

Review of "Controls of ocean carbon cycle feedbacks from different ocean basins and meridional overturning in CMIP6" by Anna Katavouta and Richard G. Williams

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Referee comment on "Ocean carbon cycle feedbacks in CMIP6 models: contributions from different basins" by Anna Katavouta and Richard G. Williams, Biogeosciences Discuss., <https://doi.org/10.5194/bg-2020-487-RC1>, 2021

The authors present a detailed analysis of ocean carbon cycle feedbacks, which is a useful extension of the recently published global CMIP6 carbon cycle feedback paper (Arora et al. 2020). The manuscript goes beyond the Arora et al. study in that it focuses on the contribution of different ocean basins to the feedbacks and also explores circulation changes (using AMOC strength as a proxy) and their relation the feedbacks. The authors diagnose the basin wide contributions of preformed (saturated/disequilibrium) and regenerated carbon pools to the feedbacks. The description of the methodology is more detailed and contains additional diagnostics compared to the Arora et al. study. This manuscript is clearly within the scope of Biogeosciences, and I believe it will be of great interest to ocean carbon cycle community. The manuscript is generally well written (with some exceptions pointed out below) and I recommend it for publication in Biogeosciences after a few points detailed below have been addressed by the authors.

Main points

1) Title: I don't think "controls of feedbacks from different ocean basins" is a good title. I am not a native speaker, but this sounds a bit odd to me. What the authors present is the "contribution of different

ocean basins to carbon cycle feedback", and feedbacks are also attributed to different processes (including AMOC). As also noted further down, I don't think that the wording "control of AMOC on feedbacks in CMIP6 models" is appropriate. For CMIP6 models, the authors show a correlation between pre-industrial AMOC and AMOC weakening. "Control" implies a detailed mechanistic explanation, in my opinion. This is beyond the scope of this study, but therefore I would avoid using the word "control" here.

2) The authors base their definition of feedbacks on changes in DIC-inventories, which, as they note, makes only very little difference at the global scale. However, at the regional scale the difference can be large (see Fig. 3), and therefore I suggest to use a different symbol for the feedbacks based on DIC-inventories. Since the feedbacks derived from carbon fluxes are the standard definition, I would use something like β^*/γ^* for the feedback estimate based on inventory changes. This would also simplify the discussion at the beginning of Section 3, where the two β/γ definitions are compared (the authors could then just write " β^* " instead of "beta, estimated from the regional ocean carbon storage").

Also, it seems the authors point out that the feedback definition based on inventory changes makes more sense than the "traditional" one based on accumulated fluxes (line 290-292: "...to gain more mechanistic insight, so as (i) to account explicitly for the ocean transport of carbon..."). Here I would disagree: From the feedback perspective, the flux at the air-sea interface (and changes to it) is the process we are interested in. Transport of carbon below the ocean surface leads to a disconnect between the actual feedback process at the surface, and where DIC-inventory changes are diagnosed (nicely illustrated by Fig. 3). Don't get me wrong here: I think the method the authors use is extremely useful to gain a global to large-scale regional understanding of ocean carbon cycle feedbacks, but there is a price to pay. In lines 290-292 it sounds like the authors are selling this "price to pay" as an advantage. Maybe the authors can re-consider their wording here?

3) Figure 3 and related discussion at the beginning of Section 3: This is one of the core figures of the manuscript, but it doesn't account for model uncertainty. I think it would strengthen the manuscript if the authors could expand this figure by 4 panels visualising the model spread (or standard deviation) for the 2 β/γ pairs, and add a brief discussion of where the main model uncertainty lies (and how and why this is different for the two

definitions of regional feedbacks). Also for beta/gamma based on the DIC inventory, it would be great to split this further into the components (sat/diss/reg; this Figure could go into the Appendix).

4) Figure 5 and related discussion: This Figure seems to be flawed:

- why does the total ocean volume add up to only 99%?
- why do the different contributions to beta/gamma add up to different percentages (between 91 and 99%)?

The discussion in lines 332-337 is unclear to me: "In the well ventilated Atlantic Ocean, the additional heat penetrates into the ocean interior and is not confined to the ocean surface, which limits the effect of the reduction in solubility with warming". But the definition of gamma_sat doesn't care whether a water parcel is at the surface or not. I don't think that this is can be the explanation (same in the next paragraph for the Pacific). Please clarify this.

5) Nowhere in the manuscript it is stated how the ocean basins are defined. Where is the delineation between the Southern Ocean and the other basins? What about the Arctic Ocean? Is it included in the Atlantic or omitted? What about marginal seas? Also, the definition of AMOC strength is not given. Please add this information.

Minor points

lines 1-3: This sentence is complicated, and the wording isn't very precise (it is not a "competition between the increase in atmospheric CO₂" but "a competition between the response to the increase in atmospheric CO₂..."). Please consider rewording this sentence and maybe splitting it into two.

Equation 9: "Delta f" is not defined. Here, I would find it worthwhile writing the equation first in terms of DIC_sat, and state that $DIC_{sat}=f(CO_2,...)$. Then write down explicitly what Delta f means.

Equation 10 and 11: Here, I think, it would be easier and shorter to just express beta and gamma in terms of I_sat (without writing out the f-terms explicitly - since I_sat is already defined in terms of f just a few lines above).

Equation 18 and 19: Here, I also think it is much easier to understand if beta and gamma are just expressed in terms of I_{reg} , which is defined just a few lines above in Eq. 17.

line 84-85: This is an assumption, not a conclusion, so starting the sentence with "Hence..." is not appropriate. Maybe use "In the carbon cycle feedback framework introduced by Friedlingstein et al. (2003,2006) it is assumed that..." or similar.

line 266: "...where γ_n includes the non-linearity of ocean carbon cycle feedbacks". This is confusing, it sounds like the definition of γ_n would be different from that of γ , which is not the case. I suggest to delete this.

Figure 3: To make this figure consistent with all other results, please add NorESM2-LM (fgco2 for the BGC run is available).

line 286: What do the authors mean by "asymmetries"? Please clarify.

line 323-324: "By definition, the contribution of each basin to β_{sat} and β_{dis} is approximately proportional to the ocean volume contained in each basin...". I see that this is the case for β_{sat} , but for β_{dis} this depends on ventilation which is not related to the volume. Maybe delete β_{dis} here?

line 445-447: Please check and reword this sentence (consider splitting in two).

line 476-477: "...which is mainly due to the disequilibrium carbon pool and the reduction in the physical ventilation with climate change." The second part of this sentence is a conclusion, isn't it? Then it would be more appropriate to write: "...which is mainly due to the disequilibrium carbon pool, indicating that the Atlantic has the strongest reduction in the physical ventilation with climate change."

line 487: This is also seen in Schwinger et al. 2014

line 491-492: "The inter-model variability in gamma amongst CMIP6 models is relatively large compared with beta...".

I think it is worth mentioning that this is not true in terms of the absolute feedback strength: In terms of PgC taken up by the ocean, it is still the uncertainty in beta that plays the dominant role.

line 513: "...controlled by the AMOC weakening..." As pointed out above, the authors find a correlation, so in my opinion the term "control" should be avoided here. Please consider rewording.

Technical

lines 26-28: Please check the grammar and logic of this sentence.

line 35: modes -> models

line 43: "defined on" -> "defined based on"

line 83: "..such as for example leading to..." please check grammar

line 121: "at the surface" is confusing. Maybe better: "is the part of DIC that has been transferred from the surface into the ocean interior..."

line 129: "is a unit conversion" please spell out from which to which unit.

line 180: "The term inside the first {} brackets..." -> "The first term in curly brackets..."

line 199: "to the alkalinity" -> "to alkalinity"

line 285 South -> Southern

line 328: necessary -> necessarily

line 349: "by the ocean ventilation" -> "by ocean ventilation"

line 356: delete "now"

line 372-373: "is the preindustrial" -> "denotes the preindustrial state" (or similar)

line 470: "...of 26% to 30%..." -> "...between 26% and 30%..."