9	96	13	It doesn't seem that Table 5.10 has actually been referred to in the text, despite being critical to this whole section. Perhaps it can be mentioned near the start of section 5.6.2.2.1 as context for the following discussion of each method? [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable. Table 5.10 has been merged with Table 5.9 in Section 5.6.2.1, which is cited a couple of times in the text.
111869	96	9	96	13	Table 5.10 needs to be coordinated with WG3 (ch12), to avoid giving contradicting information [Oliver Geden, Germany]	Accepted. Table 5.10 has been merged with Table 5.9 in Section 5.6.2.1. CDR potentials are not longer presented and discussed in Chapter 5, as they overlap with WGIII Chapters 7 and 12. As WGIII will be published later, they will present updated results on CDR potentials.
58315	96	9	97	1	Table 5.10: The factors are sometimes very specific ("fires, extreme weather, drainage") and sometimes only broad ("soil management"). "Soil management" is very general and could be defined further. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. We replaced it by "Soil and crop management", which cover a large number of processes. For further details, the reader is referred to the main text.
58317	96	9	97	1	Wetland restoration: Favtors that affect C storage are also fires, drought (extrme weather), anthropogenic disturbance [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. These factors were included to the Table. Please note that Table 5.9 has been merged with Table 5.9 in Section 5.6.2.1.
58435	96	9	97	2	Forest CDR potential is stated in Table 5.10 as 0.5-2.8 PgC/y, but SRCCL gives 1.8-26.9 GtCO2e/y (=0.5-7.3PgCe/y) in Table 6.14 - is there a source for the reduced upper limit within SRCCL, or is this linked to looking at just CO2 for CDR purposes? [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable. Table 5.10 has been merged with Table 5.9 in Section 5.6.2.1. CDR potentials are not longer presented and discussed in Chapter 5, as they overlap with WGIII Chapters 7 and 12.
58437	96	9	97	2	Some of the Table5.10 estimates match the cited literature, but lie quite far beyond other papers discussed in the text. For example, ocean fertilisation is 0.32-12 based on Fuss et al. (2018), but Oschlies et al. (2010a) and Keller et al. (2014) discssed on p.97 l.28-30 give a tighter range of more like 0.5-4PgC/y. The upper range estimate of 12GtC/y from Fuss et al. is based on two studies, one quite old (Sarmiento & Orr, 1991) and the other Oschilles et al. (2010a), but the latter figure doesn't seem to match the estimates here or in the paper itself (Fuss et al. give ~36GtCO2/y [=9.7GtC/y] max from Oschlies et al., which is rather larger than the max ~2.5GtC/y in the first year that Oschlies et al actually seem to show). Based on this, and unless I've missed something from one of these papers, I'd suggest using the more constrained figures discussed here as the central estimate in place of Fuss et al. in Table 5.10 & Fig 5.36, with perhaps the 12GtC/y used as an outlier value. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable. Table 5.10 has been merged with Table 5.9 in Section 5.6.2.1. CDR potentials are not longer presented and discussed in Chapter 5, as they overlap with WGIII Chapters 7 and 12. As WGIII will be published later, they will present updated results on CDR potentials.
58439	96	9	97	2	For blue carbon, a cost-effective max of ~0.05PgC/y is given in Table 5.10, but the cited Griscom et al. (2017b) rate 0% of blue carbon options as low-cost. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected - cost-effectiveness of blue carbon CDR approaches are covered in WGIII, Chapter 12.3.2.3

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58441	96	9	97	2	0.05-26 PgC/y from Fuss et al. (2018) is given for enhanced weathering in Table 5.10, but SRCCL previously estimated 0.1-1.1PgC/y as the more likely feasible range (a figure closer to Fuss et al's authors' assesment range for EW). [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable. Table 5.10 has been merged with Table 5.9 in Section 5.6.2.1. CDR potentials are not longer presented and discussed in Chapter 5, as they overlap with WGIII Chapters 7 and 12. As WGIII will be published later, they will present updated results on CDR potentials.
69197	96	9	97	2	Presenting table 5.10 seems better place where all the CDR method are explained, while it is now table 5.10 just after the explanation of land-based CDR but before ocean-based CDR. Moving table 5.10 to the place after section 5.6.2.2.3 (Geochemical and chemical CDR methods) is suggested. [Kaoru Magosaki, Japan]	Not applicable. Table 5.10 has been merged with Table 5.9 in Section 5.6.2.1
112127	96	9	97	2	Over what time frame is the CDR potential listed here? How does this change over time? How quick is the spin-up? What is the implied cumulative CDR potential until 2100? How representative are the values across the literature? Some of this is covered for some of these numbers in the respective text sections, but most of it is not covered for most of the numbers, which makes the table useless in its present form. Very extensive literature analyses of exactly this are given by Minx et al. and Fuss et al. (Environ. Res. Lett., 2018). and to a lesser extent by Lawrence et al. (Nature Communications, 2018) - these could easily be made use of here. [Mark Lawrence, Germany]	Not applicable. Table 5.10 has been merged with Table 5.9 in Section 5.6.2.1. CDR potentials are not longer presented and discussed in Chapter 5, as they overlap with WGIII Chapters 7 and 12. As WGIII will be published later, they will present updated results on CDR potentials.
10271	96	11	96	11	Table 5.10 is NOT referred to in the text. [Chris Vivian, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. Table 5.10 has been merged with Table 5.9 in Section 5.6.2.1. Table 5.9 is cited in the text a couple of times.
115443	96	11	96	11	The table confuses natural carbon sinks (coastal vegetation) with technological ones, The use of the term CDR to englobe all kinds of carbon removal, including natural ones, such as forests and wetlands and coastal vegetation is extfremely confusing. CDR in all litterature before SR1.5 and still in most litterature today is referred to and associated with TECHNOLOGICAL removals and withg geoengineering. Those promoting geoengineering are pleased that a body as IPCC confuses natural cycles with technological risky proposals, such as all geoengineering tecniques. PLEASE remove the use of these terms for natural carbon sinks. They are different in all senses and it is not helpful for neihter researchers or policy makers. Teh table seems biased in the consideration of potential of a lsit of technology that haven´t been proved nowhere. Even CCS, and this all other referred to it, such as BECCS and DACSS, are not proven they woukld function or fulifll the attributed characteristic. Table should be reviewed to integrate critical views, not just repeat the promoters list of promises that are not proven. In the case of Enhance Weahtering, the issue of the extreme demand of mining should be acknowledged. [SILVIA RIBEIRO, Mexico]	Rejected. As defined in the Glossary as well as in Section 5.6.1, "CDR refers to anthropogenic activities that seek to remove CO2 from the atmosphere and durably store it in geological, terrestrial or ocean reservoirs, or in products". Previous reports follow a similar definition and a similar list of methods (see, for example, AR5, Tables 6.14 and 6.15). This report does not use the term "geoengineering" anymore, and instead uses separate terms for Carbon Dioxide Removal and Solar Radiation Management. WGIII also discusses CDR.
18935	96	11	96	11	Table 5.10 is not called out in the text [Govindasamy Bala, India]	Not applicable. Table 5.10 has been merged with Table 5.9 in Section 5.6.2.1. Table 5.9 is cited in the text a couple of times.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
10273	96	11	97	1	Based on the GESAMP (2019) report, a number of additional marine-based CDR techniques could be included in Table 5-9. Notable techniques omitted from Table 5.9 are those that store carbon in the ocean and some of the ocean alkalinity techniques. [Chris Vivian, United Kingdom (of Great Britain and Northern Ireland)]	Noted - note that this section does not cover carbon injection methods or storage in geological structures. Aspects related to solar radiation modification are covered in section 5.6.3. The GESAMP report is now cited in the text.
10275	96	11	97	1	Table 5.10 - Ocean fertilisation - The top of the range for the CDR potential attributed to Fuss et al. (2018) is incorrect: a) Fuss et al. (2018) in their paper have misinterpreted the data from Sarmiento and Orr (1991) (https://doi.org/10.4319/lo.1991.36.8.1928). The figure of 12 Pg yr-1 in Table 5.10 was taken by Fuss et al. (2018) from the statement "This initial pulse is rapidly followed by much lower perturbation new production values of order 14 Gt C yr-1 , gradually settling down to - 12 Gt C yr-1 after 1,500 yr" on page 1935 of the Sarmento and Orr paper. However, that statement refers to the increase of surface waters productivity NOT to the long term uptake of carbon by the ocean. The latter is addressed on pages 1940-1941 of the Sarmento and Orr paper where it states "If Southern Ocean nutrients are depleted, the reduced surface pC02 that results will enhance the oceanic uptake of anthropogenic CO2 by 1 - 12 Gt C yr-1 in the first year, decreasing rapidly to between 1 and 2 Gt C yr-l (Fig. 8b). The latter point is confirmed by Table 2 of Strong et al. (2009) (https://doi.org/10.5670/oceanog.2009.83) where it quotes a maximal estimate of C sequestration by Sarmiento and Orr of 1-1.5 Gt yr-1 integrated over a century. Table 2 from Strong et al (2009) provides additional estimates of C sequestration rates by 4 additional authors. b) Fuss et al (2018) in their paper have also misinterpreted the data from Oschlies et al. (2009) and (2010) (https://doi.org/10.5194/bg-6-1603-2009 and https://doi.org/10.1029/2009GL041961). In their paper on p. 23/24 Fuss et al. (2018) said: “The overall reported minimum sequestration value for OF is 1.52 × 105 t CO2	Not applicable. Table 5.10 has been merged with Table 5.9 in Section 5.6.2.1. CDR potentials are not longer presented and discussed in Chapter 5, as they overlap with WGIII Chapters 7 and 12. As WGIII will be published later, they will present updated results on CDR potentials.
10327	96	11	97	1	Table 5-10, Artificial ocean upwelling, CDR potential - I cannot see where the figure of 4.3. comes from. I suggest it be deleted since Oschlies et al. (2010b) says "Our study suggests that artificial upwelling by ocean pipes may, under the hypothetical and most optimistic assumption of a massive deployment of perfect ocean pipes, be able to sequester atmospheric CO2 at a rate of about 0.9 PgC/yr-1". Also, delete the reference Keller et al. (2014) as it derives its data for AOU from Oschlies et al., 2010b [Chris Vivian, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable - discussion of CDR potentials has been moved to WGIII, Chapter 12

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
12693	96	11	97	1	Table 5.10 - Ocean fertilisation - The top of the range for the CDR potential attributed to Fuss et al. (2018) is incorrect: a) Fuss et al. (2018) in their paper have misinterpreted the data from Sarmiento and Orr (1991) (https://doi.org/10.4319/lo.1991.36.8.1928). The figure of 12 Pg yr-1 in Table 5.10 was taken by Fuss et al. (2018) from the statement "This initial pulse is rapidly followed by much lower perturbation new production values of order 14 Gt C yr-1 , gradually settling down to - 12 Gt C yr-1 after 1,500 yr" on page 1935 of the Sarmento and Orr paper. However, that statement refers to the increase of surface waters productivity NOT to the long term uptake of carbon by the ocean. The latter is addressed on pages 1940-1941 of the Sarmento and Orr paper where it states "If Southern Ocean nutrients are depleted, the reduced surface pC02 that results will enhance the oceanic uptake of anthropogenic CO2 by 1 - 12 Gt C yr-1 in the first year, decreasing rapidly to between 1 and 2 Gt C yr-l (Fig. 8b). The latter point is confirmed by Table 2 of Strong et al. (2009) (https://doi.org/10.5670/oceanog.2009.83) where it quotes a maximal estimate of C sequestration by Sarmiento and Orr of 1-1.5 Gt yr-1 integrated over a century. Table 2 from Strong et al (2009) provides additional estimates of C sequestration rates by 4 additional authors. b) Fuss et al (2018) in their paper have also misinterpreted the data from Oschlies et al. (2009) and (2010) (https://doi.org/10.5194/bg-6-1603-2009 and https://doi.org/10.1029/2009GL041961). In their paper on p. 23/24 Fuss et al. (2018) said: “The overall reported minimum sequestration value for OF is 1.52 × 105 t CO2	Not applicable - discussion of carbon sequestration potentials of ocean CDR approaches has now been transferred to WGIII Chapter 12.3.3.3
103161	96	11	97	13	Chapter 5 table 5.10. is located between subchapters 5.6.2.2.1. Land-based biological CDR methods and 5.6.2.2.2. Ocean-based biological CDR methods. Yet, the table contains all CDR methods, both on land and in the ocean. Maybe it would make sense moving the table 5.10. to page 94 line 11, between 5.6.2.2 Effects of specific CDR methods on biogeochemical cycles and climate and 5.6.2.2.1 Land-based biological CDR methods. [Philippe Tulkens, Belgium]	Not applicable. Table 5.10 has been merged with Table 5.9 in Section 5.6.2.1
103163	96	11	97	13	Table 5.10 - the table could include a 6th column with the negative impacts (for example for wetlands to include emissions of CH4, for ocean fertilisation and alkalinisation the negative impacts on marine ecosystems, etc. as well as the unknown potentially catastrophic and irreversible dammage; negative impact on the NPP and reduction of the natural ocean carbon sink). As the table includes the column 'factors that affect C storage', it might be taken out of context and interpreted that the table presents all the impacts of the CRD methods. One can also very clearly note in the same table that some CDR methods are prohibited by the London Protocol. [Philippe Tulkens, Belgium]	Taken into account. Table 5.10 has been merged with Table 5.9 in Section 5.6.2.1. Negative impacts are presented in Figure 5.36. There is a brief discussion about the London Protocol and ocean fertilisation in Section 5.6.2.2.2

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
18245	96	13	96	13	Table 5.10 - Ocean fertilisation - the 'timescale of C storage' is stated as 'centuries to millenia' but the factors that affect the timescale are stated as ocean stratification and circulation. I agree that ocean stratification and circulation will affect the shorter timescales, i.e. centuries but the main controls on whether C will be stored for millenia is the amount of C stored at the seafloor which will be controlled by the magnitude of sinking organic material that escapes remineralisation as it transits through the water column. This likely to depend on the region of fertilisation, phytoplankton community structure etc. Not all fertilisation experiments have been successful in increasing the efficiency of the biological carbon pump and so the millenia timescale result is not guaranteed. Some additions should be made to the table to make the differences of these timescales/ processes clearer. [Chelsey Baker, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - sentence altered
69779	96	13	96	13	define 'terminal effects' [Gyami Shrestha, United States of America]	Accepted. Table 5.10 has been merged with Table 5.9 in Section 5.6.2.1. Termination effects are now defined in the Table caption.
82093	96	13	96	13	It is stated that the storage timescale of ocean fertilization is centuries to millenia. I think that it would be better to state that it is "decades to millenia" as some studies (Robinson et al., 2014) have shown that storage can be much shorter. Storage time really depends on where fertilization is done. In some areas storage will only be for decades, while in other areas storage can be for much longer. Robinson, J., E. E. Popova, A. Yool,M. Srokosz, R. S. Lampitt, and J. R. Blundell (2014), How deep is deep enough? Ocean iron fertilization and carbon sequestration in the Southern Ocean, Geophys. Res. Lett., 41, doi:10.1002/2013GL058799. [David Keller, Germany]	Taken into account - sentence altered
82095	96	13	96	13	For ocean alkalinization it's suggested that ocean stratification and circulation affect the C storage time scale. While this plays a role, it's really ocean carbonate chemistry that controls this C storage. The following can be cited concerning this point: Renforth, P., and G. Henderson (2017), Assessing ocean alkalinity for carbon sequestration, Rev. Geophys., 55, doi:10.1002/2016RG000533. [David Keller, Germany]	Taken into account - sentence altered
82097	96	13	96	13	For the termination effect of ocean alkalinization, listing the Gonzalez results can be a bit misleading. Most studies with realistic amounts of alkalinization show no termination effect. In the Gonzalez study massive (unrealistic from most points of view) amounts of alkalinity were added, with abrupt termination during a very high emission scenario. A scenario like this is very, very unlikely. Thus, I would caution about putting this in as a termination effect since it's only an effect under extreme and unrealistic circumstances. [David Keller, Germany]	Taken into account - sentence altered
58835	96	13	96	13	What is a 'termination effect'? This instance in the table is the only usage of it. It would be expected that these are things that stop the process from continuing but the column seems to be adverse side effects. This should be renamed or explained in the table legend. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. Please note that Table 5.10 has been merged with Table 5.9. The concept of termination effects is explained in Section 5.6.2.1.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
10277	96	13	97	1	Table 5.10, ocean alkalinisation, termination effects - the text comes from Gonzalez et al. 2018. In the paper they say "In the AOA simulations surface ocean alkalinity is enhanced as much as needed to stabilize atmospheric CO2 concentrations to RCP4.5 levels under RCP8.5 emissions...". These are very high amounts of ocean alkalinisation against a high emissions scenario. Given that most experts on CDR/NETs believe that it is much more likely that a portfolio of CDR/NETs options will be deployed and the significant logistic issues involved in carrying out such large-scale ocean alkalinisation, it seems unlikely that such high amounts of ocean alkalinisation would ever be deployed. More realistic modelling of smaller scale ocean alkalinisation with a portfolio of other ocean CDR methods would be useful. [Chris Vivian, United Kingdom (of Great Britain and Northern Ireland)]	Noted - yes, the modelling suggested would be useful but is not available for our assessment
38525	96	13	97	1	In table 5.10 DACCS does not have any informationin the termination effects column [Siv K Lauvset, Norway]	Taken into account. All columns/cells have been filled out.
116471	96		96		Confidence in the characteristics and results of the assessment? What is new compared to SR15 and SRCCL here? [Valerie Masson-Delmotte, France]	Table 5.10 has been merged with Table 5.9 in Section 5.6.2. CDR potentials are not longer presented and discussed in Chapter 5, as they overlap with WGIII Chapters 7 and 12. As WGIII will be published later, they will present updated results on CDR potentials. A new statement introducing Table 5.9 makes explicit what is new in this report compared to previous ones.
18937	96		97		Table 5.10: A column with some key recent papers could be cited [Govindasamy Bala, India]	Taken into account. Table 5.10 has been merged with Table 5.9. For reasons of space, the citations remain in the main text.
51177	97	1	97	1	Table 5.10 with DACCS potential - authors also may wish to consider "UK Energy Research Centre: TPA of Negative Emissions"- http://www.ukerc.ac.uk/asset/A4D68D42-D266-4778-B86E8432B0119AC2/ [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. Table 5.10 has been merged with Table 5.9 in Section 5.6.2.1. CDR potentials are not longer presented and discussed in Chapter 5, as they overlap with WGIII Chapters 7 and 12. As WGIII will be published later, they will present updated results on CDR potentials.
791	97	5	98	1	Much of the new literature has not been cited and the text is heavily associated with a decade old literature. In addition, some aspects are discussed in SROCC that should further be cited instead of the older literature [Baruch Rinkevich, Israel]	Taken into account - this section has been extensively rewritten

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
10279	97	5	98	28	This section has not taken into account the GESAMP report published in 2019 that reviewed some 18 marine-based CDR techniques (including variations of approaches) as well as reviewing methane capture and destruction/degradation - see Table 4.2 and section 5 of the report. A number of additional marine-based CDR techniques could be included in this section. Notable techniques omitted are those that store carbon in the ocean and some of the ocean alkalinity techniques. Reference is GESAMP (2019). “High level review of a wide range of proposed marine geoengineering techniques”. (Boyd, P.W. and Vivian, C.M.G., eds.). (IMO/FAO/UNESCO-IOC/UNIDO/WMO/IAEA/UN/UN Environment/ UNDP/ISA Joint Group of Experts on the Scientific Aspects of Marine Environmental Protection). Rep. Stud. GESAMP No. 98, 144 p. http://www.gesamp.org/site/assets/files/1996/rs98e-1.pdf [Chris Vivian, United Kingdom (of Great Britain and Northern Ireland)]	Noted - note that this section covers ocean biological CDR methods, therefore carbon injection methods or storage in geological structures is not covered. Weathering, alkalinisation, DAACS etc is covered in section 5.6.2.2.3 and WGIII Section 12.3 (a reference to this section has been added). Aspects related to solar radiation modification are covered in section 5.6.3. We have now included reference to the GESAMP report.
127855	97	5	98	28	[RISK] The language clearly outlines the drawbacks and potential negative side effects of ocean-based biological CDR methods, especially ocean fertilization and artificial upwelling. Coastal plant community restoration is far more benign and well-aligned with natural ecosystem function, yet it has limited C sequestration potential on a global scale. Place more emphasis on nature-simulating land based (photosynthesis + soil microbial process) biological CDR as a safer and more practical strategy. [Trigg Talley, United States of America]	Noted. The focus of the WGI assessment in on the biogeochemical and biophysical effects of CDR methods. An assessment of the "safety" and "practicality "of these methods requires considerations of co-benefits, trade-offs, costs and other feasibility considerations, and is beyond the scope of the WGI assessment (and is more in the scope of the WGIII assessment).
40857	97	11	97	11	Suggest to add 'Artificial ocean upwelling (AOUpw)' to the glossary. [TSU WGI, France]	Accepted - definition added
83835	97	11	97	21	The description of AOUpw here is quite general. Instead, applying the AOUpw in aquaculture areas in some Asian countries would be a typical case (please see the SROCC 5.5.2.2.3). Artificial upwelling powered by green energy (solar, wind, wave or tidal energy) to seaweeds can moderate the amount of deep water upwelled to the euphotic zone to just meet the demands of nutrients and DIC by the seaweed for photosynthesis, while avoiding the acidification and hypoxia that often occur in natural upwelling systems. In addition, a recent study (Fan et al., 2019, Waters) reports that applying AOUpw in aquaculture could increase the natural kelp yield and the removal of inorganic nutrients from water bodies, that is, this approach in aquaculture could increase carbon fixation and reduce risk of blooms. [Nianzhi Jiao, China]	Rejected - this example is not a carbon dioxide removal method
73273	97	12	97	12	Poor expression. Delete () [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change made
73275	97	15	97	15	Subscript 2 required for CO2 [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change made
58443	97	17	97	17	Oschlies (2010) should be Oschlies et al. (2010b) - there appears to be a double citation to this in the reference list. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - change made
39745	97	20			"from cooling-induced enhancement of the terrestrial carbon sink" what do you mean? I find it unclear [TSU WGI, France]	Not applicable - this sentence has been removed in editing
39769	97	20			"is likely" assessment does not seem traceable [TSU WGI, France]	Accepted - changed to a confidence statement

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
58445	97	21	97	21	Might be worth briefly reinforcing that AOUpw has significant termination effects as noted in Table 5.10, along the lines of "AOUpw is likely to have widespread side effects (Figure 5.36) and if stopped results in significant termination effects (Table 5.10)". [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable - this sentence has been removed in editing
40597	97	23	97	24	Note that there is a definition for 'ocean fertilisation' in the glossary. [TSU WGI, France]	Taken into account - pointer to Glossary added
112129	97	23	97	35	As an example of the inconsistencies in the numbers here: In the text, it says the initial maximum for OIF is 4 PgC/yr, reducing to 1 PgC/yr after the initial decade, but in the table it says 0.32-12, citing Fuss et al. (2018), although Fuss et al. and the companion paper by Minx et al. claim that "accounting for economic and biophysical limits, we identify relevant potentials for all NETs except ocean fertilization...". More care is clearly needed here and similarly throughout. [Mark Lawrence, Germany]	Not applicable - discussion of potentials of CDR methods has been moved to WGIII, Chapter 12
58447	97	26	97	26	There's no Yoon et al. (2018) in the reference list, but I assume that this refers to doi:10.5194/bg-15-5847-2018 (and would indeed be an appropriate citation). [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - change made
58673	97	27			The reference "Smetacek et al., 2012" is missing in the Ref chapter. The full reference is : "Deep carbon export from a Southern Ocean iron-fertilized diatom bloom Victor Smetacek1,2*, Christine Klaas1 *, Volker H. Strass1 , Philipp Assmy1,3, Marina Montresor4 , Boris Cisewski1,5, Nicolas Savoye6,7, Adrian Webb8 , Francesco d'Ovidio9 , Jesu´s M. Arrieta10,11, Ulrich Bathmann1,12, Richard Bellerby13,14, Gry Mine Berg15, Peter Croot16,17, Santiago Gonzalez10, Joachim Henjes1,18, Gerhard J. Herndl10,19, Linn J. Hoffmann16, Harry Leach20, Martin Losch1 , Matthew M. Mills15, Craig Neill13,21, Ilka Peeken1,22, Ru¨diger Ro¨ttgers23, Oliver Sachs1,24, Eberhard Sauter1 , Maike M. Schmidt25, Jill Schwarz1,26, Anja Terbru¨ggen1 & Dieter Wolf-Gladrow 2012 Nature doi:10.1038/nature11229" [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - change made
58449	97	28	97	31	Should be Oschlies et al. (2010a) on both l.28 and l.31 [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - change made
58451	97	29	97	30	These ranges seem a bit broad as far as I can tell - for the initial sequstration rates I make it ~2.3GtC/y from Keller et al. for the first 10 years & ~2.5GtC/y from Oschlies et al. for the first year, and over 100 years I make it ~0.6 GtC/y from Oschlies et al. and ~1GtC/y from Keller et al. If so, then 2-3GtC/y & 0.5-1.0GtC/y seem like more tightly constrained & rounded ranges. However, these numbers also differ from those in Table 5.10 & Figure 5.36 for ocean fertilisation, which instead both use the wider range from the review in Fuss et al (2018) going up to 12GtC/y. This begs the question: if the numbers of Fuss et al. are more appropriate for Table 5.10 then should it not be discussed here in the text as well? I instead suggest using the more constrained figures discussed here as the central estimate in place of Fuss et al. in Table 5.10 & Fig 5.36, with perhaps the 12GtC/y used as an outlier value. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable - discussion of potentials of CDR methods has been moved to WGIII, Chapter 12

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
58453	97	33	97	34	Robinson et al. (2014) report that 66% of carbon sequestered below 1000m is reexposed to the atmosphere, while ~29% of the carbon sequestered below 2000m is reexposed, so either this sentence needs to specify this deeper standard or be modified to say "up to two thirds of the sequestered carbon may be returned...". [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable - this sentence has been removed in editing
10281	97	34	97	34	Insert "currently before "prohibited"as this could potentially be changed in due course. [Chris Vivian, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - sentence changed
10283	97	35	97	35	Add GESAMP (2019) to the Dixon et al. (2014) reference. See above for reference details. [Chris Vivian, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - sentence changed
58459	97	37	97	43	This paragraph repeats the mention of artificial upwelling side effects on l.21, but then mostly cites studies about the side effects of fertilisation. Consider for brevity either moving and merging in the discussion of upwelling side effects (& termination effects) from l.21 here, or focus this paragraph on just fertilisation side effects. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable - this sentence has been removed in editing
10285	97	38	97	38	The Summary for Policymakers on page SPM-41, line 31 refers to "Wide-ranging potential side-effects of CDR methods…". Thus, I suggest inserting "potential" before "negative" here . [Chris Vivian, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable - this sentence has been removed in editing
10287	97	41	97	43	Williamson et al. (2012a) state "...large-scale fertilization would lead to substantive additional CO2 sequestration at depth, hence increasing the rate of acidification of ocean interior waters (Cao and Caldeira, 2010; Oschlies et al., 2010a)". Thus, the current text here is inaccurate as OF would not enhance acidification in surface waters. I suggest revising the text of this sentence to "Additionally, both AOUpw and OF would enhance subsurface ocean acidification, AOUpw would also enhance surface ocean acidification and both methods would perturb marine ecosystems via reorganisation of community structure (high confidence) (Cao and Caldeira, 2010; Oschlies et al., 2010; Williamson et al., 2012a)". New reference - Cao and Caldeira (2010) http://dx.doi.org/10.1007/s10584-010-9799-4. [Chris Vivian, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable - this sentence has been removed in editing
10289	97	43	97	43	Insert "et al." after "Oschlies". [Chris Vivian, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change made
58455	97	43	97	43	Should be Oschlies et al. (2010a). [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - change made
58457	97	43	97	43	Consider moving Oschlies et al. (2010a) to l.39, as it is relevant to all the discussed side-effects. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected - for clarity of text the reference has not been moved

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
90129	97	45	97	47	This statement uploaded by Ted Schuur on behalf of a graduate class that reviewed the chapter (this was communicated with IPCC TSU in advance): Will these coastal ecosystems continue to persist during climate change. Many studies have documented shifts in sea grass beds to cooler water or the deterioration of coral ecosystems. [Edward Schuur, United States of America]	Noted. This point is a valid one, but is already covered in the text: "These rates could be reduced in future, since these habitats are vulnerable to changing conditions, such as temperature, salinity, sediment supply, storm severity and continued coastal development" (lines 5-7, p 98). No change necessary.
67859	97	45	97	50	It is stated that the main cause of degradation of the blue carbon ecosystems is 'aquaculture expansion'. In addition to the main issue mentioned in literatures that stated aquaculture expansion as a cause of damage to mangroves, aquaculture also creates additional emissions from the use of energy that is not environmentally friendly and from feed waste. [Ruandha Agung Sugardiman, Indonesia]	Noted. The issue that aquaculture can be a source of additional emissions is an important one, but is not relevant here (in the context of biological CDR methods). No change necessary.
40593	97	45	97	50	Note that there is a definition for 'blue carbon' in the glossary. [TSU WGI, France]	Taken into account - pointer to Glossary added
69199	97	45	98	16	Similar concepts seems to be referred to in several different terms, such as coastal wetland, coastal ecosystem, coastal land, wetland, etc. Consistency in terminology as far as possible is requested. In particular, the relationship between coastal wetland and wetland is unclear (we can see both at the same time in Table 5.10). [Kaoru Magosaki, Japan]	Noted - not all wetlands are coastal, hence the distinction in the text. Coastal ecosystems refer to a broader range of conditions, e.g. mangroves.
10291	97	47	97	50	However, Chung et al. (2017) (https://doi.org/10.1080/09670262.2017.1359678) states "There is considerable potential for increased drawdown of CO2 by SABs (seaweed aquaculture beds), though its effectiveness in amelioration of atmospheric CO2 increase will depend on the fate of the resulting biomass". [Chris Vivian, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The quote from Chung et al (2017) is valid, and there are other references that could have been cited that make a similar point. However, the key issue is "the fate of the resulting biomass"; whilst long-term removal is theoretically possible, natural long-term sequestration is highly uncertain (and cannot be relied on at climatically-significant scales) and techniques to ensure long-term storage (equivalent to BECCS) have not yet been developed nor shown to be cost-effective.
116473	97		97		Artificial ocen upwelling was not discussed in recent reports, I think (AR5, SR15, SROCC).The statement on "likely widespread side effects" is not linked to any references (missing?). [Valerie Masson-Delmotte, France]	Accepted - upwelling is discussed in SROCC 5.5.1 Text has been revised to point to Table 5.A.2 which includes citations for negative side effects
67861	98	3	98	28	There is a need to add "ecosystem service function", in addition to the blue carbon function of mangrove and seagrass.. On p.98, line 27 their function is only mentioned as 'coastal protection'. Actually there are many other ecosystem functions of wetlands (e.g. mangrove and seagrass), not only blue carbon and coastal protection. [Ruandha Agung Sugardiman, Indonesia]	Noted. The phrase "[providing] biodiversity benefits and many other valuable ecosystem services" is already included in the text (lines 28- 29, p 98). No change necessary

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
10293	98	3	99	3	I suggest some additional text is needed in this paragraph to cover the significant amounts of carbon stored by these systems and the threats to them. I suggest that the following text should be inserted on line 7 after "Bindoff et al. (2019).": "These ecosystems store very significant amounts of carbon (Alongi, 2012); Chmura et al., 2003; Donato et al., 2011; Nelleman et al. 2009; Fourqurean et al., 2012; Pendleton et al. 2012; Siikamaki et al., 2017) although there have been some challenges to the scale of their significance (e.g. Johannessen and Macdonald, 2016; Howard et al., 2017). However, these ecosystems are under threat from anthropogenic conversion and degradation and are being lost at rates between 0.7% and 7% per annum with consequent carbon dioxide emissions (Howard et al. 2017, Hopkinson et al., 2012; Nelleman et al. 2009; Pendleton et al. 2012; McLeod et al. 2011). Protecting these habitats from further destruction and allowing them to re-establish where possible will be important to preserve these carbon sinks." New references: Alongi (2012) https://doi.org/10.4155/cmt.12.20 Chmura et al. (2003) doi:10.1029/2002GB001917 Donato et al. (2011) DOI: 10.1038/NGEO1123 Fourqurean et al. (2012) DOI: 10.1038/NGEO1477 Hopkinson et al. (2012) https://doi.org/10.1016/j.cosust.2012.03.005 Howard et al. (2017) doi:10.1002/fee.1451 Johannessen and Macdonald (2016) doi:10.1088/1748-9326/11/11/113001 Nelleman et al. (2009) https://gridarendal-website-live.s3.amazonaws.com/production/documents/:s_document/83/original/BlueCarbon_screen.pdf?1483646492 Pendleton et al. (2012) doi:10.1371/journal.pone.0043542 Siikamaki et al. (2017) www.pnas.org/cgi/doi/10.1073/pnas.1200519109 [Chris	Accepted. Some of the suggested text and references were added to the text.
10295	98	4	98	4	Nelleman et al. (2009) give the range as 0.12-0.33 Pg C yr-1. see https://gridarendal-website-live.s3.amazonaws.com/production/documents/:s_document/83/original/BlueCarbon_screen.pdf?1483646492 [Chris Vivian, United Kingdom (of Great Britain and Northern Ireland)]	Noted. McLeod et al (2011) is cited instead of Nelleman et al (2009) since it is more recent, more comprehensive (based on many more sources), and published in a peer-reviewed journal. However, the range of values in the two reviews is closely similar. No change necessary.
82099	98	5	98	6	One of the major factors of vulnerability for seagrasses has been left out. This is eutrophication, which leads to phytoplankton blooms that then reduce light penetration and light availability for seagrasses to grow. Eutrophication has led to much of our current seagrass losses. [David Keller, Germany]	Taken into account. "Eutrophication" has been added (after "storm severity"). It was not originally included because eutrophication is not climate-related.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
58461	98	15	98	15	Griscom et al. (2017) give the low cost proportion of coastal restoration as 0%, so I'm assuming the 0.05-0.05 PgC/y is coming from the other reference (I only have access to a free sample of the book, and haven't directly find and check these numbers myself) [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. The reviewer is correct in stating that there is no dark grey shading for "low cost" coastal restoration in Fig 1 of Griscom et al (2017), i.e. for costs <10 USD per tonne CO2e. However, there is light grey shading, for costs <100 USD per tonne CO2e - and it is in that context (to compare with "no cost limits", the theoretical upper maximum) that the term "cost effective" is used in the text on p 98 and Table 5.10.
33373	98	16			Change: "…Sciences and Medicine, 2019)(Griscom et al., 2017)." By "…Sciences and Medicine, 2019) (Griscom et al., 2017).". [Guiomar Rotllant, Spain]	Accepted - change is made.
51181	98	31	98	31	why have geochemical and chemical CDR methods been combined in this section, whereas on page 89, they are clearly put into two separate categories. Suggest making this consistent across the chapter, either by combining in the original definition on p89 or separating out in section 5.6.2.2.3? [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. DACCS has now been separated as an own section.
127857	98	31	99	19	[RISK] Regarding enhanced weathering achieved by spreading finely-ground alkaline rocks or minerals on land, the quantity of mined material required for an effective amount of CO2 removal will either be economically unfeasible or require huge mining operations with severe environmental impacts on biodiverisity, water quality, and rural communities near the mining activities. In addition alkalizing the soil on a broad scale will alter soil microbiomes, and may disrupt microbial ecology or have other adverse "side effects" on natural plant communities, and on lands used for agriculture or forestry. In addition, while the delivery of more alkaline river waters to the ocean could help ameliorate ocean acidification, the impacts of alkalization on stream and river biota and ecosystems is unknown and likely disruptive. Alkalizing the ocean would, at least in theory, reverse a negative effect (acidification) of increasing atmospheric CO2. However, the mineral additions needed to effect alkalization may upset ocean ecological balance and, in any case, may be economically infeasible. The language on page 99 acknowledges that impacts of alkaline mineral applications in the amounts needed to effect substantive CO2 removal (114 Pmol to remove 27 Pg C by the end of the century) are unknown and possibly detrimental. Technological chemical CDR including direct air capture and carbon storage (DAC) seems risky if the CO2 is stored deep underground or under water, as the risks of leakage or failure of containment would exist in perpetuity. However, if captured carbon can be combined into a stable, solid mineral substance that can be either used to fabricate durable products or stored, this can be an important part of the climate solution provided that the process is both financially and ecologically cost-effective (i.e., process is energy-efficient and/or can be powered through solar or wind energy). DAC seems far less efficient than CO2 capture at points of emission, such as the smokestacks of power plants, whether they are burning fossil fuels or biofuels as in BECCS. Land plant based photosynthesis coupled with soil microbial carbon stabilization appears far safer and more practical than any	Noted. The focus of the WGI assessment in on the biogeochemical and biophysical effects of CDR methods. An assessment of the economical feasibility or biodiversity/ecological consequences of these methods requires considerations of co-benefits, trade-offs, costs and other feasibility considerations, and is beyond the scope of the WGI assessment.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
73277	98	32	98	32	Change 'process' to 'processes'. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change was made.
40595	98	32	98	35	Note that there is a definition for 'enhanced weathering' in the glossary. [TSU WGI, France]	Accepted - the definition was modified to better match with those in the glossary.
51179	98	32	98	35	This is a very long sentence, is it possible to split it in two to be clearer, for example by adding a full stop after 'pores'? [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - sentence was spit
58463	98	36	98	36	Taylor et al. (2015) is cited twice in the reference list and so 2015b is unecessary, and furthermore the final published version was in April 2016 and so should be updated to Taylor et al. (2016). [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not relevant - the reference was removed when the definition was modified
51183	98	36	98	38	Is there a reference or more content to back up this high confidence statement on CDR sequestration potential? Other land-based methods have similar, or higher, carbon sequestration potential (Royal Society and Royal Academy of Engineering 2018 report on Greenhouse Gas Removal). Sequestration potential (and duration of sequestration) of CDRs are generally poorly known and studies produce wide estimates (as shown with the wide error bars on Figure 5.36). [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Not relevant - the sequestration potentials are not anymore assessed in WGI, this will be done in WGIII. The sentence was deleted.
115447	98	36	98	39	The statement that EW would have larger potential than other methods or that the side effects could be "beneficial or adverser" is not substantiated by litterature. As all geoengineering techniques, the large scale deployment of EW would have large adverse effect, both on ladn or oceans and also because of the enormous mining requirements [SILVIA RIBEIRO, Mexico]	Not relevant - the sequestration potentials are not anymore assessed in WGI, this will be done in WGIII. The sentence was deleted. We have elaborated both the adverse and beneficial side effects
52341	98	40	98	40	Add reference on CO2 uptake and carbonate minerals in glacial water: Fransson et al. (2015) with Beerling et al. 2018 [Agneta Fransson, Norway]	Rejected - not relevant here given the limited space.
52343	98	43	98	43	add reference Hopwood et al. (2020) for addition of bedrock-derived trace elements to ocean to trigger primary production [Agneta Fransson, Norway]	Rejected - not relevant here given the limited space.
58465	98	43	98	46	Although the citation given links current enhanced weathering with freshwater salinisation, the citations given do not specifically state salinisation as an impact of EW (even though it does seem likely). There's also some overlap between these two sentences, so I suggest simplfying things by merging them together and softening the can to a might, e.g. "As another negative side effect, EW might also contribute to freshwater salinisation as a result of increases salt inputs and cation exchange in watersheds, and so may adversely impact drinking water quality (medium confidence) (Kaushal et al. 2010)". [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - thank you for the suggestion to improve the text.
611	98	46		49	It can be mentioned that the use of waste materials such as concrete demolition or steel slags for EW can reduce the need for mining. See for example Renforth, P. (2019). The negative emission potential of alkaline materials. Nature Communications, 10(1), 1401. doi:10.1038/s41467-019-09475-5 [Vicca Sara, Belgium]	Accepted

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
58467	98	47	98	49	The given citation (Smith et al. 2018b) state that the biodiversity impact of mineral spreading is likely neutral (rather than a mix of beneficial & adverse), with no beneficial impacts mentioned. If combined with re/afforestation (as suggested by Taylor et al. (2015)) then there could be potential for positive biodiversity impacts through enhanced forest growth, but this should be explicitly stated if this is what is meant here. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted
2809	98	51	98	51	Alkaline mineral is a difficult term, as it needs to not be confused with the old igneous rock classification (acid basic, as opposed to mafic felsic). It would be better to say minerals which weather to ions that raise the pH of water (e.g. olivine). Note also olivine does not dissociate to quicklime. [Stephen Wilkinson, United Arab Emirates]	Rejected - alkaline mineral is the term commonly used in the literature
40859	98	51	98	53	Suggest to add 'Ocean alkalinisation' to the glossary. [TSU WGI, France]	Accepted - a definition has been added to the Glossary
10297	98	51	99	8	This is a very limited coverage of ocean-based geochemical/chemical CDR methods compared to that for the biological methods in section 5.6.2.2.2. as mentioned above. This section has not taken into account the GESAMP report published in 2019 that reviewed ocean-based CDR techniques including 7 different approaches to ocean alkalinisation as well as the mineralisation of rocks under the seabed. See also the recent publication by Kirchner et al. (2020) (https://doi.org/10.1021/acs.est.9b07009) reporting on an accelerated weathering of limestone reactor installed at coal-fired power plant in Germany. [Chris Vivian, United Kingdom (of Great Britain and Northern Ireland)]	Noted - Where CDR methods have not been assessed in the framework of ESMs, they have not been included here. However, more detail on weathering and ocean alkalinisation approaches are in WGIII 12.3.2.2 and 12.3.2.3. A reference has been added here, as has the GESAMP reference.
116475	98		98		Could the assessment of SROCC for blue carbon be used as a starting point, summarized, and updated? [Valerie Masson-Delmotte, France]	Noted. Early mention of SROCC is given (line 1, p 98) with two subsequent citations (lines 8 and 29, p 98). The text does summarise that report's main findings in the context of more recent literature. Whilst the text could be further re-structured along the lines suggested by the reviewer, this does not seem necessary.
51185	99	4	99	5	The Royal Society and Royal Academy of Engineering 2018 report on Greenhouse Gas Removal suggests much is unknown about the carbon sequestration potential - e.g. if alkalinity decreased in the oceans this could reverse CO2 uptake, and dependant on the source of the alkaline minerals there could be 50 - 100% reversal of CO2 uptake. Figure 5.36 also shows large error bars on the CDR sequestration potential. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Noted - Figure 5.36 has been altered to remove the potentials.
18247	99	4	99	8	Can references be added in to support these claims? [Chelsey Baker, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - a reference has been added to Table 5.A.2 which details the side-effects and associated references

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
44657	99	6	99	6	Ocean alkalinisation decreases surfaces ocean acidification'. The effect on pH is small compared to the effect on ocean carbon uptake (see Hauck et al 2016, doi:10.1088/1748-9326/11/2/024007). It is a common misconception that alkalinisation also is THE solution to ocean acidification, which is not the message that should be conveyed by the IPCC. [Judith Hauck, Germany]	Taken into account - the sentence has been altered
10299	99	7	99	7	The text "but also releases toxic trace metals from deposited minerals" is not applicable to all minerals that could be used for ocean alkalinisation. Note that on page 5-98 it says "The dissolution of minerals is likely to stimulate biological productivity of croplands (Hartmann et al., 2013; Beerling et al., 42 2018), but can also liberate toxic trace metals (such as Ni, Cr, Cu) into soil or water bodies (Keller et al., 2018a; Strefler et al., 2018)". Hartmann et al. (2013 says "The mobilization of potentially toxic elements contained in some silicate rocks may detrimentally effect primary production and/or accumulate in the food chain, of which could be harmful to human populations". Thus, I suggest replacing "releases" in line 7 with "can release". [Chris Vivian, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable - this sentence has been removed in editing
82101	99	7	99	7	Concerning the release of toxic trace metals, not all minerals for ocean alkalinization contain or will release toxic metals. This is mostly a concern when using a mineral like olivine. If something like quick lime (calcium hydroxide) is added to increase alkalinity there is no problem with toxic metals. [David Keller, Germany]	Not applicable - this sentence has been removed in editing
112131	99	10	99	19	The DACCS text is really weak - given its tremendous long-term potential and the current spinup of (still small) industrial-scale facilities like ClimeWorks, this is really inappropriate. Please see the reviews by Minx et al., Fuss et al., Nemet et al. and Lawrence et al. (all 2018) for condensed insights into what is important to cover on this topic. [Mark Lawrence, Germany]	Noted. The DACCS text was revised
58469	99	10	99	19	Almost no references are given for DACCS, with the only exception being Rau (2014) on alternative ocean storage options. Although DACCS has not been hugely explored, at least a general reference should be provided for some of the background here - Fuss et al. (2018), which Table 5.10's carbon uptake potential for DACCS is based on, would be an appropriate starting point (and references therein), as well as Minx et al. (2018,doi:10.1088/1748-9326/aabf9b) and Keller et al. (2018a). [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. The DACCS text was revised
613	99	10		19	Perhaps include this reference: Realmonte, G., Drouet, L., Gambhir, A., Glynn, J., Hawkes, A., Köberle, A. C., & Tavoni, M. (2019). An inter-model assessment of the role of direct air capture in deep mitigation pathways. Nature Communications, 10(1), 3277. doi:10.1038/s41467-019-10842-5 [Vicca Sara, Belgium]	Noted. The DACCS text was revised
115449	99	16	99	18	CCS is not proven netiher ecologically of economically feasible, so DACSS fall under the same. Furhtermore, the energy demands for DACCS are so high, that it would enhance GHG emissions or have to use other sources of energy, that could be better used as replacement for fossil fuel based energy in other places, [SILVIA RIBEIRO, Mexico]	Noted. The DACCS text was revised

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
32007	99	20			No mention is made in this section of methane mitigation and possible removal. Recent review: Nisbet, E.G., Fisher, R.E., Lowry, D., France, J.L., Allen, G., Bakkaloglu, S., Broderick, T.J., Cain, M., Coleman, M., Fernandez, J., Forster, G., Griffiths, P.T., Iverach, C.P., Kelly, B.F.J., Manning, M.R., Nisbet-Jones, P.B.R., Pyle, J.A., Townsend-Small, A., al-Shalaan, A., Warwick, N., and Zazzeri. G. 2020. Methane mitigation: methods to reduce emissions, on the path to the Paris agreement. Reviews of Geophysics, 58(1), p.e2019RG000675 [Euan G. Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We added a section of methane mitigation.
58471	99	24	99	32	This is a useful figure, but the text is a bit small to read as presented, and the caption should refer to Table 5.10 as the data source as well. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted - the font was increased. The reference to Table 5.10 (or 5.9 in the new version) has been added.
40213	99	24			Figure 5.36: nice figure! [TSU WGI, France]	Noted - thank you
127859	99	38	101	36	[RISK] Regarding Solar Radiation Modification (SRM) through stratosphere aerosol injection, the low confidence in its net benefits for climate stabilization and agricultural production, and unpredictable "side effects" suggest that it merits at best a low priority among climate mitigation strategies. The one "high confidence" conclusion is that SRM would increase ocean acidification, a very serious side effect that must be avoided, and thus might merit removal of SRM from the list of CDR methods under consideration by decisionmakers. [Trigg Talley, United States of America]	Taken into account. The section assesses biogeochemical effect of SRM based on published literature. The assessment of the priority of SRM and CDR is beyond the scope of this section.
111871	99	38	101	36	It seems that you primarily/solely talk about Stratospheric Aerosol Injection and you should clearly say so, because Marine Cloud Brightening is moving higher up on the SRM agenda (due to ongoing experiments in Australia), and should be clear whether (and if so: where) your assessment also covers effects from deploying MCB [Oliver Geden, Germany]	Taken into account. In the revised text, we clearly state what SRM methods are referred to. This section focus on the biogeochemical effect of stratospheric aerosol injection because most existing literature focus on the effect of SAI, There are only a few studies looked the biogeochemical effect of MCB and CCT, which are also assessed in this section.
40735	99	38			section 5.6.3: introduction lacking [TSU WGI, France]	Accepted - introduction to SRM added.
71713	99	40	99	42	This section has very little on the effects that SRM can have on atmospheric chemistry and what we have learned about that from the Pinatubo eruption. While the relevance of Pinatubo has been covered when considering biophysical effects on vegetation, e.g. Kalidindi et al, 2015, its effect on atmospheric chemistry is not covered. Examples of relevant papers for this are: Dlugokencky, et al, (1996, Changes in CH4 and CO growth rates after the eruption of Mt Pinatubo and their link with changes in tropical tropospheric UV flux. Geophys. Res. Letts, 23, 2761-2764) and results from the GeoMIP project given by Pitari, et al (2014, Stratospheric ozone response to sulfate geoengineering: Results from the Geoengineering Model Intercomparison Project (GeoMIP). J. Geophys. Res, 119, 2629-2653). [Martin Manning, New Zealand]	Taken into account. Effect of SRM on stratospheric chemistry is mainly assessed in Chapter 4, section 4.6.3. Here we briefly assess effect of SRM on surface ozone. Text is revised. Pitari et al. (2014) is now cited. In the revised text, we also include discussion on the marine cloud brightening effect on atmospheric chemistry based on a recent study.
8853	99	47	99	47	Sounds like it is going to happen. Replace "is expected to" with would. [Vaishali Naik, United States of America]	Taken into account - "is expected to" with "would be expected to".

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
68285	99	52	100	7	It would be important to note that SRM proposals that would use sulfate emissions to cool the planet risk destroying stratospheric ozone, which would cause significant biological harms. WMO, et al. (2019) Scientific Assessment of Ozone Depletion: 2018, Global Ozone Research and Monitoring Project-Report No. 58, 6.16 ("Column ozone changes as the result of stratospheric aerosol geoengineering therefore depends on the injection amount, timing (ODS loading), and injection strategy (influencing aerosol size and location; Appendix 6A). Relatively small and constant injections of 2.5–4 Tg S yr−1 between 2020 and 2070, which would result in 0.5°C of surface cooling, are calculated to lead to an approximately 4% reduction in the global stratospheric column ozone for 2020 and only 1% reduction by 2070 (Pitatry et al., 2014; Xia et al., 2017). Much larger injection amounts that would lead to a surface temperature cooling of around 2°C in 2040–2050, based on a single model study, would result in reductions in column ozone of 28–40% in October over Southern Hemisphere (SH) high latitudes and 8–18% for NH high latitudes in March, with varying values depending on the injection altitude (Tilmes et al., 2018). Injections closer to the tropopause cause a stronger dynamical response and could result in up to an 8% increase in column ozone in NH winter mid- and high latitudes. A single modeling transient simulation based on RCP8.5 greenhouse gas forcings with continuously increasing SO2 injections between 2020 and 2099 and decreasing ODSs would result in approximately constant change in column ozone in high polar latitudes (20–23% in October over the SH and 10–12% in March over the NH polar latitudes) and slightly larger (3–5%) column ozone values compared to non-geoengineering conditions for tropics and winter northern mid-latitudes by the end of the 21st century (Richter et al., 2018)."). [Durwood Zaelke, United States of America]	Taken into account. The effect of SAI on stratospheric ozone is assessed in Chapter 4, section 4.6.3. In the revised text, here we also briefly discuss the effect of SAI on stratospheric and surface ozone.
127861	99	52	100	23	The effects on direct vs diffuse radiation being discussed here are specific to SAI -- not to all SRM mechanisms (e.g., CCT will not increase diffuse radiation). Edit to reflect this. It's less clear if page 100, lines 9-23, are specific to SAI or to both SAI and MCB. Again, CCT is also included here as SRM so clarity as to which mechanisms authors are referring to is important. [Trigg Talley, United States of America]	Accepted - text clarified to make clear which SRM methods are referred to.
1851	99	52			Change "acts" to "would act" Using "acts" implies that it is possible or has been observed. The technology does not exist and it has never been done. Make sure that you make clear that this is a theoretical model result. And make the change for the other verbs in the paragraph and section, too. [Alan Robock, United States of America]	Accepted - text changed throughout to avoid giving the impression that SRM exists as a viable technology.
1853	99	55			Change "photosynthetic" to "photosynthetically" [Alan Robock, United States of America]	Accepted - correction made.
1855	100	5	100	7	This is incorrect. Changing the altitude of stratospheric aerosols changes their lifetime, but does not change the ratio of direct to diffuse. And the reference is wrong. You have the first and last names of the author mixed up. And the paper has now been accepted. The correct reference is: Krishnamohan, K.-P. S.-P., Bala, G., Cao, L., Duan, L., and Caldeira, K.: Climate system response to stratospheric sulfate aerosols: sensitivity to altitude of aerosol layer, Earth Syst. Dynam., 10, 885–900, https://doi.org/10.5194/esd-10-885-2019, 2019. [Alan Robock, United States of America]	Taken into account. In the revised text, we updated the reference.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
18923	100	5	100	7	This could also depend on the hygroscopic growth of aerosols as shown by a recent study: hygroscopic growth allows the aerosols to become bigger and hence more diffuse radiation. Reference: Krishnamohan, K. S., Bala, G., Cao, L., Duan, L., & Caldeira, K. (2020). The climatic effects of hygroscopic growth of sulfate aerosols in the stratosphere. Earth's Future, 8, e2019EF001326. https://doi.org/10.1029/2019EF001326 [Govindasamy Bala, India]	Taken into account. In the revised text, we updated this reference.
58473	100	7	100	7	Citation in reference list should be upgraded from preprint discussion paper to final published version: https://doi.org/10.5194/esd-10-885-2019 [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. In the revised text, we updated the reference.
58475	100	9	100	10	A sentence briefly describing the global trends in Fig5.37 would be useful here. Although it's implictly discussed in the subsequent sentences on low-latitute trends, it's be useful to first explicitly state that across the models GPP and Ra tend to be lower with SRM, while NPP can be higher or lower depending on the model (and then the following sentences explain why the models differ). [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. In the revised text, we removed this figure, which is from a paper that is more than five years old (Glienke et al. 2015)
96649	100	9	100	23	Text is not completely wrote out in full, seems to consist partly of keywords and is full of typos. Please revise. [Nicole Wilke, Germany]	Accepted - typos corrected and text clarified.
1857	100	9			Correct the spelling of "biogeochemical" [Alan Robock, United States of America]	Accepted - change was made.
8855	100	11	100	11	replace will with would [Vaishali Naik, United States of America]	Accepted - change made.
58477	100	12	100	12	Dagon & Schrag (2019) shows NPP decline to be concentrated in the mid-latitude regions rather than high-latitudes, so either this statement needs expanding to include both high- and mid-latitudes or this citation should move (possibly to three sentences later, as they also discuss the effect of nitrogen cycle inclusion on their model response). [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - reference moved.
98353	100	18	100	18	After the reference to Dagon and Schrag, 2019, the following could be inserted: SRM may lead to a reduction in precipitation (Trenberth & Dai, 2007) which could impact fire danger (Burton et al, 2018) Trenberth, K. E., & Dai, A. (2007). Effects of Mount Pinatubo volcanic eruption on the hydrological cycle as an analog of geoengineering. Geophysical Research Letters, 34, L15702. Burton, C., Betts, R. A., Jones, C. D., & Williams, K. (2018). Will fire danger be reduced by using Solar Radiation Management to limit global warming to 1.5 °C compared to 2.0 °C? Geophysical Research Letters, 45, 3644– 3652. https://doi.org/10.1002/2018GL077848 [Chantelle Burton, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The analog of volcanic eruption for stratospheric aerosol injection is discussed in section 4.6.3, chapter 4. The discussion of Burton et al. (2018) is now added.
23713	100	18	100	18	causes… [Massimo Lupascu, Singapore]	Accepted - change was made.
41713	100	18	100	18	"cause" [Katharina Meurer, Sweden]	Accepted - change was made.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
98351	100	18	100	20	This sentence contains spelling mistakes (cauese->cause; troposhere->troposphere; implication-> implications; modeling-> modelling) : In addition, SRM would cauese changes in troposhere and surface ozone concentrations with the response strongly sensitive to SRM approaches, which has important implication for vegetation response (Xia et al., 2017). Due to the highly idealised nature of these modeling studies [Chantelle Burton, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - corrections made.
41987	100	18			Chapter 5 page 100 Line 18. Spelling of 'cauese' and troposphere. [Stephen Salter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted -Text is revised
1859	100	18			Correct the spelling of "cause" [Alan Robock, United States of America]	Accepted - change was made.
1861	100	18			Correct the spelling of "troposphere" [Alan Robock, United States of America]	Accepted - change was made.
1863	100	19			Change "implication" to "implications" [Alan Robock, United States of America]	Accepted - change was made.
73279	100	28	100	28	Change 'colored' to 'coloured' [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - figure caption rewritten.
58479	100	28	100	32	In the final figure caption, it would be useful to explicitly define Ra as respiration and state that NPP is the difference between GPP and Ra. Clear experiment labels (e.g. 4xCO2=Warming with no SRM, G1=Warming with SRM, picontrol=No warming) on the final figure itself would also be useful. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account - more descriptive figure caption now included.
8857	100	37	100	37	replace increases with would increase [Vaishali Naik, United States of America]	Taken into account, text is revised.
58481	100	37	100	44	Given that the penultimate sentence contradicts the results reported in the first sentence, I'd consider inserting "could" into the first sentence (e.g. "...aerosol injection could increase crop yields...") and moving the penultimate sentence up to follow the first sentence and insert something like "However, based on the response of crop yields to the eruption of Mount Pinatubo...". [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. Text is revised.
127863	100	37	100	44	More care needs to be taken in referring to SRM (which covers multiple mechanisms, including CCT which acts on long-wave radiation) and when referring to results that apply to specific SRM mechanisms (e.g., SAI). [Trigg Talley, United States of America]	Taken into account - text has been clarified to make clear that it refers to SAI.
41717	100	41	100	41	stratospheric [Katharina Meurer, Sweden]	Accepted - change was made.
1865	100	41			Change "the eruption" to "the 1991 eruption" Pinatubo has erupted many times. Specify which eruption you mean. [Alan Robock, United States of America]	Accepted - change made.
1867	100	41			Correct the spelling of "stratospheric" [Alan Robock, United States of America]	Accepted - change was made.
38527	100	48	100	50	add reference to Lauvset et al (2017) Lauvset, S. K., J. Tjiputra, and H. Muri (2017), Climate engineering and the ocean: effects on biogeochemistry and primary production, Biogeosciences, 14(24), 5675-5691, doi:10.5194/bg-14-5675 - 2017. [Siv K Lauvset, Norway]	Taken into account. Text is revised and the suggested reference is added.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
73285	100	53	100	53	Capital 'S' for 'system' [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable - there is no word "system" on the line indicated. In any case, "system" is not with a Capital throughout in the chapter, as per previous published IPCC reports (see the 1.5 degree Special Report).
90131	100		100		This statement uploaded by Ted Schuur on behalf of a graduate class that reviewed the chapter (this was communicated with IPCC TSU in advance): Further work needs to conducted to determine aerosol injection effects on wildlife health. [Edward Schuur, United States of America]	Noted. We assess SRM effect based on published literature. This section is on biogeochemical effect of SRM. We are not aware of any published study of aerosol injection effects on wildlife health and this topic is beyond the scope of this section.
1869	101	3	101	9	You need to include discussion of the effects of diffuse radiation here, too. You can't have SRM without impacts of diffuse radiation, and these affect the NPP, too. [Alan Robock, United States of America]	Taken into account. The effect of diffuse radiation has been discussed in the subsection of 5.6.3.1.
73281	101	8	101	8	The hyphen looks like a negative sign. Please improve clarity [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The hyphen is removed.
81529	101	8	101	8	The phrase "due to CO2-induced ocean acidification" could be clarified. I guess the point is that "elevated CO2" is due to unchecked CO2 emissions, and not a result of SRM directly. That is, SRM has been analyzed in scenarios where CO2 concentration keeps rising. It could be mentioned more explicitly that SRM doesn't address the ocean acidification impacts of elevated CO2 concentration (the next para does consider the impacts of SRM on the carbon cycle, though). One paper looking at scenarios that limit both temperature increase and ocean acidification and allowing for SRM is Ekholm, T. & Korhonen, H., Climate change mitigation strategy under an uncertain Solar Radiation Management possibility, Climatic Change 139 (3-4), 503-515, 2016. dx.doi.org/10.1007/s10584-016-1828-5 [Tommi Ekholm, Finland]	Accepted - we now clarify that SRM doesn't lead to ocean acidification itself.
115421	101	8		15	Ln 8-9 says that the consequences of SRM for the ocean sink are poorly known, but ln 15 says that SRM will enhance the ocean sink. These should be made consistent. [Gillett Nathan, Canada]	Taken into account. Text is revised.
8859	101	12	101	12	It would be helpful to have a confidence statement on the impact of SRM on atmospheric CO2 burden [Vaishali Naik, United States of America]	Taken into account. Confidence level is added.
40731	101	12			section 5.6.3.3: assessment lacking [TSU WGI, France]	Taken into account. Text is revised.
8861	101	14	101	14	replace reduces with would reduce [Vaishali Naik, United States of America]	Accepted - change made.
78549	101	14	101	14	here you say SRM will enhance land sinks, but in section 5.6.3.1. you expressed “low confidence” in the impact on land. Check for consistency. [Chris Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - inconsistencies corrected.
96651	101	14	101	23	Is it possible to provide confidence levels for the statements provided? [Nicole Wilke, Germany]	Taken into account. Confidence level is added.
105961	101	14	101	23	It may be valuable to compare the Tjiputra et al. Sink strength with that reported by Yang et al. (in revision) from the GLENS simulation, which used SAI to mitigate the full/total temperature increase after 2020 under the RCP8.5 concentration-forced simulation. The GLENS simulations used CLM4.5, which included N limitation. [Forrest Hoffman, United States of America]	Taken into account. The suggested reference is added.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
1871	101	15			At end of sentence include reference to Xia et al. (2016). [Alan Robock, United States of America]	Taken into account. References are added to support this statement, including Xia et al. (2016)
58483	101	16	101	16	Can refer Figure 5.37 (and/or its source citation, currently Glienke et al., 2015) here as a demonstration. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. Several references were included to support this statement.
96653	101	17	101	19	Sonntag et al 2017 (10.1002/2017EF000620) also find about 100 PgC decrease in atmospheric CO2 in SRM that brings RCP8.5 down to RCP4.5 radiative forcing. [Nicole Wilke, Germany]	The reference of Sonntag et al. (2017) is added.
89433	101	18			Keith et al. 2017 is a commentary piece and it is not clear, if it was peer reviewed. More importantly, the idea that SRM reduces airborn CO2 is not factoring in the need for CDR to be able to terminate SRM. With declining CO2 concentrations, also the natural carbon sinks would eventually be turned into sources again (probably with the exception of the deep ocean carbon sink), which would strongly reduce the effect. This has not been sufficiently analysed in Keith 2017. Furthermore, the attributed 'sinks' also include avoided impacts on the carbon cycle in extreme warming scenarios (e.g. Permafrost release). The study thus does not provide sufficient evidence for such a generalisable statement. [Carl-Friedrich Schleussner, Germany]	Taken into account. Text is revised to include the effect of SRM on permafrost and associated carbon feedbacks.
58485	101	20	101	21	It would be useful context to provide the CO2 reduction estimate itself to allow direct comparison to the previous RCP8p5 estimate (Tjiputra et al. (2016) report -15ppm, so approx. 32 PgC). [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. In the revised text, we do not compare the numbers of SRM-induced CO2 reduction because different modelling studies use different GHG and SRM scenario, which makes direct comparison of numbers less useful.
18249	101	22	101	23	Last sentence of the paragraph does not flow well/ make sense. Please amend. [Chelsey Baker, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We removed this statement in the revised text.
40733	101	26			section 5.6.3.4: assessment lacking [TSU WGI, France]	Taken into account. Assessment is included in the revised text.
112489	101	31	101	31	Reference suggested: Llanillo et al. 2010 doi: 10.3390/atmos1010062 [Pedro LLanillo del Rio, Germany]	Taken into account, but most references included are post-AR5.
58487	101	32	101	33	Given that NPP response to SRM is pretty uncertain (as discussed p.100 l.48-51) the reader at this point doesn't necessarily know which way this perturbation might actually be, so it may be worth preceding this statement with something like "In one model where SRM enhances ocean NPP a sudden termination of SRM causes a rapid return of ocean NPP to high-CO2 non-SRM values". [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. In the revised text, we removed this statement because only one model analysed ocean NPP response to SRM.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
81525	101	33	101	35	The phrase "However, a scenario with gradual phase-out of SRM under emission reduction […]" provides a very lopsided view of the situation. If a substantial amount of SRM would be phased-out (even gradually), the society might not be able to scale up emission reductions rapidly enough to compensate for the SRM phase-out. I suggest extending the sentence e.g. as follows: "[...]; although this would be limited by how rapidly emission reductions can be scaled-up (Ekholm and Korhonen, 2016)." The cited paper is Ekholm, T. & Korhonen, H., Climate change mitigation strategy under an uncertain Solar Radiation Management possibility, Climatic Change 139 (3-4), 503-515, 2016. dx.doi.org/10.1007/s10584-016-1828-5 [Tommi Ekholm, Finland]	Taken into account - additional text and reference added.
1873	101	33	101	36	Your job as authors is to assess the new work, and not just report it.I think you have to say that even though it would not be rational to terminate SRM instantly, it is not hard to imagine circumstances when it could occur. Please do not give the impression that this danger does not exist. As I wrote in Robock (2018), "What if fleets of airplanes were flying up into the stratosphere every day spraying sulfur to create an artificial sulfuric acid cloud to reflect sunlight to cool Earth and counteract global warming, and then they had to stop? Imagine a severe drought in China or flooding in Bangladesh, and they said, 'You [expletive deleted] geoengineers are causing this and you have to stop now,' even if they could not attribute those climate extremes to the aerosol cloud. Or what if a global financial crisis or war or pandemic eliminated the budgets for geoengineering? Or hackers or terrorists destroyed the equipment? Stopping cold turkey would not be wise, but it is not hard to imagine scenarios where it could happen." Robock, Alan, 2018: Geoengineering could have animals running for their lives, Nature Ecology & Evolution, January 22, 2018. https://natureecoevocommunity.nature.com/users/77232-alan-robock/posts/29475-geoengineering-could-have-animals-running-for-their-lives [Alan Robock, United States of America]	Taken into account. We write "A hypothetical, sudden and sustained termination of SRM …".
115451	101	33	101	36	Delete all these lines. There are no "simple policies" that could be applied to the governance of SRM in any case. Although the Parker -Irvine article referes to "simple policies", all other authors on governance of SRM coincide that it is an extremely difficult issue, full of conflict of interests, etc. To suggest that termination shock could be "managed" by simple policies is completely wrong. The management of policies related to cliamte change, without including the many new challenges and variables of SRM and geongineerring has proven extremely difficult at UNFCCC and other UN fora. Please of respect to the seriousness of the challenges, delete the reference to Parker-Irvine article. Futhermore, there are not many doubts related to the termination effects, as line 29 says. Most articles referred to the issue show it would be very negative, with rebound effects and strong negative impacts on biodiversity (Trisos et al, 2018). [SILVIA RIBEIRO, Mexico]	Taken into account. In the revised text, we delete the statement 'Climate policy could also prevent sudden SRM termination and a termination shock might be much less likely'

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
115453	101	33	101	36	Delete these lines,a s they are not enough substantied by independent scientists. There are very few studies, all speculative and from scientists biased towards SRM on the use of "moderate" SRM, as proposed by Irvine. And the proposal / conlusions in the cited article ignore many other aspects that will still remain, such as ozone depletion, acid rain, ecosystem impacts, agricultural impacts, aesthetics, ethics, and other unknowns. To have a better picture, please take into consideration the article Robock, Alan, 2020: Benefits and risks of stratospheric solar radiation management for climate intervention (geoengineering). The Bridge, 50, 59-67. http://climate.envsci.rutgers.edu/pdf/RobockBridge.pdf. Specially see table 2 [SILVIA RIBEIRO, Mexico]	Taken into account. In the revised text, we write "However, a scenario with gradual phase-out of SRM under emission reduction could reduce the large negative effect of sudden SRM termination'. Compared to the sudden SRM termination, a gradual phase-out of SRM together with emission reduction would have less negative effect.
58489	101	35	101	36	While I accept that policy measures and backups would likely accompany SRM and potentially reduce the risk of termination shock, I think "prevent" is too strong a word here as it seemingly implies a guarantee (which given generally weak policy responses on climate so far and evidence from the last few years of the fragility of international multilateralism I would suggest cannot be considered guaranteed) - I would instead suggest "Climate policy could reduce the risk of sudden SRM termination...". [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. In the revised text, we delete the statement 'Climate policy could also prevent sudden SRM termination and a termination shock might be much less likely'
8863	101	35	101	36	I don't see the usefulness of this statement. Climate policy can reduce CO2 emissions thereby eliminating the need for SRM in the first place. Suggest deleting. [Vaishali Naik, United States of America]	Taken into account. In the revised text, we delete the statement 'Climate policy could also prevent sudden SRM termination and a termination shock might be much less likely'
13459	101	39	101	39	Another approach that is important to develop more broadly is the role of biogeophysical processes in increasing the amount of CO2 and other gaseous cycles. [Maria Amparo Martinez Arroyo, Mexico]	Not applicable - Section 5.7 has been completely re-drafted from a discursive knowledge gaps to bullet points that could strengthen the existing assessment
71749	101	39	104	10	In this 'Knowledge gap' section, please mention the BVOC-SOA-CCN feedback loop as major source of uncertainty in net climate impact of land use sector and forests. [Tuomo Kalliokoski, Finland]	Not applicable - Section 5.7 has been completely re-drafted from a discursive knowledge gaps to bullet points that could strengthen the existing assessment
58515	101	39	104	10	There needs to be better integration of the palaeo content in Section 5.7 (Knowledge gaps). As it stands, palaeo is briefly mentioned, only once (lines 21-22). Knowledge gaps in palaeo do exist, and I would think some indication these gaps is essential. This could be plaed either in a paragraph in Section 5.7, or in a paragraph at the end of Section 5.1. This would be a really useful indicator of future priorities for the palaeo research community. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable - Section 5.7 has been completely re-drafted from a discursive knowledge gaps to bullet points that could strengthen the existing assessment
114857	101	39	104	10	A comprehensive list of knowledge gaps. [Roxana Bojariu, Romania]	Not applicable - Section 5.7 has been completely re-drafted from a discursive knowledge gaps to bullet points that could strengthen the existing assessment
21871	101	39			This section is much longer and more involved than the equivalent sections I have read in other chapters thus far. Is the level of detail in here proportionate or does it need to be dialled back for consistency with how this issue is handled in the remainder of the report? [Peter Thorne, Ireland]	Not applicable - Section 5.7 has been completely re-drafted from a discursive knowledge gaps to bullet points that could strengthen the existing assessment

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
20017	101	41	101	45	To the reader, this short paragraph seems undoubtedly aimed at preparing and issuing the statement contained in its last sentence. Is there anything specific in this chapter which makes necessary to take such precautions? In any case, the authors are certainly aware that this statement might act like a boomerang and create suspicion [philippe waldteufel, France]	Not applicable - Section 5.7 has been completely re-drafted from a discursive knowledge gaps to bullet points that could strengthen the existing assessment
37757	101	49	102	1	In my view, wildfire or biomass burning is a tremendous source of uncertainty in terms of carbon budget magnitude and its trend. Therefore, it can be the third key aspect. [Akihiko Ito, Japan]	Not applicable - Section 5.7 has been completely re-drafted from a discursive knowledge gaps to bullet points that could strengthen the existing assessment
58491	101	51	101	53	This sentence seems fragmented, with the listed elements before the semi-colon not leading anywhere. I suggest editing to read something like: "Improving model resolution and including representations of land management, such as forestry, grazing and cropland management, along with remote sensing observations can provide additional constraints.", and move "which covers three quarters of the ice-free land surface" to the previous sentence ("Firstly, further constraining the flux from land use, land use change and forestry, which covers three quarters of the ice-free land surface"). [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable - Section 5.7 has been completely re-drafted from a discursive knowledge gaps to bullet points that could strengthen the existing assessment
58321	101	54	101	54	The hyphen in the parenthesis separating the ecosystems should have a space on either side: (land - freshwater bodies - rivers - coastal zones - open ocean). The way you wrote it, the words do not make sense and you rather make it seem, as if you create new strange words like "land-freshwater" and "bodies-rivers-coastal" and "zones-open". [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable - Section 5.7 has been completely re-drafted from a discursive knowledge gaps to bullet points that could strengthen the existing assessment

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
16031	101				5.7 Knowledge gaps Finally, broadly speaking chapter 5 under-estimates bio-sourced or nature-based climate solutions in soils, and, from pyrogenic carbon capture and sequestration in materials. Indigo Agriculture, a Boston-based company founded in 2015 to help farmers improve profitability while also contributing to the welfare of the planet. It announced Chris Prevatt as 2020 winner of the Carbon Cup competition, which is designed to recognize farmers and ranchers cultivating the nation's healthiest soils. According to Prevatt, his 600 acres farm offsets the carbon impact of over 850 flights from JFK International Airport in New York to Los Angeles International Airport in California. One farm. While those numbers need to be double checked, Gabe Brown in North Dakota announces similar results in his book, Dirt to Soil, multiplying by up to 20 times the carbon in his 3000 acres land since he started regenerative agriculture 40 years ago — Brown is a well known, a star speaker on no-till and cover crops in the mid-west USA. He declares 5 universal principles to feed microbial life in soils. See North American Carbon World and Climate Action Reserve webinar: https://www.youtube.com/watch?v=90EjCd51s_4 Also on grazing : Teague, W. R., Apfelbaum, S., Lal, R., Kreuter, U. P., Rowntree, J., Davies, C. A., R. Conser, M. Rasmussen, J. Hatfield, T. Wang, F. Wang, Byck, P. (2016). The role of ruminants in reducing agriculture's carbon footprint in North America. Journal of Soil and Water Conservation, 71(2), 156-164. doi:10.2489/jswc.71.2.156 http://www.jswconline.org/content/ 71/2/156.full.pdf+html Machmuller, M. B., Kramer, M. G., Cyle, T. K., Hill, N., Hancock, D., & Thompson, A. (2015). Emerging land use practices rapidly increase soil organic matter. Nature Communications, 6, 6995. doi:10.1038/ncomms7995 https://www.nature.com/articles/ncomms7995 Stanley, P. L., Rowntree, J. E., Beede, D. K., DeLonge, M. S., & Hamm, M. W.	Noted. We now explicitly refer to nature-based solutions as a subset of land-based CDR options. A comprehensive assessment of the potentials, co-benefits and trade-offs of land nature-based solutions is beyond the scope of this chapter and can be found in the WGII and WGIII reports.
4247	102	1	102	55	See previous remark; also here N2O should be mentioned. [Jacobus (Ko) van Huissteden, Netherlands]	Not applicable - Section 5.7 has been completely re-drafted from a discursive knowledge gaps to bullet points that could strengthen the existing assessment
39739	102	3		4	"data gaps …knowledge" it's not explicit that you're talking about this ocean here [TSU WGI, France]	Not applicable - Section 5.7 has been completely re-drafted from a discursive knowledge gaps to bullet points that could strengthen the existing assessment
78551	102	13	102	18	I would suggest knowledge of response of wetland CH4 to increasing CO2 is also a key uncertainty [Chris Jones, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable - Section 5.7 has been completely re-drafted from a discursive knowledge gaps to bullet points that could strengthen the existing assessment

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
32009	102	13			Good understanding of the methane trends? NO!!! - What planet is that? This extraordinary statement is just plain wrong. To quote Schaefer (2020- ref below) "The methane system is underconstrained by observations, yet even analyses of the same parameter and studies with similar modelling design find differing explanations for the renewed rise." Methane looked like it was equilibrating until 2007 and we really don't know why it started rising again, and even less do we understand why the rise accelerated in 2014. Schaefer, Hinrich. "On the Causes and Consequences of Recent Trends in Atmospheric Methane." Current Climate Change Reports 5.4 (2019): 259-274. Also: Nisbet, E.G., Manning, M.R., Dlugokencky, E.J., Fisher, R.E., Lowry, D., Michel, S.E., Myhre, C.L., Platt, S.M., Allen, G., Bousquet, P. and Brownlow, R., 2019. Very strong atmospheric methane growth in the 4 years 2014–2017: Implications for the Paris Agreement. Global Biogeochemical Cycles, 33(3), pp.318-342. Also: Turner, Alexander J., Christian Frankenberg, and Eric A. Kort. "Interpreting contemporary trends in atmospheric methane." Proceedings of the National Academy of Sciences 116, no. 8 (2019): 2805-2813. [Euan G. Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable - Section 5.7 has been completely re-drafted from a discursive knowledge gaps to bullet points that could strengthen the existing assessment
58493	102	17	102	18	Permafrost doesn't have "stocks" of methane (the exception being some relict metastable methane hydrate deposits beneath permafrost, but this is not widespread) - permafrost has stocks of organic carbon which then produce CO2 and methane when thawed. Consider editing and expanding this sentence to discuss uncertainty on permafrost carbon stocks and likely release rates on thawing (e.g. "This may also apply to permafrost, for which carbon stocks and projected thaw and greenhouse gas release rates are also subject to considerable uncertainty.") [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable - Section 5.7 has been completely re-drafted from a discursive knowledge gaps to bullet points that could strengthen the existing assessment
20019	102	29	102	29	This presentation where acidification and de-oxygenation appear so to speak as twins is a bit misleading, inasmuch as these (important) phenomena deeply differ; their differences are particularly significant in the context of IPCC's work. As expressed by Doney, acidification is "the other problem", in the sense that, while resulting from increase in atmospheric CO2 concentrations, it has little to do with climate change. On the contrary, deoxygenation has two main causes (diminishing solubility and stratification), both resulting from warming. [philippe waldteufel, France]	Not applicable - Section 5.7 has been completely re-drafted from a discursive knowledge gaps to bullet points that could strengthen the existing assessment
52161	102	29	102	29	In this section I am missing a discussion aboput he effects of acidification on N2O production/emissions, see Rees, A. P., Brown, I. J., Jayakumar, A., and Ward, B. B.: The inhibition of N2O production by ocean acidification in cold temperate and polar waters, Deep-Sea Research Part II-Topical Studies in Oceanography, 127, 93-101, 2016.and Breider, F., Yoshikawa, C., Makabe, A., Toyoda, S., Wakita, M., Matsui, Y., Kawagucci, S., Fujiki, T., Harada, N., and Yoshida, N.: Response of N2O production rate to ocean acidification in the western North Pacific, Nature Climate Change, 9, 954-958, 2019. [Hermann Bange, Germany]	Not applicable - Section 5.7 has been completely re-drafted from a discursive knowledge gaps to bullet points that could strengthen the existing assessment

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
36393	102	31	102	40	This paragraph should include a mention of the importance of conducting studies to show the interactions between changing ocean carbonate chemistry and biological responses that will affect the biological soft tissue ahd hard tissue pumps in the ocean interior. [Adrienne Sutton, United States of America]	Not applicable - Section 5.7 has been completely re-drafted from a discursive knowledge gaps to bullet points that could strengthen the existing assessment
30579	102	31	102	40	This paragraph should include a mention of the importance of conducting studies to soow the interactions between changing ocean carbonate chemistry and biological responses that will affect the biological soft tissue ahd hard tissue pumps in the ocean interior. [nina bednarsek, United States of America]	Not applicable - Section 5.7 has been completely re-drafted from a discursive knowledge gaps to bullet points that could strengthen the existing assessment
26931	102	34	102	34	We suggest tro replace "coastal regions" with ", polar ocean (Arctic and Antarctic) and coastal areas" [Eric Brun, France]	Not applicable - Section 5.7 has been completely re-drafted from a discursive knowledge gaps to bullet points that could strengthen the existing assessment
58495	102	36	102	37	I'd also add that further research can help us better understand the direct ecological impacts of ocean acidification and the heterogeneity of this impact, which at the moment remains quite uncertain. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable - Section 5.7 has been completely re-drafted from a discursive knowledge gaps to bullet points that could strengthen the existing assessment
26933	102	45	102	51	The sentence is very long. We suggest to split it and to add in this paragraph: The resurgence interannual variability could bring up additional CO2 to the atmosphere which is necessary to understand ocean emissions of CO2 produced in the OMZ areas. [Eric Brun, France]	Not applicable - Section 5.7 has been completely re-drafted from a discursive knowledge gaps to bullet points that could strengthen the existing assessment
26935	102	45	102	51	We suggest to capture the following idea :Understanding specific and strong remineralization occurring in the OMZ will help to perform the model [Eric Brun, France]	Not applicable - Section 5.7 has been completely re-drafted from a discursive knowledge gaps to bullet points that could strengthen the existing assessment
18055	102	49	102	51	Somewhere the new work by Saito et al. on N20 consumption in OMZs should be cited [Lisa Levin, United States of America]	Not applicable - Section 5.7 has been completely re-drafted from a discursive knowledge gaps to bullet points that could strengthen the existing assessment
26937	102	51	102	51	We suggest to mention that that there are presently few monitoring system allowing to determine acidification rate in key areas for acidification, upwelling-OMZ, polar ocean and coastal region, autonomous buoys and automatic last generation sensor (CO2, pH, Alkalinity and CO3) would help fill this critical data gap. [Eric Brun, France]	Not applicable - Section 5.7 has been completely re-drafted from a discursive knowledge gaps to bullet points that could strengthen the existing assessment
73283	103	1	103	1	Delete hyphen. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable - Section 5.7 has been completely re-drafted from a discursive knowledge gaps to bullet points that could strengthen the existing assessment
109665	103	8	103	11	I would also add soil fauna dynamics. [Carolyn-Monika Görres, Germany]	Not applicable - Section 5.7 has been completely re-drafted from a discursive knowledge gaps to bullet points that could strengthen the existing assessment
37759	103	8	103	11	I completely agree with the importance of mineral stabilisation for soil carbon. However, in the text body, this point was explained only once (page 57 line 12). More references to recent studies are strongly recommended. [Akihiko Ito, Japan]	Not applicable - Section 5.7 has been completely re-drafted from a discursive knowledge gaps to bullet points that could strengthen the existing assessment

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
44659	103	14	103	19	Yes, ocean carbon sink is better understood than the land sink, but observation-based data-products and inverse models suggest that the models underestimate the ocean carbon sink variability (e.g. DeVries et al., 2019, already in the reference list); and analysis of the seasonal cycle (Kessler and Tjiputra, 2016; Mongwe et al 2016, 2018, already in the ref list) suggest that the models get the processes for the ocean carbon sink variability wrong. They are not robust to project into the future, if they don't get shorter term variability in agreement with observations. [Judith Hauck, Germany]	Not applicable - Section 5.7 has been completely re-drafted from a discursive knowledge gaps to bullet points that could strengthen the existing assessment
26939	103	18	103	18	We suggest to add after 210 "and taken into account its specific role on coastal ecosystem and polar region" [Eric Brun, France]	Not applicable - Section 5.7 has been completely re-drafted from a discursive knowledge gaps to bullet points that could strengthen the existing assessment
58497	103	21	103	22	While palaeo is indeed an important source for understanding tipping points, I'd say that models have been equally important for the discovery and assessment of tipping points. For example, while discussion of AMOC shutdown was inspired by evidence of past shutdowns it has also long been explored by models, improvements in ice sheet modelling have confirmed the possibility of Greenland or West Antarctic collapse implied by palaeo sea level data, and understanding of Amazon dieback is almost entirely model-based. Analysis of CMIP5 output by Drijfhout et al. (2015) also identified several abrupt climate shifts which were not so obvious from palaeo data alone, such as Labrador Sea convection collapse or sea ice regime changes. So while I support the statement suggesting new models and runs to further explore tipping points, it might be worth adding a short precursor sentence saying something like "Models have helped improve our understanding of tipping dynamics, but large uncertainties remain. New conceptual models...". [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable - Section 5.7 has been completely re-drafted from a discursive knowledge gaps to bullet points that could strengthen the existing assessment
58517	103	21	103	25	This statement on palaeodata is a little too dismissive (also see the above comment on lack of integration of knowledge gaps in palaeo data), given the high importance placed on palaeo aspects in several sections of the report. The way the first sentence is currently written implies that palaeo data provide completely inadequate analogues for tipping points under anthropogenic GHG rise because the timeframe of the latter is less than 100 years. I think this might be a little too simplistic. A great deal of the content of the report (see in particular the discussion of palaeo constraints in Chapter 7), is focussed on palaeo data which is relevant to C-cyle tipping points on relatively rapid timescales - not as fast as 100 years, but nonetheless geologically rapid, and relevant for Earth System modellers. For instance, several recent papers show significant reductions in NH glaciation during the LIG, which is directly relevant to tipping points on 100 to 1000 yr timescales. One suggestion for the authors is that the word "conservative" needs to be better defined and clearly explained in this context, and brought more into line with IPCC terminology. As a whole, knowledge gaps wrt palaeo need to be better addressed in this section. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable - Section 5.7 has been completely re-drafted from a discursive knowledge gaps to bullet points that could strengthen the existing assessment

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
58499	103	30	103	32	Spell out the full name of TCRE (like with ZEC on l.30) for readers who may have skipped straight to reading the Knowledge Gaps section. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable - Section 5.7 has been completely re-drafted from a discursive knowledge gaps to bullet points that could strengthen the existing assessment
96655	103	31	103	31	Please introduce TCRE. [Nicole Wilke, Germany]	Not applicable - Section 5.7 has been completely re-drafted from a discursive knowledge gaps to bullet points that could strengthen the existing assessment
41719	103	31	103	31	please explain "TRCE" - te reader might not have read the full report [Katharina Meurer, Sweden]	Not applicable - Section 5.7 has been completely re-drafted from a discursive knowledge gaps to bullet points that could strengthen the existing assessment
66687	103	34	103	36	Should probably refer back to the tipping points section on permafrost. [Dave Frame, New Zealand]	Not applicable - Section 5.7 has been completely re-drafted from a discursive knowledge gaps to bullet points that could strengthen the existing assessment
58501	103	40	103	42	Could an example or two be given here of promising approaches on this (such as GWP*)? [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable - Section 5.7 has been completely re-drafted from a discursive knowledge gaps to bullet points that could strengthen the existing assessment
96657	103	44	104	10	It may be good to discuss how complex a comparison of different CDR and SRM measures is, as it depends on a careful choice of variables and inclusion of side-effects and feedbacks. Normalization helps to make different scenarios better comparable. See Sonntag et al 2017 (10.1002/2017EF000620). [Nicole Wilke, Germany]	Not applicable - Section 5.7 has been completely re-drafted from a discursive knowledge gaps to bullet points that could strengthen the existing assessment
18925	103	44	104	10	Excellent job by the lead authors in their discussion of the gaps in knowledge! [Govindasamy Bala, India]	Not applicable - Section 5.7 has been completely re-drafted from a discursive knowledge gaps to bullet points that could strengthen the existing assessment

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
127865	103	46	103	50	The first paragraph under the heading "Carbon Dioxide Removal and Solar Radiation Modification" notes low confidence in global CO2 sequestration potential of land-based and ocean-based CDR methods, and the need to verify that they "are regionally feasible, present an actual and verifiable negative regional carbon balance, and have no negative unintended consequences." Land-based biological CDR, including soil health practices in organic farming and other agroecological production systems, agroforestry, advanced rotational grazing management, and restoration of native forest, prairie, and wetland plant communities, come closest to meeting these criteria. Although more research is needed to quantify CDR potential and enhance regional feasibility, recent literature reviews indicate that these practices offer substantial CDR potential (NSAC, 2019; Schonbeck et al., 2018, cited above). For example, Lal et al. (2018) cites a range of estimates for cumulative potential global CO2 removal over the next 60 years through land-based biological CDR of 208-458 PgC, resulting in a reduction in atmospheric CO2 concentration of 97 to 214 ppm. Even at the low end of this range, this could make the difference between climate change to which ecosystems and human civilization can adapt, versus utter catastrophe that would leave the planet unrecognizable. In addition, unlike ocean-based, geochemical, and technological CDR methods, the "side effects" of photosynthesis-based CDR on currently managed lands are mostly beneficial, and include reduced soil erosion; increased agricultural, community, and ecosystem resilience based on improved soil health and biodiversity; and improved water quality through enhanced nutrient cycling. Holistic and locale-appropriate application of land-based biological CDR methods can protect and enhance both food security and biodiversity. The uncritical application of certain land-based biological CDR methods can undermine biodiversity, food security, and community resilience, as noted in the AR6 WGI report. Pitfalls to avoid include afforestation of areas not historically in forest, reforestation with monoculture	Not applicable - Section 5.7 has been completely re-drafted from a discursive knowledge gaps to bullet points that could strengthen the existing assessment
82103	103	46	104	4	In this section it would be good to mention that a better understanding of interactions (i.e., synergies or opposition) between CDR methods is needed as it is unlikely that if CDR is deployed it will be only one method. A portfolio of CDR methods will likely be used (if CDR is done) and the carbon and climate response to this portfolio must be understood. Carbon cycle responses will be especially important in this context for monitoring and accounting purposes. [David Keller, Germany]	Not applicable - Section 5.7 has been completely re-drafted from a discursive knowledge gaps to bullet points that could strengthen the existing assessment
115423	103	47		50	This appears to be a research recommendation, which I don't think we are allowed to make. Also IPCC recommending SRM field experiments is likely to be contentious. [Gillett Nathan, Canada]	Not applicable - Section 5.7 has been completely re-drafted from a discursive knowledge gaps to bullet points that could strengthen the existing assessment
96659	103	48	103	48	"Large-scale and long-term experiments or assessments that involve independent verification are needed" is stated undifferentiated for CDR and SRM measures. It is not appropriate for the IPCC to provide the recommendation to conduct large scale experiments in CDR and SRM approaches. Please address the pros and cons of such research but refrain from recommendations. The current text is imbalanced since it omits addressing potential risks that some of the approaches would have if conducted in "large-scale experiments". [Nicole Wilke, Germany]	Not applicable - Section 5.7 has been completely re-drafted from a discursive knowledge gaps to bullet points that could strengthen the existing assessment

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
16997	103	53	103	53	"Earth system Models" should probably read "Earth System Models". Or "Earth system models" as on the next page line 3. [Andreas Krause, Germany]	Not applicable - Section 5.7 has been completely re-drafted from a discursive knowledge gaps to bullet points that could strengthen the existing assessment
1875	104	6	10		I think you can add the effects of diffuse radiation on carbon uptake in various types of vegetation, including crops. Also, SRM will affect UV and surface ozone concentrations, and we know little about those impacts. [Alan Robock, United States of America]	Not applicable - Section 5.7 has been completely re-drafted from a discursive knowledge gaps to bullet points that could strengthen the existing assessment
40483	105	0			it'd be good to mention the relative importance of both land and ocean sinks [TSU WGI, France]	Accepted - text revised
40093	105	0			doesn't FAQ5.1 contradict ch5 p19, L42-45? "In conclusion, both ocean and land CO2 sinks have grown and continuous to growth with the rising of atmospheric CO2 and consistent with anthropogenic emissions, but the growth of the combined ocean and land sink has been slower than the accumulation of anthropogenic CO2 in the atmosphere (medium confidence)." [TSU WGI, France]	Taken into account - This is not a contradiction. The response relative to the emissions has been constant, but the response relative to the excess CO2 in the atmosphere has not. Text has been revised to make that distinction clear.
40145	105	0			FAQ5.1 is nice! [TSU WGI, France]	Noted
40669	105	0			reading the question, one might wonder why it's being asked. I'd put more emphasis on the "why should we care" or "why are we even answering this question" aspects [TSU WGI, France]	Noted - The FAQ has been revised to answer the why should we care aspect clearer.
41191	105	0			why are there 3 paragraphs on the land sink and only one on the ocean sink? It makes it looks as if it's less important [TSU WGI, France]	Accepted - text has been revised to reduce detail from the land description
39677	105	0			the summary would be clearer with the answer upfront and the mention that sinks are expected to take up less CO2 [TSU WGI, France]	Accepted - text has been revised
19315	105	1	111	43	Shorten the FAQs. Answer the question and leave out some of the facts stated there which are already in the main text. [Benjamin Lamptey, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - The FAQs have been reworked for accessibility. However, as FAQs build on the assessment and are meant to be read as a standalone by a lay audience, some overlap cannot be avoided. Furthermore, the length of the FAQ is defined in the TSU guidelines.
7169	105	3			The entire section FAQ 5.1 in its current form is arguable. There are some indications of slowing down. See for example: Peñuelas, J., Ciais, P., Canadell, JG., Janssens, I., Fernandez-Martinez, M., Carnicer, J., Obersteiner, M., Piao S., Vautard, R., Sardans, J. (2017). Shifting from a fertilization-dominated to a warming-dominated period. Nature Ecology & Evolution 1, 1438–1445. doi:10.1038/s41559-017-0274-8 Wang, T., Liu, D., Piao, S., Wang, Y., Wang, X., Guo, H., Lian, X., Burkhart, J., Ciais, P., Huang, M., Janssens, I., Li, Y., Liu, Y., Penuelas, J., Peng, S., Yang, H., Yao, Y., Yin, Y., Zhao, Y. 2018. Emerging negative impact of warming on summer carbon uptake in northern ecosystems. Nature communications (2018) 9:5391. doi: 10.1038/s41467-018-07813-7 Fernández-Martínez, M., Sardans, J., Chevallier, F., Ciais, P., Obersteiner, M., Vicca, S., Canadell, J., Bastos, A., Friedlingstein, P., Sitch, S., Piao, S., Janssens, I., Penuelas, J. 2019. Global trends in carbon sinks and their relationships with CO2 and temperature. Nature Climate Change, 9, pages73–79 (2019). doi: 10.1038/s41558-018-0367-7. [Josep Penuelas, Spain]	Accepted - while there is no evidence based on atmospheric observations and large-scale constraints on the land and ocean C sink that the land and ocean fraction of anthropogenic emissions have changed, there are observable changes in the processes that affect land and ocean C uptake, which together suggest that in the future the land and ocean uptake will be weakened. The FAQ has been revised to make this clear.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
109853	105	8	105	55	Starting "There is as yet no ….." till the end of the page. There is a contradiction with what was stated in the executive summary in page 6 line 53 to line 55, page 7 line 32 to line 34 and page 7 line 42 to line 43. [Rehab El-Maghraby, Egypt]	Accepted - It is incorrect to state that the ES and FAQ are contradictory, however, the difference between the land-/ocean fraction of emissions (which has not changed, as discussed in the FAQ and the sink fraction (the ocean/land uptake relative to the excess CO2 in the atmosphere, which has declined) does not become clear. ES and FAQ have been revised to remove this apparent contradiction
39947	105	26		30	are you only talking about the land carbon sinks or both land and ocean it is a bit confusing. [TSU WGI, France]	Accepted - it has been clarified that this text regards land carbon changes
38529	105	32	105	38	While this statement is true it is also true that the ocean sink will continue to grow as long as emissions, and thus the atmospheric partial pressure, continues to grow. This is a direct consequense of Henry's Law. The temperature effects are much too small to overcome the proportionality between atmospheric partial pressure and ocean concentrations. When growth in atmospheric partial pressure stops, or begins to decrease, the temperature effects are likely to become much more important. This should be reflected in the reply to the FAQ because the assumed temperature dependence of the ocean carbon sink very often is misunderstood and misconstrued among the general public [Siv K Lauvset, Norway]	Accepted - The text following line 48 says exactly this. It has been revised to make the proportions clear
81027	105	32	105	38	Should a reference be made to the realtionship between a warmer ocean and reduced capacity to retiain dissolved CO2 be made here? [Jeffrey Philip OBBARD, Singapore]	Rejected - this is already implied in the text. A detailed treatment of the underlying processes is presented in Section 5.3
18195	105	33	105	33	'(approximately the upper 50 m but change seasonally)' - and also regionally? It could be argued that different regions will have larger differences than during a seasonal cycle, for example at lower latitude locations? [Chelsey Baker, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable - text was removed
73287	105	36	105	36	Change to 'CO2 enriched' [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change was made.
36395	105	40	105	44	The authors need to be clear whether or not they believe the ocean fraction of total CO2 emissions is decreasing, or is it still within the uncertainty of the measurements. [Adrienne Sutton, United States of America]	Noted - The text is clear in that the fraction has remained unchanged within the uncertainty, however, process-level evidence supports a beginning/future change in the uptake fraction
30581	105	40	105	45	The authors need to be clear whether or not they believe the ocean fraction of total CO2 emissions is decreasing, or is it still within the uncertainty of the measurements. [nina bednarsek, United States of America]	Noted - The text is clear in that the fraction has remained unchanged within the uncertainty, however, process-level evidence supports a beginning/future change in the uptake fraction

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
58503	105	43	105	44	There has been at least some recent evidence of a slowdown in Amazon rainforest carbon sink capacity (Hubau et al., 2020, doi:10.1038/s41586-020-2035-0), and observable permafrost thaw represents a slow carbon sink shifting to become a net source (admittedly running parallel to Boreal/Tundra greening acting as a large sink in the same region, but this would then represent the processes underlying the global carbon sink singificantly changing, and at least one study suggests recent permafrost thaw C loss is currently greater than tundra greening C gain [Natali et al., 2019,doi:10.1038/s41558-019-0592-8]). As such stating there's no evidence seems potentially over-confident, and I'd suggest replacing "no" with "limited" both here and on l.8. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account - the statement as written is correct (the land/ocean borne fraction do no exhibit any trend, however, the reviewer is correct that underlying process have begun to change. The FAQ has been revised to take this into account
104711	105	48	105	49	This implies that even if countires manage to strongly reduce .... As it is the sentence implies that IF we reduce emissions, sinks become smaller. [Marco Tulio Cabral, Brazil]	Noted - this interpretation is correct so far natural sinks (and not those from anthropogenic action) are concerned
84011	105	48	105	49	This implies that even if countires manage to strongly reduce .... As it is the sentence implies that IF we reduce emissions, sinks become smaller. [Marco Tulio Cabral, Brazil]	Noted - this interpretation is correct so far natural sinks (and not those from anthropogenic action) are concerned
33375	105				FAQ 5.1. L8-9. “There is as yet no observable evidence that this natural removal is slowing down or that the processes underlying this removal are changing.” This sentence might be followed by another taking in account that in the future it will as indicted in the last paragraph: “The ocean sink will become smaller in the future because the buffer capacity to continue uptake CO2 will diminish at the same time the warming of the ocean will further reduce its capacity to remove CO2. For the land sink, model simulations suggest that if emissions are not reduced sufficiently to cap warming at 2°C, the combined effect of reduced the CO2 fertilisation effect and climate change is likely to weaken the land sink in the second half of this century. In summary, CO2 sinks will change in the future and understanding the magnitude of change will be important to design mitigation pathways.” Something like: “However, in the future the buffer capacity to continue uptake CO2 will diminish in ocean and land.”. I suggest to include this sentence or similar in the first part of the sentence in italics. [Guiomar Rotllant, Spain]	Accepted - text revised
58505	106	7	106	8	The caption states 1960-2019 but the citation given is the 2018 global carbon budget - depending on the actual data source for the figure this should either be updated 1960-2018, or the citation updated to Friedlingstein et al. (2019) for the 2019 GCB. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - citation has been revised
41721	106	53	106	53	"reducing" [Katharina Meurer, Sweden]	Not Applicable - Line 53 of page 106 doesn't exist.
40993	107	0			the numbers and units (PgC C, TgCH4/yr) given in the text are not really self-speaking to non-specialists…maybe giving an order of magnitude or explain a bit more what it is could help? [TSU WGI, France]	Accepted, numbers are changed in text.
40335	107	0			generally I think the language of FAQ5. could be simplified to be more accessible to a lay audience (e.g. I wouldn't assume people know what permafrost is) [TSU WGI, France]	Accepted, we have simplified the language

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
40387	107	0			I think the text would be stronger if the you added a last paragraph as a conclusion to both elements developed in the text (permafrost thawing + gas hydrates) [TSU WGI, France]	Accepted, sentence added
40399	107	0			I would consider rephrasing the title to make it shorter and snappier: e.g. "can thawing permafrost [strongly] intensify global warming?" [TSU WGI, France]	Accepted, title clarified
40147	107	0			FAQ5.2 is very interesting and has a logical flow and a lot of useful information [TSU WGI, France]	Noted with thanks.
40933	107	0			The FAQ focuses on the Arctic but what about the carbon stored in the permafrost elsewhere in the world? [TSU WGI, France]	Accepted, we refer to Tibet permafrost as well.
18251	107	1	107	1	The question is a little confusing in terms of permafrost under the ocean. I assume this relates to methane clathrates and I think rephrasing the question to something similar to '… or within the seafloor' would be better. [Chelsey Baker, United Kingdom (of Great Britain and Northern Ireland)]	Accepted, title clarified.
88209	107	1	107	1	Permafrost is a sub-surface phenomenon. It would be better to refer to "terrestrial permafrost" rather than "permafrost on land" (you could also refer to permafrost under the land surface) [Sharon Smith, Canada]	Accepted.
41723	107	4	107	8	even if not specifically accounted for in the budgeting, but N2O emissions from permafrost thaw cannot be beglected and should be mentioned here. [Katharina Meurer, Sweden]	Rejected due to space constraints in this FAQ, but covered in permafrost box.
41181	107	4		9	When you say "it should be considered". Has I been considered in this report and it wasn't the case before (i.e. is it a new improvement)? If so I would state that fact more explicitly [TSU WGI, France]	Accepted this refers to how the estimate is used more clearly now.
88211	107	7	107	8	"deeper on land" - Are your referring to gas hydrates here and permafrost that is is 100s metres thick? [Sharon Smith, Canada]	Accepted, this is clarified in text now.
88213	107	10	107	10	What is meant by "deep soils"? It would be better to say "….where the ground remains frozen…." This would be more inclusive - include soils, unconsolidated sediments, rock etc. [Sharon Smith, Canada]	Accepted, this is clarified in text now.
88215	107	13	107	13	We usually say "perrenially-frozen" rather than "permanently-frozen" when referring to permafrost. Maybe it would be helpful to provide a definition for permafrost up front in the FAQ and then the term "permafrost" can be used throughout the text. [Sharon Smith, Canada]	Partially accepted, we now say "ground that is frozen throughout the year."
39803	107	15			"permafrost soils to act as carbon sinks historically" what do you mean by historically? Thousands of years or decades ago? [TSU WGI, France]	Accepted, this is clarified in text now.
16081	107	16	107	16	Strictly speaking, the carbon as such does not decompose (except 14C if you like, but that's not sensitive to warming). Organic matter does. [Gerhard Krinner, France]	Accepted
88217	107	18	107	19	A time period is required here as changes in permafrost will lag behind the changes in air temperature. [Sharon Smith, Canada]	Accepted
88219	107	24	107	25	Revsions suggested to better refelct process "…processes that can enhance permafrost thaw and loss of carbon…" (note you don't necessarily need to have massive ice bodies to have settlement etc. Excess ice is required - ice-rich material) [Sharon Smith, Canada]	Accepted
88221	107	26	107	27	I assume you are referring to expansion of thermokarst lakes and ponds and the link to permafrost thaw needs to be made. Revision is suggested: "….can expand as permafrost thaws." (also delete "move across the landscape"). You could also mention here that ponds can form in response to permafrost thaw (thermokarst ponds) [Sharon Smith, Canada]	Accepted

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
98495	107	27	107	27	After, "the landscape" I'd add "or can drain quicky if a confining permafrost layer thaws beneath" or something to that effect. Something capturing the impact of sunlight passing through surface water bodies warming the permafrost beneath the water body and thawing that area faster, potentially causing the pond/lake to drain. [Emily Romano, United States of America]	Accepted
88223	107	27	107	28	You should probably be clear here that it isn't the fire itself that results in thaw but the changes in vegetation and surface conditions, i.e. damage to vegetation and organic layer which alters the micro climate. [Sharon Smith, Canada]	Accepted
88225	107	28	107	30	However, growth of shrubs in tundra can also result in snow accumulation and warming of the ground and potentially permafrost thaw so there could be the opposite effect. [Sharon Smith, Canada]	Accepted
58507	107	32	107	35	It might be worth specifying that only the slower, wider permafrost processes have been included in permafrost-enabled ESMs so far and do not yet include fine-scale processes like thermokarst formation and slumps in ice-rich terrain, which Turetsky et al. (2019,doi:10.1038/d41586-019-01313-4; 2020,doi:10.1038/s41561-019-0526-0) suggest could double emission rates, and so this sensitivity might prove to be conservative. However, the insights following these sentences still remain sound. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted, we now use consistent numbers here.
109855	107	34	107	34	the amount od carbon release as CO2 from premafrost is stated to be 20 PgC as CO2 per degree C global temperature change, this number is different to the number stated in Page 8 Line 52 and Line 53 (75 GtCO2 per degree of additional warming ….) [Rehab El-Maghraby, Egypt]	Accepted
21873	107	34	197	34	Given that the purpose of FAQs is to be accessible I am not convinced that the addition of 20 +/- 13 PgC as CO2 per C is entirely in keeping with this. Would a lay audience have any idea what this is and how much it matters? Would it not be better to say what that is in e.g. equivalence to the total anthropogenic emissions in 2019? [Peter Thorne, Ireland]	Taken into account; we now do take this into account in the budgets, hence the must.
32011	107	39			This paragraph seems rather overconfident and model dependent, not really recognisant of field observations in the Arctic. See for example Youmi Oh's work: Oh, Youmi, Qianlai Zhuang, Licheng Liu, Lisa R. Welp, Maggie CY Lau, Tullis C. Onstott, David Medvigy et al. "Reduced net methane emissions due to microbial methane oxidation in a warmer Arctic." Nature Climate Change 10, no. 4 (2020): 317-321 [Euan G. Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Accepted, wording changed.
88229	107	41	107	46	Hydrates can also occur beneath the deep ocean that are not associated with permafrost. I assume that is not what is considered here but rather hydrates beneath the Arctic continental shelf that are associated with permafrsot that formed during glaciation when sea levels were much lower - as sea level rose with deglaciation, warming and thawing occurs. [Sharon Smith, Canada]	Accepted, wording changed.
88227	107	45	107	48	Define "deep" when discussing terrestrial permafrost and gas hydrates. Earlier in the FAQ reference is made to deep soils which are quite shallow compared to depths where hydrates occur - depths greater than 200 m (where as soils disussed earlier are in the upper 10 m or so). It is also important to give some idea of time period for this deeper carbon in hydrates to be released compared carbon associated with shallow soils. [Sharon Smith, Canada]	Accepted, the units of these numbers are all changed.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
21875	107	48	107	48	Again, 2000 PgC is probably not accessible without some analogy such as e.g. either annual fossil fuel combustion or total fossil fuel combustion to date. [Peter Thorne, Ireland]	Accepted, the units of these numbers are all changed.
58509	107	48	107	48	The methane hydrate reserve is quoted as 1500-2000 PgC earlier in the text, so I suggest inserting "up to" before 2000 PgC. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected, no enough space to get into this process.
81029	107	49	107	54	Should reference be made here to the senisitivity of CH4 from methane hydrates reaching the surface being dependent on ocean depth, where shallow ocean depths (<50m) result in CH4 reaching the surface? I believe there is some evidence for this in the literature. [Jeffrey Philip OBBARD, Singapore]	Accepted, wording changed.
16305	107	52	107	54	It is not clear 'next century' means 21st or 22nd century [Cunde Xiao, China]	Accepted, wording changed.
40199	108	0			fig FAQ5.2: I wonder if a map with all the carbon stored/extent of permafrost in the arctic couldn't do the job? [TSU WGI, France]	Taken into account - the figure was not available in time for the SOD but was included for the FGD.
81031	108	37	108	37	Is it possible to more quantitiative here in terms of the amount of sea level rise and the period of time for continued sea level rise as a function of continued ocean thermal expansion. More broadly in FAQ 5.3 what about the implications of negative CO2 emissions for ocean pH change? [Jeffrey Philip OBBARD, Singapore]	Rejected: the primary FAQ to discuss sea level rise is FAQ 9.2. Additional information on the negative implications to pH was not added to FAQ5.3 to avoid imbalanced coverage of topics in this FAQ and due to space contraints.
39975	109	0			At the moment the text doesn't really answer the question of "what is THE remaining carbon budget" it's more an explanation of "what is A carbon budget" so the reader is somehow left disappointed [TSU WGI, France]	Taken into account - FAQs are intended as explainers to a broad audience and should not contain the assessment. In the revised FAQ, better context is provided.
40795	109	0			shouldn't you mention (more clearly) that if you remove carbon from the atmosphere the sinks react by releasing carbon in the atmosphere and/or something about the fact that "The climate-carbon cycle response to the removal of CO2 from the atmosphere (negative emissions) is not always equal and opposite to the response to positive emissions" (p9 L19-20)? This is something that seems very important to me. [TSU WGI, France]	Rejected. These are important points but are not crucial to answering the FAQ.
41107	109	0			To improve the readability of the text I would avoid the use of words like "anthropogenic" etc and acronyms, as it sounds a bit jargony [TSU WGI, France]	Accepted. We replaced "anthropogenic" with "human-caused".
40395	109	0			I wonder is the title shouldn't be adapted a little bit, to make it more self-explanatory to a lay audience? (I think negative emission is not properly understood by a lay audience). For instance: " Can we reverse climate change by removing carbon from the atmosphere?" "Can carbon capture and negative emission reverse climate change" [TSU WGI, France]	Accepted. The title was changed to " Can we reverse climate change by removing carbon from the atmosphere?"
40149	109	0			FAQ5.3 is a nice FAQ with a clear structure so that it's easy to follow the flow [TSU WGI, France]	Noted. No edit required.
39667	109	0			it's a pity that the conclusion statement "yes it can but to a certain degree" is not presented as clearly in the summary. [TSU WGI, France]	Accepted. The conclusion is now repeated in the summary.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
111877	109	1	109	1	I think "net negative" would be better than "negative", because it better highlights that this FAQ is not really about CDR as such [Oliver Geden, Germany]	Rejected. We prefer to keep the title more general and clarify in the text that a decline in CO2 can only be achieved with net negative CO2 emissions.
17001	109	1	109	55	Shouldn't it also be mentioned that it is very uncertain whether substantial negative emissions can be achieved at all? [Andreas Krause, Germany]	Accepted. This point has been clarified.
96661	109	1	110	9	The FAQ on negative emissions is purely theoretical and offers interesting information. However, it can lead to misunderstandings if it is not clearly stated that it is not about methods, potentials or risks. Therefore, please add "This FAQ does not assessment methods, potentials or risks of potential approaches to realize negative emissions. [Nicole Wilke, Germany]	Accepted. It was clarified that negative emission technologies have not yet been proven at scale and some proposals remain entirely theoretical.
40963	109	3	109	26	The glossary definitions distinguish between negative emissions for CO2 alone vs. for multiple greenhouse gases. The term 'negative greenhouse gas emissions' is used to refer to removal of multiple GHGs, while CDR only refers to removal of CO2. 'Net negative CO2 emissions' is the equivalent of 'Net negative greenhouse gas emissions' but only for CO2. The term 'Net emissions' isn't defined in the glossary but if it were, it would be the balance for multiple GHGs rather than just CO2. [TSU WGI, France]	Accepted. It was clarified that the FAQ refers to negative CO2 emissions.
40931	109	20	109	26	The FAQ doesn't talk about what will happen if the the natural carbon sinks change, which is the cause of much confusion. Another source of confusion is what is required to stabilise global temperature, it is simply stable CO2 concentration, or is it a slowly declining CO2 concentration, given that the ocean release heat when this happens? [TSU WGI, France]	Rejected. These points are important but beyond the scope of this FAQ.
58323	109	23	109	23	Comma-error: …if CO2 release equals removal COMMA the (erase comma here) atmospheric CO2 …. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted -error was corrected.
73289	109	25	109	25	Net should not be in italics, given that italicisation is reserved for probablity statements throughout the document. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Underlining is now used for emphasis.
16999	109	26	109	26	Dot missing at the end of the sentence. [Andreas Krause, Germany]	Accepted - change was made.
33377	109	26			Change: “…removals)” by “removals).”. [Guiomar Rotllant, Spain]	Accepted - change was made.
39715	109	31	109	32	"… to decline within a few years…" -> Need to explain how this is reconciled with FAQ 4.2 we says "it will take 20 to 0 years to see the effect of greenhouse gas reduction on global temperature" [TSU WGI, France]	Accepted. We clarified that it will take decades for the change to be detectable.
39811	109	31		32	"Recent studies ….a decline in atmospheric CO2" this seems to contradict FAQ4.2 which says it will take decades to be detectable [TSU WGI, France]	Accepted. We clarified that it will take decades for the change to be detectable.
58511	109	49	109	51	I would also consider adding here that negative emission technologies are still unproven at scale and some remain very theoretical, and so we can't rely on them scaling up (along the lines of Anderson & Peters, 2016, 10.1126/science.aah4567) - perhaps can add something at the end like "Furthermore, negative emission technologies have not yet been proven at scale and some proposals remain entirely theoretical, and so their utility cannot be guaranteed". [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Sentence was included.
73291	110	5	110	5	Don't use etc, give all details. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The sentence was a placeholder and has been replaced with a proper figure caption.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
17003	111	1	111	43	CO2 is sometimes written CO2 and sometimes carbon dioxid. [Andreas Krause, Germany]	Taken into account - The FAQs have been reworked for accessibility.
18253	111	3	111	3	remove the word 'to' [Chelsey Baker, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - The entire FAQ has been edited
58325	111	3	111	43	If you could specify the chemical spelling of the gases (methane (CH4)… carbon dioxide (CO2)… nitrous oxide (N2O)) the first time they are mentioned in this paragraph, it could help laymen follow easier and then stay consistent with this spelling. You jump between writing the name out and using the chmical spelling. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted
58513	111	20	111	28	I know the FAQ is meant to be relatively untechnical for easy reading, but would it not be worth including at least some actual headline carbon budget estimates based on the work in this report to show what the current best knowledge on this is? (Much like the other chapter 5 FAQs which include at least a few key numbers.) The Chapter 5 summary states "The remaining carbon budgets starting from the year 2020 for limiting warming to 1.5°C... and 2.0°C with a probability... of at least 50% are 390 GtCO2... 1140GtCO2...[these] correspond to reaching net zero in about 20... and 55 years, respectively". A somewhat simplified version of these headline figures would help illustrate the point and provide key information to a reader only coming across this FAQ. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted
81033	111	23	111	24	Should reference be provided here to the negative radiative forcing from atmospheric aerosols and the implications for achieving temperature targets? What effect of CO2 emission reductions have on the atmospheric burden of CO2, for example with the switch from aerosol-generating fossil fuel combustion to non-aerosol generating renewable energy sources? [Jeffrey Philip OBBARD, Singapore]	Accepted
58327	111	24	111	24	I do not understand this sentence: These choices can be informed by science… . Do you mean to say, "these choices can be made according to information we gained through scientific research"?? [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted
58329	111	25	111	28	VERY long sentence! Can you break this up and make it easier to understand by having shorter sentences? [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted
58331	111	34	111	34	At the end of the sentence "to start with" is supposed to say what? Are there more Earth systems besides the oceans, the land and the atmosphere, that we can release CO2 into? [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted
37761	112	40	112	40	Add volume number to Anav et al. (2015). Anav, A. et al. Spatio-temporal patterns of terrestrial gross primary production: A review. Reviews of Geophysics 53, doi:10.1002/2015RG000483 (2015). [Akihiko Ito, Japan]	Accepted.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
93437	114	12	114	13	The ref should be Barker, S. and Elderfield, H., and you may want to add a recent ref showing the same: Moy, A. D., Palmer, M. R., Howard, W. R., Bijma, J., Cooper, M. J., Calvo, E., Pelejero, C., Gagan, M. K., & Chalk, T. B. (2019). Varied contribution of the Southern Ocean to deglacial atmospheric CO2 rise. Nature Geoscience, 12, 1006–1011. https://doi.org/10.1038/s41561-41019-40473-41569 [Carles Pelejero, Spain]	Accepted (Elderfield is added to citation).
37763	114	18	114	18	Remove 'LP'. [Akihiko Ito, Japan]	Accepted
58675	115	35	115	36	DOI is missing : doi:10.3334/CDIAC/00001_V2017 [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted
37765	117	54	117	55	Cavan et al. (2019) appears twice and the latter should be deleted. [Akihiko Ito, Japan]	Accepted
58677	118	41	118	46	References "Ciais et al. 2019a" and "Ciais et al., 2019b" are listed in the same way. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted
37923	118	41	211	46	Same references [Junhee Lee, Republic of Korea]	Accepted
37767	118	44	118	45	Ciais et al. (2019) appears twice and the latter should be deleted. [Akihiko Ito, Japan]	Accepted
58679	121	51	121	52	DOI is missing : 10.1038/NGEO2633 [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted
83989	123	21	123	22	Delete repeated reference [Marco Tulio Cabral, Brazil]	Accepted
84005	123	21	123	22	Repeated reference [Marco Tulio Cabral, Brazil]	Accepted
58681	123	21	123	25	The two differently cited papers are the same. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted
58683	123	29	123	30	The year of publication is false. "Finkel et al., 2010" --should be changed to--> "Finkel et al., 2009" [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted
52263	124	19	124	26	Fransson A. M. Chierici, D. Nomura, M. A. Granskog, S. Kristiansen, T. Martma, G.Nehrke (2015). Effect of glacial drainage water on the CO2 system and ocean acidification state in an Arctic tidewater-glacier fjord during two contrasting years. Journal of Geophysical Research-Oceans, 120, doi:10.1002/2014JC010320. [Agneta Fransson, Norway]	Noted - added to section 5.3.4
52265	124	19	124	26	Fransson A., M. Chierici, I. Skjelvan, A. Olsen, P, Assmy, A. Peterson, G. Spreen and B. Ward (2017) Effect of sea-ice and biogeochemical processes and storms on under-ice water fCO2 during the winter-spring transition in the high Arctic Ocean: implications for sea-air CO2 fluxes. JGR- Oceans, N-ICE special issue, doi: 10.1002/2016JC012478. [Agneta Fransson, Norway]	Noted - added to section 5.3.4
52275	124	19	124	26	Fransson, A., M. Chierici, and Y. Nojiri (2009). New insights into the spatial variability of the surface water CO2 in varying sea ice conditions in the Arctic Ocean. Cont. Shelf Res., 29, 1317-1328, doi:10.1016/j.csr.2009.03.008. [Agneta Fransson, Norway]	Noted - we have decided to however to use more recent references from the same authors to this assessment (melt water, sea ice cover and CaCO3 dynamics (precipitation or dissolution) in high latitude (northern and southern) coastal areas.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
52277	124	19	124	26	Fransson A., M. Chierici., P. Yager., W.O. Smith (2011). Antarctic sea ice carbon dioxide system and controls. J. Geophys. Res. 116, C12035, doi:10.1029/2010JC006844. [Agneta Fransson, Norway]	Noted - we have decided to however to use more recent references from the same authors to this assessment (melt water, sea ice cover and CaCO3 dynamics (precipitation or dissolution) in high latitude (northern and southern) coastal areas.
52279	124	19	124	26	Fransson A., M. Chierici, L.A. Miller, G. Carnat, H. Thomas, E. Shadwick, S. Pineault, T.M.Papakyriakou, (2013). Impact of sea ice processes on the carbonate system and ocean acidification state at the ice-water interface of the Amundsen Gulf, Arctic Ocean. Journal of Geophysical Research: Oceans, Vol.. 118, 1–23, doi:10.1002/2013JC009164. [Agneta Fransson, Norway]	Noted - we have decided to however to use more recent references from the same authors to this assessment (melt water, sea ice cover and CaCO3 dynamics (precipitation or dissolution) in high latitude coastal areas.
52261	124	20	124	24	suggest adding references: Fransson, A., M. Chierici, and Y. Nojiri (2006), Increased net CO2-outgassing in the upwelling region of the southern Bering Sea in a period of variable marine climate between 1995 and 2001, Journal Geophysical Research, 111, C08008, pp. 1-21, doi:10.1029/2004JC002759. [Agneta Fransson, Norway]	Noted - there are however more recent references to the Bering Sea area that were based in this suggested references.
52259	124		124		add reference: Chierici et al. (submitted) [Agneta Fransson, Norway]	Rejected - it is unclear what the publication is about without title, journal of submission, authors.
58685	126	17	126	18	DOI is missing : https://doi.org/10.1002/2016GL068576 [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted
52347	129	40	129	41	Hopwood, M. J., Carroll, D., Dunse, T., Hodson, A., Holding, J. M., Iriarte, J. L., Ribeiro, S., Achterberg, E. P., Cantoni, C., Carlson, D. F., Chierici, M., Clarke, J. S., Cozzi, S., Fransson, A., Juul-Pedersen, T., Winding, M. S., and Meire, L. (2020): Review Article: How does glacier discharge affect marine biogeochemistry and primary production in the Arctic? The Cryosphere, 14, 1347–1383, 2020, https://doi.org/10.5194/tc-14-1347-2020. [Agneta Fransson, Norway]	Noted - This citation has been included in the FGD in Section 5.3.4
84007	131	48	131	50	Is it the same reference as Janssens-Maenhout, G., Crippa, M., Guizzardi, D., Muntean, M., Schaaf, E., Dentener, F., Bergamaschi, P., Pagliari, V., Olivier, J. G. J., Peters, J. A. H. W., van Aardenne, J. A., Monni, S., Doering, U., and Petrescu, A. M. R.: EDGAR v4.3.2 Global Atlas of the three major Greenhouse Gas Emissions for the period 1970–2012, Earth Syst. Sci. Data Discuss., https://doi.org/10.5194/essd-2017-79, 2017. or the above reference is missing? [Marco Tulio Cabral, Brazil]	Noted - Both 2017 and 2019 citations of the same authors have been included in the FGD
74663	132	39	132	46	Jones et al. (2019a) and (2019b) are the same. [Kaoru Tachiiri, Japan]	Accepted
93447	138	18	138	19	Liu et al ref is 2014 and not 2015 [Carles Pelejero, Spain]	Accepted
52345	141	12	141	13	Mattsdotter-Björk M., A. Fransson, A. Torstensson, and M. Chierici (2014). Ocean acidification state in western Antarctic surface waters: controls and interannual variability. Biogeosciences, 11, 57-73, doi:10.5194/bg-11-57-2014. [Agneta Fransson, Norway]	Noted
52283	142	10	142	11	Meire L, Mortensen J, Meire P, Juul-Pedersen T, Sejr MK, Rysgaard S, Nygaard R, Huybrechts P, Meysman FJR (2017) Marine-terminating glaciers sustain high productivity in Greenland fjords. Glob. Chang. Biol., 23(12), 5344–5357, (doi:10.1111/gcb.13801) [Agneta Fransson, Norway]	Noted

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
52267	144	45	144	47	Olafsson J., S. R. Olafsdottir, A. Benoit-Cattin, M. Danielsen, T. S. Arnarson, and T. Takahashi (2009) Rate of Iceland Sea acidification from time series measurements.Biogeosciences, 6, 2661–2668 [Agneta Fransson, Norway]	Noted
10301	144	53	144	53	3 authors of this paper are missing - Add "Pahlow, M., Yool, A. and Matear, R.J." [Chris Vivian, United Kingdom (of Great Britain and Northern Ireland)]	Accepted
58687	144	53	145	4	references "Oschlies et al., 2010" (p144 / l53-54) and "Oschlies et al., 2010b" (p145 / l3--4) are the same. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted
96663	144	53			Please delete Oschlies 2010 references (same reference occurs again on next page). [Nicole Wilke, Germany]	Accepted
93451	145	48	145	49	Pelejero ref has a number of coauthors: Pelejero, C., Calvo, E., McCulloch, M. T., Marshall, J. F., Gagan, M. K., Lough, J. M., & Opdyke, B. N. (2005). Ocean Science: Preindustrial to modern interdecadal variability in coral reef pH. Science, 309(5744). https://doi.org/10.1126/science.1113692 [Carles Pelejero, Spain]	Accepted
365	148	22	148	22	Typo: Replace "Burr Ows" by "Burrows" [Michael Buchwitz, Germany]	Accepted
74665	149	21	149	24	Rogelj et al (2019a) and (2019b) are the same. [Kaoru Tachiiri, Japan]	Accepted
52269	150	1	150	4	Rysgaard S, Glud RN, Sejr MK, Bendtsen J, Christensen PB (2007) Inorganic carbon transport during sea ice growth and decay: A carbon pump in polar seas. J. Geophys. Res. 112, C03016, (doi:10.1029/2006JC003572) [Agneta Fransson, Norway]	Noted
52271	150	1	150	4	Rysgaard S, Bendtsen J, Pedersen LT, Ramløv H, Glud RN (2009) Increased CO2 uptake due to sea ice growth and decay in the Nordic Seas. J. Geophys. Res. 114, (doi.org/10.1029/2008JC005088) [Agneta Fransson, Norway]	Noted
52273	150	1	150	4	Rysgaard S, Glud RN, Lennert K, Cooper M, Halden N, Leakey RJG, and others (2012) Ikaite crystals in melting sea ice-implications for pCO2 and pH levels in arctic surface waters. Cryosphere, 6, 901-908 [Agneta Fransson, Norway]	Noted
96665	153	53			Sonntag reference has wrong title and is incomplete. It should be Reforestation in a high-CO2 world—Higher mitigation potential than expected, lower adaptation potential than hoped for; Sebastian Sonntag, Julia Pongratz, Christian H. Reick, and Hauke Schmidt, GRL, 2016. [Nicole Wilke, Germany]	Editorial. Professional copy-editing to be completed prior to publication. This kind of issues will be fixed then.
29185	156	43	156	46	I believe that the two Tian et al. Nature submissions cited here as "a" and "b" are actually one in the same. The title and author order changed in various drafts, but I think that there is only one manuscript submitted. [Eric Davidson, United States of America]	Editorial. Professional copy-editing to be completed prior to publication. This kind of issues will be fixed then.
4257	157	4	157	7	The same reference (Tokarska and Gillett 2018) is listed twice. [Claude-Michel Nzotungicimpaye, Canada]	Accepted
74667	157	4	157	7	Tokarska and Gillett (2018a) and (2018b) are the same. [Kaoru Tachiiri, Japan]	Accepted
35131	160	24	160	27	Reference duplicated in 2017b and 2017c [Richard Williams, United Kingdom (of Great Britain and Northern Ireland)]	Accepted
37769	160	26	160	27	Williams et al. (2017) appears twice and the latter should be deleted. [Akihiko Ito, Japan]	Accepted
35133	160	28	160	33	Reference duplicated in 2017d and 2017e and should not contain Williams et al. [Richard Williams, United Kingdom (of Great Britain and Northern Ireland)]	Accepted
37771	160	31	160	32	Williams et al. (2019) appears twice and the latter should be deleted. [Akihiko Ito, Japan]	Accepted

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
52281	161	38	161	39	Yasunaka S., E. Siswanto, A. Olsen, M. Hoppema, E. Watanabe, A. Fransson, M. Chierici, A. Murata, S.K. Lauvset, R. Wanninkhof, T. Takahashi, N. Kosugi, A. M. Omar, S. van Heuven, and J. T. Mathis (2018) Arctic Ocean CO2 uptake: an improved multiyear estimate of the air–sea CO2 flux incorporating chlorophyll a concentrations. Biogeosciences, 15, 1–18, 2018 https://doi.org/10.5194/bg-15-1-2018. [Agneta Fransson, Norway]	Noted
71555	164				I believe that for the results of the inverse analysis, the means and their standard deviations from multiple models (ex. TransCom, RECCAP, etc) should be shown, not the individual model results. [Takashi Maki, Japan]	Taken into account - the multi-model means and standard deviations are now shown in the table.
20021	165	0	166	0	This 2nd table of this appendix is the 3rd one (following tables 5.9 and 5.10) in this chapter which lists the CDR methods. Admittedly information given is not redundant. Besides, CDR is basically a mitigation issue; it is expected that WG3 will discuss it again in detail, possibly adding more tables. An optimized presentation of CDR should be looked for in common. [philippe waldteufel, France]	Noted. Tables 5.9 and 5.10 were merged to eliminate redundancies. The CDR assessment was coordinated with WGIII to eliminate overlap and ensure consistency.
5671	165	1	166	1	Please either include "weakens … through decreased [CO2]" in any case where removal of CO2 is an issue, or delete this phrase where it is shown now. Or provide a justification why land and ocean based measures are treated unequally. [Joachim Rock, Germany]	Noted. We have only mentioned carbon cycle feedbacks of such specific CDR methods from which there is evidence (=published paper). If there is no paper that demonstrates an impact the cells was left white.
127867	165	1	166	30	[RISK] The summary table of impacts and side effects of CDR strategies is quite thorough, accounting for both desirable and negative "side effects" of each CDR strategy. Under afforestation, the table correctly notes that, if not applied wisely and in a site-appropriate manner, this strategy can pose risks to food security and biodiversity, and can threaten water supply in dry regions. As noted earlier (comments on pages 95) tree-based CDR strategies can be integrated with agricultural production and food security through agroforestry, use of locally-adapted food-producing tree species, silvopasture, and permaculture systems. Under ocean fertilization and artificial upwelling, the table notes side effects of increased acidification, hypoxic zones, N2O emissions, perturbation of oceanic ecosystems and communities, and possible toxic algal blooms. These are serious enough to consider taking these CDR strategies off the table, or at least making them much lower priority than land-based CDR strategies that utilize diverse crop, tree, and native plant species, and use no or minimal fertilizer and other agrochemical inputs. [Trigg Talley, United States of America]	Rejected. Agroforestry, although not mentioned explicitly, belongs to the CDR category "soil carbon sequestration". No space to specify all the detailed methodology under this category.
10309	165		165		Unnumbered table - Ocean fertilisation row, 'biogeochemical and biophysical effects' - The 1st line states "Enhanced ocean acidification", however, this only applies to subsurface waters - see page 50, lines 13-14, page 72, lines 34-36 and comments above to page 97, lines 41-43. [Chris Vivian, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
11383	165		165		1. Most DAACS that have demonstrated sigificant negative impacts on ecosystems and biodiversity directly inject carbon underground or in the ocean. They have a reputation to alter the ocean chemistry and affect marine and some other biodiversity in different ways. 2. It they are carried out on a very large scale, DAACS can decrease atmospheric CO2 concentration to sub-optimal levels. Such a decrease has a strong potential to lead to negative plant physiological responses. For example, at one hand, leaf-level CO2 fertilization and photosynthetic efficiency, which strongly are controlled by the leaf structure and CO2 intake, will likely decrease. Plant growth will be constrained, although the vegetation may not be impacted significantly. On the other hand, the reduced photosynthetic efficiency and CO2 fertilization has some potential to increase nutrient availability in the leaf. It is important to add that such negative responses by plants will also be mediated by the effects of climate change (precipitation, temperature), sunlight, land cover and soil or geological composition (https://climate.nasa.gov/news/2436/co2-is-making-earth-greenerfor-now/; Commun. Biol. 3, 125 (2020) - https://doi.org/10.1038/s41558-020-0777-1) 3. There are various DACCS methods/technologies and infrastructure. Some have been applied on commercial scale for decades. Some that are still at pilot and/or experimental scales. The efffects of all DAACS are not the same. It would therefore be important to rearrange accordingly the cell under the column labelled Method of the table summarizing the effects of DAAC on p. 166 (indicated as 5-166). The cell should be spited into sub-sell including selected types of DAACS and their potential effects. Likewise, it would also be useful to have a small section or table (1 page) in the chapter classifying or differentiating DAACS. This will avert unnecessary the confusion that may be generated by amalgamating all the available methods or technologies and claiming that their effects are unknown or dangerous. Averting to claim anything about such effects	1) Taken into account. Side-effects are now briefly discussed in section 5.6.2.2.4. 2) Rejected. All CDR methods aim to reduce the amount of CO2 in the atmosphere, so this consequence is not only specific to DACCS but all the CDR methods listed here. The Earth system impacts of reduced [CO2] are already addressed and assessed in section 5.6.2.1.1. 3) Rejected. It is beyond the scope of this section to discern between different types of DACCS abd their effects. DACCS is assessed in detail in WGIII Section 12.3.
69781	165		165		How can biochar reduce ocean C sequestration effect? Maybe cite a few more papers that support or contradict this. [Gyami Shrestha, United States of America]	Rejected. This happens through reduced atmospheric CO2 concentration, which is evident from idealized model simulations, as explained in Keller et al. 2018.
69783	165		165		No earth system feedback from wetland restoration? Many other blank cells in the table too. What do the blank cells imply? [Gyami Shrestha, United States of America]	Accepted. White cells indicate that there are no published model simulations looking at the specific CDR available. Blank cells were replaced with relevant information.
10303	165		166		This table has no number, no title and does not appear to be referred to in the text of Chapter 5! [Chris Vivian, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - this has now been corrected

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
10305	165		166		This does seem to be a useful table, however, as with earlier comments on Tables 5-9 and 5-10, this table omits a number of ocean-based CDR methods covered in the GESAMP (2019) report. [Chris Vivian, United Kingdom (of Great Britain and Northern Ireland)]	Noted - note that this section does not cover carbon injection methods or storage in geological structures. Weathering, alkalinisation, DAACS etc is covered in section 5.6.2.2.3 and WGIII Section 12.3 (a reference to this section has been added). Aspects related to solar radiation modification are covered in section 5.6.3.
10307	165		166		What is the meaning of the asterisks in the table? [Chris Vivian, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - a footnote was added explaining the asterisks.
103165	165		166		Chapter 5, table on pages 165 and 166, header row, add 'CDR' in front of 'Method'. [Philippe Tulkens, Belgium]	Accepted - change made
103167	165		166		Chapter 5, table on pages 165 and 166, header row, 'side effects on water quantity…' The usage of the word 'side effects' is a secondary, typically undesirable effect of an activity. However, for many, both positive and negative impacts/side-effects are given. Maybe consifer also adding 'benefits' after side effects or mention both 'positive and negative impacts'. The figure 5.36 on page 206 mentions 'adverse' and 'beneficial'. [Philippe Tulkens, Belgium]	Accepted. Changed "side-effects" to "positive and negative impacts"
16025	165				graphic, in relation to biochar « Side effects on water quantity and quality, food supply, biodiversity (BD) », we read « improved soil water holding capacity** (Karhu et al., 2011; Verheijen et al., 2019; C. Liu et al., 2016) ». Indeed biochar is well known for its water holding capacity. After pyrolysis, biochar maintains the original molecular structure of organic products — rice husk to bagasse to wood residues. Those pores, acting like coral reef sponges, will absorb water and release it slower than conventional humus. These pores are also home to microbial life that activate 'living soils'. Yet on page 5 - 206 in graphic « Characteristics of carbon dioxide removal (CDR) methods » we read among « side effects » that biochar has « no effect » on water quantity. Those two declarations seem to contradict each other. Biochar will not only retain water, it will absorb it, as carbon in soils do. [Benoit Lambert, Canada]	Accepted. The graphic was changed as suggested.
16027	165				we read « decreased BD* (Smith et al., 2018b) ». Biodiversity can indeed be reduced if virgin forests are cut to produce biochar. But this goes against the International Biochar Initiative principles to qualify as biochar, and get an IBI label. All biomass used should come from left behind forestry or agricultural activities 'wastes'. [Benoit Lambert, Canada]	Accepted. "decreased BD" was changed to "no impact or increased BD"
10311	166		166		Unnumbered table - Artificial ocean upwelling row, 'biogeochemical and biophysical effects' - The 1st line states "Enhanced ocean acidification", however, this only applies to subsurface waters - see page 50, lines 13-14, page 72, lines 34-36 and comments above to page 97, lines 41-43. [Chris Vivian, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
10313	166		166		Unnumbered table - 'Ocean alkalinisation' row, 'Side-effects on water quality etc.' - As indicated above to comment on page 99, the comment "Release of toxic trace metals from deposited minerals" is not applicable to all minerals that could be used for ocean alkalinisation. Thus I suggest replacing the quoted text with "Release of toxic trace metals from some deposited minerals". [Chris Vivian, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
40205	167	0			Figure 5.0 : the structure of the outline isn't really clear to me (at least not with the current names of the sections): for instance why are section 5.2 and 5.3 on the same level. [TSU WGI, France]	Accepted - Section re-organized
83993	168	0			In Chapter 2, ages were shown in BP, shouldn't this chapter follow the same nomeclature? [Marco Tulio Cabral, Brazil]	Rejected - CE or BCE are in common use
84811	168	1	168	1	In principle a nice figure, but since the x axis has different time tick marks, why not also show the holocene as a fourth expanded interval? [Martin Heimann, Germany]	Accepted - Holocene added
21877	168	1	168	1	Figure text in many places is too small to be legible. Addition of an overarching figure title describing what it is showing would aid accessability. [Peter Thorne, Ireland]	Accepted - Fig was improved
127869	168	1	168	1	Figure 5.1 is misleading because of how the x-axis is represented. The figure is inconsistent with how IPCC has presented this information in the past. The figure is confusing and should have an x-axis that does not change units. [Trigg Talley, United States of America]	Accepted - change was made.
18133	168	1	168	1	In Figure 5.1 a) and b) the blue text reads 'West Antartic Divide' but should read 'West Antarctic Divide' [Chelsey Baker, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Changed.
18135	168	1	168	1	The grey growth rate bars would benefit from some further annotations to make guide the reader through what each bar represents. The full error bars should also be shown - perhaps using a break in the y axis? [Chelsey Baker, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change was made.
58615	168	1	168	1	The first grey bar in graph 5.1.a is unlabled ; uncertainty levels are not clearly shown (the range is too wide to see the end of the spread) [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - change was made.
89635	168	1	168	7	The year label of the growth rate is inconsistent in the figure and legend. It is 1900-2018 in the figure but 1900-2017 CE in the legend. I suggest in the figure, change the bar color of the 1900-2018 growth rate from pink to blue to match the color of the x-axises of the corresponding year range in the left panel. [Xiang Li, United States of America]	Accepted - clarification was made
41597	168	1	168	15	The right panel of the figure is confusing and I am missing the time period 0 - 1900 CE. I understand that the different colours represent the different time periods with black/gray being BCE, red being 0 - 1990 CE and blue representing 1900 - 2017 CE - why is that not the case in the right panel? [Katharina Meurer, Sweden]	Accepted - change was made.
4391	168	1	168	16	It seems that the 1900-2017 period (dark blue) is missing from the figure! [Ana Bastos, Germany]	Accepted. Changed.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
88503	168	1	168	16	The right panels of Fig. 5.1 (those with rates) are confusing. Some examples: - In the legend caption three different time periods are listed while on the figure we can see only two (1900-2018 and 800k-0). - What is the first grey bar on the CO2 growth rate? - It's not possible to see whether the first grey bars are there and close to 0 or not there for CH4 and N2O. - In the growth rate panel, the reference 800k-0k seems to be related only to the middle grey bar and not to the other? - What is the meaning of the standard deviations? [Damien Cardinal, France]	Accepted - clarification was made
88505	168	1	168	16	Is this growth rate panel part of Fig. 5.1 useful? It is fully redundant for CO2 with Figure 5.2. [Damien Cardinal, France]	Rejected - it remains because it is necessary for CO2, CH4 and N2O
88507	168	1	168	16	West Antarctic Divide is never defined in the legend caption of Fig. 5.1or in the main text [Damien Cardinal, France]	Noted - removed from the Figure in FGD
723	168	1	168	16	Fig 5.1a: The CO2 data in the older part (800-600 kyr BP) need to be updated with the revised data of the EPCIA Dome C ice core found in Bereiter, B.; Eggleston, S.; Schmitt, J.; Nehrbass-Ahles, C.; Stocker, T. F.; Fischer, H.; Kipfstuhl, S. & Chappellaz, J. Revision of the EPICA Dome C CO$_2$ record from 800 to 600 kyr before present Geophysical Research Letters, 2015, 42, 542-549, doi: 10.1002/2014GL061957. Actually, I made this comment already on the FOD, but it had not been improved! [Peter Köhler, Germany]	Accepted - change was made.
127871	168	3	168	3	In Figure 5.1, the first growth rate depicted in the bar chart on the right is not labeled. The caption mentions the period 0-1900 CE, but it is not clear if this is included in the bar chart. [Trigg Talley, United States of America]	Accepted - clarification was made
93423	168	3	168	3	In Fig. 5.1, is it necessary to remark, in blue, "West Antarctic Divide"? If so, Antarctic needs an extra 'c', and perhaps indicate why it is remarked in the figure legend. [Carles Pelejero, Spain]	Accepted - change was made.
55007	168	7	168	7	The figure caption refers to "1900-2017", whereas Figure 5.1 contains the text label "1900-2018". Either pick 2017 or 2018, whichever is the correct year. [Nancy Hamzawi, Canada]	Accepted - change was made.
112297	168	11	168	12	There are new high-resolution records of CO2, CH4, N2O that cover the last 1 or 2 ka. Please check figures in Ch. 2. [Jinho Ahn, Republic of Korea]	Accepted - Fig was improved
26941	168	12	168	12	We suggest to replace "surface observations" with "atmospheric observations" [Eric Brun, France]	Accepted - change was made.
116481	168		168		Very nice representaiton of ice core datasets. Would a start in 1850 reduce the impression of discontinuity in the middle panel? How is ice core smoothing accounted for in the growth rate calculations? There is redundancy with Figure 5.2, please consider carefully what is the main message. I think that the first panel could focus on the concentrations (past, present, future), and the second (simplified) panel on dates of changes. At the moment the two aspects are mixed, making the panels more difficult to understand intuitively. [Valerie Masson-Delmotte, France]	Accepted - have clarified the specific message for each figure
90133	168		168		This statement uploaded by Ted Schuur on behalf of a graduate class that reviewed the chapter (this was communicated with IPCC TSU in advance): Fig. 5.1. Y axis is not reader-friendly. I suggest e.g. CO2 150, 200, 250.... [Edward Schuur, United States of America]	Accepted - Fig was improved

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
55005	168		168		In looking at the growth rates for atmospheric CO2, CH4 and N2O, policy makers may get the false visual impression that recent (1900-2018) growth rates are not very different from growth rates in the past 800,000 years. The y-labels "Kyr^-1" and "yr^-1" indicate a factor of 1000 difference between the pink 1900-2018 bars and the grey bars (800k-0k). The factor of 1000 should be made more visible in the graph itself, or by explicitly drawing attention to it in the caption of Figure 5.1 [Nancy Hamzawi, Canada]	Accepted - figure was clarified
83991	168				On the right panel, what does the 1st grey bar represent? Shouldn't it be "the linear growth rates for different time periods (800,000–0 BCE, 0–1900 CE and 1900–2017 CE)" as mentioned in the caption, but this is not clear on the Figure. The time interval 800,000-0 BCE is present, but 0-1900 CE is not clearly marked and in the Figure the time interval indicated is between 1900-2018. [Marco Tulio Cabral, Brazil]	Accepted - clarification was made
81573	168				Antarctic divide is misspelled. 1990 CE should be adjusted to 2018 CE or similar in the caption. The updated data by Bereiter et al., GRL, 2015 should be considered [Fortunat Joos, Switzerland]	Accepted
84815	169	1	169	1	The plotted CO2 values prior to the ice core record are much more uncertain. Please add some error bands. [Martin Heimann, Germany]	Accepted - Fig was improved
21879	169	1	169	1	For me the growth rate inserts don't work because it isn't clear immediately what they denote and because their similar size leads to an inadvertent immediate impression that the y-axis scales are equivalent measures. There is thus considerable opportunity for readers to misinterpret these as shown. I would either remov them or show them in larger form in a third row of panels and include some vertical measure stick to provide an equivalence yardstick so that the fact the third change is far more significant is made obvious to the reader. [Peter Thorne, Ireland]	Noted - have taken those points into account
18137	169	1	169	1	In Figure 5.2 the blue text reads 'West Antartic Divide' but should read 'West Antarctic Divide' [Chelsey Baker, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change was made.
18139	169	1	169	1	Insets in Figure 5.2 b - the y axis label formatting is squashed and so the 'ppm' is hard to read. [Chelsey Baker, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - Fig was improved
725	169	1	169	5	Fig 5.2a: The CO2 data in the part (800-600 kyr BP) need to be updated with the revised data of the EPCIA Dome C ice core found in Bereiter, B.; Eggleston, S.; Schmitt, J.; Nehrbass-Ahles, C.; Stocker, T. F.; Fischer, H.; Kipfstuhl, S. & Chappellaz, J. Revision of the EPICA Dome C CO$_2$ record from 800 to 600 kyr before present Geophysical Research Letters, 2015, 42, 542-549, doi: 10.1002/2014GL061957. Actually, I made this comment already on the FOD, but it had not been improved! [Peter Köhler, Germany]	Noted, added
78553	169	1			update with SSPs. Choice of 2 is fine as it would clutter the figure to show many. 5-85 and 1-19 are the highest and lowest, or could show 3-70 and 1-26. [Chris Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change was made.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
58537	169	2	169	2	The plot of pre-800 ka CO2 concentrations in Fig 5.2a would be greatly improved by plotting the error bounds already given in the Foster et al (2017) - which did a very good job at estimating them. These error bounds could be easily used to infer the corresponding uncertainties on growth rate in Fig. 5.2b, and these uncertainty estimates mentioned in the body of the chapter, where relevant. This would be consistent with IPCC guidelines on uncertainties and terminology. The authors might also consider mentioning that the PETM is not shown in the caption - although I note that this is also mentioned in the text body itself. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - Fig was improved
93425	169	2	169	2	In Fig. 5.2, is it necessary to remark, in blue, "West Antarctic Divide"? If so, Antarctic needs an extra 'c', and perhaps indicate why it is remarked in the figure legend. [Carles Pelejero, Spain]	Accepted - clarification was made
26943	169	3	169	3	We suggest to add "Scenarios (see chapter 4)" after RCP8.5. [Eric Brun, France]	Accepted - change was made.
112299	169	5	169	5	Insert in Fig5.2(b): ice core records cannot have an annual resolution because of natural smoothing in the firn. I believe that the data for older than 1940CE came only from an ice core. [Jinho Ahn, Republic of Korea]	Noted - have added Further details on data sources and processing are available in the chapter data table (Table 5.SM.6).
116483	169		169		Could the figure include a level of scientific understanding. Check use of confidence language in the caption ("likely to strengthen") not italicized. [Valerie Masson-Delmotte, France]	Accepted - Fig was improved
112937	169		169		In compiling some information for FAQ1.3, I was struck by the complete absence of CO2 concentrations provided for the different scenarios. Here the RCP concentrations are given, but why not for the SSPs? I appreciate that they are quite a bit different conceptually, and require more nuance in presenting, than RCP scenarios, but I think we are doing a disservice to the report if these are not provided. This seems. the best palce do that. CH1 has a figure where these are presented graphically, but that should really just foreshadow a more detailed presentation in CH5, to my mind. [Kim Cobb, United States of America]	Accepted - change was made.
81575	169				I remain highly sceptical about this sweeping comparison of growth rates covering the entire past 60 Mio yrs. Is this somewhere published? Has this gone through peer review? There are serious issues with measurement resolution and with the age distribution of individual samples. This makes it very hard to compare current decadal-to-century scale growth rates with past growth rates, in particular for the pre-ice core period. There is work by Zeeby and colleagues on the PETM and related growth rates. There is some information from ice cores for the pre-20 ka period (e.g. from Ed Brooks group (Ahn,. Bauska..). [Fortunat Joos, Switzerland]	Accepted - Fig was improved
84813	170	1	170	1	Add in the caption to figure 5.3 that this represents the fast global carbon cycle (processes, pools, pathways, feedbacks etc.), relevant for the anthropogenic perturbation. [Martin Heimann, Germany]	Accepted - clarification made beyond the Active carbon cycle - caption as well.
104855	170	1	170	1	Figure 5-3: Ocean deoxygenation is identified as a negative feedback on atmospheric CO2. Why? Also, ocean warming should be identified as a negative feedback on atmospheric CO2. [Timothy DeVries, United States of America]	Rejected: they are identified as a positive feedbacks

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
88509	170	1	170	19	Fig. 5.3 Difference between purple and magenta is subttle, better use more contrasted colors for this figure to be readable when projected. [Damien Cardinal, France]	Accepted - done
109667	170	1	207	27	Have the figures been checked if they are accessible for color blind people? [Carolyn-Monika Görres, Germany]	Noted - colour table checks were made across the chapter.
26945	170	3	170	3	It is necessary to add the values of the fluxes otherwise we dont understantd the % given in the legende of the figure and in the text [Eric Brun, France]	Accepted: made the links clearer in the text
26947	170	3	170	3	on figure 5.3, the role of land use is not clear. Land-use change (towards agroecology and so) can result in a negative carbon feedback (from the atmosphere point of view). Here the word is used but there is no arrow associated to it. [Eric Brun, France]	Accepted: added agro-ecology
26949	170	8	170	8	We suggest to clarify which part of soil and permafrost is considered in this calculation. Is it the whole profile or only the few upper centimeters? [Eric Brun, France]	Noted - covered in Permafrost Cross Chapter Box
108831	170	8	170	15	This is inconsistent with TS 46 line 30. [Jason Donev, Canada]	Accepted - net imbalance of 0.4PgCy-1 is correct for 2009-2018
41599	170	10	170	10	The values for the ocean sink vary throughout the report. They have been 23 % on page 6 l. 45, 24 % on page 13 l. 41 and are now 22 % [Katharina Meurer, Sweden]	Accepted - should be 23%
108829	170	12	170	12	Is this Pg/year? [Jason Donev, Canada]	Accepted - PgC/y
41601	170	12	170	12	It was 0.4 Pg C imbalance on page 6 l. 47 [Katharina Meurer, Sweden]	Accepted - corrected to 0.5 PgC
40215	171	0			figure 5.4: is there a way to be clearer about what the shading in B is (without the need to read the entire long caption)? [TSU WGI, France]	Accepted
84805	171	1	171	1	The permed units in the figure 5.4 caption is wrongly defined. Better to convert the scale to ppm (as in previous WGI reports). [Martin Heimann, Germany]	Taken into account. Because the data providers distribute data in the per meg units, and also the publications are using that unit, it is better we use per meg
84807	171	1	171	1	The various delta/Delta units and permils etc. need to be defined somewhere (perhaps in a footnote). Else a lay person does not understand what is shown here. [Martin Heimann, Germany]	Accepted
84809	171	1	171	1	In previous reports we always plotted the MLO-SPO concentration difference against the fossil fuel emissions (or the difference in emissions in the northern and southern hemisphere). The very nice linear relation over now more than 50 years is a powerful piece of evidence of the anthropogenic perturbation; originally shown in Keeling et al., 1989. [Martin Heimann, Germany]	Taken into account - we already have shown FFCO2 in the airborne fraction discussion, and this is now well discussed in Ciais et al (Nature 2019). Thus we have kept in the text only
21881	171	1	171	1	Figure would be more accessible were in addition each panel to have a title and not just (a), (b) etc. [Peter Thorne, Ireland]	Accepted
58585	171	1	171	1	Figure 5.4a: I would match the magenta color of the trendline with the right axis color. Clarify what the right axis represents? Currently only read as "MLO-SPO (ppm)" ppm of what exactly? [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted
26951	171	7	171	9	The legend of panel (d) is missing [Eric Brun, France]	Accepted
51187	171	fig 5.4	171	fig 5.4	the caption could mention explicitly that the growth rate in panel (b) is an annual growth rate (ie the seasonal cycle has been removed) . [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Accepted
32015	172	0			"Athropogenic" emissions (in figure) - typo [Euan G. Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change was made.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
127873	172	1	172	1	Typo in figure. "athropogenic" should be "anthropogenic". [Trigg Talley, United States of America]	Accepted - change was made.
58587	172	1	172	1	It is unclear to me which line in the figure represents the sink rate that is being explained on page 19, lines 30-37. It seems like the sink rate is an important observable diagnostic, I think the readers could benefit to have that in the graph. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - change was made.
18151	172	1	172	1	Figure 5.5 - figure text reads 'athropogenic emissions' but should read 'anthropogenic emissions'. [Chelsey Baker, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change was made.
109553	172	1	172	1	Figure 5.4: AF trend line should include uncertainty bounds. [Anthony Walker, United States of America]	Accepted - changes are made
108833	172	1	172	3	Unclear, could these lines be re-worked. I had trouble understanding what was being said. [Jason Donev, Canada]	Accepted - changes are made
41603	172	1	172	4	Figure: please check spelling in the legend [Katharina Meurer, Sweden]	Accepted - change was made.
78555	172	1			what are the numbers in the caption (“linear fit Y=0.068X”) – is this the gradient of the fit? Include uncertainty estimate as it likely spans zero. [Chris Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - changes are made
55009	172		172		Considering the very large interannual variability in the airborne fraction of CO2 emissions, the slope (0.068) of the linear fit is probably not statistically different from zero. Given the lack of statistical significance of this slope (trend), the linear fit to the Airborne Fraction should be removed from Figure 5.5. [Nancy Hamzawi, Canada]	Accepted - change was made.
64587	172				Fig. 5.5: In upper legend, "Anthropogenic" is misspelled [Charles Curry, Canada]	Accepted - change was made.
86627	172				Fig5.5 Mlight be good to show sink efficiency as this is discussed in the text (and SPM). [Pierre Friedlingstein, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - Sink efficiency are calculated using GCP-2020 ESSD data
2223	172				Figure 5.5: Please add "n" for anthropogenic emissions in the legend/red line. [Abed El Rahman Hassoun, Lebanon]	Accepted - change was made.
21885	173	1	173	1	The right hand key lables to panel A are not obvious and may be better in an inline key in the top left vacant portion. Panel B font size is too small and the inline key is cramped and may be better below the panel. The green shading is hardly visible. An overaching title and titles to each panel would aid accessability [Peter Thorne, Ireland]	Accepted. Both Figures redrawn for better clarity.
23715	173	1	173	1	figure 5.6 b….resolution to be improved [Massimo Lupascu, Singapore]	Accepted. Figure redrawn to improve quality
58593	173	1	173	1	I would include the data from 2018 in the 5.6a graph, it seems the data is available, based on the paragraph lines 20-35 page 20. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Fig. updated.
58595	173	1	173	1	Keep consistency on either using capital letters for the graphs or lower case. So either 5.6A or 5.6a, throughout text and graph. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Figure redrawn to improve quality
4401	173	1	173	13	If DGVM data are from Friedlingstein et al. 2019, then the DGVM data should be from TRENDY v8, not v7. [Ana Bastos, Germany]	Accepted. Updated to new version.
37965	173	2			You need to increase the resolution of figure. [Junhee Lee, Republic of Korea]	Accepted. Figure redrawn to improve quality

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
2225	173				Figure 5.6: The resolution of both plots need to be improved. [Abed El Rahman Hassoun, Lebanon]	Accepted. Figure redrawn to improve quality
44661	174	1	174	24	Fig. 5.7 a: Incredible good match between models and data-products on the global mean. The data is treated differently than what we currently do in the GCB and besides slight differences in the area correction, the big difference is that in this Figure there is no river adjustment applied to the data-products. The actual number of the river flux adjustment is highly debatable and clearly needs updates and further work, but the 0.78 from Replandy et al 2018 is the best peer-reviewed published estimate that we have - and that's what our assessments should be based on. I strongly object the argument: 'the lines match already with the areal correction, so we don't need a river flux adjustment'. Yes, some sort of area correction is needed, but if we agree that the concept is F_cont = F_nat + F_river + F_ant and that the models capture F_nat+F_ant and the data-products capture F_cont; then the river adjustment is needed to compare apples with apples and is totally independent from the areal correction. [Judith Hauck, Germany]	Noted - the content of Fig 5.7 has been fully revised to bring all 6 data products to comparable area corrections, wind product ensembles and a pre-industrial river correction of 0.62PgCy-1 (average of Jacobson2007 and Resplandy2018)
44663	174	1	174	24	Figure 5.7 b: - that's exactly what I had hoped and asked for in the last round of reviews. My main comment is that - if that figure is mainly motivated to allow constraining the models, then the model estimates should be clearly distinguished and not go into the mean and uncertainty over a certain decade. I'd keep them in the figure as a reference, but maybe in a different color, and yes a mean with and without models would be good. [Judith Hauck, Germany]	Accepted - new figure highlights Fnet, Socean and Fant - models were included with a separate mean
44665	174	1	174	24	There are two individual estimates for the 1994-2007 period from ocean interior estimates and in the Table 5.A.1 I can see the following: The Gruber et al 2019 estimate goes in twice, once the anthropgenic uptake alone, and once the contemporary flux (i.e. Gruber ant corrected for natural non-steady state). Clearly, only the second one should go into the contemporary estimate. [Judith Hauck, Germany]	Accepted
44667	174	1	174	24	Fig. 5.7b: you call this the total CO2 flux, i.e. contemporary. Yet what it is, is the anthropogenically-perturbed sink, S_ocean (as in GCB) without rivers. Rivers are part of the contemporary flux though, so the correction for rivers would need to be applied in the other direction. Here, the models would need to be adjusted for that and the Gruber estimate, but not the data-products. (need to check for the other estimates). [Judith Hauck, Germany]	Accepted - this has been fully revised
44669	174	1	174	24	Fig 5.7 follow-on comment: at least subfigures a and b of the same figure should be consistent on which definition of the ocean carbon sink (contemporary, anthropogenic, anthropogenically-perturbed) is used and whether or not a river flux adjustment is taken into account. [Judith Hauck, Germany]	Accepted - This has been done in the text and captions

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
44671	174	1	174	24	Fig.re 5.7 b: in AR4, seven methods were used for the 1990s estimate, not all of these seem to be covered here (not sure whether there are updates for all of them, but certainly for the 1990s it should be consistent with the AR4 work. AR4 said: To assess the mean ocean sink, seven methods have been used. The methods are based on: (1) observations of the partial pressure of CO2 at the ocean surface and gas-exchange estimates (Takahashi et al., 2002); (2) atmospheric inversions based upon diverse observations of atmospheric CO2 and atmospheric transport modelling (see Section 7.2.3.4); (3) observations of carbon, oxygen, nutrients and chlorofluorocarbons (CFCs) in seawater, from which the concentration of anthropogenic CO2 is estimated (Sabine et al., 2004a) combined with estimates of oceanic transport (Gloor et al., 2003; Mikaloff Fletcher et al., 2006); (4) estimates of the distribution of water age based on CFC observations combined with the atmospheric CO2 history (McNeil et al., 2003); (5) the simultaneous observations of the increase in atmospheric CO2 and decrease in atmospheric O2 (Manning and Keeling, 2006); (6) various methods using observations of change in 13C in the atmosphere (Ciais et al., 1995) or the oceans (Gruber and Keeling, 2001; Quay et al., 2003) and (7) ocean General Circulation Models (Orr et al., 2001). --> CFCs and 13C does not seem to be used here anymore? [Judith Hauck, Germany]	Accepted - have updated where possible
44673	174	1	174	24	Figure 5.7b: The Gruber et al 2019 estimate seemed to follow the expectations from the atm CO2 increase pretty well. So, with this assumption, it could also be used to make estimates for the 90s and 2000s. Maybe worth checking with Nicolas Gruber? [Judith Hauck, Germany]	Noted - we have used the published estimates here
63621	174	1	174	24	Figure 5.7 is not consistent between the panels in the models and observation-based products used. There are differences in the adjustment of the products with river fluxes. The labeling of the observation-based products is inconsistent (observation-based products in a vs empirical models in b. I've had conversation with the people working on this part of the chapter and I am aware that revisions are in process. [Galen McKinley, United States of America]	Accepted - Figure and its content fully updated and made products comparable
21887	174	3	174	3	Panel b text and many symbols are barely legible [Peter Thorne, Ireland]	Noted - the figure has been redrawn
58609	174	4	174	4	Graph 5.7b is a bit confusing. I keep wanting to read with x axis as continous time steps, but some of the decades overlap, with the past ones. I would suggest making each line of evidence more delineated (larger separations between them, or instead of shading, use lines) from the other by making a separate graph or mentioning they are lines of evidence and not purely decades on the x axis. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted - the figure has been redrawn
84817	174	6	174	6	Why are atmospheric inversion results shown only for some of the time spans displayed? There are inversion results from several groups available for the whole period 1990-2019. [Martin Heimann, Germany]	Noted - the figure has been redrawn
37967	174	6			The author represents the observationally-based products as red color not blue color. [Junhee Lee, Republic of Korea]	Noted - the figure has been redrawn

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
26953	174	8	174	8	"blue" has to be changed with "pink" in the legend [Eric Brun, France]	Noted - the figure has been redrawn
88511	174	8	174	8	It's not blue, it's black [Damien Cardinal, France]	Noted - the figure has been redrawn
18105	174	8			No colour in the figure looks blue to me even though it is stated in the caption. [Vlad Macovei, Germany]	Noted - the figure has been redrawn
21889	175	1	175	1	Font sizes are too small in titles and the colour bar labels [Peter Thorne, Ireland]	Accepted - fonts and legibility improved
18153	175	1	175	1	The colorbar label units in the Figure 5.8 are hard to read. i.e the 'yr-1' reads as 'yr1' [Chelsey Baker, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - label made clearer
88513	175	1	175	8	Could illustration of errors on these estimates be highligted? i.e. which zones show no significant trend? (this could be the case for rates around 0 but also in regions where density of observations is too scarce. [Damien Cardinal, France]	Noted - the trends and variability are depicted in Figure 5.9
109203	175	1	175	10	Figure 5.8 could benefit from more contextual text, even just along the color legends -- does a positive flux mean the ocean or atm is the sink? [Steph Courtney, United States of America]	Accepted - The caption provides greater context explaining the context and specifics
37969	175	3			The title of Figure 5.8b is not matched with the caption. [Junhee Lee, Republic of Korea]	Accepted - changes made
26955	175	5	175	5	We suggest to replace "Observational data set" with "Observational pCO2 (air and sea) date set" [Eric Brun, France]	Accepted - Caption was clarified
26957	175	6	175	6	Please specifiy that (b) shows the storage of anthropogenic CO2 [Eric Brun, France]	Accepted - this was clarified
88515	175	6	175	7	add "anthropogenic" before CO2 to be consistent with panel b title. [Damien Cardinal, France]	Accepted - anthropogenic added
37971	176	2			There is no definition of the thick and thin lines and shadings. [Junhee Lee, Republic of Korea]	Accepted - resolved in caption
116485	176		176		what are "regional ocean biomes" as used in this figure? [Valerie Masson-Delmotte, France]	Taken into account - figure 5.8 defines the regions. Now noted in caption
21891	177	1	177	1	I would add a title and move the key box to the bottom of the figure [Peter Thorne, Ireland]	Accepted - key box was moved to the bottom.
109555	177	1	177	1	In the strongest possible terms, panel b and c need to be removed from Figure 5.10. NDVI is a metric of greenness, and a proxy of LAI. These are only tenuously related to the land carbon sink. They are also only slightly less tenuously related to NPP. These three different variables are (possibly deliberately) confused in the climate denial world (e.g. judithcurry.com/2020/05/14/greening-the-planet-and-slouching-towards-paris/). If the IPCC cannot be clear about the difference between these very different variables that creates confusion allowing misinformation to thrive. NPP trends based on satellite proxies of NPP should not be compared with process based models. The satellite estimates are based on simple light use efficiency (LUE) models that ignore the effect of CO2 on LUE (De Kauwe et al 2017). The effect of CO2 on LUE is well established at multiple scales thus the satellite estimates of NPP cannot be used for assessing trends when CO2 is changing. De Kauwe, M. G., Keenan, T. F., Medlyn, B. E., Prentice, I. C., & Terrer, C. (2016). Satellite based estimates underestimate the effect of CO2 fertilization on net primary productivity. Nature Climate Change, 6(10), 892–893. [Anthony Walker, United States of America]	Taken into account - the satellite NPP was replaced by several GPP products from Cheng et al. (2017) and from the MODIS product. However, the greenness metrics remain since Chen et al. (2020) has demonstrated the linkage between greening signal and increasing photosynthesis carbon uptake and the land sink. While we agree that De Kauwe et al. made a very good point on satellite-derived products, it is still valid to show the trend of satellite-derived GPP, which was widely used in current literature and shows that even without well considering the physiological effect of CO2, global GPP still show sizable increment.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
109557	177	1	177	1	Follow up to above. The NPP point made in Figure 5.10 is not discussed anywhere in the text, further supporting the comment above that panel b and c should be deleted from Fig 5.10. [Anthony Walker, United States of America]	Accepted - NPP was replaced by GPP, which was discussed in length in the text.
41611	177	1	177	49	Figure 5.10: this is a very good figure and the grey arrows to the right of the plots make it very easy to understand! [Katharina Meurer, Sweden]	Accepted - thanks.
130511	177		177		Figure legends are not clear for Fig 5.10 a, c. [Panmao Zhai, China]	Accepted - figure legends was revised.
71559	177				I believe that the results of Kondo et al. (2020) should also be included in Figure 5.11. As this paper carefully removes the factors that make the results of the top-down and bottom-up analyses different. [Takashi Maki, Japan]	Rejected - Kondo et al. (2020) is a good piece of work for concept reconciliation, but their data source were the same to what is presented in Figure 5.11.
81577	177				Fig. 5.12: I guess marine biota reservoir of 3 PgC should also be in a white area. [Fortunat Joos, Switzerland]	Rejected. The marine biota of 3 PgC is represented by a white area. Its white area is just too small compared to others (e.g. 700 PgC of DOC).
81579	177				Fig. 5.12:Is there a reason why on land GPP and AR+HR is shown while in the ocean NPP and HR is shown. I would suggest to show GPP and total resp. for the marine biota for consistency. Is the burial flux with 0.2 PgC/yr not too small.? I would rather expect a best estimate of about 0.4 PgC/yr from CaCO3 burial (~0.2 PgC/yr) and POC burial (~0.2 PgC/yr) [Fortunat Joos, Switzerland]	Rejected. This choice was made according to the availability of reliable data sources.
21893	178	1	178	1	Much of the text is too small or in colours that are hard to read. The key box should likely move to bottom of figure and a self describing title for the figure as a whole be added [Peter Thorne, Ireland]	Taken into account - the key box was moved to the bottom.
18155	178	37	178	38	The figure caption text reads ' land surface air temperature (T) at the globe or'. Change to 'land surface air temperature (T) globally or' [Chelsey Baker, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change was made.
41615	178	46	178	47	Figure 5.11: Despite the figure being very informative and pedagogic, I think that this sentence is confusing and should maybe be removed from the caption [Katharina Meurer, Sweden]	Taken into account - the sentence was removed.
116487	178		178		check the coherency of ENSO index with other chapters (2, 3, 4 etc) [Valerie Masson-Delmotte, France]	Accepted - we now used Nino 3,4 in order to be consistent with Chapter 4.
3919	179	0	179	0	Fig. 5.12 Color of circles for atmospheric CO2 increases is wrong: (present-wrong) Pre-industrial: pink, Anthropogenic: Green, (correct) Pre-industrial: Gren, Anthropogenic: Pink [Makio Honda, Japan]	Noted - Figure was changed
18203	179	1	179	1	Figure 5.12 - It is unclear what numbers in the two boxes above 'Net land flux' and 'Net ocean flux' are referring to. Why are there two numbers, i.e. 0.75, 3.2 +/- 0.6? Does the +/- 0.6 apply to both numbers of just one? If the numbers are representing the anthropogenic versus natural fluxes perhaps also colour coding the text would make the figure easier to digest? [Chelsey Baker, United Kingdom (of Great Britain and Northern Ireland)]	Noted - Figure was changed
69201	179	1	179	1	In order to avoid confusion with the remaining carbon budget in Figure 5.12, the phrase "Carbon Budget" is suggested to be changed to the "Global Carbon Cycle", as in the Figure 6.1 of AR5/WGI. Also, the "Methane Budget" and the "Nitrous Oxide Budget" in Figure 5.14 (p.181) and 5.17 (p.186) is suggested to be changed to "Methane Cycle" and "Nitrous Oxide Cycle" appropriately. [Kaoru Magosaki, Japan]	Noted - Figure was changed
21895	179	1	179	1	Text is barely legible at the font size used [Peter Thorne, Ireland]	Noted - Figure was changed

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
88523	179	1	179	1	Colour codes are wrong for the atmospheric reservoir at the top of the figure 5.12: 589 should be green (or even more consistent with stock it should be in a white circle) while 275 should be pink! [Damien Cardinal, France]	Noted - Figure was changed
64459	179	1	179	1	The caption of fig. 5.12 is fine, but the drawing is not entirely satisfactory to represent inland and coastal waters. My suggestion is to define an ecosystem near the river mouth and call it "rivers, estuaries, coastal zones", a bit similar to what is done in Fig 5.17 for N2O. Then remane the vertical arrow pointing to atmosphere as "net outgassing" (since it results from the balance between river-estuarine emissions and coastal vegetation-shelf uptake) and rename the horizontal arrow "export". In this way it would be clear that the export is to the open ocean., after transit through estuaries and coastal zones. [pierre regnier, Belgium]	Noted - Figure was changed
88525	179	1	179	1	Why is the 589 PcG natural atmospheric reservoir included in the dashed circle identifying anthropogenic change? [Damien Cardinal, France]	Noted - Figure was changed
88527	179	1	179	1	For sake of clarity of the figure 5.12, better use colour code blue/green vs. pink also for numbers, e.g. for land flux 114, 113.2, 0.75 should be blue while 24, 20.8, 3.2 should be pink [Damien Cardinal, France]	Noted - Figure was changed
88531	179	1	179	1	For the ocean part, it's quite weird that DOC has a special box and is spelled out while the main C reservoir on Earth is marine DIC and not spelled out in surface, deep and intermediate ocean. Similary the POC is not mentioned next to the biological C pump estimated at 11 Pg C /yr [Damien Cardinal, France]	Noted - Figure was changed
88533	179	1	179	1	The marine component of the Fig. 5.12 should really be improved. Open white circles sometimes refer to space (e.g. surface ocean, sediments, deep & intermediate) and sometimes to chemical speciation of C (DOC), some stocks are in the middle of nowhere (e.g. marine biota). [Damien Cardinal, France]	Noted - Figure was changed
88535	179	1	179	1	The anthropogenic change of + 165 +/-20 in the ocean dashed circled is isolated from any pink arrow or reservoir. Unclear what it describes, increase of deep & intermediate DIC via physical pump? Then why no pink arrow is displayed between surface and deep ocean? [Damien Cardinal, France]	Noted - Figure was changed
88537	179	1	179	1	Some dashed circles too small to be visible e.g. -20 for vegetation, + 165 in the ocean. Better display them in pink since they are anthropogenic. [Damien Cardinal, France]	Noted - Figure was changed
109559	179	1	179	1	Figure 5.12 the colours of the atmosphere boxes appear to be opposite to the description. [Anthony Walker, United States of America]	Noted - Figure was changed
86769	179	1	179	36	Figure 5.12. Please consider adding this figure, or a slightly simplified version, to either the Summary for policymakers or at least in the Technical Summary. Together with 5.14 (CH4) and 5.17 (N2O), this would give a very good overview of the fluxes between atmosphere, land and ocean of these important greenhouse gases. [Oyvind Christophersen, Norway]	Noted - Figure was changed
41617	179	1	179	37	Figure 5.12: From the caption of Table 5.1 I understood that negative fluxes are equivalent to a gain of C in the respective reservoir. So in contrast to other figures, it is not the atmosphere that is in the main focus, but the change within each reservoir. I suggest to highlight this in the caption of Figure 5.12 right at the beginning to avoid confusion [Katharina Meurer, Sweden]	Noted - Figure was changed

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
103169	179	1			Fig. 5.12, unclear why there is a separation between anthroponic and natural pool in atmosphere. Is the "natural" pool reflecting pre-industrial situation? If yes, it should be the larger of the two circles, not the smaller one (global concentrations increased from 265 to ~407 ppm, so "pre-industrial" is larger than the increment) [Philippe Tulkens, Belgium]	Noted - Figure was changed
78557	179	1			fig 5.12 anthro and natural circles for atmospheric CO2 size are the wrong way round (589 is the natural – shold be blue) [Chris Jones, United Kingdom (of Great Britain and Northern Ireland)]	Noted - Figure was changed
78559	179	1			fig 5.12 “pink arrows represent anthropogenic…” – does this mean “perturbations to natural fluxes?” – land/ocean sinks are not anthropogenic fluxes, but the response of the natural fluxes to our emissions [Chris Jones, United Kingdom (of Great Britain and Northern Ireland)]	Noted - Figure was changed
103171	179	2			Green arrows [Philippe Tulkens, Belgium]	Noted - Figure was changed
26959	179	4	179	4	Please specify what is soil in "soils 1,700 ± 250" . These values are not in Batjes 2016. Same for permafrost. The reference says 1778 ± 198 (thus 1800 ± 200) for the upper 150 cm of soil and 30% of the total upper 2 meters (id est 30% of 2060 ± 217 = 607 ± 87) in the Northern Circumpolar Region (thus permafrost). Does this mean that more than 2 meters are taken into consideration for permafrost but less than 2 meters for "regular" soil? [Eric Brun, France]	Noted - Figure was changed
84819	179	4	179	4	The colouring of the atmospheric circles is wrong. [Martin Heimann, Germany]	Noted - Figure was changed
84821	179	4	179	4	The labels in the surface ocean and intermediate/deep sea circles should indicate that this is inorganic carbon (e.g. DIC) [Martin Heimann, Germany]	Noted - Figure was changed
84823	179	4	179	4	Why are there no ocean carbon stock changes shown? It is important to note that the anthropogenic carbon stock changes in the ocean are not in the organic or marine biota, nor in the ocean surface sediment pool. [Martin Heimann, Germany]	Noted - Figure was changed
88529	179	4	179	4	Is that colour really blue?? Looks more greenish to me…. [Damien Cardinal, France]	Noted - Figure was changed
108835	179	8	179	10	This is a reasonable estimate, but the text that refers to figure 5.12 should explain this in more detail. [Jason Donev, Canada]	Noted - Figure was changed
103173	179	11	179	14	Figure 5.12, Needs reviewing as a full stop seems to be missing after (Friedlingstein et al., 2019). [Philippe Tulkens, Belgium]	Noted - Figure was changed
18201	179	26	179	26	There is either a phrase missing or an extra 'and'. [Chelsey Baker, United Kingdom (of Great Britain and Northern Ireland)]	Noted - Figure was changed
51191	179	26	179	26	expression/word missing before "and" [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Noted - Figure was changed
88521	179	26	179	27	The term 'mixed layer' is particularly misleading here since in Emerseon & Hedges (1988) it refers to bioturbated surface sediments (i.e. 10-20 cm) which has no relation to the surface surface ocean layer shown in this figure 5.12. Replace in the legend 'organic carbon in the mixed layer' by 'organic carbon in the surface sediment' [Damien Cardinal, France]	Noted - Figure was changed
88519	179	26	179	28	It is very unclear from where the figure of 150 PgC is coming. Emerson & Hedges (1998) caculate a range of POC between 36 to 65 Pg POC in margin + open ocean surgace sediments. [Damien Cardinal, France]	Noted - Figure was changed
3921	179	28	179	28	(Archer et al. 1998) => not subscript [Makio Honda, Japan]	Noted - Figure was changed

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
23717	179	28	179	28	Figure 5.12 caption "Archer et al., 1998" is subscript [Massimo Lupascu, Singapore]	Noted - Figure was changed
88517	179	28	179	28	Typo: Archer et al. (1998) is in subscript instead of normal text [Damien Cardinal, France]	Noted - Figure was changed
51189	179	Fig 5.12	179	Fig 5.12	for atmosphere carbon stocks, the colour code here is incorrect: 589 should be in a white disk (pre-industrial stock) and 275 in a dashed circle (anthropogenic change thereof). [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Noted - Figure was changed
116489	179		179		Could it be possible to highlight on this panel what is new since AR5, and possibly what is the level of confidence for each aspect with a small symbol? [Valerie Masson-Delmotte, France]	Noted - Figure was changed
86629	179				Fig 5.12 Colour code (red/blue) is wrong for atmosphere! 589 is natural, 275 is anthropogenic [Pierre Friedlingstein, United Kingdom (of Great Britain and Northern Ireland)]	Noted - Figure was changed
32017	180	0			I assume these are all standardised on the WMO X2004ACH4 global calibration standard for NOAA network in situ measurement? Caption doesn't say: wouldn't show in the scale of the graph but maybe should be stated. Also maybe note in caption that CH4 varies with latitude. [Euan G. Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - we have mentioned the measurement scale
7343	180	1	180	2	Can you please increase the resolution of this figure for the final report? Same goes for some of the other figures (e.g. 5.3, 5.6, 5.31, 5.34, 5.35, 5.37) [Svenja Halfter, Australia]	Accepted - Improved figure resolution
71715	180	3	180	14	(Fig 5.13) As noted earlier this appears to be cited as Fig 5.12 in Box 5.1. [Martin Manning, New Zealand]	Accepted - changes are made
18209	181	1	181	1	I think Figure 5.14 should have a figure caption more in line with 5.12 so that it can be understood without reading the text/seeing previous similar figures. [Chelsey Baker, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - changes are made
21897	181	1	181	1	Some of the text is too small to be readily legible [Peter Thorne, Ireland]	Accepted - the font size of the text increased.
86771	181	1	181	5	Figure 5.14. Please consider adding this figure, or a slightly simplified version, to either the Summary for policymakers or at least in the Technical Summary. Together with 5.12 (carbon) and 5.17 (N2O), this would give a very good overview of the fluxes between atmosphere, land and ocean of these important greenhouse gases. The caption of this one seems to need some more work when compared to the caption of Fig. 5.12. [Oyvind Christophersen, Norway]	Accepted. We shall try to achieve this for better visibility of the Chapter 5 outcomes
32019	181	1			Fig 5.14 These values seem to differ in many ways from table 5.2 referenced in the caption and on p5-34 line 45. Geological sources may be less! Should show loss to stratosphere in the figure. Inclusion of O1D is confusing: also key to OH. [Euan G. Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - we have matched the numbers
88231	181	1			Fig. 5.14 - Methane release from shallow permafrost soils is not obvious in the figure. I realize methane release is asociated with wetlands that may result from permafrost thaw but not really obvious in the figure. [Sharon Smith, Canada]	Rejected. This figure is already crowded and we have received comments to increase the font size etc. To maintain simplicity only the major sectors are shown.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
108837	181	3	181	6	Tg are an uncommon unit, parenthetically comparing this to tonnes would help. [Jason Donev, Canada]	Accepted - Tg is now defined in the figure caption. But putting them on the infographic will obscure clarity with so many numbers already
37973	181	4			You need to increase the resolution of figure. [Junhee Lee, Republic of Korea]	Accepted - Improved figure resolution
51193	181	fig 5.14	181	fig 5.14	if not already the case (typo?), suggest it wouldbe better to present the 2008-2017 average rather than 2010-2017 average for consistency with figure 5.4 (even if that requires some additional assumptions) and cross-chapter box 5.1 [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The choice of the time period of the final decade is made to maintain uniformity across the 3 species, CO2, CH4 and N2O.
116491	181		181		Could it be possible to highlight on this panel what is new since AR5, and possibly what is the level of confidence for each aspect with a small symbol? [Valerie Masson-Delmotte, France]	Rejected. Interesting idea - but the figure is already too crowded. Any further complexity would be difficult to convey
86631	181				Fig 5.14 Same colours scheme as for CO2 would be nice. [Pierre Friedlingstein, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - we agree with your suggestion, but different colour schemes are used for CO2, CH4 and N2O intentionally to give distinction
58617	181				It would be good if you could add an estimate of the stocks variations as it has been done for the C-Budget [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable. Not clear what you mean by stocks variations.
21899	182	1	182	1	A title would help enormously increase accessability of this figure [Peter Thorne, Ireland]	Not applicable - this figure is revised and simplified. Because there are x-axis values at top and bottom, we haven't add a title
32021	182	1			Box 5.1 Fig1 - is this really proven? Especially for the past decade, given the great uncertainty in what is driving the present rise, and how to tell cows from wetlands in the tropics. Also why does the panel break take place at 2008/9 instead of 2006/7 when the growth started. It would be better to use 1997-2006, then 2007-present. Also explain what Growth rates Tg/y numbers refer to: burden? emissions TD? or emissions BU? [Euan G. Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - ten-year means are shown in the plot. We have simplified the plot by removing the bottom-up estimations. Not all sectorial emission changes are proven, but we are able to show using models in Box 5.1 Fig 2 (revised) that the wetland emissions are unlikely to have changed in a way that can explain the CH4 growth rate variabilities in the inter-decadal time scales.
84825	182	3	182	3	This is a poor figure. It attempts to show the changing magnitudes of the CH4 sources and sinks in the different time intervals corresponding to the observed features in the atmospheric concentration record. But differences in the displayed bars are hardly visible. A better way of showing this would be to plot anomalies from the long-term mean. [Martin Heimann, Germany]	Accepted - changes are made to expend the bars. Also the source-sink imbalances are shown.
116493	182		182		What is the main message from this figure? It is hard to understand the driver for changes in methane growth rate. [Valerie Masson-Delmotte, France]	Taken into account - the figure is simplified to send the main message how CH4 sources and sinks produce the budget imbalance that is observed as CH4 growth rate in the atmosphere

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
84013	183	1	183	1	Targetting individual countries shall be avoided. This is not the commom in IPCC method. [Marco Tulio Cabral, Brazil]	Taken into account - it is true that the name of Brazil came up in the discussion here. Please note that this for purely scientific purpose to state how well we are able to track emissions from different regions by our regional emission assessment models. Similar occurrence can also be found in AR5, Section 5.3.3.2, page 509
38325	183	1	183	9	The boundary lines of East Section and West Section of China-India Border are wrongly placed in Cross-Chapter Box 5.1 Figure 2. In order to avoid unnecessary disputes, it is suggested to delete them from the Figure. [Yaming LIU, China]	Taken into account - Please note that the regional boundaries are not strictly country specific. The grid are made at 1x1 deg resolution, which is coarse for any country boundary and grouping of countries are done based on ecoclimatic characteristics rather than political boundaries. We have made a mention in the caption
41625	183	1	183	11	Cross-Box 5.1, Figure 2: It is not clear to me if the figure present solely model results or if the results from 19 sites (Chandra et al., submitted) are measured [Katharina Meurer, Sweden]	Accepted - we have used many models of CH4 inversions in making this plot. Chandra et al., is shown as separate line for longer period (1988-2016).
32023	183	1			Should this also consider the global maps in Miller et al? See their Fig 1 especially. 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), 1-8. [Euan G. Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - we think showing the gridded emission distribution is very useful here. We are already running short of space in this 2page document and the gridded trends in XCH4 as in Miller et al. are not straightforward as that of the regional emissions trends shown by the time series already
40209	184	0			Figure 5.15: it is weird not to have the time axis of the subpanel aligned with the rest of the time, to better compare the evolution of both the abundance and growth rates [TSU WGI, France]	Accepted - figure changed
18211	184	1	184	1	Figure 5.15 part c) the legend is obscurring some data points. Can the legend be moved outside of the plots - perhaps a long thin box at the top of the plot with 2 markers in each column. [Chelsey Baker, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - figure changed
41629	184	1	184	11	Figure 5.15: The legend covers some of the data points in subplot c and should be moved to next to the plot, if possible [Katharina Meurer, Sweden]	Accepted - figure changed
18213	185	1	185	1	Figure 5.16 - the Arctic is white on the figure but there is no indication of what this means. Is there no data available? If no, I would mark it as such as done in previous similar plots in this chapter. [Chelsey Baker, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - figure revised to remove ambiguities
84015	185	1	185	1	Targetting individual countries shall be avoided [Marco Tulio Cabral, Brazil]	Accepted - Figure was revised
38327	185	1	185	10	The boundary lines of East Section and West Section of China-India Border are wrongly placed in Figure 5.16. In order to avoid unnecessary disputes, it is suggested to delete them from the Figure. [Yaming LIU, China]	Rejected - the border is the line between two regions, not the border between the states of China and India

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
18215	186	1	186	1	Figure 5.17, as with Figure 5.14, should have a figure caption similar to 5.12 so that it can be understood separately from the text. Can it be clarified why there are only atmosphere stocks? I think the key for the stocks are also incorrect - for example should the atmosphere stocks not be a white dashed line similar to the previous budget figures. Is there a logical reason why the natural versus anthropogenic fluxes have different coloured arrows between figures 5.12, 5.14 and 5.17. Consistency would aid the lector. [Chelsey Baker, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - a caption has been edited. The figures have been edited for consistency
86773	186	1	186	5	Figure 5.17. Please consider adding this figure, or a slightly simplified version, to either the Summary for policymakers or at least in the Technical Summary. Together with 5.12 (carbon) and 5.14 (CH4), this would give a very good overview of the fluxes between atmosphere, land and ocean of these important greenhouse gases.The caption of this one seems to need some more work when compared to the caption of Fig. 5.12. [Oyvind Christophersen, Norway]	Noted. The figure caption has been revised. Due to length constraints, the budget figures have not been included in TS and SPM
116495	186		186		Could it be possible to highlight on this panel what is new since AR5, and possibly what is the level of confidence for each aspect with a small symbol? [Valerie Masson-Delmotte, France]	Rejected - while this is an appealing idea, this would overcomplicate the already complex graphic
86633	186				Fig 5.17 Same colours scheme as for CO2 would be nice. [Pierre Friedlingstein, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - The figures have been edited for consistency
32025	187	1			Fig 5.18 and Fig 2.10 don't really talk to each other; might need some joint modification. Also Fig 5.18 should consider spin offs from methane such as O3. The relationship between Fig 5.18 and Fig 7.9 needs a lot of though. See my very detailed comments on page 42. [Euan G. Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - changes are made for better compliance between Ch2 & Ch7
38329	188	1	188	9	The boundary lines of East Section and West Section of China-India Border are wrongly placed in Figure5.19. In order to avoid unnecessary disputes, it is suggested to delete them from the Figure. [Yaming LIU, China]	Taken into account - Please note that the regional boundaries are not strictly country specific. The grid are made at 1x1 deg resolution, which is coarse for any country boundary and grouping of countries are done based on ecoclimatic characteristics rather than political boundaries. We have made a mention in the caption
55011	188	1	188	9	The text "Value = 10.1, 4.9" is confusing. Is it indicating that the East Asia CO2 flux is off the scale? Either this needs to be clarified in the figure caption or the figure needs to be modified. [Nancy Hamzawi, Canada]	Accepted - Figure revised and caption added to clarify that the bar height above the y-axis range which is chosen for showing the other regions and CH4/N2O emissions reasonable well
41647	188	1	188	14	Figure 5.19: The meaning of "Value = 10.1, 4.9" is not clear. Does it relate to the emissions from East Asia? [Katharina Meurer, Sweden]	Accepted - Figure revised and caption added to clarify that the bar height above the y-axis range which is chosen for showing the other regions and CH4/N2O emissions reasonable well

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
103175	188	1			Please check unit. Is it Pg CO2-eq? Otherwise consisntently Pg C is used, would be good not to change units [Philippe Tulkens, Belgium]	Not applicable - because 3 gases of different molecular composition and physical properties for radiative forcing are shown in this plot, such a unified unit is adopted (as is also done by the UNFCCC).
103177	188	1			Makeup of graph makes it almost impossible to read. Use instead a set of much larger bar-graphs, possibly a small map aside as one panel. Global total is missing. GTP50 (for CH4 only) may not be needed - leave that discussion to chapter 7 [Philippe Tulkens, Belgium]	Accepted - all changes are made. Thank you
32029	188	1			I'm rather puzzled by this figure. For CH4 it seems very different from the real world, and is hard to reconcile with some of the major post-2007 growth latitudinal zones shown in the NOAA sine latitude plots. It also looks odd for CO2 in places. From west to east 1. the upper Amazonian wetlands of Bolivia, and the Panatanal, some of the world's largest tropical wetlands (plus lots of cows). 2. The Sudd and dense cattle of moist northern tropical Africa. Ethiopia has one of the world's largest cattle populations (maybe more than the USA), and S. Sudan and the whole belt across to Senegal are heaving with cows, fires, (plus the Sudd). Lunt, Mark F.; Palmer, Paul I.; Feng, Liang; Taylor, Christopher M.; Boesch, Hartmut; Parker, Robert J.. 2019 An increase in methane emissions from tropical Africa between 2010 and 2016 inferred from satellite data. Atmospheric Chemistry and Physics, 19 (23). 14721-14740. https://doi.org/10.5194/acp-19-14721-2019. 3. In Southern Africa there are the very productive wetlands of the Upper Congo and Upper Zambesi in Zambia, the huge biomass burning input from Angola across to Tanzania and Madagascar. All these are major source regions for methane, yet in the figure they are very small. Note also for CO2 in Southern Africa that South Africa is a global-scale coal producer and has some of the world's biggest point source emitters of CO2 in SASOL and the very large 5GW coal power stations, while Zambia is undergoing intense deforestation for charcoal, though I agree other areas of Southern Africa are greening up. [Euan G. Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable - We think the confusion comes from the units of "Pg of CO2-equivalent per year" used in this plot. Normally CH4 emissions are expressed in Tg-CH4/yr. Regional total emissions from South America or Africa each are more then 80 Tg-CH4/yr (ref. Box 5.1 Fig. 2).
88233	188	1			Fig. 5.19 - Boreal North America - Is "Boreal" meant to be equivalent to "northern" rather than refer to vegetation zone (boreal forest). The region shown includes the area above tree line. [Sharon Smith, Canada]	Taken into account - The boreal terms is used here to mean boreal (cold high latitude) zone, not related to boreal vegetation alone. Similarly we use Boreal Asia
32027	188	8			Typo. 'Mean from of' [Euan G. Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - changes are made
2227	188	8			Figure 5.19: Please remove "from" in the sentence. [Abed El Rahman Hassoun, Lebanon]	Accepted - change was made.
33379	188				Figure 5.19. Why the following annotation (value= 10.1, 4.9) is inserted in the graphic? [Guiomar Rotllant, Spain]	Accepted - Figure revised and caption added to clarify that the bar height above the y-axis range which is chosen for showing the other regions and CH4/N2O emissions reasonable well

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
86635	188				Fig 5.19 Not sure why you call this attribution. Attribution to what ? Sounds like just a regional distribution to me. [Pierre Friedlingstein, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Text changed to reflect this
81581	188				Fig. 5.19: It would be advisable to use the CO2* GWP* approach developed by Allen et al. 2018 10.1038/s41612-018-0026-8 as updated by Cain et al. In order to properly reflect hte radiative forcing impacts of short-lived gases. [Fortunat Joos, Switzerland]	Rejected. Appropriate suggestion, but for the consistency with AR6 metric and relevance to the UNFCCC we have used the well accepted metrics here
90135	189	1	189	9	This statement uploaded by Ted Schuur on behalf of a graduate class that reviewed the chapter (this was communicated with IPCC TSU in advance): Figure 5.20 was understandable. I wonder why those particular sites were chosen to include pH data? [Edward Schuur, United States of America]	Noted - they were chosen because trends of ocean acidification at these time-series sites have been provided in peer-reviewed published literature.
18165	189	2	189	2	Figure 5.21 - The colorscale and tick position may be misleading. For example - would a value of 8.0 plot as dark blue or light blue? Either reposition the ticks or double the amount of colors in the colormap. [Chelsey Baker, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - figure was revised.
36397	189	6	189	6	Figure 5.20 climatological mean pH was not published in Iida et al 2015. However, a climatological mean map of global pH distributions has been provided by Jiang et al 2019. {Citation: Jiang, L.-Q., B.R. Carter, R.A. Feely, S.K. Lauvset, and A. Olsen (2019): Surface ocean pH and buffer capacity: Past, present and future. Sci. Rep., 9, 18624, doi: 10.1038/s41598-019-55039-4}. I suggest that you use the pH distribution from their publication. [Adrienne Sutton, United States of America]	Accepted - global annual mean pH in 2000 in Jiang et al., 2019, was used.
30583	189	6	189	6	Figure 5.20 has NOT been published in Iida et al 2015. The Figure has not been described in any paper that I am aware of. On the other hand, a climatological mean map of global pH distributions has been provided by Jiang et al 2019. {Citation: Jiang, L.-Q., B.R. Carter, R.A. Feely, S.K. Lauvset, and A. Olsen (2019): Surface ocean pH and buffer capacity: Past, present and future. Sci. Rep., 9, 18624, doi: 10.1038/s41598-019-55039-4}. I suggest that you use the pH distribution from their publication. [nina bednarsek, United States of America]	Accepted - see the response to the comment 36397.
21901	190	1	190	1	Much of the font is barely legible. Lack of overall figure title and accessible panel titles. [Peter Thorne, Ireland]	Accepted - font has been made bold to increase visibility
90137	190	1	190	6	This statement uploaded by Ted Schuur on behalf of a graduate class that reviewed the chapter (this was communicated with IPCC TSU in advance): I felt that Figure 5.21 was very cluttered. It was difficult to understand how they were portraying the changes in pH. The contour lines (indicating pH levels from 2002) were also confusing. I think if the figure is explained more thoroughly, it would make sense. [Edward Schuur, United States of America]	Rejected - the figure was revised for better visibility but the way of representation was unchanged because the same way of representation has been used in the peer-reviewed paper of Lauvset et al. (2020).
99437	190	4	190	4	Figure 5.21. Poor quality in the Y axis of the figures and titles too close to the figure. Arctic, N. Atlantic, etc should be in white font to make it visible over dark grey background. [Isabel Seguro, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - the figure titles have been moved further away from the panels. Regional identifiers are now in white as suggested. A label is added to the y-axis, and the font has been increased.
26961	190	4	190	6	Please indicate how the different variables are calculated : from insitu DIC, Nutrient data by X method, or by model ? [Eric Brun, France]	Accepted - a sentence "as evaluated from the data of ship-based measurements of physics and biogeochemistry in the oceans"..

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
36399	190	6	190	6	The reference for Figure 5.21 is; Lauvset, S.K., B.R. Carter, F.F. Perez, L.-Q. Jiang, R.A. Feely, A. Velo, and A. Olsen (2020): Processes driving global interior ocean pH distribution. Global Biogeochem. Cycles, 34(1), e2019GB006229, doi: 10.1029/2019GB006229 [Adrienne Sutton, United States of America]	Accepted - the reference was updated
30585	190	6	190	6	The reference for Figure 5.21 is; Lauvset, S.K., B.R. Carter, F.F. Perez, L.-Q. Jiang, R.A. Feely, A. Velo, and A. Olsen (2020): Processes driving global interior ocean pH distribution. Global Biogeochem. Cycles, 34(1), e2019GB006229, doi: 10.1029/2019GB006229 [nina bednarsek, United States of America]	Accepted - text was revised.
2353	190				Figure 5.21: It is possible to add the variations during the last 17-18 years (after 2002)? [Abed El Rahman Hassoun, Lebanon]	Rejected - the same as the response to the comment #2229.
33381	190				Figure 5.21. Labelling in the figure could be improved. [Guiomar Rotllant, Spain]	Accepted - the size of labels on both x-axis and y-axis were increased. A label to the y-axis to identify what the numbers indicate was added.
2229	190				Figure 5.21: Is it possible to add the variations during the last 17-18 years (after 2002)? [Abed El Rahman Hassoun, Lebanon]	Rejected - more recent data have been used for this analysis but data have been normalized to the year 2002. No assessment for global ocean acidification in the ocean interior has been made for the years after 2002.
21903	191	1	191	1	Text is utterly illegible at 100% projection. To discern text needs zooming to 200%. Suggest changing to a portrait top-bottom configuration. Place titles above and not below the panels. Add an overarching self-describing figure title to aid accessability [Peter Thorne, Ireland]	Accepted. We have increased the font size on the figure legends
7345	191	1	191	2	We need to increase the font size in the figure. Same goes for figure 5.36 [Svenja Halfter, Australia]	Accepted. We have increased the font size on the figure legends
55013	191	1	191	9	(1) Figure 5.22 shows the performance of ESMs relative to the mean model performance. However, the scale ranges from “worse value” to “better value” and lacks any quantitative measure. This makes it impossible to know how much worse or better a model performs. Please add the corresponding numbers, i.e. relative scores. [Nancy Hamzawi, Canada]	Accepted -- we have added the numbers that document relative scores.
55015	191	1	191	9	(2) Figure 5.22: One of the measures for computing the scores is the centrailized root mean square error (crmse, equation 17 in Collier et al, 2019). This equation compares the modeled and observed values that occur on the same date. This approach is fine when forcing a land surface model offline with observed meteorological data. In the context of ESMs, however, the approach is not useful, as the dates in the model and observations have no direct correspondance (e.g. comparing modeled and observed values for Jan 1980 is meaningless). Alternatively, the crmse should be based on a climatological mean cycle (e.g. compare modeled and observed values for Jan averaged from 1980 to 2010). References: Collier, Nathan, Forrest M. Hoffman, David M. Lawrence, Gretchen Keppel-Aleks, Charles D. Koven, William J. Riley, Mingquan Mu, and James T. Randerson. 2018. “The International Land Model Benchmarking (ILAMB) System: Design, Theory, and Implementation.” Journal of Advances in Modeling Earth Systems 10 (11): 2731–54. [Nancy Hamzawi, Canada]	Accepted -- We use the CRMSE for climatological seasonal cycles only to avoid this issue
55017	191	1	191	9	(3) There is no score for “Relationships”. Please delete this row or provide values. [Nancy Hamzawi, Canada]	Accepted -- we have deleted this row

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
55019	191	1	191	9	(4) Please inlcude information on the reference data that was selected for producing this figure. [Nancy Hamzawi, Canada]	Accepted -- this is in process of consolidating references to all of the datasets and including in caption of figure.
55021	191	1	191	9	(5) Showing only relative scores makes it impossible to know how well ESMs perform. Please add the corresponding absolute scores. [Nancy Hamzawi, Canada]	Rejected -- including both absolute and relative scores would be space-prohibitive, so wee have chose relative scores. The reason for this is that there is wide variation from one dataset to the next on the range of absolute score, which makes it difficult to perceive the relative change form CMIP5 to CMIP6 -- the key goal of this figure -- as opposed to the differences in relative scores from one dataset to the next.
78561	191	1			fig 22. Could coordinate choice of colours with chapter 3 “Gleckler plots”. (fig 3.41 in this SOD) [Chris Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We have chosen a colour bar that provides visual clarity for this figure.
52743	191	5	191	5	Figure 5.22. It would be helpful to list what observed products are these models compared to and benchmarked towards [Katarzyna Tokarska, Switzerland]	Accepted -- this is in process of consolidating references to all of the datasets and including in caption of figure.
33383	191				Figure 5.22. Figure is too small. Put both figures one above the other and make it bigger. [Guiomar Rotllant, Spain]	Accepted. We have increased the font size on the figure legends
17889	192	1	192	10	For Figure 5.23, are these all of the models that report land & ocean storage on ESGF? In some particular paper? If the former, you may want to put the date you accessed the archive since I expect it will be changing. If the latter, please clarify which paper is being used. [Katherine Calvin, United States of America]	Accepted - list of models and dates retrieved from archive now added.
78563	192	1			fig 23, be careful what is labelled as “observations” here you mean the GCP estimate? [Chris Jones, United Kingdom (of Great Britain and Northern Ireland)]	See response to 44649
86637	192				Fig 5.23 Would need to show uncertainty for the GCP estimates. Also, these GCP estimates are not really observation based. Ocean and land are only constrained by 02/N2 estimates for the 1990s. [Pierre Friedlingstein, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - we now include uncertainties in GCP estimates.
81553	193	1	193	1	Figure 5.24: I didn’t find the claimed blue bands and red bands for CMIP5 and CMIP6, respectively. [Wu Minchao, Sweden]	Taken into account - Figure formatting error during the SOD compilation. Format error removed for the FGD.
87709	193	1	193	1	Fig 5.24 (at least in my version) is missing CMIP5/6 results (while it does not say that it is a placeholder Figure); this makes it difficult for a reviewer to assess the Figure interpretation. [Ivy Frenger, Germany]	Taken into account - Figure formatting error during the SOD compilation. Format error removed for the FGD.
55023	193	1	193	1	Figure 5.24 only shows results from the inversion models, not from CMIP5/6. Please add results from CMIP5/6. [Nancy Hamzawi, Canada]	Taken into account - Figure formatting error during the SOD compilation. Format error removed for the FGD.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
102077	193	1	193	10	Figure 5.24: Distributions of carbon sink - there are CMIP5 and CMIP6 data missing despite they are explained in figure caption. Is this because those are yet unpublished or the results are pending? This needs to be clarified as this is second order draft and the last chance for scientific community to comment upon the report. [IAPSO ECS group review, United States of America]	Taken into account - Figure formatting error during the SOD compilation. Format error removed for the FGD.
17891	193	1	193	10	I don't see either the CMIP5 or CMIP6 results in the figure. [Katherine Calvin, United States of America]	Taken into account - Figure formatting error during the SOD compilation. Format error removed for the FGD.
41669	193	1	193	11	Figure 5.24: I cannot see the blue or red bands in the figure that should show the results of CMIP5 and CMIP6 [Katharina Meurer, Sweden]	Taken into account - Figure formatting error during the SOD compilation. Format error removed for the FGD.
52793	193	1	193	15	Neither CMIP6 nor CMIP5 results are visible in either of the panels in Figure 5.24 (macOS: Adobe Acrobat, Apple Preview, Skim) [Sergey Malyshev, United States of America]	Taken into account - Figure formatting error during the SOD compilation. Format error removed for the FGD.
127875	193	1	193	60	In Figure 5.24, CMIP6 and CMIP5 results are not visible in either of the figure panels. [Trigg Talley, United States of America]	Taken into account - Figure formatting error during the SOD compilation. Format error removed for the FGD.
84827	193	4	193	4	This figure is incomplete - there are no color lines shown. [Martin Heimann, Germany]	Taken into account - Figure formatting error during the SOD compilation. Format error removed for the FGD.
58845	193	4	193	10	Figure 5.24 seems to be missing the actual CMIP5 ESM (blue) and CMIP6 ESM (red) modelled outputs. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account - Figure formatting error during the SOD compilation. Format error removed for the FGD.
37975	193	4			There is no results for CMIP5 and CMIP6. [Junhee Lee, Republic of Korea]	Taken into account - Figure formatting error during the SOD compilation. Format error removed for the FGD.
63617	193		193		The CMIP5 and CMIP6 models don't show up on the figure 5.24 [Galen McKinley, United States of America]	Taken into account - Figure formatting error during the SOD compilation. Format error removed for the FGD.
33385	193				Figure 5.24. Latitude, negative means north and positive means south? I will suggest to use N and S instead. [Guiomar Rotllant, Spain]	Taken into account - figure caption now explains the meaning of -ve latitude.
86639	193				Fig 5.24 Is this still a placeholder ? There are no CMIP5/6 data on that figure ! [Pierre Friedlingstein, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - Figure formatting error during the SOD compilation. Format error removed for the FGD.
52795	194	1	194	1	wedges are not visible in Fig.5.25, only lines (macOS: Adobe Acrobat, Apple Preview, Skim) [Sergey Malyshev, United States of America]	Taken into account - Figure formatting error during the SOD compilation. Format error removed for the FGD.
55025	194	1	194	1	Figure 5.25 lacks the uncertainty ranges mentioned in the Figure caption. Please add. [Nancy Hamzawi, Canada]	Taken into account - Figure formatting error during the SOD compilation. Format error removed for the FGD.
17893	194	1	194	11	The shaded area is not visible in the pdf [Katherine Calvin, United States of America]	Taken into account - Figure formatting error during the SOD compilation. Format error removed for the FGD.
127877	194	1	194	60	Wedges are not visible in Figure 5.25, only lines. [Trigg Talley, United States of America]	Taken into account - Figure formatting error during the SOD compilation. Format error removed for the FGD.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
78565	194	1			fig 5.25 – need to ensure consistency here with chapter 4 who show these but for all 5 core scenarios. See fig 4.5 in this SOD [Chris Jones, United Kingdom (of Great Britain and Northern Ireland)]	Noted, but we have decided to stay with the 4 scenarios because this allows us to use a common set of ESMs for each scenario
84829	194	3	193	3	This figure is incomplete - there is no shaded area shown. [Martin Heimann, Germany]	Taken into account - Figure formatting error during the SOD compilation. Format error removed for the FGD.
52745	194	3	194	3	Figure 5.25. Please note that the top panel in this figure seems to be the same as Fig. 4.5 in Chapter 4. Perhaps it would be good, for consistency, to only keep one version, and just add more SSP scenarios to this figure here? [Katarzyna Tokarska, Switzerland]	Noted.
52747	194	3	194	3	Figure 5.25. The vertical axis labels in panels and b may be confusing. Maybe refer to them as atmosphere to ocean or atmosphere to land carbon flux. (Also, for consistency with fig. 4.5 in CH4 that shows the same information) [Katarzyna Tokarska, Switzerland]	Taken into account - axis labels revised.
58619	194	3			The projections don't seem to stop in 2090 (or there is an error in the plot of time axis) [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - axis labels corrected.
37977	194	5			There is no shaded area. [Junhee Lee, Republic of Korea]	Taken into account - Figure formatting error during the SOD compilation. Format error removed for the FGD.
26963	194	9	194	11	thick lines and shaded areas are missiong on the Figure [Eric Brun, France]	Taken into account - Figure formatting error during the SOD compilation. Format error removed for the FGD.
58847	194	10	194	10	Figure 5.25 the standard deviations (shaded areas) seem to be missing from the figure. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account - Figure formatting error during the SOD compilation. Format error removed for the FGD.
18229	194	10	194	11	Figure 5.25 caption states that the shaded areas represent one standard deviation around the mean but there is no shading in the plot. Please amend the figure. [Chelsey Baker, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - Figure formatting error during the SOD compilation. Format error removed for the FGD.
116497	194		194		I cannot see the shared adeas. [Valerie Masson-Delmotte, France]	Taken into account - Figure formatting error during the SOD compilation. Format error removed for the FGD.
51217	194		194		Figure 5.25: the caption mentions a shaded areas representing +- 1 standard deviation, but there is no shaded area. Please include an indication of uncertainty such as this. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - Figure formatting error during the SOD compilation. Format error removed for the FGD.
86641	194				Fig 5.25 Is this still a placeholder ? There are no uncertainty for the CMIP6 data on that figure ! [Pierre Friedlingstein, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - Figure formatting error during the SOD compilation. Format error removed for the FGD.
58935	195		195		The quality of Fig. 5.26 is rather poor. Is it possible to improve it? [Subchair note: I assume referring to resolution] [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted and addressed
58859	195		195		Figure 5.26. For clarification, it would be good to add axes titles for the zonal mean latitude profiles (figures on the right of the maps). [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted and addressed

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
18233	196	1	196	1	The key in Figure 5.27 does not match the plots or information in the plots are missing, particularly for observation and emergent constraint. Please amend the plot. [Chelsey Baker, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - Figure formatting error during the SOD compilation. Format error removed for the FGD.
21905	196	1	196	1	Font size is barely legible. [Peter Thorne, Ireland]	Accepted - font sizes increased.
55027	196	1	196	1	Figure 5.27 lacks the grey bar and the uncertainty ranges mentioned in the figure caption. [Nancy Hamzawi, Canada]	Taken into account - Figure formatting error during the SOD compilation. Format error removed for the FGD.
41677	196	1	196	18	Figure 5.27: The green bar in the subplots is different to the green bar given in the legend (emergent effect) [Katharina Meurer, Sweden]	Taken into account - Figure formatting error during the SOD compilation. Format error removed for the FGD.
37979	196	2			The legend of observation is missed. Maybe blue botted line. [Junhee Lee, Republic of Korea]	Taken into account - Figure formatting error during the SOD compilation. Format error removed for the FGD.
26965	196	16	196	16	there are not different thicknesses in the figure [Eric Brun, France]	Taken into account - Figure formatting error during the SOD compilation. Format error removed for the FGD.
33387	196				Figure 5.27. Fig. a. Observations lack representation (blue dashed line?) [Guiomar Rotllant, Spain]	Taken into account - Figure formatting error during the SOD compilation. Format error removed for the FGD.
86643	196				Fig 5.27 panel (a): why 2060 and not end of century ? [Pierre Friedlingstein, United Kingdom (of Great Britain and Northern Ireland)]	Noted - as published study.
18235	197	1	197	1	This an excellent figure and really conveys the information clearly. The end of the figure caption is cut off the end of the page. [Chelsey Baker, United Kingdom (of Great Britain and Northern Ireland)]	Editorial - this kind of issues will be taken care of during the production phase, which includes professional copy-editing.
21907	197	1	197	1	Font size is barely legible. [Peter Thorne, Ireland]	Rejected - it is difficult to increase font size at the Figure without interference between different phrases
12691	197	1	197	22	See also page 68 as figure placement. Figure doesn't include the numbers from above reference. Gedney et al 2019 gives a wetland CH4 response to climate with the range ~0.01-0.08Wm-2K-1 [Nicola Gedney, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - N39this paper mixes different feedback, and it is unclear how to place its numbers in the figure. However, this paper is cited and discussed in the text (see reply to comment id 12689).
41679	197	1	197	22	Figure 5.28: I have troubles understanding the gray colour in the legend of panels a and b. They are supposed to represent the permafrost and CMIP6 model, respectively, but I cannot identify them in the plots. Are they supposed to fill every second row? [Katharina Meurer, Sweden]	Accepted - the respective note is added to the Figure caption
32031	197	4			Fig 5.28 Maybe I'm just going on qualitative anecdotal evidence (my eyes, having done a lot of boots on the ground fieldwork in both the Arctic and tropics) but I'd think this over values permafrost feed backs and undervalues tropical wetland/cow/vegetation feedbacks. [Euan G. Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - unclear what is to be changed
2231	197	13			Figure 5.28: Please remove "by with" [Abed El Rahman Hassoun, Lebanon]	Taken into account - see comment id 19995
58851	197		197		Figure 5.28 panel A. (legend) has a symbol for ´fire and permafrost feedback (panel c)´, it is not clear to what this refers to in panel C. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - the figure is revised accordingly

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
41691	198	1	198	12	Figure 5.29: It is not clear to me why each scenario has two pairs of values and, conseuqently, two lines in each plot [Katharina Meurer, Sweden]	Partially accepted -- fig. now includes larger set of models, so no longer only two to differentiate, and instead treating as an ensemble plume
2233	198	1			Please make sure to separate between the caption of Figure 5.28 and Figure 5.29. [Abed El Rahman Hassoun, Lebanon]	Accepted. This was corrected in final version.
26967	198	4	198	4	there are 4curves in each figure, indicate what represent each kind of curves [Eric Brun, France]	Partially accepted -- fig. now includes larger set of models, so no longer only two to differentiate, and instead treating as an ensemble plume
37981	198	4			The legend of models' name is missed. [Junhee Lee, Republic of Korea]	Partially accepted -- fig. now includes larger set of models, so no longer only two to differentiate, and instead treating as an ensemble plume
58853	198		198		Figure 5.29: it is not clear which line is ESM and which is EMIC. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Partially accepted -- fig. now includes larger set of models, so no longer only two to differentiate, and instead treating as an ensemble plume
33389	198				Figure 5.29. How we can differentiate CanESM5 from UVIC-ESCM? [Guiomar Rotllant, Spain]	Partially accepted -- fig. now includes larger set of models, so no longer only two to differentiate, and instead treating as an ensemble plume
30011	199	1	199	1	Figure 5.30: It is not easy to distinguish symbols with filled dots from simple open symbols. Is it possible to use filled symbols instead of symbols with filled dots? [Michio Kawamiya, Japan]	Not applicable. Figure removed
21909	199	1	199	1	panels and their text are very small and hard to make out. Better as a vertical stack of 3 panels? Increase font size so legible. Add overarching title for accessability? [Peter Thorne, Ireland]	Not applicable. Figure removed
26969	199	5	199	5	Please indicate what means Hist, Hind1, Hind2, Hind 3, Hind 4 Hind 5 [Eric Brun, France]	Not applicable. Figure removed
33391	199				Figure 5.30. Add description of X axis. [Guiomar Rotllant, Spain]	Not applicable. Figure removed
2235	199				Figure 5.30: Please separate between the enumeration (A, B, C) and the plots' titles by "-" or "/" or something else. [Abed El Rahman Hassoun, Lebanon]	Not applicable. Figure removed
99369	200	1	200	1	Could you provide a range of scenarios in the left panel, as done in AR5 and SR15, rather than just one? Even if the concept is to use TCRE rather than directly deriving carbon budgets from scenarios containing a variety of non-CO2 changes, it might be interesting to have a figure showing how the warming evolves wrt CO2 emissions for a set of scenarios. That might perhaps highlight the need for the TCRE approach. [Philippe Marbaix, Belgium]	Taken into account - additional scenarios have been added.
109205	200	1	200	10	Figure 5.31, right-side theoretical panel, needs a lot more guidance and/or less content to be understandable. Consider numbering or lettering various text to make clear the order that makes most sense and changing important text to full sentences (i.e., remaining allowable warming before X target is reached, then point on the graph, then, therefore there is this much more carbon to spend before reaching that target) [Steph Courtney, United States of America]	Accepted - The caption has been amended to clarify these aspects.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
21911	200	2	200	2	Text is barely legible at the font size used. Figure lacks overarching title and panel titles that may aid accessability [Peter Thorne, Ireland]	Noted - choice of resolution of figures is not in the hands of the authors. All figures are provided in vector format.
18501	200	2	200	15	The RH panel shows a scheme for estimating the RCB based on net zero carbon (it includes the ZEC in the adjustment). I don't think this is quite what the text says. [Jim Skea, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - the text now more clearly spells out the ZEC contribution
52751	200	5	200	5	Figure 5.31. It would be helpful to clarify in the caption that carbon budgets should not be direcly read off the red curve on the left hand side, following the new methodology. Currently, that is only implicit, and some readers may not realise it. [Katarzyna Tokarska, Switzerland]	Accepted - The caption has been amended
52755	200	5	200	5	Figure 5.31. Perhaphs it would be good to keep the same colour on both panels for the non-CO2 contribution (e.g. red), in order to make the connection easier between the two plots. [Katarzyna Tokarska, Switzerland]	Noted - The figure has been further improved for internal consistency.
26971	200	5	200	5	Please detail the description in the legend allowing to read the figure on the right [Eric Brun, France]	Accepted - more information has been added to the caption
58825	200	5	200	15	Left panel of figure is fine but the right panel is not clear at all. The ZEC and non CO2 commitment are one way arrows when I think it would be clearer if they were both two way arrows. What is the purpose of the three triangles on the dashed line and what does their direction represent? Is the unrepresented earth system feedbacks also meant to be the contribution where the black triangles to the left are? Are these black triangles meant to represent a bounding, if so use two way arrows as is convention. There figure could be explained better in the figure legend, there is very little information about the right panel. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - triangles have been changed to arrows. The ZEC and non-CO2 commitment only work in the direction indicated by the arrows. More clarifications have been added.
40167	200	5			Fig 5.31: the black triangles are a bit confusing here [TSU WGI, France]	Accepted - they have been removed
52749	200	8	200	9	It would be helpful to clarify the data source of the global mean temeprautre time-series used in this plot. Box 2.3 includes different temperautre definitions, but is is not explicitly clear what time series are shown here? Is is average of different datasets? [Katarzyna Tokarska, Switzerland]	Accepted - The data source (i.e. the Chapter 2 assessment) has been highlighted.
26973	200	9	200	9	It is difficult to see the brown [Eric Brun, France]	Accepted - Figures have been edited for visibility.
52753	200	12	200	13	It would be helpful to clarify why the grey TCRE cone in the right hand side increases with cumulative CO2 emissions. I thought the uncertainty in TCRE is constant (assumed as a normal distribution), so the uncertainty should be parallel to the black curve? The re-basing to the present-day makes the total uncertainty in the remaining budgets grow with time (and cumulative emissions), but I do not think it comes directly from TCRE, since TCRE is treated as a constant in this new framework? [Katarzyna Tokarska, Switzerland]	Taken into account - the absolute uncertainty in warming implied by the uncertainty in TCRE increases proportionally to the amount of additional future CO2 emissions. Therefore, the appropriate uncertainty representation is a cone.
116503	200		200		Figure 5.31 could help show differences compared to earlier assessments. [Valerie Masson-Delmotte, France]	Rejected - this would overload the figure
18241	201	1	201	1	Figure 1 part a) - if the solid line is a 'major' exchange it implies that the dashed line is a 'minor' exchange, is this the case? [Chelsey Baker, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Legend was revised.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
52757	201	4	201	4	Box 5.2. I found this figure confusing. Please consider simplyfing the bottom panels, especially panel d, as outgassing of natural carbon sinks is very difficult to spot in the bottom panel, and it is unclear if the land and ocean carbon sinks increase and decrease in size? Also, the transfer between land and ocean carbon storage is confusing. Please consider clarifying it in the caption. [Katarzyna Tokarska, Switzerland]	Accepted. Figure was revised.
58829	201	4	201	10	This figure is not clear. The label for the red stripes is 'Natural carbon redistribution in response to a perturbation' when in actually it should be 'intermediate sink before natural carbon system redistribution'. The red area in the land and ocean sink in figure C should look visibly smaller than the red area in the land ocean sink of B. The decision for empty white boxes to be removal from the reservoir should be deisgnated another style e.g horizontal or vertical lines. not left white.When carbon changes sink it appears that the sink size is proportional to the area of the boxes, this is not true and is deceptive. Obviously the sinks are not to scale. If this figure is not to scale it should be stated. Reorder legend so the ones relevant to figure A and B are top. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Figure was revised.
10315	201	10	201	10	Keller et al. (2018) should be Keller et al. (2018a) [Chris Vivian, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Citation was revised.
116505	201		201		The representation of the size of the land and ocean reservoirs in the various panels is puzzling. [Valerie Masson-Delmotte, France]	Accepted. A statement clarifying that the size of the boxes does not reflect reservoir sizes was added.
18917	201		201		Box 5.2 Figure 1: There is a kind of hatching for "Natural carbon redistribution in response to a perturbation" in the legend for panel b). What does this mean? This may be explained in the caption. I would expect this redistribution in response to a perturbation should be on all 3 reservoirs (atm., land and ocean) in panels b), c) and d). May be I am not understanding. That is why explaining this in the caption would be helpful [Govindasamy Bala, India]	Accepted. Figure was revised.
26975	202	3	202	3	Please indicate the shape of the remove and the time period in the legend [Eric Brun, France]	Accepted. We clarified that the figure illustrates the response to a 100 GtC instantaneous removal.
18183	202	10	202	10	Change (red lines) to orange lines or amend the figure. [Chelsey Baker, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change was made.
18921	202	10	202	10	"red lines"? To me, they appear "orange" [Govindasamy Bala, India]	Accepted - change was made.
116507	202		202		What is the robustness of the result if a multi model approach is used? [Valerie Masson-Delmotte, France]	Accepted. The figure was redrawn with CDR-MIP results.
108827	203	2	203	3	The waterfall charts should match from the corner of the last negative to the next positive, I think, if I'm understanding this correctly, which I'm not sure I am. [Jason Donev, Canada]	Not applicable. Figure was replaced.
51195	203	Fig 5.33			Fig 5.36 Please can you make the caption clearer? It says the figure is showing flux components, defined in the glossary as "flows of matter", but the vertical axis is labelled ppm. I think the numbers on the blue bars are concentration at the beginning of the time period, and the numbers on the other bars are the effect on concentration by the end of the time period due to that source/sink? Which aren't fluxes. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. Figure was replaced.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
18927	203		203		Figure 5.33: The budgets do not tally for CO2 concentration in year 2050 and 2100. For instance, the CO2 concentration in 2050 should be 449 ppm when the numbers are added up. Please make sure the budgets are correct. I see that the caption discusses but it may be a good idea to show this imbalance in the figure. Also, there are no labels such as (a), (b), etc. in the figure but captions indicate labels in the figure. [Govindasamy Bala, India]	Not applicable. Figure was replaced.
33393	203				Figure 5.33. Erase the following text from figure legend since it is already in the figure: “(a) Large positive CO2 emissions, (b) Small net positive CO2 emissions, (c) Net negative CO2 emissions (short-term response), (d) Net negative CO2 emissions (long-term response).” [Guiomar Rotllant, Spain]	Not applicable. Figure was replaced.
18185	204	1	204	1	The legends in Figure 5.34 are confusing - add what the symbols mean into the legends as well as detailing them in the figure caption. Or if the differences between the different symbols are not relevant then use a boxplot? [Chelsey Baker, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - change was made.
26977	204	2	204	2	Please indicate the complete definition of CDR in the title too [Eric Brun, France]	Accepted - change was made.
26979	204	3	204	3	Please indicate the complete definition of RCP [Eric Brun, France]	Accepted - change was made.
18931	204		204		Figure 5.34, panel a): There are four symbols here but which symbal removes more CO2 and which one removes least is not indicated in the panel or the caption. However, for panel b) this information is provided in the caption. [Govindasamy Bala, India]	Accepted - change was made.
58621	204				Why plot the airborne fraction after an equilibration of 100 years when you're studying the period 2020-2099 and saying the global plot is showing the situation in 2100 (and not 2120) ? Is it only to try to reach steady state ? If yes, would the global system reach ss in 2100 ? [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - change was made.
21913	205	1	205	1	Figure without effort at adding self describing titles is not possible to interpret alone. [Peter Thorne, Ireland]	Accepted - change was made.
96667	205	1	205	1	Figure 5.35 would be easier to read by adding labels "removal" and "emission" above the left and right half of the figure, respectively. [Nicole Wilke, Germany]	Accepted - change was made.
51203	206	1	206	1	While it is fair that A/R/Forest Management generally increases N20 emissions, authors could caveat that in some cases this leads to decreased N20 emissions – i.e. when planting is on previously highly fertilised cropland/grassland rather than marginal land (ref (https://www.sciencedirect.com/science/article/pii/S0168192314001816) For a study in Ireland). [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - this was how it was stated in the text previously. Now the figure has been revised accordingly.
51207	206	1	206	1	Some studies suggest methane uptake can increase from soil carbon sequestration e.g. Nisbet et al geophysics 2020 https://doi.org/10.1029/2019RG000675 p.33. Suggest this reference is considered. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Accepted, the figure was revised to show decreased CH4 emissions due to afforestation

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
51209	206	1	206	1	Wetland restoration – suggest modifying to imply decreased N20, in line with IPCC 2013 wetland supplement (chapter 3.2.3 N2O emissions from rewetted organic soils) in this regard: “Raising the depth of the water table will cause N2O emissions to decrease rapidly, and fall practically to zero if the depth of the water table is less than 20cm below the surface (Couwenberg et al., 2011). Saturated conditions may promote denitrification and the consumption of N2O, but in practice this effect is very small and considered negligible in this chapter. This is because anoxic conditions and low NH4+ availability reduce the rates of mineralisation and nitrification, two processes that are prerequisites for denitrification.” [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - part of the suggested sentence was added in the text
18187	206	1	206	1	Figure 5.36. Very nice figure. De-oxygenation is spelt incorrectly in the key. What do missing squares mean? For example, there is no box for ocean fertilisation Ocean C sequestration. Not well quantified? [Chelsey Baker, United Kingdom (of Great Britain and Northern Ireland)]	Thank you. Accepted - de-oxygenation corrected. Missing squares indicate that the effect is not relevant. For OF, the missing square was filled with white box to indicate it is not known (there are studies but it is not clear)
103179	206	1	206	1	Figure 5.36: for ocean fertilisation no box is mentioned for ocean C sequestration. [Philippe Tulkens, Belgium]	Noted - the missing square was filled with white box to indicate it is not known (there are studies but it is not clear)
51211	206	1	206	1	There is evidence that biochar can decrease and increase CH4 emissions from soil – suggest modifying to reflect this: 'CH4 emissions have reduced by 20-51% (Wang et al., 2018), 47-86% (Dong et al.,) or even suppressed almost completely (Rondon et al., 2005) – however some studies suggest increases in CH4 due to biochar application (Zhang et al., 2012; Pandey et al., 2014; Singla and Inubushi, 2014).' See Song et al 2016. for a meta analysis that shows mixed results for methane fluxes. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - there is no space to go into such details, the statement that impacts on CH4 are not clear well reflect the variation in results highlighted in the suggested papers.
103181	206	1	206	1	Figure 5.36: in the text explaining the figure, for the time scale for the CDR sequestration potential, more explanation is needed as to what the numbers 0 to 27 stand for, i.e. PgC y-1. The unit is barely visible in the chart. [Philippe Tulkens, Belgium]	Not relevant - the sequestration potentials are not anymore assessed in WGI, and were removed from the figure.
103183	206	1	206	1	Figure 5.36: an additional column could be added mentioning the CDR methods that are prohibited by the London Protocol (GESAMP2019). [Philippe Tulkens, Belgium]	Not relevant - the focus of WGI is to assess the biogeochemical, biophysical and other side-effects
5673	206	1	206	1	Please revise text. A/R or FM can have no, adverse or beneficial effects on water quantity, depending on what is done where. Afforstations are one means to stabilize water outflow from a region to improve water availability "downstreams". Literature on forests and water is abundant. [Joachim Rock, Germany]	Accepted. Both text and Fig. 5.36 were revised.
21915	206	1	206	1	Text is barely legible at the font size used [Peter Thorne, Ireland]	Accepted - The font size was increased
111031	206	1	206	1	Please more clearly define side effect and explain the ratings. E.g. Why does Wetland restoration has no effect on evapotranspiration? Would the "bio-energy with carbon capture and storage" effect on evapotranspiration maybe depend on vegetation types? And according to Sonntag et al. (2018) even afforestation might have an effect on Marine NPP. Sonntag et al. 2018, https://doi.org/10.1002/2017EF000620 [Julia Nabel, Germany]	Accepted - was changed to 'unknown'. We are not aware of studies focusing on changes in ET due to wetland restoration.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
58319	206	1	206	1	Figure 5.36: Writing is very small and hard to read, especially the "legend" explaining the colouring of "earth system feedbacks" and "side effects" at the top. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - The font size was increased
58837	206	1	206	10	Direct air carbon capture and storageI is listed as having an adverse effect on biodiversity, this is misleading as it assumes storage in the ocean, it may be necessary to break this down dependent on storage solution. Direct CCS is non invasive. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. The DACCS text was revised
17903	206	1	206	10	What are the adverse side effects from DACCS on biodiversity? This isn't mentioned in the text, but appears on the figure. Please elaborate and provide citations. [Katherine Calvin, United States of America]	Noted - the biodiversity impact is given in Table 5A1
26981	206	2	206	2	SR1.5 and SRCCL had a much more nuanced approach to this, especially for afforestation & BECCS. In particular, the side effects depended a lot on the scale to which the CDR is applied. This should be reflected in the right column of Figure 5.36 [Eric Brun, France]	Noted - This is true and was discussed in
51201	206	Fig 5.36	206	Fig 5.36	There is no shaded square against EW for ocean acidification, but on p.98 rows 39-40 of ch.5 it says "EW increases the alkalinity and pH of natural waters, helps dampen ocean acidification and increases ocean carbon uptake (Beerling et al., 2018)." [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - thank you for noticing. A shaded square indicating decreased OA was added to the figure
51197	206	Fig 5.36	206	Fig 5.36	Fig 5.36: Please can you add references to the caption? [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Noted - references are found in Table 5.A.1.
51199	206	Fig 5.36	206	Fig 5.36	It's hard to understand from Figure 5.36 the impacts on ocean carbon sequestration. Won't all CDR methods weaken ocean C sequestration, as the ocean equilibrates with an atmosphere with a reduced CO2 concentration? Why is this more uncertain for some CDRs? It would be helpful to point the reader in the direction of the relevant text in the report within the figure caption. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Noted - references are found in Table 5.A.1. In the figure the strengthening/weakening label is given to a method only if this specific method was a investigated in a modelling study (published paper)
10317	206		206		Figure 5.36 - 'Ocean fertilisation' row, 'ocean acifidification' side effect - This is scored as increasing ocean acidification. However, this only applies to subsurface waters - see also comments to page 97, lines 41-43 and the comments to page 165, ocean fertilisation row. [Chris Vivian, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - this information has been added to the figure
10319	206		206		Figure 5.36 - 'Ocean fertilisation' row, 'ocean deoxygenation' side effect - This is scored as 'can increase or decrease' ocean deoxygenation. Is this correct? As far as I could see in the text of chapter 5, there is no mention of ocean fertilisation. [Chris Vivian, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - ocean fertilisation is discussed in Section 5.6.2.2.2
10321	206		206		In the figure under 'Side Effects' it indicates that ocean alkalinization will increase emissions of N2O and CH4. However, there does not appear anything in the text of this chapter to support this assessment. Is there some literature to support this assessment? If not, I suggest that these 2 assessments should be classed as unknown. [Chris Vivian, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - these effects have been classed as unknown

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
18939	206		206		Fig. 5.36: This is an excellent figure that very nicely summarizes the many characteristics of CDR. I have a few comments though. a) Can we really say the timescale tmescale of C storage for Direct air capture is "permanent"? What if the captured CO2 is used to produce products such as synthetic fuels or making soda? Shouldn't the time scale be the same for BECCS and Direct Air Capture if they use the same type of storage? b) Why would enhanced weathering weaken the ocean C sequestration? I believe weathering helps to transfer C from atmosphere to oceans. c) I don't think there is any study that shows that the ocean c sequestration will be reduced by DAC. You can leave this box white (unknown) d) Shouldn't ocean fertilization increasee c sequestration in the ocean. [Govindasamy Bala, India]	Noted - thank you! A) this is correct, the time scale depends on the method how the storage is done. The figure was revised; B) According to Keller et al. (2018a) it will initially reduce the ocean CO2 sequestration, but after enough weathering products are transported into ocean to increase alkalinity, will increase ocean CO2 sequestration. See Table 5.A.1; C) agree, figure was changed; D) The evidence that OF increases ocean C sequestration is not clear (see also text in 5.6.2.2.2)
116479	206		206		Consider adding levels fo confidence or scientific understanding in the figure. [Valerie Masson-Delmotte, France]	Accepted - this was added to the figure
33395	206				Figure 5.36. Font letter is too small to be readable. [Guiomar Rotllant, Spain]	Noted - the font was increased.
16029	206				graphic, regarding « Earth system feedbacks », « Land C sequestration » and « temperature » are declared « unknown ». Man-made terra pretas in the Amazon basin that inspired biochar proves the long-term sequestration of pyrogenic biomass, 500-8000 years in the case of terra preta. Made fertile by man, terra preta soils cover huge areas (1-3% of the Amazon basin). They have obviously sequestered carbon as terra preta/biochar is reputed 'recalcitrant to decomposition' by science for long periods (decades, up to millions of years). Made from plants/trees through photosynthesis (that uses carbon from the air), terra preta has affected temperatures, even if marginal. Regarding biogeochemical effects, it would be surprising if biochar had « no effect » on CH4 emissions as written — unknown might be a better choice. That said, we note your comment 5 - 89 « The IPCC Special Reports on the global warming of 1.5 degrees (SR1.5, IPCC, 2018) and the Special Report on climate change and land (SRCCL) assessed CDR potentials and side effects but did not address the effects of CDR on carbon and other biogeochemical cycles in detail. » We would add « non soils » biochar uses are not mentioned at all. In particular, biochar could replace marin sand in concrete. Marin sand is the most used product in the world. The extraction of marine sand around the world has recently been covered by mainstream press. It is very destructive of marin ecosystems, getting scarce, and, often associated with illegal activities. Besides its use in concrete, more than ten uses for biochar as a material have been identified. See A Review of Non-Soil Biochar Applications. Non-soils uses of biochar are also found in Burn: Igniting a New Carbon Drawdown Economy to End the Climate Crisis. Those two major documents should be in chap. 5 references. A Review of Non-Soil Biochar Applications by Mattia Bartoli 1, Mauro Giorcelli 1, Pravin Jagdale 2, Massimo Rovere 1 and Alberto Tagliaferro 1,3,*	Accepted - a sentence was added
58839	207	27	207	31	It is difficult to determine the NPP:GPP ratios from this figure, so why is NPP not just plotted on it's own? The text for this on page 100 discusses high/low latitude differences but this is absent from the figure. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted - but this is as the published figure.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
41715	207	28	207	28	please explain "Ra" [Katharina Meurer, Sweden]	Taken into account - "Plant Respiration (Ra)" now included in figure caption
26983	208	6	208	6	We suggest to develop a connexion with the other figures because there are the same CO2 fluxes than in figure 5.8. In general there are few connexions between figures using same data or same models [Eric Brun, France]	Accepted - Figure FAQ5.1 has been revised accordingly
2355	209				FAQ 5.2, Figure 1: Not there! [Abed El Rahman Hassoun, Lebanon]	Taken into account - the figure was not available in time for the SOD but was included for the FGD.
33397	209				FAQ 5.2, Figure 1. Missing. [Guiomar Rotllant, Spain]	Taken into account - the figure was not available in time for the SOD but was included for the FGD.
2237	209				FAQ 5.2, Figure 1: Not there! [Abed El Rahman Hassoun, Lebanon]	Taken into account - the figure was not available in time for the SOD but was included for the FGD.
33399	210				FAQ 5.3, Figure 1. Missing. [Guiomar Rotllant, Spain]	Accepted. A figure is now included.
33401	212				FAQ 5.4, Figure 1. Missing. [Guiomar Rotllant, Spain]	Accepted - The FAQ figure has been completed.
98583	AII-4				Comment: for PETM CO2, a recent publication is Harper, D. et al. (2019). The Magnitude of Surface Ocean Acidification and Carbon Release During Eocene Thermal Maximum 2 (ETM-2) and the Paleocene-Eocene Thermal Maximum (PETM). Paleoceanography and Paleoclimatology. 35. 10.1029/2019PA003699, and recalculated with consistent assumptions to the other B isotope CO2 records in early Cenozoic in Anagnostou E., John E.H., Babila T.L., Sexton P.F., Ridgwell A., Lunt D.J., et al. State-dependence of climate sensitivity in the Eocene greenhouse, Nat. Comm. (in review), a reference already used for the EECO [Eleni Anagnostou, Germany]	Noted - Changed
98589	AII-4				Comment: References for pH values of sea surface ocean are not included for the Deep past (Cenozoic) periods in the Annex. These are Penman et al., 2014; Gutjahr et al., 2017; Babila et al., 2018; Harper et al., 2019 for the PETM, and Anagnostou et al., 2016; Anagnostou, in review for the EECO. Depending if the discussion on ocean acidification in the Eocene is expanded, then the ETM2 record of Harper et al. 2019, and the MECO record of Henehan et al. 2020 should also be included. [Eleni Anagnostou, Germany]	Noted - Changed
98591	AII-4				Comment: MPWP Bartoli et al. 2011 CO2 estimates [Eleni Anagnostou, Germany]	Noted - Changed
98585	AII-7	8			Comment: the updated reference is "Anagnostou E., John E.H., Babila T.L., Sexton P.F., Ridgwell A., Lunt D.J., et al. State-dependence of climate sensitivity in the Eocene greenhouse, Nat. Comm. (in review)" [Eleni Anagnostou, Germany]	misplaced comment. We cannot identify what it refers to.
98587	AII-8	8			Comment: the updated reference is "Inglis, G.N., Bragg, F., Burls, N. Cramwinckel, M.J., Evans, D., Foster, G.L., et al. Global mean surface temperature and climate sensitivity of the EECO, PETM and latest Paleocene. Clim. Past (in review)". [Eleni Anagnostou, Germany]	Thank you.
99031		45	6	45	I'd suggest changing "was" to "is estimated to have been" or change sentence and start sentence with "Observations and reconstructions show that …" [Michael MacCracken, United States of America]	Sentence has been completely rewritten, comment no longer applies.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
8705					Overall, the chapter has all the relevant material needed to convey how global biogoechemical cycles relevant to CO2, CH4 and N2O have evolved in the past, present and will evolve in the future. I think some sections need better organization, especially highlighting what is new since AR5. [Vaishali Naik, United States of America]	Accepted. More sections now start with what we have learned since ar5.
33289					All over the chapter. Avoid starring sentences with abbreviations AOUpw, AR5, BECCS, CDR, CH4, CMIP, CO2, DAC, DACCS, ESM, EW, IPCC, SRM, OMZs or ZEC. [Guiomar Rotllant, Spain]	Accepted. We have reduced the number of acronyms.
33291					All over the chapter. Tables format can be improved. [Guiomar Rotllant, Spain]	Accepted. Table format improved.
116499					Aspects related to carbon cycle feedbacks need to be better addressed in the TS, including insights from emergent constraints. [Valerie Masson-Delmotte, France]	Accepted.
39973					Assessments regarding Urban, which is an important and new content in AR6, are not mentioned in SPM. [TSU WGI, France]	Ch5 doesn't address urban carbon.
40487					Key assessments of Chap 5 (e.g. changes and budgets of GHGs) are included in the TS, but are not very explictly stated. [TSU WGI, France]	Accepted. Traceability of CH 5 messages elevated to TS have been strengthend
71733					Figure 5.3 Shouldn't there be arrow/arrows between land and ocean depicting carbon fluxes between these two compartments? [Tuomo Kalliokoski, Finland]	Accepted - arrow added
71735					Figure 5.24 Figure does not contain elements described in figure legend. [Tuomo Kalliokoski, Finland]	Taken into account - Figure formatting error during the SOD compilation. Format error removed for the FGD.
71737					Figure 5.25 Figure does not contain elements described in figure legend and text. [Tuomo Kalliokoski, Finland]	Taken into account - Figure formatting error during the SOD compilation. Format error removed for the FGD.
64589					Fig 5.14: Despite statement on caption, ranges in the figure for different source & sink categories don't match those in Table 5.2 [Charles Curry, Canada]	Accepted - we have matched the numbers
52305					An eight-year times-series (2011-2019) in the Arctic outflow water on the Greenland shelf and in the East Greenland Current (EGC; 3-11°W) shows a significant pH decrease of 0.006/year (0.06 per decade) in the depth interval of 50-150m, coinciding with increased pCO2 of 5 µatm/year and increased temperature of 0.06/year (Chierici et al., submitted). This Arctic outflow water has decreased buffering capacity, which is transported further south (Chierici et al., submitted). [Agneta Fransson, Norway]	Noted
115295					This chapter is well-written and consistent, with a clear structure, and no major weak points. It is a mature draft. [Gillett Nathan, Canada]	Thank you.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
115297					Calibrated uncertainty is not always used correctly in this Chapter. In particular likelihood language (likely, very likely etc.) should be used only if there is sufficient quantiative evidence to evaluate likelihood. Otherwise use confidence assessment only (or evidence and agreement if you can't assess confidence). Also likelihood language should only be used with with clear quantified statements, not with terms like 'approximately' etc. I think that confidence should be either 'low', 'medium', 'high' or 'very high', not 'low to medium' or similar. See Box 1.1. Related to this, I found that the term 'unequivocal' or 'unequivocally' was overused in this chapter - this isn't part of the calibrated language. Statements of fact can be made without using the word 'unequivocal'. [Gillett Nathan, Canada]	Accepted. Improved, and fixed problems.
40803					Some key definitions needs to be more carefully used. For example, "land use", "land use change", "land use and land use change" are all used throughout this chapter and should be consistently defined. Suggest to link to the Glossary. [TSU WGI, France]	Accepted. Changed.
115299					The only main unanswered scientific question I had after reading the chapter was how confident we are in the very high pre-1750 anthropogenic CO2 emissions assessed here? And if these LUC emissions are as high as assessed (328 PgC on Pg 21, ln 38), then following the TCRE concept and proportionality of warming to emissions, wouldn't it follow that we would expect 0.3-0.6C of anthropogenic warming in 1750? Further, are ESMs able to simulate such high preindustrial LUC emissions, while also being consistent with observed paleo-temkperatures? If not, this would seem to be a hole in our understanding which deserves further assessment. [Gillett Nathan, Canada]	Accepted. WE show the very high uncertainty on those emissions.
115301					The standard version of CanESM5, which uses the CMOC ocean carbon model is used throughout the chapter. But there is also a second version of the model - CanESM5-CanOE, which has a more complex ocean biogeochemistry model, which could also be assessed (everything else about this model is the same as the standard CanESM5 except the ocean biogeochemistry, and it is launched from the same initial conditions as CanESM5 simulations, so CanESM5 and CanESM5-CanOE prescribed-CO2 simulations are identical for all variables except ocean biogeochemical ones). See Swart et al. (2019) - https://www.geosci-model-dev.net/12/4823/2019/. [Gillett Nathan, Canada]	Accepted. More models added to section 5.6
115303					In the ES and chapter text there were multiple inconsistent statements on attribution of the observed GHG increase to anthropogenic emissions. For example pg 6, ln 36-37 says that 'it is unequivocal that the increase of CO2, CH4 and N2O in the atmosphere over the industrial era is the result of human activities', whereas line 4-5 just says that it is unequivocal that emissions from human activities were the main driver of the increases. Pg 7, ln 6-7 says only that it is 'very likely' that human activities were the main driver of the increase in CH4. Pg 13 ln 4-6 says 'there are multiple independent lines of evidence that make the relationship between growth of excess GHGs and human actitivities virtually certain'. So the link between the methane increase and human activities is variously assessed as 'very likely', 'virutally certain' and 'unequivocal' in different places, and in different places anthropogenic activities are assessed to be entirely responsible for the increase or just the main driver. [Gillett Nathan, Canada]	Accepted. We have made the statements consistent.

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116587					Please note that Chapter 5 is too long by around 5%, so attention to length is needed when revising the text, figures etc. One option can be to build more on earlier assessments and reduce duplication with other chapters. [Valerie Masson-Delmotte, France]	Accepted.
29317					awesome! [Zangari del Balzo Gianluigi, Italy]	Thank you
127879					Does C burial in lakes and reservoirs get accounted for anywhere in this chapter? [Trigg Talley, United States of America]	Yes, in the budget sections, particularly C budget 5.2.1.5
127881					The relative role of non-CO2 gases should be explained better throughout the chapter. [Trigg Talley, United States of America]	Accepted. Improved.
127883					[PROGRESS] According to the SPM, this report builds on AR5 and the three Special Reports in the AR6 cycle. Chapter 5 does reference SRCCL at all, particularly in terms of land C and CH4 emissions as well as C mitigation options. [Trigg Talley, United States of America]	Accepted. Include more references and build upon from previous reports.
41101					Time periods used for fluxes estimations need to be consistently used for comparison. For example, the period of 2008-2017 is used for anthropogenic fluxes estimations in Figure 5.12 while the period of 2009-2018 is used in Table 5.1 in the same paragraph. [TSU WGI, France]	Accepted. Made consistent.
127885					[RISK] The chapter does not cite Terrer et al. 2019 (reference below), which is the first global-scale empirical estimation of the effect of elevated CO2, synthesizing data from 138 CO2 experiments. The authors conclude that nitrogen and phosphorus strongly constrain the magnitude of the CO2 fertilization effect on biomass expected by the end of the century. Furthermore, in the corrected version of the paper, the authors conclude that the CO2 fertilization effect derived from CO2 experiments is several times lower than the CO2 fertilization effect estimated by ESMs in recent decades. This suggests that the CO2 fertilization effect may strongly slow down in coming decades due to the constraining role of soil nutrients and that models may overestimate the projected potential of plants to store carbon. Citation: Terrer, C., Jackson, R.B., Prentice, I.C. et al. Nitrogen and phosphorus constrain the CO2 fertilization of global plant biomass. Nat. Clim. Chang. 9, 684-689 (2019). https://doi.org/10.1038/s41558-019-0545-2 [Trigg Talley, United States of America]	Accepted. Cited.
127887					Spelling of "fertilization" needs to be consistent. [Trigg Talley, United States of America]	Accepted. Made consistent.
127889					The switching from C to CO2 units is confusing throughout. Choose the IPCC standard and use throughout. [Trigg Talley, United States of America]	We use C for all chapter, but one section CO2 to link to policy previous work. We included C in that section too, both C and CO2
127891					Ensure that key terms (e.g. GTP, GWP, TCRE, ZEC) are defined in the Glossary. [Trigg Talley, United States of America]	Accepted. Defined.
116379					Congratulations for the maturation of the draft chapter, including results from CMIP6. I encourage the chapter team to look carefully at the TS and SPM, and make sure that key findings from the chapter are included. For instance, the box on irreversibility in the TS. [Valerie Masson-Delmotte, France]	Thank you. Accepted, contributions to TS and SPM.

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116381					I am looking for an update of the AR5 ch 6 assessment of the residence time of excess CO2 emitted in the atmosphere (AR5 had a box and a FAQ on this matter). Also, other chapters have an empty box when referring to CO2 lifetime, and please check the glossary definition of lifetime and the order of magnitude given for CO2. [Valerie Masson-Delmotte, France]	Accepted. Added.
72095					Figure 5.2: I like that a different color is used when changing to a different time scale. This is very similar to what is done in Figure 1.3. I do think that in Figure 1.3, they did an even better job, and it would be nice to use a similar approach. [Elke Zeller, Republic of Korea]	Noted - Changed
116389					It is not easy in the chapter ES and in the main text to find what is new compared to findings of earlier reports (AR5, SRCCL, SROCC, SR15). It would be good to be explicit on changes. [Valerie Masson-Delmotte, France]	Accepted. Improved structure of subsections with clearer identification of what is new.
115369					Much of the chapter assesses changes for the RCPs. Will these be updated to the SSPs for the FGD? Or if not, could the authors somewhere insert some text relating changes in the RCPs to those in the SSPs? Can the readers assume that changes assessed for RCP8.5 apply to SSP5-8.5 etc? [Gillett Nathan, Canada]	Accepted. More ssp discussion added.
116395					The coupling of energy, carbon and water fluxes is not well addressed currently in the AR6 chapter drafts. I suggest to coordinate esp with chapters 4, 5, 7 and 8 so as to develop a consistent approach (assessment of related literature and findings). [Valerie Masson-Delmotte, France]	Accepted. A new cross-chapter box included.
116399					Mapping the interplays with other chapters in the introduction and visual abstract of the chapter would be helpful. [Valerie Masson-Delmotte, France]	Accepted..
116401					PETM is addressed twice in this chapter, and there is some duplication with ch 2 and ch 7.At least cross references are needed but maybe a common box could save space and avoid duplication. [Valerie Masson-Delmotte, France]	Accepted. Cross references added, minimized repetition within chapter.
115893					FAQ5.1 Why refer to "wind throw" here? Would it make sense to explicitely refer to soil respiration and carbon in soils? To the link with the observed greening trend? Please explain which of the ocean and land carbon sink is known with better accuracy. Under which conditions (when) is it expected to detect a reduced ocean sink? No mention of blue carbon in this FAQ? What controls a reduction in the fertilization effect? why only above 2°C would the land sink be reduced? What about permafrost (link to FAQ5.2)? Figure = it could be good to provide key numbers and also show the lack of trends in the figure ; the figure does not show figure trends wwhich are discussed in the text. [Valerie Masson-Delmotte, France]	Taken into account - see response to comment 115821
116405					The starting points for the chapter sections are not alwayws explicit. It could be nice to summarize related findings from AR5 and SRCCL, SR15 or SROCC as starting points, and then focus on the assessment of recent literature. Some parts of the chapter still have a "text book" tone (with descriptive parts, without use of the confidence language, and clear summary statements). [Valerie Masson-Delmotte, France]	Accepted. We have improved the structure of the subsection including departing point.
115895					FAQ5.2 does not permafrost thaw also release N2O (missing with the focus on carbon)? What about Tibet permafrost (size of the carbon pool)? What about abrupt thaw? (check coherency / SROCC). The text is not clear enough on the timescale of potential emissions (gradually, decades-centuries?), and the potential cliamte amplifying effect (how to convert extra PgC per °C to additional °C). [Valerie Masson-Delmotte, France]	Partially Accepted -- adding discussion of permafrost N2O to permafrost box

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115897					FAQ5.3 check coherency with FAQ4.3 on detectability of consequences of reduced emissions. [Valerie Masson-Delmotte, France]	Accepted.
115899					FAQ5.4 it would be good to remind the reader of the timescale of the climate effect of the residual CO2 fraction remaining in the atmosphere. AR5 chapter 6 FAQ said "After 2000 years, the atmosphere will still contain between 15% and 40% of those initial CO2 emissions", is it still valid? [Valerie Masson-Delmotte, France]	Accepted
116415					Ch 5 has the potential to reflect emergent literature related to the effect of COVID19 on emissions and atmospheric composition and chemistry, to coordinate with other ch (2, 6, 7?). There was agreement that the assessment would need to be placed in a specific cross chapter box as a specific item introduced after the SOD. [Valerie Masson-Delmotte, France]	Accepted. New cross chapter box added and referred in chapter 5. Also add estimate for 2020 CO2 emissions in emissions section.
5315					'methane' and 'CH4' are used interchangebly throughout chapter, suggest using CH4 after defining on first appearance [Sheel Bansal, United States of America]	Accepted. Changed to CH4.
40907					The concern is that key assessements of this chapter are missing in the SPM with little to SRCCL. The carbon and other biogeochemical cycles do not have a separate section in SPM, which did exist in AR5 SPM. [TSU WGI, France]	Accepted. Respective section of the SPM have been adapted to include information from Chapter 5, most notably Figure SPM7, 10, and text in Section B4, D1
116447					Coordination with ch 9 and 11 is needed to provide an assessment of compound events in the ocean [Valerie Masson-Delmotte, France]	Noted - Compound events in the ocean are largely the remit of WG2 which assesses the impact of multi stressors on ecosystems
58607					Make sure all figures in the chapter/report use color blind-safe colors. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted.
116477					Ch4 explores simulations in response to future volcanic eruptions, and implications for near term changes. Could insights from ch 5 on impacts of eruptions on carbon fluxes be also used in that discussion? [Valerie Masson-Delmotte, France]	Accepted. There is some literature but with high uncertainties.