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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.

Anonymous Referee #3

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The authors study long term ocean carbon cycle feedbacks over a time horizon of 2000 years by using an offline ocean biogeochemistry model driven by climate model output. By using different combinations of output fields from a control simulation (no global warming) and a global warming simulation, they separate the carbon cycle feedback into components originating from SST-changes, circulation changes, changes of the biological pump, and a few others. They find that changes in the biological pump contribute most to the carbon uptake reduction under climate change followed by solubility changes. The authors claim that this finding is "contrary to most previous studies".

The manuscript is clearly within the scope of Biogeosciences. The main conclusions,

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however, are partly inconsistent and not well enough supported by the results. Also, the manuscript as it stands now, it is not very novel. Many similar studies on ocean carbon-climate feedbacks have been published during the past 20 years, most of them with simpler models. However, the authors do not convincingly make the point as to why significantly different results could be expected because of enhanced model complexity. There are (or potentially are) interesting new aspects in the present study, but the authors do not elaborate these (see below).

Major points:

1) The statement that the results are "contrary to most previous studies" is not convincingly supported by the results presented in this manuscript. Since the experimental set-up is different from (most of the) previous studies, it remains unclear what the effect of these differences might be. This is briefly discussed in section 5, following speculations (page 12, lines 20-30) about why models in previous studies possibly gave different results. These speculations are not convincingly supported by the results or the cited literature either. In my opinion it is most likely that differences in the experimental set-up explain much of the differences. The authors follow Zickfeld et al. (2008) in designing their experiments, and use the tendencies of DIC and ALK due to biological production/remineralisation from the CTL-experiment in the GW-experiment (and vice versa) to determine the effect of biology on CO2 uptake. This mimics pre-industrial organic matter and CaCO3 production/remineralisiation under a reduced circulation. I find this design questionable, since it weakens the upward transport of remineralised carbon and nutrients (leading to enhanced C-uptake), but at the same time keeps the export production at pre-industrial levels (leading also to enhanced uptake). The experiment design used in some of the previous studies (Joss et al. 1999, Plattner et al. 2001) is different: Here, archived pre-industrial surface sPO4 and sALK fields are used in a global warming simulation to separate the "effect of biology". If I am not mistaken, the effect of reduced upwelling of DIC is cancelled out in this experiment. Other studies (Sarmiento et al. 1998, Matsumoto et al. 2010) use abiotic experiments. In order

to demonstrate that the feedback-mechanisms are really substantially different from previous studies, the authors would need to quantify the differences arising due to the different experimental set-up (or different interpretations of the "biological effect"). This could be done by running additional sets of experiments following the design of previous studies. A discussion of which experimental set-up or definition of "biological pump contribution" is more useful or correct should also be provided. The authors state in the abstract that "quantifications of the contributions from different processes to the overall reduction in ocean uptake are still unclear". Instead of adding to the confusion they could take the opportunity to assess what the experimental set-up in different studies contributes to this. This would also be a novel and useful contribution to the field.

In this context it would be also useful to discuss the limitations of the approaches to separate the feedback-mechanisms in a non-linear system. The authors have already performed two sets of experiments (GW-base and CTL-base), which could serve this purpose. Results from these sets of experiments are presented in Table 2, but are only mentioned in one brief sentence (page 9, lines 24-25) in the manuscript. Particularly, for the "Biology"-contribution, the authors find a considerable dependence on the base state (GW or CTL; 118 PgC difference while the total is 402 PgC). An explanation for this would be useful. Do the authors expect that the individual contributions would add up to the total, and is the residual given in Table 2 thus an indicator of non-linearity?

2) The authors state towards the end of the introduction section that the "second purpose of this study is to investigate the usefulness of EMIC for long-term simulations of the ocean carbon cycle by comparing our results to previous studies." This sounds like "EMIC" would be a well defined class of models with homogeneous properties, which is not the case. Some of the cited EMICs (e.g. Zickfeld et al. 2008) employ a 3d state-of-the art ocean model, which is not fundamentally different from the ocean model used in this study. The authors do not discuss sufficiently why specific feedbacks could be expected to be present in their model but not in a simpler model. They also do not provide an in depth comparison of their results with previous EMIC studies (which I

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would expect for an issue that is the "second purpose of this study"). I actually do not belive that the question as to the "usefulness of EMIC for long-term simulations of the ocean carbon cycle" could be answered in this study - this would require a dedicated model intercomparison study with a common experimental design. I would recommend to drop this "second purpose", and discuss results compared to previous EMIC studies as necessary to place the present study in the scientific context.

Further, the conclusions regarding the "usefulness of EMIC" are inconsistent. On page 9, lines1-2, it is stated that "results support the usefulness of EMIC for long-term projections of the ocean carbon cycle". Later in the "Summary and Discussion" it is speculated about why the simpler models used in previous studies would have significantly different feedback mechanisms. Should this be interpreted as "simpler models are right for the wrong reason, but this is still useful"?

Minor points

page 1, line 14: at year 2000 -> after 2000 model years

page 1, line 22: "...circulation change becomes a second order process." This is in contradiction to the statement that "changes in the biological pump via ocean circulation" is the dominant process.

page 2, line 4: "...over a 1000-year period" -> "on millennial time scales" or similar

page 2, lines 16-18: "In those previous studies...". This assertion is not correct. E.g. Maier-Reimer et al. 1996 state that both biological and physical carbon-climate feedbacks are small compared to the carbon concentration feedback. I do not think that the other cited studies make the point that biology is a second order process (but I have not checked in-depth).

page 4, line 11: setting -> settling (?)

page 4, line 22: "As for spin-up,..." -> "For the spin-up..."

page 5, line 9: an -> the

page 5, lines 22-29: It should be made clearer here which effect is included in which experiment. E.g., the authors state that the experiments GW_om and GW_ca "evaluate the contributions of changes in the organic matter and CaCO3 cycles." This is not very precise, since these experiments evaluate changes in one part of the "cycles" only (changes in production and remineralisation rates, but the rate of upward transport of reminerelised OM is not included).

page 5, line 30: "...are included in not..." check grammar

page 5, line 31: I guess the pre-industrial sea ice fractions are used only in the gas exchange calculations? Please clarify.

page 6, line 8: "... are likely to reflect the non-linearity..." Please describe what the experiments reflect. There is no need to speculate ("likely").

page 7, line 11: "According to..." -> "Consistent with the..."

page 7, line 18: "...rain ratio increasing from 0.09 to 1.13..." Please check the numbers.

page 7, lines 16-17: Please explain briefly why PP increases and export decreases. It is not obvious from the model description why this could happen (if necessary or helpful, please amend the model description accordingly)

page 8, line 5: "of the same simulation using models..." -> "of the corresponding simulations"

page 12, line 33: Plattner et al. 2001 do have abiotic experiments, but they do not use this to quantify the contribution of biology

Figure 3: a separation into panels for surface and deep ocean would be useful (or a stretch of the depth scale in the upper 1000m)

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