# Columbia University

IN THE CITY OF NEW YORK
LAMONT-DOHERTY EARTH OBSERVATORY

March 18, 2021

Dear Associate Editor Fortunat Joos –

Thank you for your comment and those of the Reviewers. They have helped us to substantially improve this manuscript.

In addition to the detailed responses to all three sets of comments that are appended, we wish to highlight several key changes made with this Revision:

1. The need to clarify the impact of warming was highlighted in the review, and this led us to explicitly separate its effects in our IRF decomposition (Table 1, Figure 5). The effects remain small, as originally stated, now they are explicitly quantified and clearly presented.

2. In reviewing IRF scripts so that Figure 5 could be remade, we identified a unit conversion problem, and this resulted in the need to retune the IRF model to match the CESM. We now use h=51m to replicate CESM. This leads to ocean carbon uptake reductions in RCP8.5 and RCP4.5 both being dominated by the impact of carbonate chemistry. Now, only the 1.5C scenario has significant slowing attributable to reduced transport of carbon from surface to depth --- this transport change is attributable to a large reduction of the global-mean vertical gradient of Cant from surface to depth in the ocean (Figure 3c, Figure 4), given the assumed constant ocean circulation.

3. We have removed all mention of "one-dimensional model" and replaced this terminology with IRF. In addition, we have explicitly stated in several places that the decomposition equation including Kz is meant to be conceptual and to assist in understanding decomposition approach, but does not represent the IRF itself.

4. We have made substantial revisions to the text throughout to integrate the updated results and to clarify the presentation.

Thank you again for your careful attention to this manuscript,
Galen McKinley

Sincerely,

Galen McKinley, Professor, mckinley@ldeo.columbia.edu
Sean Ridge

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**Associate Editor Decision: Publish subject to minor revisions (review by editor)** (02 Mar 2021)
by Fortunat Joos
Comments to the Author:
Dear authors

Your revised manuscript has been evaluated by the two reviewers. Both find your manuscript a useful contribution to the field and suggest publication after minor or technical revision. Based on their recommendations and my reading, I ask you to further revise the manuscript before a potential publication.

My own comments and suggestion are added below. Most notably, I find the terminology regarding the "gradient effect" misleading. The effect termed "gradient effect" arises, in my understanding, from the time-varying evolution in atm. CO2 and its deviation from an exponential curve. This should be reflected in the name for this effect. Section 2.3 is, in my opinion, not very clear and requires further improvement. I also suggest renaming "one dimensional model" to Impulse Response Function (IRF) model throughout the manuscript. As noted by one of the reviewers, there is some confusion regarding the representation of gradients in the IRF model and how this is described in the manuscript.

Thank you for submitting your work to Biogeosciences. I am looking forward receiving your revised version.

Yours sincerely,
Fortunat Joos

**Major Comments from the Associate Editor**

1) The terminology of the attribution of Canth to mechanisms is confusing and misleading:
The CESM and IRF model results for Canth are compared to a historical scaling approach. The deviations of ocean uptake are attributed to "carbonate chemistry" and "a vertical gradient effect" (Eq. 11).

The historical scaling is taken as a reference to discuss mechanisms that lead to deviations from these scaling results. The assumptions of the historical scaling to work are (i) the system is linear, i.e. no change in carbonate chemistry and circulation, and (ii) the forcing of the system is exponential, i.e. the atmospheric CO2 follows an exponential curve. Given this, it seems natural to attribute deviations to (i) carbonate chemistry (as done by the authors), and (ii) to deviations from exponential forcing – an atmospheric pCO2 history effect.

The deviation in the vertical gradient between Canth estimated from historical scaling and simulated by CESM (Fig. 3 and 4) arises both from the carbonate chemistry (changes in buffer factor) as well as due to deviations of the atmospheric pCO2 evolution from an exponentially increasing curve. (In addition, there may be changes in circulation, wind speed and air sea gas transfer coefficient and in the marine biological cycle that also may affect the simulated vertical gradient in Canth in CESM – albeit these effects are likely of minor importance here as postulated by the authors).

We concur that the historical scaling is based on these assumptions, and that it is already established the historical scaling will not hold as the atmospheric becomes sub-exponential (Raupach et al. 2014). This is described in the paragraph immediately after equation 2.

Our goal is to better understand what are the ocean physical / chemical processes that will cause the deviation from historical scaling. CESM does not allow a separation of the chemical and the physical effects, and so we use the IRF model to estimate this separation.

The historical scaling is a useful reference point because it has been very useful in understanding the carbon sink to date, and analyzing observed interior Cant accumulation (Gruber et al. 2019, Science). But it will not hold going forward, and it is important to understand that the mechanisms by which the sink will diverge will depend on emissions.

The problematic notation is evident when comparing section 3.3 and 3.2. On line 300 it is stated: "These two effects reduce the ocean carbon sink by a total of 31% from the historical scaling, with approximately 1/3 of the effect due to the vertical gradient and 2/3 due to carbonate chemistry." In section 3.2 and Fig. 3 the deviation in the Canth gradient between the historical scaling and as simulated by CESM is shown. It is clear that the integral of these deviations (shown by gray shading) must equal the difference in cumulative air-sea flux of Canth for the historical scaling and for CESM. Thus, about 2/3 of the difference between the vertical gradient in Canth as simulated by CESM and the historical scaling is due to ocean chemistry and not due to the "vertical gradient effect".

What is termed "vertical gradient" effect in this paper, reflects, in my opinion, rather the effect of a time-varying CO2 evolution which deviates from an exponential curve. Thus, I find it misleading and conceptually confusing to talk about a "gradient effect". The terminology should be changed.

What we have been calling the "vertical gradient" effect is not the deviation of the atmosphere from its exponential curve. Instead, it is the excess Cant stored in the thermocline that modifies the near-surface gradient of Cant, and that raises surface ocean pCO2 via re-entrainment / reemergence from below. It is a primary goal of this work to illustrate that as the atmosphere pCO2 growth rate slows or becomes negative, this effect will be increasingly important to setting the magnitude of the ocean carbon sink.

To make our focus on the physical transport of carbon more clear, and because we are focused mostly on the global-mean profile, we have renamed this "vertical transport of Cant" throughout the manuscript.

The profiles in Figure 3 and 4 are for the full CESM, including both the effect of carbon chemistry and of ocean vertical transport of Cant. These figures illustrate how the vertical profile differs from the historical scaling under each emission scenario. With Figure 5, we use the IRF model to attribute how much of the change in these profiles is caused by carbon chemistry as opposed to by the ocean vertical transport of Cant.

We have carefully reviewed the text and made an effort to clarify these points throughout.

2) Please replace the term "one dimensional model" with "Impulse Response Function model " and "IRF model". Reason: The IRF model is not a 1-d model and may represent 3-d, 2-d, as well as 1-d models. The IRF model does not resolve a 1-d, e.g. vertical, gradient.
Thank you. We have made this change, and it has much clarified the manuscript.

3) Assumptions for the diagnostic framework and the IRF should be clearly stated.
a) It seems that you apply the response function from the HILDA model and adjust mixed layer depth to emulate the anthropogenic carbon uptake by CESM. Thereby it is assumed that the time scales of anthropogenic carbon removal from the mixed layer and thus of ocean overturning are the same (or at least scale in some sense) in the HILDA model and in CESM-POP2. The IRF model does nicely emulate CESM over the 21st century. However, I doubt that this is true on longer time scale as the ventilation of the deep Pacific and Indian is much too sluggish in POP2 as evident from radiocarbon simulations. It should be noted that the IRF model may not equally well represent CESM results on longer time scales.
This is correct. We use the HILDA response function to emulate CESM. We state this more clearly in the last paragraph of section 2.3. We add a last sentence here " It is important to note that despite the ability of the IRF model to emulate CESM behavior through 2080, this does not mean it would be able to emulate longer timescales; particularly under high emissions, greater ocean circulation and biogeochemical changes are expected beyond 2100 (Randerson et al., 2015)."

b) In section 2.5, it is assumed that the gas exchange velocity k_gas is time invariant. Again this may not hold under future climate change in CESM and in reality. Similarly, changes in the marine biological cycle are neglected.
Yes, this is correct. To make this assumption more clear, we now start this section (now section 2.4) with "Considering anthropogenic perturbations on top of a background natural state, the air-sea flux of anthropogenic carbon is a function of the pCO2 in the atmosphere and ocean (Equation 8), and pCO2 is a function of the anthropogenic carbon content (Cant) and the temperature (T): Fant(pCO_2^atm, pCO_2^ocn(C_ant, T)). Change gas-exchange rates are assumed negligible, and because the biological pump is part of the background natural cycle, it is also assumed constant."

c) ocean circulation is assumed to be time invariant and not affected by global warming. This is stated. However, it also applies to Eq. 14 where the last term only reflects the impact of warming on carbon chemistry, but not on ocean transport and stratification.
This is correct, we have added note of this. We have also explicitly separated out the impact of warming on solubility for additional clarity.

4) There is technically nothing wrong with section 3.4 and and Fig. 6. However, I do not see that this discussion and analysis adds any particular insight. I suggest to delete this text and figure from the MS. It is well established that the surface ocean pCO2 equilibrates quickly with the atmospheric pCO2 (typical time scale of 1 yr, see e.g. Broecker and Peng, Tracers in the Sea, 1984) and that the air-sea disequilibrium in pCO2 is small relative to the absolute pCO2 value. Correspondingly, the gross air-to-sea and the gross sea-to-air CO2 flux are very comparable. This fact needs not to be iterated.
We have removed this section as suggested.

Further Comments

Line 17: I find the number of 39% misleading. Ocean uptake should be related to total anthropogenic emissions, including those from land use. LUC emissions are substantial over the historical period. A more conventional estimate is that the ocean has taken up between 25 to 30 percent of anthropogenic emissions.
We respectfully disagree on this point. It is our carefully-considered conclusion that as long as the denominator is clearly cited as being fossil and cement emissions, as is the case here, it is not inaccurate to cite this number. The value is taken directly from the Global Carbon Budget 2019 (their Figure 9), and the same choice of denominator has been used and presented very prominently – i.e in the abstract - of Sabine et al. 2004 (Science). We recognize that many authors choose to include the Land Use source in the denominator to arrive at "total anthropogenic emissions", but cumulatively, the land was a net source until ~1950 and only after that began to trend to a sink. Moreover, the Land Use component on its own is very poorly quantified. On the other hand, from ocean data (Sabine et al. 2004, Gruber et al. 2019), we have a good estimate of what the cumulative uptake by the ocean of excess carbon has been, and from this we can get a relatively-low-uncertainty estimate of the net land flux, as demonstrated by Khatiwala et al. 2009 and replicated in Figure 1 of McKinley et al. 2017 (Annual Rev. Marine Science). We see no reason to incorporate the very uncertain land-use source into the denominator, and would like to maintain consistency here with our group's approach by citing the magnitude of the cumulative ocean sink relative to fossil fuel and cement emissions. To make it more explicitly clear that we are talking about "excess carbon in the ocean", which is what the studies of Sabine 2004 and Gruber 2019 are able to quantify, we have revised the sentence. It now reads "The ocean has absorbed excess carbon equivalent to 39% of the $CO_2$ from industrial era fossil fuel combustion and cement production (Friedlingstein et al., 2019). "

L30: effective surface diffusivity: The term "surface" seems misleading. It is a vertical diffusivity applied to operate over the upper and deep ocean
We have removed the mention of diffusivity from this sentence.
l. 38: natural sink – suggest deleting "natural"
Thank you. We have made this change.
l40: expand to say: "are strictly exponential and the system is linear". As this holds only for linear systems.
Thank you. We have made this change.

L124: historical period: 1920 to 2006: Should this not read 1820 to 2006? Starting the simulation in 1920 would lead to a massive cold start problem. I think the RCP simulations started in 1820.
Thank you. This now reads "Following a long preindustrial spin-up, all simulations used here are forced for the historical period (1850-2005) with observations of $pCO^{atm}$. The individual ensemble members of the Large Ensemble are branched off at 1920 (Kay et al., 2015)."
Section 2.3: I found this section somewhat unclear and confusing.
It is not clear to me what is done. I guess that you use the IRF function of HILDA from Joos et al. and adjust the mixed layer depth h to 109 m to match the anthropogenic carbon uptake as simulated by CESM for the historical period. Eq. 7 and Eq. 10 (together with the chemistry described in the appendix) are then used to compute ocean uptake. If this is correct then please state this explicitly. It would also help to bring Eq. 10 next to Eq. 7.
It is also my interpretation that Eq. 8 and 9 are not applied in this study, but added for illustration and to conceptually separate mechanisms. I recommend moving these two equations and the related text to section 2.5 to keep the description of the IRF model strictly separate from the description of the separation into individual terms.
Thank you. We have removed these equations and streamlined section 2.3 to clarify the IRF model.
L145: typo: in -> on
Thank you. We have made this change.
L145: Change "one dimensional" to "reduced form" or to "substitute".
Thank you. We have changed this to "reduced form" throughout the manuscript.
L160ff, eq. 8: Eq. 8s represents rather a conceptual than an operational description and the text on l.160 is in my opinion not correct. Even in the case of 1-d box diffusion model, many ocean layers are used to determine the carbon flux at the base of the mixed layer. Here it is not clear how the gradient dC_ant^ML/dz would be derived. I suggest rewriting the text to read something like "The convolution integral (Eq. 7) may be conceptually linked to the following tendency equation of anthropogenic carbon in the surface ocean mixed layer:
Eq. 8
The change in the mixed layer carbon concentration results from the air-sea flux (F_anth) and from the flux between the mixed-layer and the deeper ocean, here formally written as a diffusive flux.
Thank you. We have fully changed this discussion, and moved it to section 2.4.
Suggest deleting "The one-dimensional model'sKz;eff must match that of the ocean component of the ESM being emulated (Gnanadesikan et al., 2015)." as this sentence is not needed and rather confusing given that it remains unclear how the gradient is determined.
Thank you. We have made this change.
L 167: Please specify from what the "diffusive flux term" is estimated. I first guessed from the air-sea flux and the change in surface layer concentration as simulated by CESM. However, then in section 2.4 it becomes clear that the separation is done using the IRF model.
Thank you. We have substantially clarified these sections and this test no longer appears.
L198: only true under the assumption of a constant k_gas and in a spatially-averaged framework. These assumptions may not necessarily hold for CESM. Please clarify the limitation.
Thank you. We have fully changed this discussion; moved it to section 2.4. We clarify that the

use of Kz and the whole equation 12 is meant to be conceptual only.
Line 207: expand to read: “impact of warming on the carbon chemistry”
Thank you. We have made this change.
Line 259: typo: fro -> from
Thank you, correction made.
Section 3.2: The deviations in Canth between the historical scaling and POP2 are due to changes in the buffer factor and due to deviations from an exponential forcing in the scenarios. Correct? I suggest to state this for clarity and to avoid confusion with the attribution done in the next sections.
We believe that our clarification in terminology to “ocean vertical transport of Cant” has resolved this issue.
Line 318: two verbs: “acts increases”
Thank you, correction made.
L 324: typo: around
Thank you, correction made.
L385 to 391: This paragraph is misleading and should be deleted. An IRF model is applied and not a 1-d diffusion-advection model.
We have substantially modified this paragraph to correctly state that the IRF model emulates and array of physical processes in the CESM.
L457 ff: Suggest to use the term Impulse Response Function model instead of 1-d model throughout the MS.
Thank you, we have made this change throughout.

**Review 1** of the first revised version of "Ocean Carbon Uptake Under Aggressive Emission Mitigation" by Sean Ridge and Galen McKinley, submitted to Biogeosciences Discussions https://doi.org/10.5194/bg-2020-254

General Comments

The revised manuscript by Ridge and Galen takes into account most of the critical points that another reviwer and I have raised in the first version. It is much more concise now, much clearer in its nomenclature, and also puts the results more into relation with previous work.

As a result, the manuscript has become quite helpful for understanding how future uptake of carbon in the ocean will evolve at least in one particular earth system model. The methodology proposed here may also be useful to analyze the results from other models. I think it can now be accepted for publication in Biogeosciences.
Thank you for these detailed comments. We have addressed them, as noted below.

Line 108: empty space missing after $C_{ant}^{ML}$.
Thank you, correction made.
Line 198: Isn't there a minus sign missing in the inlined equation?
Thank you. Yes, this was incorrect, but it has been removed in the revision to section 2.4.
Line 259: 'fro' should be 'from'
Thank you, correction made.
Line 289: Capitalise the 'V' in DeVries, 2014
Thank you, correction made.
Line 307: the degree in missing in 1.5C
Thank you, correction made.
Line 324: 'aroudn' should be 'around'
Thank you, correction made.
Last line, figure caption 6: Missing space after $C_{ant}$
This section has been removed per the recommendation of the associate editor.
Line 355: The sentence beginning with 'If emissions' is inconsequent in its usage of a 'the stronger $\ldots$ the larger' construction.
This has been revised to read “As emissions are mitigated, the back-pressure effect grows (Figure 3-5).”
Line 361-362: I don't understand this sentence.
This sentence has been removed as part of an overall effort to clarify this paragraph.
Line 368: Too much space after $C_{ant}$
Thank you, correction made.
Line 400: 'Magnitude' should be plural, I think
Thank you, correction made.

References:

At least in Friedlingstein et al, 2019, and in Sanderson et al., 2017, the http-form of the doi contains errors.
CO$_2$ is written without subscripting the 2 in Khatiwala et al.,2009, Peters et al., 2017, Tanhua et al., 2007.
In Sanderson et al., 2017 a special character has sneaked into the ASCII text
In Takahashi et al., 1993, the journal name is incomplete.
Thank you, these corrections to the references have all been made.

**Review 2** of a revised version of "Ocean Carbon Uptake Under Aggressive Emission Mitigation" by Sean Ridge and Galen McKinley

I have reviewed a first version of this manuscript, and I am happy to see that the revised manuscript has improved a lot. I have a few remaining issues (listed below), and I would recommend the manuscript for publication in Biogeosciences after the authors have addressed these.
Thank you for these detailed comments that have helped us to improve the presentation of our work. We address all the comments, as indicated below.

Main point:
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The analogue of vertical diffusion used by the authors is misleading as far as the impulse response function is concerned. In lines 160-168 it is stated: "The convolution integral (Equation 7) is derived from the model's surface anthropogenic carbon tendency equation", which is not correct. The impulse response function (IRF) doesn't know the gradient of anthropogenic carbon, and I don't see how equation 9 and 7 are related. Of course, it is possible to reproduce results from a box-diffusion model sufficiently well by fitting an IRF (as in Joos et al. 1996). But an IRF is not based in any way on the assumption that the vertical mean ocean state can be represented by a 1d-diffusion approach as in equation 8. The IRF is an empirical fit to model results, representing all processes that the fitted model included.
Thank you for your helpful comment. We agree that this was not clear. We have removed the terminology "one-dimensional model", replaced it with IRF throughout. We have made it clear that the discussion of Kz is only a conceptual component, not an actual fitting in section 2.5. We have made the most changes in section 2.3, and have made substantial terminology changes also in the abstract and discussion.

Later we find statements like

*lines 385-388: "Our one-dimensional ocean carbon cycle model represents multiple physical processes that remove carbon to depth as a single diffusive process that is constant in time (Equation 8) using an effective vertical diffusivity, K_z,eff . The value for this term in the one-dimensional model has been set (Section 2.3) so as to mimic advective, eddy-diffusive and watermass transformation processes occurring in CESM.", which wrongly suggests that the IRF would use a vertical diffusivity as a parameter.
Thank you for this comment. We have clarified this paragraph, and also in methods, to make it clear that the IRF model does not have Kz,eff as a tuning parameter. The point that is now more clear is that the IRF is emulating advective and diabatic processes.

*lines 406-408: "The remainder of the climate-carbon feedback is related is due to changing physical transport, which in the one-dimensional model is due only to the vertical carbon gradient and ocean circulation is constant." Again, the IRF does not know the vertical gradient

of carbon.
Thank you for this comment. We have clarified that the impact of “ocean vertical transport” has been diagnosed using CESM in this work.

I would strongly suggest that the authors revise the above mentioned parts of the manuscript. I see that the authors need to introduce the vertical C-gradient somehow, to be able to define the "gradient effect", but please do this in a way that avoids the impression that this is parametrized in the IRF.
Thank you for this comment. This is the most important clarification with this round of review.

Also, it would be good to mention/discuss that the "gradient effect" as used by the authors is found by residual (difference between the "historical scaling" and the ccc-simulation) so you don't actually need a model that models a vertical gradient to derive it.
Thank you for this comment. We have clarified that the impact of “ocean vertical redistribution” has been diagnosed using CESM in this work.

Minor points:
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The concept of a constant sink rate, which allows for defining the "historical scaling" is a central point, but it is introduced too late in the text (lines 63-70). It would make the text easier to understand if these lines could be moved up to somewhere after the definition of k_S (lines 34-44). I see that lines 63-70 deal with the ocean sink only while lines 34-44 more generally deal with the land and ocean sink, but then please generalize lines 63-70 (you write about "...the theoretical prediction of constant sink efficiency..." already in line 40, and for a reader not familiar with the concept this is unclear).
Thank you, we have moved this paragraph up as requestred. We have also removed the potentially-confusing, statement “This result is as expected because the theoretical prediction of constant sink efficiency is only valid if CO2 emissions are strictly exponential, and the system is linear.” The theory for the ocean will shortly be explained, and this is what is critical for the reader to understand.

line 50-51: Please consider replacing "climate-carbon feedbacks" by "carbon cycle feedbacks" (here both, carbon-climate and carbon-concentration feedbacks are meant).
Thank you, this change has been made.
line 74: RCP8.5 is not a "business-as-usual" scenario, it should be called a "high emission" scenario.
Thank you, this change has been made.
line 100-105: Please double check whether this could be more concise. It seems to me some sentences are more or less duplicate.
Thank you, we have made this section more concise.
Section 2.2: Please spell out which version of CESM you are using and add a reference for the whole model. Please also add a suitable reference for the RCPs (e.g. 10.1007/s10584-011-0148-z). It remains unclear to me why the authors are reluctant to properly acknowledge CESM scientists and engineers, as I commented in my first review.

Thank you. We apologize for this oversight. We now reference Hurrell et al 2013 at the start of section 2.2. To further emphasize, to the acknowledgements, we add “We acknowledge the CESM Large Ensemble Community Project and supercomputing resources provided by NSF/CISL/Yellowstone. This material is based upon work supported by the National Center for Atmospheric Research, which is a major facility sponsored by the National Science Foundation under Cooperative Agreement No. 1852977. We thank all the scientists, software engineers, and administrators who contributed to the development of CESM We thank all the scientists, software engineers, and administrators who contributed to the development of CESM.”

lines 177-178: "...we perform two sensitivity experiments...". It is only one sensitivity experiment that is performed, isn't it? The "historical scaling" is not an experiment with the 1d-model. Also, please describe the sensitivity experiment briefly in the main text (not only in Table 1). E.g. "...we perform a sensitivity experiment ... where the buffer factor is kept at pre-industrial level" or similar.

Yes, you are correct that in the last version there was only one sensitivity experiment. Now that we have added the explicit separation of warming, there are two experiments as stated.

line 188: From Figure 6 in Randerson et al. (2015) I roughly read that AMOC is reduced by 10 Sv in 2080. This is quite a significant reduction. So the point is not that CESM hasn't significant circulation changes but rather that the carbon cycle in CESM seems to be relatively insensitive to these changes (on a global scale). Please consider revising this sentence.

We have modified this to “The physical circulation is assumed fixed in the IRF model, consistent with the carbon cycle in CESM not being sensitivity to changes in circulation over 1920-2080 (Randerson et al., 2015).”

line 199-200: "The pCO2ocn closely follows pCO2atm , with the same sign." I don't understand what the authors mean with "with the same sign". Please clarify.

Thank you, we have removed “with the same sign”, as it is unnecessary.

line 217: "Projected Spatial Redistribution of the Anthropogenic Carbon Air-Sea Flux". I think "redistribution" is not a good wording here. Maybe better "Projected Spatial Patterns of Anthropogenic Air-Sea Carbon Flux"?

Thank you, we have changed this to read as suggested.

lines 290-291: "...the ocean would absorb 158 Pg C_ant from 2020 to 2080". You mean would absorb 158 Pg C_ant in addition, right? Please consider making this clearer.

This now reads “Due to the fact that ocean chemical capacity changes in the future, uptake is reduced significantly, -233 PgCant from 2020 to 2080 from what it would be without chemical change (light blue shade). “

line 304-305: Please check the logic of this sentence (it is not clear what "because" refers to).

Thank you, we have clarified this. It now reads “The $\Delta C_{chem}$ effect is the weakest in this scenario, -37 Pg C in 2080. The weak $\Delta C_{chem}$ effect is consistent with this scenario taking up the least anthropogenic 305 carbon because chemical capacity decreases as anthropogenic carbon uptake increases.”

line 392: "differences" is a bit unclear. I think the authors mean "changes".

We have revised this to read "For the historical period, global-mean air-sea fluxes and anthropogenic carbon storage are not substantially different across models, despite these models having substantial differences in the ocean circulation …"

lines 400-402: "We use our one-dimensional model to estimate climate-carbon feedbacks for CESM." If this is really the case a bit more explanation would be appropriate, but it seems the authors use the feedback values from Arora et al. (2013). Please add either more explanation as to how you estimate the feedback with the 1d-model, or delete this sentence. Also CESM's gamma_o is not only weaker than the CMIP5 mean, but it has the weakest ocean carbon climate feedback of all models.

We have eliminated this paragraph as it was not central to our discussion.

lines 403-404: "For CESM, decline in ocean carbon uptake due to climate-carbon feedbacks in high emission scenarios is an order of magnitude smaller than due to change in ocean chemistry (Randerson et al., 2015)." I cannot see that the Randerson paper supports this statement. They cannot separate climate-carbon feedbacks from feedbacks due ocean chemistry with their experiments. Please clarify/justify this statement.

We have eliminated this paragraph as it was not central to our discussion.

line 431: Please specify after which year carbon in the upper 100m starts to decrease.

We have revised this to read "… the anthropogenic carbon concentration begins to decrease starting in 2038, just two years after the maximum pCO2atm of 437ppm is achieved."

Technical:

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line 24: "is controlled" -> "is further controlled"

This change has been made.

line 27: "...dominates regional patterns anthropogenic..." -> "...dominate regional patterns of anthropogenic..."

This change has been made.

line 48: "that the future" -> "that in the future"

This change has been made.

line 59: This sentence duplicate the previous one, please consider merging the two sentences.

This has been revised to read "Climate driven effects stem from the warming of the surface ocean, which reduces gas solubility, and ocean circulation, thus reducing the efficiency of ocean uptake (Friedlingstein et al., 2013)."

line 82: Please consider writing "... referenced to the year 1990, and expressed as a percentage".

This change has been made.

line 145: "based in impulse response functions" -> "based on a impulse response function"

This change has been made.

line 147: "...to the RCP4.5 and RCP8.5 CO2 concentration pathways" -> "...to the RCP CO2 concentration pathways" (applies to all RCPs, not only 4.5 and 8.5)

Thank you, this sentence now reads "….and is also used for all RCP scenarios to convert projected emissions to $CO_2$ concentrations (Meinshausen et al., 2011)."

Section 2.5: There is a couple of instances where the superscript "ML" is missing in the notation for "C_ant^ML". Please check this throughout this section.

Thank you, we have checked to make sure that CantML is referenced when it should be. When

Cant in the ocean more broadly is intended, we use Cant.
line 198: There is a minus sign missing in the equation, please check this.
Thank you. This was incorrect, but has been removed in the revision to section 2.4.
line 233: "Globally-mean" -> "Global-mean"
This change has been made.
line 239: "CESM-simulated air-sea anthropogenic carbon uptake" -> "CESM-simulated anthropogenic carbon uptake"
This change has been made.
line 233: "(Figure 2b)" -> "(Figure 2c)"
This change has been made.
line 249: "at that location" -> "at that depth"
This change has been made.
line 259: "fro" -> "from"
This change has been made.
line 276: "by the the" -> "by which the"
Thank you, correction made.
line 318: delete "acts"
This section has been removed per the recommendation of the associate editor.
line 324: "aroudn" -> "around"
This section has been removed per the recommendation of the associate editor.
line 326: "to a less net uptake" -> "to less net uptake"
This section has been removed per the recommendation of the associate editor.
line 362: "...are renewed connection..." Please check grammar of this sentence.
This sentence has been removed to clarify this paragraph.