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Biogeosciences Discussions

Interactive comment on "Impact of rapid sea-ice reduction in the Arctic Ocean on the rate of ocean acidification" *by* A. Yamamoto et al.

A. Yamamoto et al.

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Dear Editor
We have addressed all the comments made by the two referees, as follows, and we hope that our explanations and revisions are satisfactory. We have to confess that there was an error with the previous calculation of pH and Ωarag, which is corrected in the revised manuscript. Our main conclusions, "The reductions of pH and Ωarag in the Arctic surface waters is significantly affected by the reduction rate of sea-ice extent", does not change.

Dear refree1
We appreciate your attentiveness to our paper and your comments that have helped

us to improve it. Specific replies are as follows.

Comment 1-1:
It would be good to add a table that compare the model components of the two different models.

Response:
Thank you for the suggestions. Accordingly, we added the comparison of model components in the revised manuscript (Table. 1).

Comment 1-2:
Interestingly, both models use identical physical ocean and sea ice models as well as the same ocean biogeochemical module, but different atmospheric components. I am curious to know why the inclusion of a more sophisticated atmospheric model (with increased vertical resolution) leads to a more rapid sea ice decrease? Any comments on that would be helpful.

Response:
It is a good point. We conducted spin-up of both versions with the same parameter. As a result, the average thickness of the September sea-ice in "CMIP5" is about half of that in "C4MIP" (Fig. 1). This thinner sea-ice causes a rapid decrease of sea-ice extent. Unfortunately, we have not been able to identify the reason why the sea-ice thickness in "CMIP5" is thinner than in "C4MIP", since it is a result of many modifications in the model code and tuning of many parameters. We added the description of sea-ice thickness in the revised manuscript.

Comment 2-1:
The authors state that the spin-up simulation for the carbon cycle component in the old model was conducted by running the model for 250 years and by starting the model

from initial conditions based on climatological data sets. Please specify if preindustrial or present-day initial conditions were used and please indicate literature reference for the used initial conditions.

Response:
As the referee points out, the explanation in the original manuscript was insufficient. We described the initial condition used for both models (Line 7-11 and 17-18, Page 9) in the revised manuscript.

Comment 2-2:
Furthermore, I am a bit puzzled about the short spin-up time. Please show that the deep ocean reaches quasi equilibrium.

Response:
Due to limited computational resources, we do not perform thousands of years of spin-up. Our short spin-up time may be insufficient for complete spin-up for the global terrestrial and oceanic carbon cycle, but is still long enough to drive the model to a quasi-steady state. The globally averaged seawater temperature and dissolved inorganic carbon in the deep ocean under control run are shown in Fig. 2.

Comment 2-3:
What do you mean with 'until globally integrated net CO2 fluxes at land and sea surfaces vanished'? Are the transient simulations drift-corrected?

Response:
Certainly, this sentence was unclear. We rewrote this sentence as follow. "As a result, the globally integrated CO2 fluxes between the atmosphere and land/ocean reach a quasi-steady state. The global net atmosphere-ocean/land CO2 exchange becomes sufficiently small compared to its interannual variability (Land drift: +0.1 PgC year-1;

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Ocean drift: -0.2 PgC year-1).”

Comment 2-4:
Additionally, please describe the spin-up procedure of the new model.

Response:
We added the explanation as below. Because further details are described in a separate (and more technical) paper, we kept the explanation in the revised manuscript relatively concise. “Similarly, “CMIP5” was spun-up for 280 years with the pre-industrial initial condition from offline spin-up of SEIB-DGVM and ocean model including ocean carbon cycle (for detail, see Watanabe et al., 2011).” (in the revised manuscript Line 17-19, Page 9)

Comment 3:
On page 10620 the authors discuss different regions where water becomes undersaturated with respect to aragonite during this century. They miss the discussion of the Eastern Boundary Upwelling Systems, such as the California Current System. Theses systems are naturally more acidic than the mean surface ocean (Feely et al. 2008) and are especially prone to progress toward widespread undersaturated conditions with regard to carbonate in a future high CO2 climate. Please discuss that in the introduction.

Response:
Following the referee’s suggestion, we mention now the undersatureted coastal waters in in the revised manuscript (Line 11-14, Page 5).

Comment 4:
It would be good to put the results of this study into context to other simulations conducted for CMIP3. As most CMIP3 models underestimate the rate of sea ice

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decrease, one would expect that current ESMs underestimate the rate of OA changes in the Arctic Ocean. Please discuss that in an additional paragraph.

Response:
We agreed with the referee's comment. We added this discussion in the revised manuscript.

Comment 5:
The naming convention of the models ("new" version vs. "old" version) is confusing. I assumethat the model will be developed further in the next couple of years. Therefore, I would prefer to write the name of the model version throughout the entire MS.

Response:
We newly named the two model “CMIP5” and “C4MIP”.

We also thank the referee for the specific comments and technical corrections. The misspellings have been corrected, and the reference is added.

Dear refree2
We are grateful for you're the careful review. The referee's useful comments helped us to improve our paper. We streamlined our paper, and took all suggestions into account in the revised version, as indicated in the responses as follows.

Comment 1
These concerns related to the response at the seasonal scale where the results of this paper are completely different to the aforementioned paper. This result, if correct is very interesting, however the authors devote only two paragraphs this and do not discuss these differences. I struggle also to understand the response

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that solubility alone in a high-latitude region can drive 180-degree phase shift in the period 2090-2099 relative to 1990-1999 or provide adequate seasonal variance. The transition period of little variance in the seasonal cycle also interesting although not explained.

Section 4.3 is inadequate and I remain whole unconvinced of the results in section (discussed above).

Response:
The explanation in the previous manuscript was not clear and insufficient, as the referee pointed out. The change in the seasonal cycle of Ωarag is caused by phase shift of seasonal cycle of CO2 uptake due to the significant winter sea-ice reduction and solubility. In 1990's, the greater CO2 uptake in summer than in winter led to lower Ωarag in summer. This is because summer sea-ice reduction enhances CO2 uptake, and winter sea-ice cover suppresses CO2 uptake (Steinacher et al., 2009). This seasonal cycle of CO2 uptake is unchanged under the condition that most winter sea-ice remains until 2100 such as predicted by "C4MIP" version and CSM1.4-carbon in Steinacher et al., 2009. In our "CMIP5" version, the significant winter sea-ice reduction enables winter CO2 uptake. In addition, the solubility in winter is larger than summer. Consequently, the greater CO2 uptake in winter than in summer causes the lower Ωarag in winter than summer.

We adequately rewrote Section 4.3 including the above discussion and added the explanation of the little variance in the seasonal cycle.

Comment 2
it might be equally interesting if the authors contrast the results of different RCPs e.g. RCP 4.5 vs 8.5 While is true that at the RCPs in the coming decades are similar this is also a result and has important implications for the transition response discussed in

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Cai et al (2010; Science)

Response:
We are most grateful to the referee for helpful comments. We compared the results of RCP4.5 and RCP8.5. Interestingly, the similar reduction rate of sea ice extent with atmospheric CO2 concentration is projected under both scenarios (Fig. 3a). Under this condition, we can see the effect of scenario by comparing the results of two scenarios. The comparison shows the differences of pH and Ωarag at the same CO2 concentration between two scenarios is much less than the difference between two model versions (Fig. 3b,c).

We added this comparison to Section 3.3.

Comment 3
The breakdown of drivers in the discussion of the changes in omega is poorly explained and hard to follow; it needs more explanation

Response:
We added Table. 2 and rewrote Section 4.1 and 4.2 clearly.

Comment 4
There numerous typogographical errors that needs to be addressed

Response:
We corrected some errors.

Interactive comment on Biogeosciences Discuss., 8, 10617, 2011.

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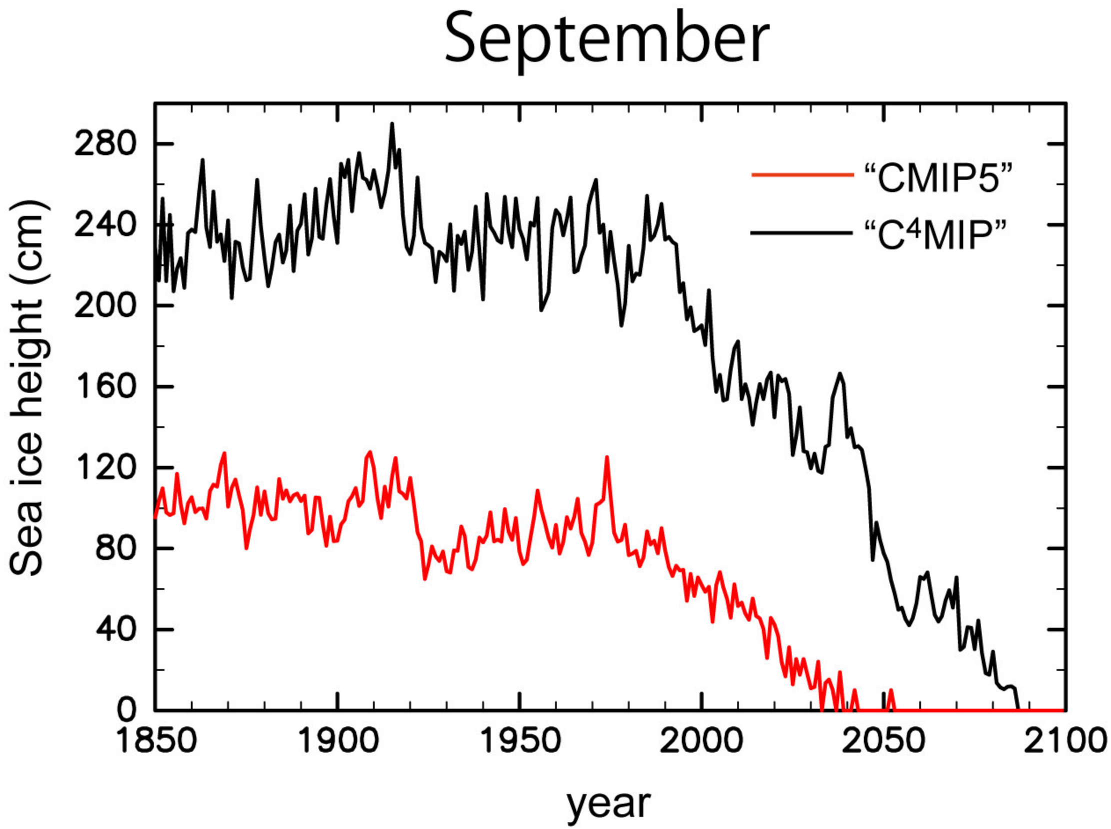

Fig. 1. September sea-ice height in the Arctic Ocean for both versions.

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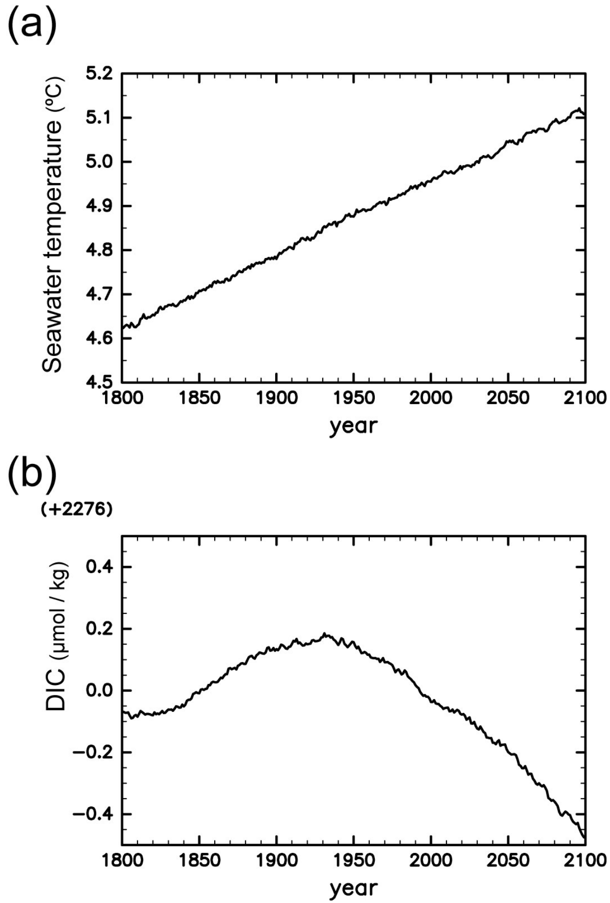

Fig. 2. Temporal variations of (a) globally averaged seawater temperature and (b) dissolved inorganic carbon at a depth of 1000-4000m for "C4MIP" under control run.

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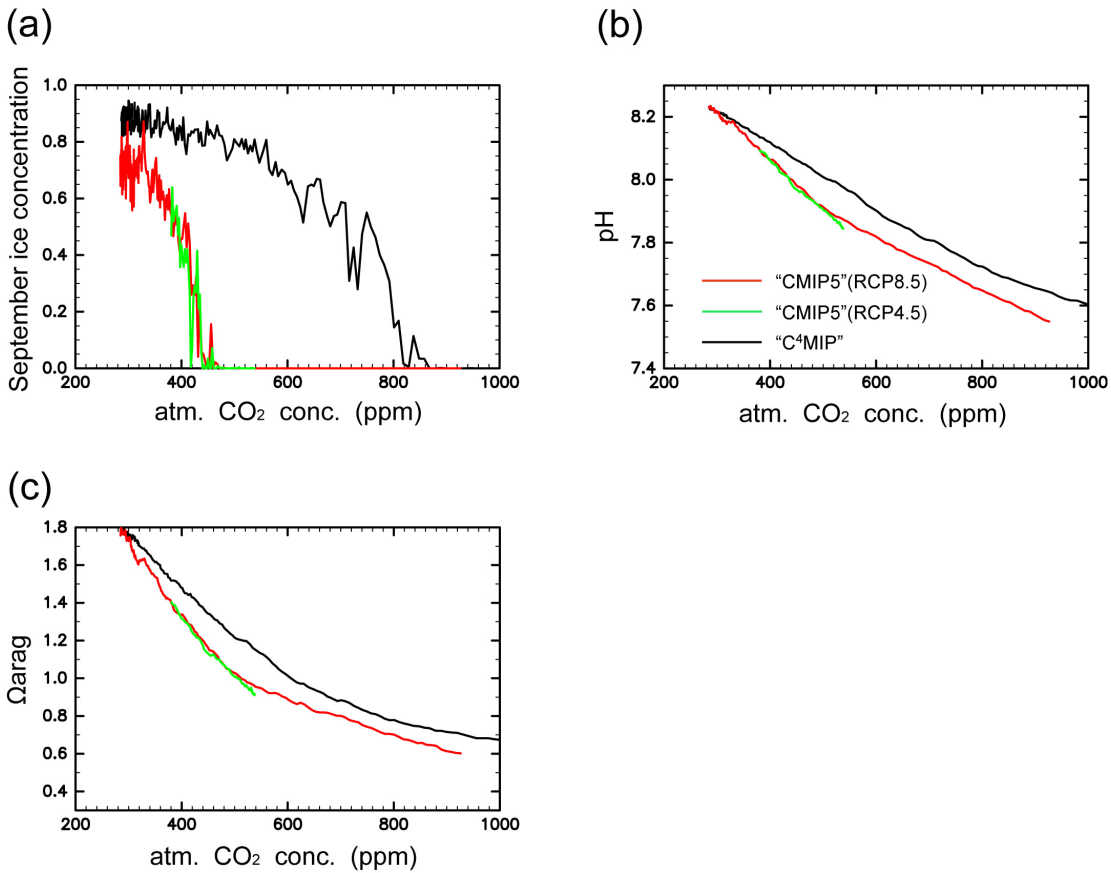

Fig. 3. (a) September sea-ice concentration and modified annual mean of (b) pH and (c) Ωarag with atmospheric CO2 concentration in the Arctic Ocean for both versions under RCP8.5 and "CMIP5

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