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Biogeosciences Discussions

Interactive comment on "Sensitivity of the marine carbonate cycle to atmospheric CO_2" *by* R. Gangstø et al.

R. Gangstø et al.

reidun.gangsto@meteoswiss.ch

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We would like to thank the reviewer for comments that improved our manuscript.

Answer to general comment:

The second reviewer criticizes the lack of novelty presented and suggests focusing more on the true originality of the study, such as the response of different groups of phyto- and zooplankton to increasing ocean acidification. He/she also suggests considering more simulations to determine the array of feedbacks of the carbonate cycle to perturbations. In addition, the reviewer finds the structure of the manuscript confusing, the title inadequate and mentions several inconsistencies.

Our study contains several novelties as also noted by reviewer 1. We are the first to explicitly include calcification by nanophytoplankton corresponding to foraminifera in a global biogeochemical model, to study it's response to increasing atmospheric CO2, and to investigate the associated feedbacks. Only one study (Gangsto et al., 2008) has so far included aragonite production and dissolution in a global biogeochemical model, and here this work has been extended by also investigating the feedback on atmospheric CO2 from the decrease in aragonite production. Other studies have investigated changes in calcification with increasing CO2 and associated feedbacks, but in addition to including new forms of CaCO3, we also use a different model. And by confirming results from similar studies they become more robust. This article is also the first one to present the new Bern3D/PISCES model and its validation.

However, we realize that the novelty of our study was not well enough emphasized, and the structure of the manuscript was not always clear. In our revised manuscript we have taken into account all the critics and suggestions of the 2. reviewer. We have highlighted the new model and the new findings, such as the implementation of foraminifera calcite in the model. The introduction has been revised to highlight the goals of the studies and the novel features as early as in the second paragraph. We have also added more simulations and quantified the potential upper limit of the CO2-calficiation feedback. Furthermore, the structure of the manuscript has been improved by adding discussions at the end of individual result sections, removing repeating parts and separating out the conclusions. Finally we have corrected the inconsistencies pointed out by the reviewer and changed the title to better reflect the content of the paper.

Answers to specific comments:

p. 7031, l. 29: Citation of Heinze (2004) is incorrect.

We have now removed the word “dissolution” so that the citation becomes correct.

p. 7032, l. 24: Please reconsider the formulation of the question on reversibility.

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We have added some more about this topic in the conclusion and the abstract, so that the question is more clearly answered.

p. 7034, l. 5: should be: “of temperature . . .” instead of “for temperature . . .”

This is now corrected.

p. 7035, l. 25-26: such assumption would lead to overestimation of dissolution fluxes. Please discuss this.

The following sentences have been added: This additional dissolution has only a minor effect on the ocean chemistry. Only the open water dissolution is included in our further calculations and comparisons to observations.

p. 7036, l. 14: is Rp,c calcite production by nanophytoplankton? It corresponds to calcite production by nanophytoplankton; the fraction Rp,c multiplied with the loss terms as shown in Eq. 3 gives the production. An explanation of Rp,c has been added:

the following parameterization is applied for the fraction of calcite production by nanophytoplankton, RP,C (Aumont and Bopp, 2006). . .

p. 7036, l. 16: where is scaling factor f given?

Please see response to first reviewer. We have added: The values of the scaling factors are not given as they depend largely on the carbonate chemistry and circulation fields of the model used.

p. 7037, l. 14: here and below, where does this numeration belong to?

The numeration is removed.

p. 7038, l. 10: see comment above

See answer above.

p. 7039, l. 8: Which Michaelis Menten curve is meant here? as most of the paper discusses aragonite cycle, consider adding a dependency for aragonite also in fig. 1.

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We have now explained better in the text which one it is. The dependency for aragonite has been included in Fig 1.

p. 7041, l. 3: the tuning factor f used in equation (11) is not given.

See previous answer.

p. 7042, l. 17: Annual means of what?

We have added: All the variables presented in this study (alkalinity, saturation state, $CaCO_3$ production etc) are given as annual means.

p. 7042, l. 26-27: Emissions are set to zero in high and medium emission scenarios, not in the low one.

This is now corrected.

p. 7043, l. 2: In this section present-day model results are evaluated, not preindustrial.

Pre-industrial model output is compared to pre-industrial observations where these are available, i.e. for DIC and saturation state we have used values from the GLODAP data set after removing the anthropogenic perturbations. For other variables we follow common praxis and compare the pre-industrial model output to present-day observations (circulation and $CaCO_3$ production), assuming that the differences are not large. We have now specified better in the text what we have done.

p. 7043, l. 10-23: Discussion of radiocarbon data is speculative and not supported by presented results.

Following the reviewer's suggestion we have removed the sentence: "as evidenced by the comparison between modeled and data-based radiocarbon fields from GLODAP (not shown)". We have however kept the sentence "too high radiocarbon values at the surface of the North Pacific compared to observations may indicate too little upwelling of old water masses here". The radiocarbon values are supported by the presented results. Note that this statement concerns the North Pacific, not the Equatorial Pacific.

Whereas in the Equatorial Pacific the model provides too much upwelling, in the North Pacific the results indicate the opposite (in addition to the streamfunctions, see for instance surface maps of modeled versus observation-based alkalinity and DIC: in the North Pacific the model underestimates the observed values, indicating too little upwelling here).

p. 7043, l. 26-28: this will have implications for CaCO3 production, this has to be discussed.

We now discuss this in section 4.2 when comparing the results to observations.

p. 7044, l. 4: largest differences in CaCO3 production are in upwelling regions – this is not explained.

This is now mentioned: However, whereas the estimates from satellite images show little calcification in lower latitudes compared to higher latitudes, a larger part of the total modeled calcification occurs in low-latitude upwelling areas. At least for the Equatorial Pacific, this may be linked to model deficiencies as mentioned in the previous section.

p. 7044, l. 6: simply stating the values are low and high is insufficient, illustrate by numbers.

We have now compared to calcification rates from Balch et al (2007).

p. 7044, l. 24: it is not clear from here if this feature is really due to higher abundance of mesozooplankton or due to bias in the circulation model.

We have now included maps of mesozooplankton and nanophytoplankton distribution (Fig. 4) showing that more of the total mesozooplankton distribution is located in coastal areas, than equivalently for nanophytoplankton. The model has the same bias in the circulation for all model versions.

p. 7045, l. 17-18: spatial distribution of mesozooplankton is not given here, consider showing it, otherwise this conclusion is speculative.

We have included maps of mesozooplankton and nanophytoplankton distribution (Fig. 4). We have also compared mesozooplankton values to observations from Buitenhuis et al. (2006) as a zonal mean plot (Fig. 5b). Although the spatial patterns are similar, the concentration is in fact somewhat underestimated in the north, and this is now mentioned as an explanation in addition to the missing temperature effect.

p. 7046, l. 30-33: what about corals?

Corals are not considered in this study, as we use a pelagic ocean model.

p. 7046, l. 10-11: is this inline with increase diffusivity of the circulation model?

We have deleted this sentence as there may be many reasons for the too low surface value.

p. 7047, l. 5-7: I don't see how this discussion is supported by figure 5.

This sentence has been removed.

p. 7047, l. 11-15: above, the authors suggest that including aragonite cycle improves model performance, why is correlation coefficient higher for model scenarios without aragonite?

Some mismatches for alkalinity may be seen in the deep Pacific and Atlantic Ocean, which probably lead to a lower correlation for alkalinity. We have now explained these mismatches better in the text: The alkalinity and DIC concentrations at depth are slightly improved in the versions including aragonite, due to the rearranging of DIC and alkalinity concentrations in the water column caused by shallow water aragonite dissolution (Fig. 6b). An exception is the alkalinity in the deep Atlantic and Pacific, which is more over- and underestimated, respectively, in the model versions including aragonite (Fig 7a). The differences between the model versions with calcite only and the model versions including aragonite appear in the Taylor diagram (Fig. 9), where modeled alkalinity and DIC are compared to pre-industrial values from the Global Ocean Data Analysis Project (GLODAP) (Key et al., 2004). Due to the mismatches in the deep

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Atlantic and Pacific, the correlation coefficient r between modeled and data-based alkalinity is $\sim$ 0.8 for the versions without aragonite, and $\sim$ 0.7 for the versions including aragonite. For DIC, the correlation coefficient is higher than 0.9 for all versions and independent of the form of CaCO3, whereas the standard deviation becomes closer to unity when aragonite is included. To conclude, except for some discrepancies in the Atlantic and Pacific mainly related to deficiencies in modeled circulation, the observed alkalinity and DIC concentrations are fairly well represented by the model.

p. 7048, l. 8: Fig. 5b does not show that DIC in Pacific Ocean is underestimated.

It does, but only for the upper 1000 m in question. But these figures are too small, so it may be difficult to see. We have split the figure up in two, so that the sizes are increased in the revised manuscript. We deleted this detailed sentence to shorten and clarify the paragraph a little.

p. 7049, l. 8-9: The meaning of this sentence is unclear.

The sentence has been changed to: The pelagic CaCO3 dissolution of all model versions is within the range of estimates of 0.5 ± 0.2 (Feely et al., 2004).

p. 7049 l. 21 – p. 7050 l. 7: Comparison with NEMO/PISCES is too extensive.

This paragraph has been removed.

p. 7050, l. 17: Section 4.2.5 does not exist.

This is corrected.

p. 7050, l. 27 – p. 7051, l. 2: this is self-evident, the sentence is redundant.

OK, the sentence is removed.

p. 7051, l. 20: this discussion is repetitive.

We have removed the sentence.

p. 7052, l. 2: this is repetition of the statement in p. 2050, l. 19.

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OK, this sentence is now removed.

p. 7052, l. 2-4: the sentence doesn't make sense.

The sentence has been changed to: Under the High scenario, the $CaCO_3$ production decreases to between 0.79 and 0.82 Pg C yr^{-1} by the year 2100, for most of the calcite-only versions (Fig. 10c).

p. 7052, l. 11-15: isn't this obvious? In both cases production decreases with saturation state and there are no other forcing factors.

There has been some discussion in the community concerning which shape to use when fitting the response in calcification to decreasing saturation state, whether a linear curve should be used or some other. Here we show that it doesn't matter much which one we use, this is therefore an interesting result. We have added this argument in the introduction.

p. 7053: l. 16-25: I don't see what is this discussion is based upon.

We have now included a figure with the relative dissolution (fig 10e), so that it may be seen directly.

p. 7054, l. 1-15: This whole paragraph's discussion is not illustrated. Note that this is "Results" section.

We have moved this paragraph to the introduction.

p. 7054, l. 27: I wouldn't call a feedback of a few ppm "substantially stronger".

The word "substantially" has been removed.

p. 7055, l. 6-9: this has been concluded already in other studies and is not a novel conclusion (see general comments).

This sentence has been removed here. The conclusion is however new in the sense that nobody has got this result with the same type of parameterizations. Nobody has

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looked at differences in using linear and Michaelis-Menten shapes, nobody has explicitly included aragonite production and calcite production from mesozooplankton when studying the sensitivity. We have now emphasized this novelty more in the introduction.

p. 7056, l. 14: why discussing possible implications for corals?

We have now made clear in the text that our discussion concerns pelagic pteropods.

p. 7059, l. 1-6: this is literature review which belongs to introduction.

We have removed part of it and concerning the remaining part we have discussed their results more in context with our results so that they become a more natural part of the results/discussion section.

p. 7079 (7059?), l. 16-27: this discussion is not supported by results presented in the manuscript and thus is out of context here.

This is removed.

p. 7060, l. 20: Section 5.2 almost entirely repeats discussion in section 4.2.4.

We have removed this repetition and restructured the results and discussion sections.

p. 7062, l. 1: Discussion in section 5.3 is repetition of the discussion in section 4.2.3

We have removed this repetition too.

Figure 1: since great portion of the manuscript discusses aragonite cycle, if would be useful to have a subfigure showing all scenarios for aragonite.

The aragonite parameterization used in the model versions with aragonite has been included.

Figure 5: subfigures are too small and hard to read. Different notations are used for model scenarios (inconsistent with Table 1). Colors used for different scenarios in the upper panel plots (a, b and c) do not match those in the lower panel (d and e). This is confusing.

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We let the first 3 subfigures represent a single figure in order to increase figure size. The remaining profiles (calcification and dissolution) become a second figure, and here we have changed the color and names in the legend so that they are consistent with the previous figure.

Figure 8: is color scale the same for all subfigures? Consider relocating it. Difference between saturation horizons for different scenarios is invisible.

Yes, the color scale is the same for all, we have now included it next to each subfigure. The saturation horizons for the two other model versions were left out by mistake and are now added as grey and white lines.

Figure 10: Does this figure show zonal means? What is the difference between this figure and fig. 14a. As far as I can see, both show Omega for aragonite under high CO2 scenario.

We have now removed Fig 14a.

Interactive comment on Biogeosciences Discuss., 7, 7029, 2010.