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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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Response to Reviewer 1 (N.C. Swart)

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 orga-

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Discussion paper

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nized 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.

Response: We appreciate the positive evaluation and many thoughtful comments from the reviewer. Referring to the comments, we will carefully revise the manuscript. Specific replies are as follows.

Specific comments o 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?

Response: Thank you for a useful suggestion. In our simulation, globally averaged preformed PO4 increases from 1.15 mmol/m3 in the pre-industrial condition to 1.40 mmol/m3 at the end of the simulation. Export production decreases from 8.1 PgC/yr to 6.3 PgC/yr. Considering the previous literatures pointed out by reviewer, the reduction of oceanic CO2 uptake due to global warming would be attribute to decrease in biological pump efficiency rather than EP reduction in our simulations. We will add the description of relationship between export production, biological pump efficiency and carbon uptake in the revised manuscript. We will also describe the importance of reduction in biological pump efficiency to decreasing CO2 uptake under global warming in the abstract and conclusion.

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o 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.

Response: As mentioned below and manuscript, we compared passive salinity tracer in the offline model to online salinity in the AOGCM. There were no significant differences in the salinity distribution between two simulations. Therefore, the short-term processes have limited impact on our results. We will add the discussion in the revised manuscript.

o 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).

Response: According to the reviewer's comment, we compare our results to other AOGCMs and EMICs under high CO2 scenario (e.g. 4xCO2 and RCP8.5). The weakening of AMOC and AABW formation in the first 140 years of our simulation are consistent with the results of CMIP5 models under RCP8.5 (Weaver et al., 2012; Heuzé et al., 2015). However, the longer-term responses of AMOC and AABW formation are very uncertain. In our simulation, AMOC shutdown

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continues to the end of the simulation without recovery. Partial or full AMOC recovery to the pre-industrial level has emerged in other long-term AOGCM and EMIC simulations [Schmittner et al., 2008, Weaver et al., 2012, Li et al., 2013]. AABW formation recovers and overshoots after 1000 years in our simulations. These responses have not been reported in previous multi-millennium simulations [Schmittner et al., 2008, Li et al., 2013]. The uncertainties of circulation changes would have impacts on millennial-scale CO2 uptake. We will add the description of uncertainty of circulation change and its impact on long-term carbon cycle to the discussion in the revised manuscript.

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.

Response: According to the reviewer's comment, we will modify this sentence in the revised manuscript.

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".

Response: We agree the reviewer's comment. Following the comment of reviewer 3, we will change from "at year 2000" to "after 2000 model years" in the revised manuscript.

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

Response: We will correct this.

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

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Response: We will remove this in the revised manuscript.

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).

Response: We will revise this sentence following the reviewer's comment.

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".

Response: We agree the reviewer's comment. We will modify this sentence in the revised manuscript.

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

Response: We will remove "primarily" in the revised manuscript.

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.

Response: Thank you for the nice suggestion. We will add Randerson et al. (2015) to the reference in the revised manuscript.

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

Response: We will add these information in this paragraph.

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

Response: According to the reviewer's comment, we will remove "However".

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

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Response: We do not know this kind of study using CMIP5. We will add "To our knowledge," to this sentence in the revised manuscript.

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"

Response: Thank you for pointing out. We will correct these in the revised manuscript.

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".

Response: We will change the sentence to the latter one in the revised manuscript.

pg 4: ln 11: "setting flux" -> "settling flux"

Response: Thank you for pointing out. We will fix typo in the revised manuscript.

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.

Response: As reviewer said, we compared passive salinity tracer in the offline model to online salinity in the AOGCM. We will add this description to the revised manuscript.

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 esti-

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mated PI DIC and ALK to compare against? Not a big deal but worth clarifying.

Response: We compared a pre-industrial simulation with modern observations. We will add this information to revised manuscript for 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.

Response: We will move this sentence to section 2.2 following the reviewer's comment.

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

Response: The reviewer is right. We will add this information in the revised manuscript.

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.

Response: We agree the reviewer's comment. We will add the assumption of linearity of the feedbacks to this paragraph in the revised manuscript.

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.

Response: This is our mistake. Interior T and S are not used in the sensitivity experiments. We will just remove the mention of "interior T and S values" in the revised manuscript.

pg 6: ln 12 :"after the summary of the global mean" - a bit confusing as written. Maybe

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"...and ocean biogeochemical variables. A full summary of the global mean changes is reported in..."

Response: According to the reviewer's comment, we will modify this sentence.

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.

Response: We agree the reviewer's comment. We will add the line for year 2000 in the revised manuscript.

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?

Response: Global NO3 at the surface also decreases by about 20%. Regional NO3 changes are similar to the PO4 changes. Diazotrophs and "other" phytoplankton increase slightly, which is consistent with previous study (Schmittner et al., 2008). Increase in "other" phytoplankton is caused by faster nutrient recycling due to seawater warming. We will add these description to the results in the revised manuscript.

pg 8: ln 6: "...during constant atmospheric CO2..." - I would include the year 140, as in "...constant atmospheric CO2 after year 140..." for clarity.

Response: We will correct this sentence.

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).

Response: We agree the reviewer's comment. We will add a Hovmoller figure in the supplementary information.

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pg 9: ln 23-24: I suggest you reference these "uptake change" numbers back to table 2.

Response: We will add the reference to table 2 in the revised manuscript.

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.

Response: Following the reviewer's comment, we will delete the reference to Le Quere et al 2008. We will also add the description of carbon redistribution due to wind stress induced circulation changes to the revised manuscript.

Figures: 1. e : please add line for year 2000

Response: We will add the line for year 2000 in the revised manuscript as mentioned above.

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.

Response: We will change the color bar in the revised manuscript.

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

Response: Thank you for pointing out. We will fix typo.

References: Heuzé, C., Heywood, K. J., Stevens, D. P., and Ridley, J. K.: Changes

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in global ocean bottom properties and volume transports in CMIP5 models under climate change scenarios, J. Climate, 28, 2917-2944, doi:10.1175/JCLI-D-14-00381.1, 2015 Weaver, A. J. SedláËĞcek, J., Eby, M., Alexander, K., Crespin, E., Fichefet, T., Philippon-Berthier, G., Joos, F., Kawamiya, M., Matsumoto, K., Steinacher, M., Tachiiri, K., Tokos, K., Yoshimori, M., and Zickfeld, K.: Stability of the Atlantic meridional overturning circulation: a model intercomparison, Geophys. Res. Lett., 39, L20709, doi:10.1029/2012GL053763, 2012.

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