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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 2

The manuscript by Yamamoto et al explores through a large suite of experiments under fixed atmospheric concentrations the role physical changes in climate play on ocean carbon uptake. Their conclusions suggest, in contrast to other papers, that the change of circulation dominate the response. It took me a little while to get into this paper, but once there I enjoyed the paper much and really appreciate the larger number of simulations that went into this work - thank you. Overall this is well conceived and executed piece of work, that will be of interested to a wide readership. I do have some

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Discussion paper

minor comments that I feel once addressed would strengthen the paper, otherwise I am happy to recommend this paper for publication.

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

Minor Comments: 1. The authors predicate the study on global warming, and state that global warming will decrease ocean carbon uptake. However in the present day, as CO2 levels continue to rise - the ocean will take up carbon at a rate proportional to this i.e. gradient driven. I do understand in this study, if we assume fixed CO2 levels then this supposition is correct, but I do think this needs to clarified in the text.

Response: The reviewer is right. We will refer to the assumption of fixed CO2 level in the revised manuscript.

2. The study puts more heat and carbon into the ocean over a much shorter period than under CMIP3/5 change changes runs, even the business-as-usual scenario; this of course has implications for where the heat and carbon are stored. As the authors make a number comparison to these climate change runs - could they comment on what the implications of this maybe - perhaps on the timing of events e.g. sinks to sources etc, and whether its a fair comparison?

Response: As reviewer pointed out, input of heat and carbon into the ocean during the first 140 year of our experimental design are larger than SRES A2 and RCP8.5. In the first 140 year, the response of climate and oceanic carbon cycle would be somewhat different from the RCP8.5 simulations. After year 140, the influence of the initial differences of heat and carbon input on oceanic carbon cycle would weaken since CO2 concentration is similar between 4xCO2 and RCP8.5. Therefore, we think that the differences between 4xCO2 and RCP8.5 have a limited impact on long-term response of climate and carbon cycle to global warming.

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3. The experimental methods section is super critical to this paper, however I needed to read this at least 5 times to be really clear. I recommend that the authors break up the 3rd paragraph to make it more accessible

Response: We agree the reviewer's comments. We will break up the 3rd paragraph in the revised manuscript.

4. The study uses offline simulations, which make sense, could the authors comments on whether on or offline makes much difference - given the challenges of capturing short-term processes in the fields needed to run the model. I am sure that they have tested this somewhere, and if not it should be acknowledged.

Response: The reviewer is right. We compared passive salinity tracer calculated in the offline model to online salinity in the AOGCM. There were no significant differences in salinity distribution between the two simulations. We will add this information to the revised manuscript.

5. The timescales calculated in the paper are based on a fixed atmospheric concentrations. In the real world i.e. driven by emissions, the ocean carbon uptake would significantly slow as the gradient between the ocean and atmosphere decreases. I think this probably needs to be mentioned in the discussion, as do the implications for timing of changes.

Response: As reviewer pointed out, our simulation with prescribed CO2 concentrations are idealized. On the other hand, there is an advantage of the simulations with prescribed CO2 concentrations compared to the simulations with prescribed emissions. The simulations with prescribed CO2 concentrations allow for a more rigorous separation of feedback processes since carbon sinks respond to the same atmospheric CO2 concentration in all simulations (Zickfeld et al., 2011). We will mention the difference between emission driven runs and concentration driven runs and usefulness of concentration driven runs in the revised manuscript.

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6. Otherwise some minor typos etc need to be addressed, but I am sure they will be picked in the proofs.

Response: We will carefully correct typo and errors in the revised manuscript.

References: Zickfeld, K., Eby, M., Matthews, H.D., Schmittner, A., and Weaver, A.J.: Nonlinearity of carbon cycle feedbacks, J. Climate, 24, 4255-4275, 2011.

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