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Biogeosciences Discussions

Interactive comment on "Long-term response of oceanic carbon uptake to global warming via physical and biological pumps" *by* Akitomo Yamamoto et al.

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General comments

This paper seeks to understand the source of carbon climate feedbacks arising in the ocean on multi-centennial timesscales. This is an important question in the Earth System Modelling community, including for understanding future climate change, and interpreting carbon budgets. The authors use a well thought out experimental design to quantify the sensitivity of different aspects of the ocean carbon cycle (e.g. biology, circulation, solubility etc) to climate change. The approach is based on previous work,

but fairly novel in this particular application. The paper is well organized and written, and the results, including the graphics are clear. Most uncertainties are addressed and the results are placed in the context of previous work. I thoroughly enjoyed this paper. Almost every time I had a question it was answered in the follow sentence or section. Overall I assess the quality as very high, and I recommend publication. I don't have any major issues. I do have some comments which I think could help to clarify the paper and address the few lingering questions that I did have.

Specific comments

- The authors describe a decreasing ocean CO2 uptake under global warming, and attribute this in large part to a reduction in export production. However previous literature (e.g. de Vries et al. [2012], Marinov et al. [2008] and references therein) has shown that ocean CO2 uptake is not directly tied to export production (as one might guess), but rather to the so called "efficiency of the biological pump". Please clarify how export production, biological pump efficiency and carbon uptake relate in this study. Specifically, is it really export production which is important - and if so why is this different from the above literature?
- The authors force the offline ocean biogeochemical model with monthly mean fields from the AOGCM (including for insolation, velocity, temperature, salinity etc). This means that much variability is being averaged over, including the diurnal cycle, synoptic scale variability and so on. There is a known sensitivity of ocean model response to forcing frequency. Obviously forcing at a higher frequency means more data, and is more expensive. But please discuss how the results might be sensitive to the forcing frequency. I don't necessarily need any more experiments, just a clear caveat on this point.

- Circulation plays a small direct role, but a large indirect role through nutrient transport. The circulation changes are large (and mostly consistent with expectations). In various parts of the manuscript, the authors do a good job of comparing their results to those from CMIP and other studies. If possible it would be interesting to know how the MIROC simulated circulation changes under 4xCO2 compare to other CMIP models. More generally a comment on how sensitive the results are to uncertainties, for example in the climate model response to increasing CO2, would be helpful. (I note the authors do discuss the need for similar studies using different models, but the reasons for this could be fleshed out).

Technical comments and typos (by pg and ln)

pg 1 / Abstract:

ln 8: "accelerate an increase in CO2" - Is there really an "acceleration". I'm not sure that this is the right word. I think just "decrease oceanic carbon uptake and therefore increase atmospheric CO2 and global warming" would sound better and be more accurate.

ln 14: "...first 140 years (at year 2000)" - the meaning of this because clear later when reading the methods, but this could be a little confusing in the abstract, because readers do not know at that point what experiment you are conducting. For example, on first reading I was thinking "calendar year 2000".

ln 19: "...gradient of DIC substantially" - add a comma after "DIC"

ln 23-4: "uptake through natural carbon cycle" - suggest removing "natural carbon cycle". I don't think this is needed.

pg 2:

ln 5-6: "...long-term evolution of climate systems with slow response times..." -> "...long term evolution of climate system components with a slow response time..." (i.e. there is only one climate system, which is made up of many components).

ln 10: "accelerating the rate of CO2 accumulation" - again I'm not sure if "accelerating" is accurate? Maybe just "increasing CO2 accumulation in the atmosphere".

ln 13: "primarily alter"...delete "primarily". There are only the natural and anthropogenic CO2 cycles.

ln 15-16: Another good study to reference is Randerson et al. (2015). They show that ocean carbon feedbacks become larger than land carbon feedbacks, but only on very long time scales. There is a nice tie in with this work.

ln 15-20: I suggest mentioning here that you will explain later why those studies came to that conclusion (and are different from yours).

ln 25 "However the contributions"...suggest deleting "However". This sentence is not really a continuation of the previous sentence.

ln 26-27: There are no studies doing this breakdown for CMIP5?

ln 28 "with AOGCM" -> "with an AOGCM"

pg 3:

ln 3 "using AOGCM" -> "an AOGCM".

ln 13 "with MIROC 4m AOGCM" -> "with the MIROC 4m AOGCM"

ln 27-28 "according to AOGCM climate simulations" - I got what you meant, but this could be clearer. Maybe something like "following the physical evolution of AOGCM climate simulations", or "forced by output from AOGCM climate simulations".

pg 4:

ln 11: "setting flux" -> "settling flux"

ln 16-18: "we confirmed..." - I found this confusing. At the bottom of page 3, it says that salinity is specified from the AOGCM simulations - but here you are saying you are using salinity from the offline simulation to validate against the AOGCM simulation. Something is missing. Do you simulate a passive salinity tracer in the offline model, to compare against the "online" salinity in the AOGCM? Please clarify.

ln 25-31: Just noting that the comparison is between a pre-industrial simulation and modern observations. This could have some impact. Are you using GLODAP estimated PI DIC and ALK to compare against? Not a big deal but worth clarifying.

pg 5:

ln 5-7 "This model does not include..." - it seems like these sentences belonged in section 2.2 to me. They are about the model, not the experiment.

ln 9 "We conducted additional experiments"...these were only run for 500 years, right? Maybe worth mentioning here.

ln 9-20: It is mentioned briefly below, but I think it is worth mentioning clearly here at the outset that the experimental design assumes linearity of the feedbacks.

ln 23: "and oceanic interior temperature and salinity". When I thought about the experimental design - as far as I can tell these interior T and S values are not used for anything in the offline model for this particular experiment, since the organic matter cycle is specified. The SST is, I believe, still be specified as GW. If this is all true, I would just remove the mention of "interior T and S values", since it is not relevant, and could be confusing. If these values are used for something, please clarify.

pg 6:

ln 12 :"after the summary of the global mean" - a bit confusing as written. Maybe "...and ocean biogeochemical variables. A full summary of the global mean changes is reported in..."

pg 7:

ln 2 / fig 1 e: I suggest you add the line for wind stress at year 2000 to Fig 1e (most other panels in fig. 1 are showing a year 2000 result). It would be helpful to see the recovery.

ln 6-15: PO4 is shown, but what about NO3? More generally, the paper discusses export production in general, but does not mention how diazotrophs and "other" phytoplankton react?

pg 8:

ln 6: "...during constant atmospheric CO2..." - I would include the year 140, as in "...constant atmospheric CO2 after year 140..." for clarity.

ln 27-33: I was interested in this section, and would like to see more spatial information. If possible, it would be really nice to see a Hovmoller, like Fig 1a, but for CO2 uptake/flux anomaly (of GW - CTL) (maybe in the SI).

pg 9:

ln 23-24: I suggest you reference these "uptake change" numbers back to table 2.

pg 10:

ln 19-23: Le Quere et al 2008 claim that the westerly wind increase is reducing Southern Ocean CO2 uptake (i.e. the opposite of what is being said here). Therefore, it is strange to cite as evidence without further explanation. I suggest it would be better to reference the Zickfeld et al. response to Quere et al. (who show that the CO2 uptake response to wind changes is time-scale dependent).

The effect of circulation change on sDIC (Fig 5) is essentially a redistribution of carbon from the Atlantic to the Pacific. Interestingly, we saw a similar redistribution due to to wind stress induced circulation changes in Swart et al. (2012), which we linked back to changes in the Agulhas leakage and overturning circulation.

Figures:

1. e : please add line for year 2000

3. The colorbar is not perceptually uniform, which makes it hard to determine where large changes have actually occurred. Please consider using a perceptually uniform colorbar.

6. Caption "Global upper-ocean" - fix typo

References

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