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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 3

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 solu-

bility 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, 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).

Response: We are grateful for the careful review. The reviewer's comments helped us to improve our manuscript. Referring to the comments, we will carefully revise the manuscript. Specific replies are as follows.

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.

Response: As for the upward transport of remineralized carbon and nutrients, we think that the lack of our explanation of experimental design misled the reviewer. To quantify the effect of biology on CO2 uptake, we compared the GW-experiment to the GW-experiment with biological production/remineralization from the CTL-experiment (GW_om) ((2) - (6) in Table 2). In the GW-experiment, the upward transport of remineralized carbon and nutrients are weakened by both circulation change and the reduction of remineralization. In the GW_om, the upward transport of remineralized carbon and nutrients are weakened by only circulation change since biological production/remineralization are kept at pre-industrial levels. Comparing the GW_om to the GW-experiment, the upward transport of remineralized carbon and nutrients are enhanced, leading to reduced carbon uptake. We will describe experimental details in the section 2.3 "Experimental design".

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.

Response: We agree the reviewer's comments. In fact, before submitting the manuscript, we conducted the abiotic experiments following the previous studies (Sarmiento et al. 1998 and Matsumoto et al 2010) (attached Table 1). In our abiotic experiments, the reduction in oceanic CO2 uptake associated with global warming is caused by changes in the ocean circulation and SST (attached Figure 1). Biological effect slightly enhances CO2 uptake. These contributions of individual mechanisms are consistent with previous estimation using abiotic experiments (Sarmiento et al. 1998 and Matsumoto et al 2010). Therefore, the different results are caused by the differences of experiments set-up, as pointed by reviewer.

We attribute these different results to the vertical gradient of DIC under the pre-industrial condition of the abiotic and biotic experiments (i.e., CTL-base/GW-base experiments) (attached Figure 2). The effect of circulation change in the abiotic experiments mainly represents the reduced CO2 uptake due to weaker downward transport of anthropogenic CO2 from the surface to the deep ocean. The enhanced CO2 uptake associated with the reduced upward transport of natural CO2 from the deep ocean to the upper ocean is underestimated. This is because the vertical gradient of DIC in the abiotic experiments is much smaller than the observed DIC gradient. The increase in CO2 uptake due to weaker equatorial upwelling is not found in the abiotic experiments (attached figure 3).

In the circulation effect of CTL-base/ GW-base experiments with the realistic vertical gradient of DIC, the enhanced CO2 uptake associated with the reduced upward transport of natural CO2 largely offsets the reduced CO2 uptake due to weaker downward transport of anthropogenic CO2 (as mentioned in section 4.2) . Therefore, the reduction in CO2 uptake due to circulation change is larger in the abiotic experiments than in the CTL-base/GW-base experiments.

In the abiotic experiments, the contribution of biological effect is calculated as the residual (attached Table 2). The enhanced CO2 uptake owing to the reduced upward transport of natural CO2 is included in the biological effect. This effect overcomes the reduced CO2 uptake due to the weakening of the biological pump, so that biological effect shows an increase in CO2 uptake. Our results show that CTL-base/GW-base experiments are useful for quantifying the contribution of circulation and biological change.

We will add these comparison and discussion in the revised manuscript. Unfortunately, due to computer resources, it is difficult to conduct additional experiments based on other previous studies (Joss et al. 1999, Plattner et al. 2001) immediately. However, we believe the comparison of two different experimental set-up (CTL-base/GW-base and abiotic experiments) is useful for understanding of the feedback mechanisms.

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?

Response: According to the reviewer's comment, we will explain the dependence of biological effect on the base state in the revised manuscript.

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 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"?

Response: We agree the reviewer's comment. We will remove this "sec-

ond purpose", the sentence on page 9, lines 1-2, and speculation about different feedback mechanisms of the simpler models in the "Summary and Discussion". We will simply compare our results with previous EMIC studies in the revised manuscript

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

Response: We will modify this according to the reviewer's comment.

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.

Response: We agree the reviewer's comments. We will rewrite this sentence to "circulation change plays a small direct role, but a large indirect role through nutrient transport and biological pump." in the revised manuscript.

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

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

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).

Response: We will remove the reference of Maier-Reimer et al (1996) and revise this sentence.

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

Response: We will fix typo in the revised manuscript.

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

Response: Following the reviewer's comment we will modify this in the revised manuscript.

page 5, line 9: an -> the

Response: We will correct this in the revised manuscript.

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).

Response: In the comparison between GW and GW_om, the changes in upward transport of remineralized OM due to reduced OM remineralization are included as mentioned in the response to the major points 1). The changes in upward transport of remineralized OM due to circulation change are calculated in the comparison between GW and GW_circ. According to the reviewer's comment, we will add experimental details to the revised manuscript.

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

Response: Thank you for pointing out. We will correct this.

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

Response: The reviewer is right. We will clarify this sentence in the revised manuscript.

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

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

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

Response: Following the reviewer's comment we will correct this.

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

Response: Thank you for pointing out. We will change from 1.13 to 0.13 in the revised manuscript.

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)

Response: PP increase are also found in Schmittner et al (2008) and Taucher and Oschlies (2011). Our model is based on Schmittner et al (2008). Taucher and Oschlies (2011) show that PP increase is caused by temperature effects on biological processes such as remineralization and the microbial loop. We will add these information in the revised manuscript.

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

Response: Thank you for pointing out. We will correct this.

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

Response: We will delete this reference in the revised manuscript.

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)

Response: According to the reviewer's comment, we will revise Figure 3.

Reference:

Taucher, J., and Oschlies, A.: Can we predict the direction of marine primary production change under global warming?, Geophys. Res. Lett., 38, L02603, doi:10.1029/2010GL045934, 2011.

Figure caption

Figure 1. Contributions of the mechanisms to the reduction in the oceanic CO2

uptake due to global warming at 500 years for the abiotic experiments. The total change and the contribution of the individual mechanisms are calculated as summarized in Table 2. We will add this figure to the Figure 4 in the manuscript.

Figure 2. Vertical profile of the difference in salinity-normalized DIC from the surface under the pre-industrial condition. The black and red lines show the abiotic and biotic models, respectively.

Figure 3. Zonal mean change in the salinity-normalized DIC induced by circulation changes for the abiotic experiments at 500 years. The left and right panels show the Atlantic and Indo-Pacific Oceans, respectively. We will add this figure to the Figure 5 in the manuscript.

Table 1. Description of the abiotic experiments and results of the oceanic CO2 uptake. We will add this table to the Table 1 in the manuscript.

Table 2. The contributions of individual mechanisms to the reduction in the CO2 uptake due to global warming in the first 500 years under the abiotic experiments. We will add this table to the Table 2 in the manuscript.

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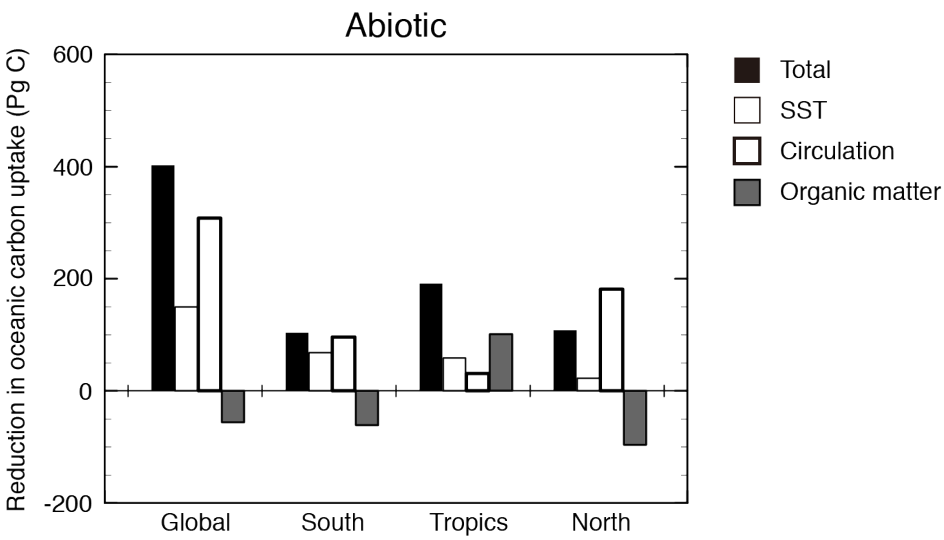

Fig. 1.

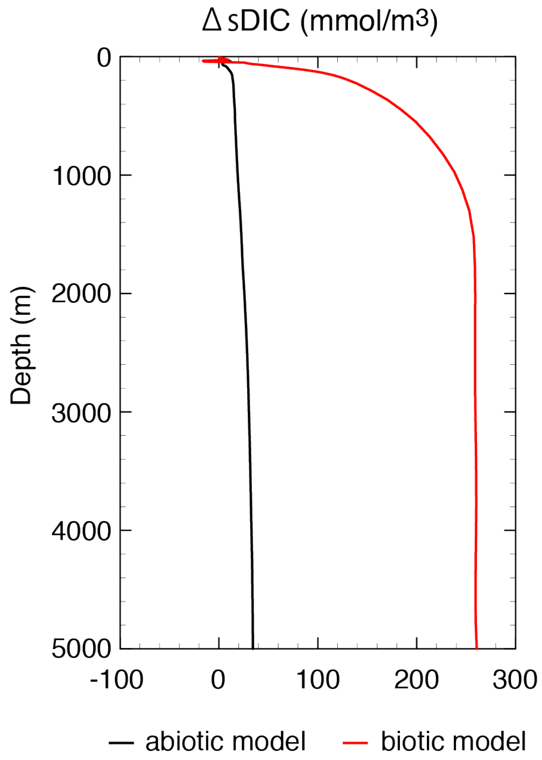
Δ sDIC (mmol/m3)
Depth (m)
0
1000
2000
3000
4000
5000
-100
0
100
200
300
abiotic model
biotic model

Fig. 2.

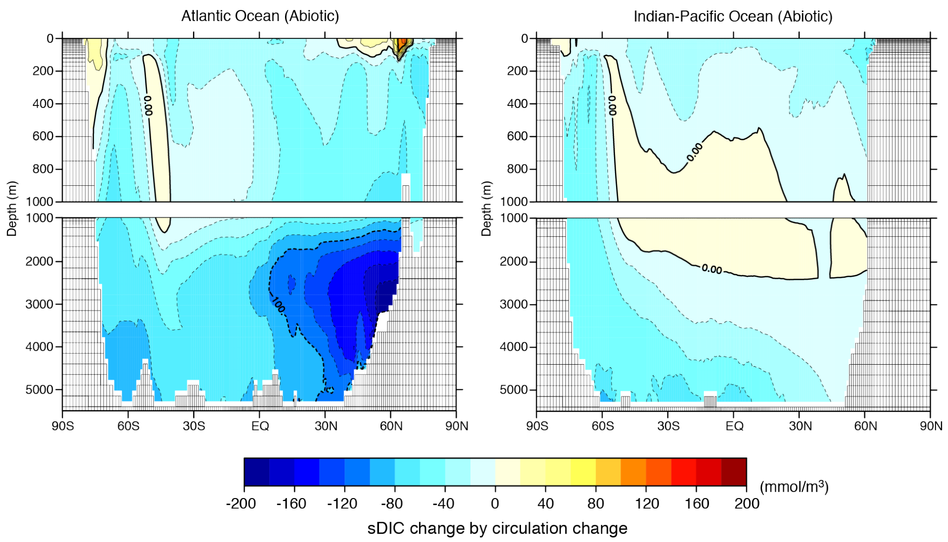
Atlantic Ocean (Abiotic)
Indian-Pacific Ocean (Abiotic)
Depth (m)
0.00
100
90S
60S
30S
EQ
30N
60N
90N
-200
-160
-120
-80
-40
0
40
80
120
160
200
(mmol/m3)
sDIC change by circulation change

Fig. 3.

	Experiments	Changing mechanisms	Cumulative uptake (Pg C) 500year	2000year	SST	Dilution	Circulation	Organic matter cycle	$CaCO_3$ cycle
1	CTL	–	1629	2888	CTL	CTL	CTL	CTL	CTL
2	GW	all	1227	2028	GW	GW	GW	GW	GW
3	CTL_abio	–	1819		CTL	CTL	CTL	–	–
4	GW_abio	SST circulation	1371		GW	GW	GW	–	–
5	GW_abio_SST	SST	1511		CTL	GW	GW	–	–

Fig. 4. Table 1

Mechanisms		uptake change (Pg C)
Total	(2) − (1)	-402
SST	(4) − (5)	-140
Freshwater	–	–
Circulation	(5) − (3)	-308
Biology	(Total) − (SST) − (Biology)	46

Fig. 5. Table 2