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Interactive comment on "Ocean Carbon Uptake Under Aggressive Emission Mitigation" *by* Sean Ridge and Galen McKinley

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Response to Reviewer #1

Thank you for your careful review of our manuscript, your suggestions have helped us improve our manuscript. We have shortened our manuscript by removing all of the text you identified in your detailed comments and we have removed Appendix A. Also, the detailed summary in section 4.1 has been removed and replaced with a much more succinct version that better links with the cited literature. Finally, we have added sentences to section 4.2 contextualizing our work with that

of the climate-carbon feedbacks literature. Our detailed responses can be found below.

The nomencalture of anthropogenic carbon is handled quite confusingly, with Cant meaning very different things in different places in the manuscript without proper distinction. In some places, Cant(z,t) is used to describe the time-and depth-dependent anthropogenic carbon in the 1-d model, in other places, either the depth or time dependency is left away (e.g. line 142). In equation 7, then again Cant means the surface ocean concentration only. I suppose that is also, what is meant on the lefthand side of equation 8, while on the right hand side the depth- and time-dependent field is meant. In that form, the equation cannot hold in the interior of the ocean. I suggest to clarify what is meant in each instance e.g. by adding additional superscripts, e.g. CML ant for the mixed-layer Cant.

We understand that our current naming convention is potentially very confusing. We have followed your suggestions and updated the symbols to the following:

Name	Symbol	Units
Time, depth, and space dependent anthropogenic carbon concentration	$C_{ant}(x, y, z, t)$	mmol m^{-3}
Mixed layer C_{ant} concentration	$C_{ant}^{ML}(t)$	mmol m^{-3}
Atmospheric anthropogenic carbon inventory	$C_{ant}^{ATM}(t)$	Pg C

I think readers are able to understand the linear equation 4 and its consequence equation 5 without the lengthy explanation in lines 114 to 124.

We agree and have updated this section

A similar statement holds for the explanation of the impulse response function 7

from lines 179 to 195; this is a fairly standard mathematical technique and does not need to be explained in so much detail.

We have removed this lengthy explanation, which we agree is too verbose.

An important point in the methods chapter is line 220 to 222, where it is stated that the relation between CantML(t) andpCO2ocn contains the effects of changing buffer factor and of changing temperature. Here would be a good place to discuss how the decomposition of effects made here is related to the more traditional feedback analysis that results on the two feedback factors β and γ. I must admit that I wondered why it is necessary to have fitted relation between the two (Appendix B), rather than using standard carbonate chemistry and calculating one from the other, assuming constant alkalinity

We agree that this needs to be better linked to the introduction. Quantifying carbon climate feedbacks requires a different set of simulations, but their effects are included in our analysis. We have added a discussion of this to discussion section 4.2:

“While uncertainty in the mean state of ocean circulation is most important over the next 60 years, as warming increases, the magnitude of climate-carbon feedbacks related to ocean circulation increase. For simulations made with the one-dimensional model and the CESM, we simulate the effects of real-world climate-carbon feedbacks. The strength of ocean climate-carbon feedbacks (o) in CESM is weaker (-2.4 Pg C K^{-1}) than the CMIP5 multi-model mean (-7.8 Pg C K^{-1}) (Arora et al., 2013; Friedlingstein et al. 2015). Compared to Cchem, the decline in ocean carbon uptake due to climate-carbon feedbacks in high emission scenarios is an order of magnitude smaller. The one-dimensional model only simulates the reduced CO2 solubility feedback, but is close to the CESM response to warming (Figure 2d), thus indicating solubility effects dominate climate carbon feedbacks prior to 2080. The remainder of

the climate-carbon feedback is related to changes in ocean circulation. In simulations out the year 2300 with CESM (Randerson et al. 2015), or simulations with models featuring a rapidly declining AMOC (Sarmiento et al., 1996), AMOC collapse plays a large role in reducing carbon uptake. The small effect of changing ocean circulation in our simulation is likely because AMOC has yet to collapse by 2080 (Randerson et al. 2015). While assuming that climate-carbon feedbacks related to ocean circulation are small prior to 2080 is consistent with the behavior of the CESM (Randerson et al. 2015), this may not be hold true for the Earth System itself. The uncertainties associated with the timing and magnitude of climate-carbon feedbacks can be avoided by mitigating climate change (Randerson et al. 2015).”

We have added the following justification for the fitted solution to Appendix B:

“There are two main reasons for using a fitted solution. First is that if every variable other than temperature and carbon are held constant, using the full carbonate system equations provides no additional accuracy, thus the additional model complexity is unjustified. Second, we want to be consistent with other models: the concentration scenarios used in CMIP5 (RCP4.5, RCP8.5) are generated using the same model and thus the same representation of ocean chemistry.”

Line 314: "Assuming ocean circulation remains constant" To what extent is that assumption justified?

We justify this assumption in an updated discussion section 4.2. This updated text is in the response to your comment that starts with “An important point in the methods chapter is line 220 to 222...”

I do not completely understand, how the fields shown in Figure 4 have been calculated. Is this the zonal average of the 3-d model output minus the

expected profile from the 1-dimensional model? How does advection enter this picture?

We have updated our description of Equation 6, which was understandably hard to interpret. Your interpretation is close, except that the calculation of the expected Cant is derived from the CESM zonal mean Cant in 1990. Three dimensional advection is thus accounted for.

Section 4.1 contains very little discussion and is in large parts a repetition of results. Then, in the end (lines 468-471), some other results on future Cant uptake are cited, but without giving any connection to what this study has shown. Is there a relation between these results, or is it just inserted here without too much meaning?

Section 4.1 has been revised for clarity and brevity. The cited works provide the additional context of what is going on with the land sink during rapid mitigation, and what happens to the ocean sink beyond 2080.

"We find . . .": Is this really a finding, or not rather an assumption that went into the methodology?

We agree that the current wording is imprecise. Theory predicts that the vertical Cant profile would behave as a function of pCO2atm under exponential forcing but we show that this is true in a complex model. We have updated this line to be more precise:

“We find that an exponentially increasing pCO2atm allows for the vertical Cant profile to behave as a function of pCO2atm, as predicted by the historical scaling.”

Appendix A again explains the impulse response function, even repeats

equation 7, and gives details about using it that can be found in many textbooks. I suggest to remove it (including Figures A1 and A2), or shorten it to not more than one paragraph.

We agree and have done as you suggested, shortening it to a paragraph.

Appendix B: The coefficients shown in equation B3 to B7 all have different units, but these are not shown.

Thank you for noticing this omission, units have been added.

Lines 297 and 298: should one of the mentioned scenarios be RCP8.5 rather than RCP4.5?

We removed one of the sentences, it was repetitive.

References: Many references give two web addresses for the cited papers, one being the http-form of the doi (I would rather have the doi without the https://doi.org in front), and the journal address. Are really both necessary?

We have the same preference but we are using the Biogeoscience’s LaTeX template which automatically formats the references.