

Reply to review by Jörg Schwinger

We thank Dr Jörg Schwinger for his constructive comments.

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.

Thank you for your overall positive view.

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.

Agreed. The title will be changed to: 'Ocean carbon cycle feedbacks in CMIP6 models: contributions from different basins.'

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 " β , estimated from the regional ocean carbon storage").

Agreed, we will introduce new equations for estimates of γ and β from the cumulative flux along with new notation to distinct them from estimates using the carbon inventory.

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?

We will rewrite this part to clarify that the definition based on carbon inventory is different than the definition based on the air-sea flux and explicitly discuss strengths and limitations of both definitions.

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 beta/gamma 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).

Agreed. We will expand this figure to include the model uncertainty (please see Figure R1 below) and discuss this model uncertainty in the main text. We will also introduce a supplement figure that shows maps for the separation of beta and gamma to the different components (saturated, disequilibrium and regenerated) and relevant text.

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%)?

In retrospect we recognise that not explicitly defining our ocean basins (related to comment 6 below) may lead to confusion. The volume for the combined Atlantic, Pacific, Indian and Southern Ocean does not add up to the total volume of the global ocean (100%) as 1.4% corresponds to the volume of the Arctic and semi-enclosed seas like the Mediterranean Sea.

The regional contribution to beta and gamma and their different components is not proportional to the regional volume, and each is controlled by different processes as discussed in section 3.2. Hence, the fractional contribution from the combined Atlantic, Pacific, Indian and Southern Oceans (excluding the Arctic and semi-enclosed basins) to beta and gamma are different to each other, and are different to their combined fractional volume. This distinction can be equivalently viewed in terms of the Arctic and semi-enclosed basins having different contributions to beta and gamma, which are also different from their fractional volume. This distinction is also the case for the saturated, disequilibrium and regenerated components of beta and gamma.

We recognise that the Arctic has a high contribution, and we will revise the manuscript to introduce a separate Arctic basin (and modify all figures, tables and text to include this additional basin). We will keep only the semi-enclosed seas in the 'other regions'.

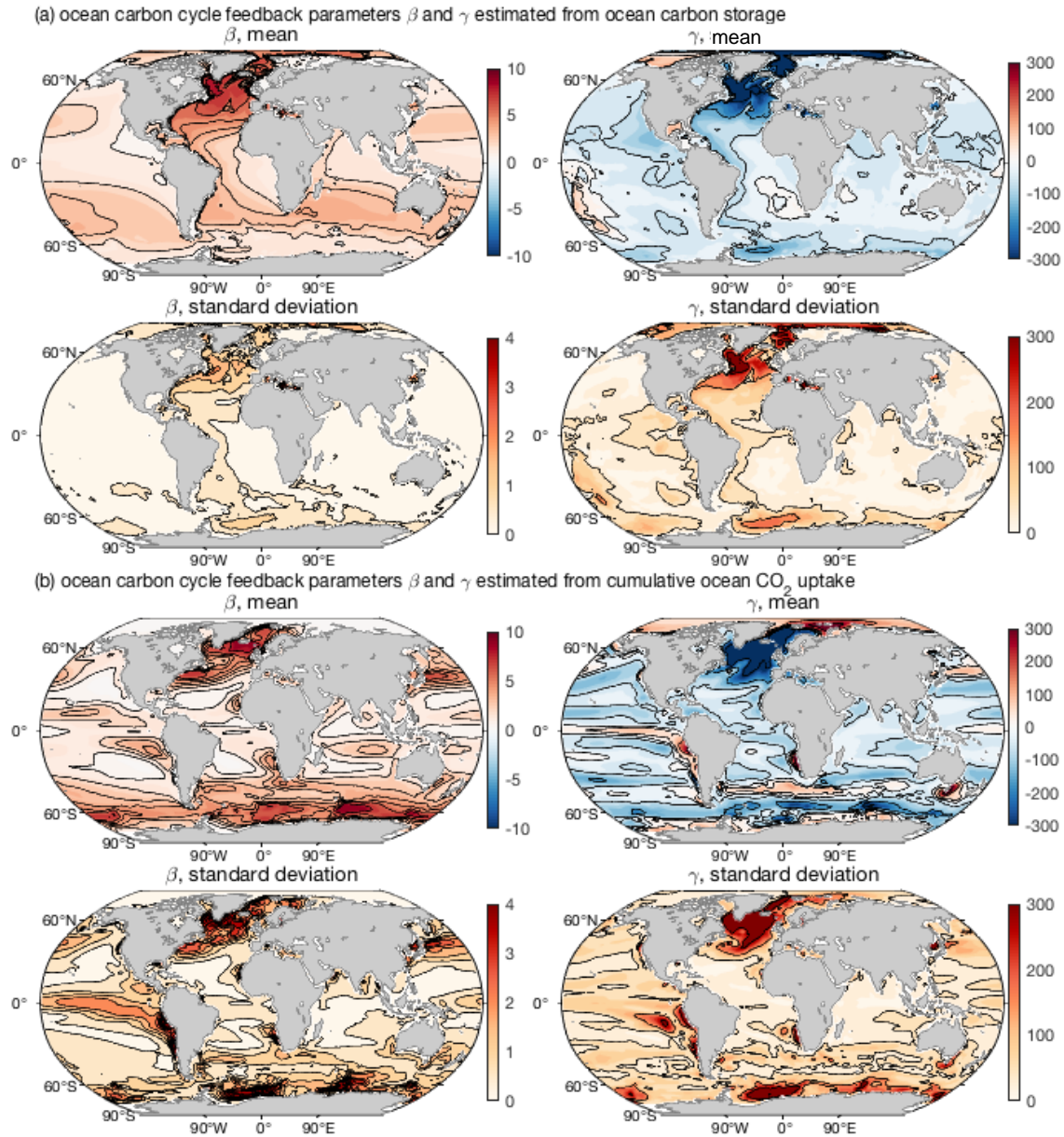


Figure R1. Geographical distribution of the CMIP6 inter-model mean and standard deviation for the carbon cycle feedback parameters normalised by area, β and γ as estimated: (a) based on the regional ocean carbon inventory changes; and (b) based on the regional cumulative ocean carbon uptake from the atmosphere. The estimates now include the NorESM2-LM. Note that this figure will be updated to use different notation for beta and gamma estimated from the carbon inventory vs the cumulative carbon uptake as suggested by the reviewer.

5) 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

Agreed, the reviewer is correct. This point is associated with the non-linearity of gamma as described in equation (16) and discussed in lines 180-189 of the original manuscript. We will update the text in the new manuscript along the lines:

The Pacific and Indian Oceans have somewhat lower contribution to gamma_sat than expected from their fractional volumes as they experience less warming than the other basins (Figure 1.d). The Atlantic Ocean has a smaller contribution to gamma_sat than expected from its fractional volume despite experiencing the largest warming among the basins (Figure 1.d), which is related to the non-linearity of the carbonate system. Specifically, the Atlantic Ocean has the largest increase in DIC (Figure 1.c) which acts to significantly reduce the magnitude of the negative gamma_sat driven solely by the effect of warming on solubility (see equation 16).

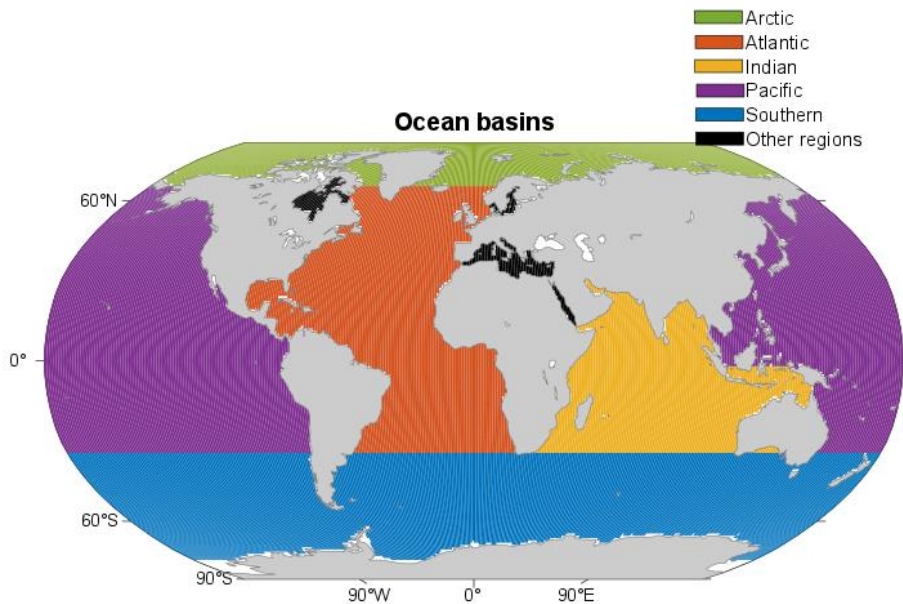


Figure R2. Geographical definition for the different basins used in our analysis. Other regions include semi-enclosed seas that are not included in any of the main 5 ocean basins.

6) 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.

Agreed. We will provide bounds in terms of latitude and/or characteristic regional features (e.g., south edge of Africa) for the basin separation in the main text and add a figure in the supplement with a map showing our definition of the Atlantic, Pacific, Indian, Southern and Arctic Oceans (please see Figure R2 above). We will clarify that semi-enclosed seas like the Mediterranean Sea, the Red Sea and Hudson Bay are not included in the definition of the 5 main basins. The Arctic will be treated as separate basin in the new manuscript.

We will add the definition of the AMOC strength and weakening in the main text, which could only be found in the caption of Table 3 of the previous manuscript: The strength and weakening

of the AMOC are defined as the maximum pre-industrial AMOC and maximum AMOC change relative to the pre-industrial between 30oN and 50oN, respectively.

Minor points

7) 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.

Agreed. We will re-write this text.

8) 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(CO₂,...). Then write down explicitly what Delta f means.

Delta f is an iterative algorithm, rather than a simple function, that estimates DIC_{sat} without prior knowledge of the [H₊]_{sat}. This algorithm follows an iteration based on a first guess of [H₊] or pH and is explicitly described in Follows et al., 2009 along with a coding example. In the limit that [H₊]_{sat} is known then Delta f function becomes equivalent to equation (15) for gamma. We will reorganise and rewrite the text to better describe what Delta f represents.

9) 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).

Agreed. We will re-organise these equations.

10) 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.

Agreed. We will reorganise these equations.

11) 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.

Agreed. We will change the text.

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

Agreed. We will reorganise this text and modify equations 22 and 23 following exactly the description of the global carbon cycle feedbacks in equations 4 and 5 to avoid any confusion.

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

Agreed. We will add NorESM2-LM (please see Figure R1 above).

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

Agreed, spatial asymmetries here refer to the regional differences/pattern in gamma (regions of positive and negative gamma) driven by the regional air-sea carbon exchange. The transport effect redistributes this regional ocean carbon loss/gain from the atmosphere due to changes in climate, such that the changes in gamma estimated based on the carbon storage are more uniform and less negative in the Southern Ocean, less negative in the North Atlantic high latitudes and switch to being negative in the Arctic relative to the gamma estimated based on

the cumulative carbon uptake. We will rewrite this text without using the word asymmetry for clarity.

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

Agreed. We will delete beta_dis.

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

Agreed. We will re-write this text

17) 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."

Agreed. We will rephrase this text.

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

Thanks. We will reference Schwinger et al. 2014 here.

19) 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.

Agreed. We will clarify that the uncertainty of gamma is larger than the uncertainty of beta in relative terms (variability in relation to the mean as described by the coefficient of variation) but that beta has more uncertainty in absolute terms for the carbon storage (variability in PgC).

20) 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.

Agreed. We will only use control when we refer to results from the sensitivity experiment for the control of the AMOC with the idealised model. When we refer to CMIP6 and the results based on the correlation we will use something along the lines of 'dependence'.

Technical

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

Agreed. We will rephrase this text.

22) line 35: modes -> models

Agreed

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

We will rephrase this to 'defined in terms of ...'

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

Agreed, we will rephrase this text.

25) 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..."

Agreed

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

Agreed, we will change to 'a unit conversion from moles to Pg of carbon'.

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

Agreed.

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

Agreed

29) line 285 South -> Southern

Agreed

30) line 328: necessary -> necessarily

Agreed (I think the reviewer means in line 318).

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

Agreed

32) line 356: delete "now"

Agreed

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

Agreed

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

Agreed, we will rephrase to 'of between 26% and 30% ...'.