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

Anonymous Referee #2

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This manuscript analyzes the efficiency of ocean carbon uptake, defined relative to an exponential scaling, for scenarios of different future CO2 concentrations (ranging from a worst case to a strong mitigation case). The authors use an Earth system model as well as a simple 1d-model of the ocean carbon cycle fitted to the ESM. The simple model is used to decompose uptake efficiency changes seen in the ESM simulations into different drivers. The results demonstrate the strong decline in efficiency (according to the chosen metric) in the strong mitigation scenario, which is driven by changes in the vertical gradient of anthropogenic carbon in the ocean.

Although the basic principle of declining uptake efficiency under strong mitigation is quite intuitive, the analysis and decomposition into drivers provided by this manuscript is definitely of interest to the readers of Biogeosciences. However, the manuscript is

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(mainly in the introduction and the methods section) not well structured, suffers from imprecise language, and is generally not very concise. Assumptions are often not clearly stated. As the manuscript stands, it does not meet the high standards of Biogeosciences. I give a few examples below (point 1), but my concerns are not limited to these examples. I urge the authors to go carefully through their text and rewrite (with a focus on conciseness) the introduction and the methods section. I have also a few other, more scientific concerns, which I also list below.

Major points:

1) Structure and conciseness of the manuscript (mainly introduction and methods):

*The concept of an exponential growth of emissions leading to a constant sink rate (under assumptions) is quite central in this work, and it needs to be introduced and put into context. Currently this concept is first mentioned in passing in line 44. It needs to be introduced to the reader before the sink rate is mentioned.

*line 50-53: " Nearly every nation..." I don't see that this sentence adds anything new here, consider deleting.

*The authors claim that "In the RCP8.5 scenario (Meinshausen et al., 2011), pCO2_atm increases exponentially..." (line 71). To me it is unclear to which degree the RCP8.5 emissions or concentraions can be approximated by an exponential, but this is not very relevant to this study either (since the baseline is an idealized exponential growth). RCP8.5 is the outcome of an advanced modelling exercise, so the emissions are not strictly exponential.

*Throughout the manuscript, the authors mention exponential historical emissions. It should be made clear that this is an idealization (e.g. by saying "roughly exponential" or similar). This is already the case in some places but missing in others.

*lines 54-67: Again, all this could be much more concise. The feedback studies mentioned are considering a single (exponential) concentration pathway, so they cannot

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quantify an uptake efficiency for different emission pathways. (And yes, in addition to this, they cannot quantify the contribution of a changing buffer factor, either).

*line 74-78: Consider moving this up to lines 40-50 where k_s in general is introduced.

*lines 102-106: This text is not necessary, we do not need a summary of subsections at the beginning of a section. Please consider removing. The same applies for lines 271-277.

*line 108: What is the first sentence of 2.1 supposed to tell us? Just start with "The efficiency metric (eta) used here is defined as k_m ..."

The text explaining equation 4 (lines 112-118) can be shortened substantially: "The historical scaling for ocean C_ant uptake (F_ant) is defined as: (equation 4). The overset "" indicates that a variable has been extrapolated using the historical scaling. Here, we diagnose F_ant(1990) from the CESM large ensemble simulations. For example...".

*Delete unnecessary words, e.g. "mathematically".

*Lines 129-130: "While k_M remains constant,...". This does not reflect the logic of this manuscript. The authors use the exponential scaling to define a baseline against which simulated quantities are compared. The actual k_M is not and does not need to be constant for this exercise.

*Line 144: "The CESM provides a realistic simulation of the response of the ocean carbon cycle to climate change." What do the authors mean by "realistic"? I would suggest to delete this sentence.

*Section 2.3: Impulse response functions are a well established tool in climate modelling. It is useful to give a short explanation for those readers that are not familiar and highlight those aspects that relevant for this study, but otherwise the authors should refer the reader to the literature and shorten section 2.3 substantially. Likewise the Appendix A is not necessary. The most important assumption of IRFs is constant circulation. The most important aspect of the Joos-IRF (hidden in the Appendix A) is

the fact that, contrary to atmospheric IRFs, the mixed layer IRF take ocean carbon chemistry (including changes in SST) into account.

*Throughout section 2, it is unclear what the symbol C_ant(t) is supposed to denote. In equation 7 it is the anthropogenic carbon content of the mixed layer, but otherwise C_ant (often) seems to denote the total ocean anthropogenic carbon. The authors also frequently use the expression "C_ant air-sea flux", which I would suggest to replace by "air-sea flux of anthropogenic carbon" (and use F_ant as a short form of this if necessary).

*Section 2.3: The remaining description of the 1d-model is verbose and confusing. Apparently, the authors "extend" the mixed layer IRF downward by "plugging" a diffusion equation under the mixed layer IRF? Yes, the downward flux can be determined "by residual", but then how are the profiles of C_ant(z) calculated?

*Section 2.5: Equation 12 can be derived by assuming F_ant = F_ant(pCO2atm(t),pCO2ocn(t)). Then, later, it is additionally assumed pCO2ocn=pCO2ocn(C_ant,T). This should be made clearer. (Again C_ant here means surface C_ant).

*Section 2.5: The equation del F_ant/del pCO2atm = del F_ant/del pCO2ocn, is this based on Equation 10? Then a minus sigh is missing. Also, the dependency of the transfer velocity on temperature is neglected in this step. How does it follow from Equation 12 that "The pCO2ocn closely follows pCO2atm, and the sign of their growth rates is the same"?

*Section 3.1: If a paragraph begins with "In the RCP4.5 scenario, changes to the spatial pattern lie somewhere between RCP8.5 and the 1.5C scenario" both scenarios should have been discussed already. This is not the case here.

As pointed out above, this list is not exhaustive.

2) In my opinion, the term "historical scaling" used by the authors is misleading or at

best confusing. The sink rate has not been constant over a substantial part of the historical period in observations (Raupach et al. 2014, cited) as well as in the model experiments used in this study (Fig S1). If the authors wish to replace the previously used term "transient steady state", why not just saying what it is, e.g. "exponential scaling" (or something similar that does not refer to the historical period)?

4) Choice of time periods: The time period 1920-2006 is not the "historical period". From a CMIP5 perspective this would be 1850-2006. Why do the authors choose 1920 as a starting year? Likewise, why do the authors not use the last 20 years of the scenarios, which would be most interesting period in the mitigation scenarios?

5) 1d-model evaluation: The space saved by shortening Section 2 could be invested in presenting a brief evaluation of the (full) 1d-model compared to CESM. How well does the fitted 1d-model reproduce F_ant in the 3 different scenarios? More important, how well are vertical profiles of C_ant simulated? To me it is not given that the 1d diffusion model has skill in reproducing the CESM global mean C_ant profiles for all scenarios, but this is the basis of the analysis of the "gradient effect".

6) Section 3.4: This section is not easy to follow. What is the main point here? I guess it is the fact that the ocean uptake in the strong mitigation scenario after 2040 is maintained by the ocean through continuous downward mixing (otherwise the surface ocean would start outgassing because pCO2atm declines already). Could the authors please add some easy to understand explanations here? Further, related to my point 5) above, how realistic is this process simulated by a 1d-model? In reality we would have upwelling of waters that have been last in contact with the atmosphere in pre-industrial times, that can potentially sustain ocean uptake even under declining CO2, but this is not the case in the 1d-diffusive model. Here the processes must be different. Could the authors please comment on this?

7) The authors should appropriately acknowledge the tremendous work of hundreds of scientists and engineers that made the CESM simulations possible. The

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NCAR CESM website specifically asks authors to do this, this is not optional (http://www.cesm.ucar.edu/publications/).

Minor points:

line 20-21: "The ocean has absorbed 39% of the CO2 from industrial era fossil fuel combustion and cement production (Friedlingstein et al., 2019). The rest of the CO2 remains in the atmosphere where it acts as the primary driver of climate change." Even if the authors seem to neglect emissions from land-use change the number 39% is not correct (it is 36% calculated from Friedlingstein page 1812), but more importantly, land use change should be included here, since it is an anthropogenic emission to the atmosphere (and is treated separately in the GCB). According to Friedlingstein et al. 2019, the ocean has absorbed 25% of the CO2 emissions from industrial era fossil fuel combustion, cement production, and land use change (Friedlingstein et al. 2019, page 1812).

line 35: "...one-dimensional diffusion models have been shown to be consistent with observations and complex models (Gnanadesikan et al., 2015; Oeschger et al., 1975)." Please consider adding "on a global scale" or similar.

line 54: "The reductions to efficiency that are attributable to a slowing pCO2_atm growth rate will be at least partially compensated by a decrease in the strength of the climate-carbon feedbacks". Climate-carbon feedbacks will decrease F_ant,M, so why would this compensate a decrease of k_M? Please clarify.

line 86-87: "... C_ant concentration at all points in space also follows the historical scaling" and "The amplifying effect..." It is unclear to me what is meant by "follows the historical scaling" and "amplifying". Please clarify.

line 176-178: The MAGICC model uses the Joos et al. 1996 IRF for the Princeton GCM, not the HILDA model as the authors do. So it would be correct to say "A very similar IRF has been used to..."

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line 293: "...the RCP8.5 scenario features a consistent spatial pattern of the C_ant air-sea flux..." What does "consistent" mean here? Please consider rewording.

line 306: "The efficiency decrease is linear in RCP8.5 and RCP4.5, but exponential in the 1.5C scenario." The decrease is neither exactly linear in the RCPs nor exactly exponential in the 1.5 scenario. Please add "approximately" or similar.

line 408: "...i.e. how closely ocean carbon uptake follows the observed proportionality between uptake and atmospheric CO2". The proportionality is not exactly observed. Please be more precise here.

line 409: Again, in RCP8.5 pCO2atm might increase roughly exponential.

line 429-430: Here it should be mentioned that changes in ocean circulation due to warming are not consistently treated in this study, since they are present in CESM, but not in the 1d-model.

line 445-446: "Therefore, climate simulations extending beyond 2100 are needed to quantify the back-pressure effect in high emission scenarios." These simulations exist, the CMIP6 SSP5-8.5 scenario has an extension to 2300.

Technical:

line 37: "is described" -> "can be described"

line 67: delete "additional"

line 69: "We will compare..." -> "We compare..."

line 115, 117: delete "mathematically"

line 228-229: "With these experiments,..." I would suggest to reword this sentence: With these experiments we can decompose the total anthropogenic carbon uptake into contributions from...

line 297-298: The 3rd sentence seems to repeat the 1st sentence? Please consider

removing.

line 314: "...and the deviation is positive" This can be deleted.

line 459: "but" should read "such that"(?)

In figure captions: Replace "Output from the ocean component of the CESM of the global mean C_ant profiles..." by "CESM global mean ocean C_ant profiles..." (and similar in other figure captions).

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