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Biogeosciences
Discussions

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

B. Schneider (Referee)

bschneider@gpi.uni-kiel.de

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The study "Sensitivity of the marine carbonate cycle to atmospheric CO2" by authors Gangstoe, Joos and Gehlen uses a cost-efficient ocean circulation model (Bern3D) in combination with a complex marine biogeochemical model (PISCES) to investigate the response (calcification, dissolution) of marine pelagic calcifying organisms to future ocean acidification (OA) and potentially arising negative feedbacks on atmospheric CO2. A large effort has been made, where the sensitivity is tested in several different ways, e.g. by applying (1) different parameterizations for the dependence of pelagic calcification on the saturation state of seawater with respect to CaCO3; (2) different mineral forms of CaCO3, such as calcite formed by phytoplankton (coccolithophorids) and zooplankton (foraminifera), as well as zooplankton aragonite (pteropods); and (3)

finally, different strengths of CO2 emission pathways are used. The main conclusion is that the negative feedback on atmospheric CO2 is rather small, and on the order of 1-11ppm by the year 2100, which is in support of previous studies.

The study is very interesting, thouroughly carried out, well explained and certainly dealing with a topic that is presently of high scientific concern and debate. Although contained in the manuscript, there is little new information highlighted. Therefore, I would suggest to add more information on theoretical considerations of the sensitivity of pelagic calcification to OA and also to focus more on the impact of the response of zooplankton calcite to OA, which has not been done explicitly in similar studies, before. I suggest to publish after some minor revisions.

General comments

The study explores the sensitivity of pelagic calcification to ocean acidification (OA) by first addressing various dependencies of calicification on the seawater CaCO3 saturation state. These dependencies are based on laboratory and mesocosm studies, since very little is known from the open ocean realm. A wide range of different parameterizations is therefore an adequate approach to address this uncertainty. However, it seems that except for LIN1 all the different parameterizations are affected in a very similar way by changes in the saturation state. It would be very interesting to explain the theoretical behavior of the different parameterizations in more detail, e.g. by calculating the derivatives of the theoretical PIC:POC ratios (Figure 1) to changes in Omega. These sensitivities could be added to Figure 1, indicating in which Omega-range which parameterization is the most sensitive (see dashed lines in Figure below).

The sensitivity of the Michaelis-Menten kinetics can be calculated as d(PIC:POC)/dOmega = k_max * (PIC:POC)_max / (k_max + Omega - 1)ˆ2 Accordingly, for Omega > 4 the parameterization LIN1 is clearly the most sensitive, about 3-4 times more sensitive than the other formulations (see figure below). Only for very severe OA (Omega < 2.5) MIC1 becomes most sensitive. Considering that preindustrial zonal average values for Omega-C were lying within the range of 2.5-6.5 and that Omega-A values will move to 0.7-2.5 in the future (Fig. 10), this means that first LIN1 will show stronger effects and then MIC1 becomes more sensitive around an Omega-value of 2.5. The aragonite sensitivity is always very low due to the weak slope of the regression line. For Omega-values approaching 1 the MIC2-formulation becomes also increasingly sensitive, reaching the sensitivity of LIN1, but still MIC1 is here clearly the most sensitive. These considerations also nicely explain the differences seen in the calcification response in Figure 9c.

Another point, probably the more important one when wondering about the weak feedback on atmospheric CO2, is the low amount of pelagic CaCO3 available for compensation. Therefore, it would be very interesting to determine the potential upper limit, e.g. by setting the calcification to zero at a certain time (e.g. in the year 2000). I guess this could easily be done by the cost-efficient model that was used in the study. The ocean CO2-uptake would then mostly be determined by the efficiency of the ocean turnover bringing alkalinity back to the surface. Since several similar studies have given similar values for the calcification feedback (varying by a factor of two; Ridgwell et al., 2007), it would be interesting to see what the maximum value to be obtained from the PISCES model would be. I assume that the higher values in Ridgwell et al., are mostly due to the fact that (a) more calcite is being formed each year (export ~1.2 Gt CaCO3-C/yr) and (b) that the higher reaction rate order η yields an increased (non-linear) sensitivity in contrast to the linear response in PISCES (where n=1). Discussing the difference between PISCES and Ridgwell et al. 2007 as well as the potential upper limit would provide a nice concluding statement on the existing efforts to determine the feedback potential.

The inclusion of foraminifera calcite in this kind of modeling exercise is certainly an innovation that should deserve more attention. As it stands, the response of the experiment including foraminifera calcite (CAL2ARAG), where foram calcite seems to contribute about 1/3 to total calcification, does not significantly deviate from the version

where approximately 2/3 of the calcite is formed by phytoplankton and 1/3 is aragonite from zooplankton, although the parameterizations are very different for phytoplankton calcite (Michaelis-Menten kinetics) and zooplankton calcite (linear response to Omega). However, this is an important result, indicating that as the abundance of zooplankton is coupled to the food source (phytoplankton) and the sensitivities of the different parameterizations of the PIC:POC ratio to Omega are very similar, it can be concluded that the response of global calcification to OA can adequately be approximated by using either of the organisms. This is a very new finding, which should be highlighted, discussed and also mentioned in the abstract.

Minor comments

Title: The title ‘Sensitivity of the marine carbonate cycle to atmospheric CO2’ sounds very general. As the topic is ocean acidification and pelagic calicification, something like ‘Sensitivity of pelagic calcification to Ocean Acidification’ would be more appropriate. As far as I could find out, and surprisingly, this title seems not to be taken, yet.

p. 7032, l. 17-19: Ridgwell et al. (2007) have already included foraminifera via an optimization of the rate order η.

p. 7036, l. 5: is there a reference for the limit of 0.8?

p. 7036, l. 17-22: explanation to equation (4): it should be mentioned that (1) the scaling factor is used to match a certain value for global calcification (please include value and reference or at least refer to Tables 1 and 2); (2) the limitation term max(1,P/2) must be in μmolC/l; (3) the average PIC_C:POC is 0.3 (if I’m correct).

p. 7037, l. 14: numbering is not consistent, I’d suggest to remove it here

p. 7037, l. 23: It might be helpful for the reader to mention where the values of (PIC/POC)_max and K_max can be found in Figure 1.

p. 7039, l. 13-15: a bit more specific information about the results from L. helicina would be helpful to understand the development of this parameterization

p. 7039, l. 21: again a scaling factor (f_M,A) is used. As I understood, this is used to tune Aragonite production to 1/3 of global calcification. Please specify.

p. 7041, l. 4: tuning factor f_M,C, please specify

p. 7041, l. 7: If the sensitivity analysis of the different paraterizations is added to Figure 1 (see general comments above), it would be good to explain this in more detail, here.

p. 7041, l. 9 – p. 7042, l. 9: Model description, please move to section 2.

p. 7042, l. 21-22: If atmospheric CO2 emissions are used to force the model, which assumptions are made for the land biosphere?

p. 7044, l. 13-14: typo – relatively

p. 7047, l. 12: Why is the correlation weaker for Aragonite (0.7) than for calcite (0.8)? Before, you mention that the surface Alkalinity distribution is better in the Aragonite version due to the shallow remineralization and faster transport back to the surface via circulation. Why does this better match not apply to deeper levels?

p. 7084, l. 12: Please add the depth of the calcite and aragonite saturation horizons to the Taylor diagram. This is another (probably more strict) constraint of the spatial distribution of the saturation state.

p. 7048, l. 23-25: this is part of the model description

p. 7049, l. 19-21: you may add: . . . and upward mixing (Friis et al., BG, 2007)

p. 7050, l. 23: add ‘growing’ before ‘extent’

p. 7050, l. 27 – p. 7051, l. 2: I do not understand this sentence. Since only the parameterizations for calicification differ, it is clear that everything is related to them.

p. 7052, l. 6-7: maybe this sentence could be continued by: . . ., over the entire Omega-range (see sensitivities added to Figure 1)

p. 7053, l. 6: add ‘. . . scenario (Fig. 9d).’

p. 7054-7055: add more information about the foram calcite effect and the potential upper limit of the feedback on the atmosphere, when considering that calcification ceases (see general comments above)

p. 7059, l. 24-27: Yes, but also these species will respond to undersaturation by dissolution.

p. 7062, l. 13-16: What is the indirect way of ballasting in PISCES? Does calcite play a role here? Please specificy.

Table 2: In the caption, I guess the CAL-MIC2 parametrization is referred to.

Table 3: if the 2nd and 3rd columns show differences between 2100 and preindustrial, it should say 2100-1766 in the column heads

Figure 1: see comments above and suggested Figure below

Figure 5e: why is in the Aragonite-run the calcite dissolution shallower than in the calcite only run? The delta-CO3 looks very similar (Fig. 5c).

Figure 7: please add the depth of the saturation horizons for calcite (CSH) and aragonite (ASH). It seems that none of the parameterizations significantly improves the model-data agreement. As the Taylor analyis only makes pattern correlations, what are the respective total amounts of DIC in the different runs? It would be interesting to see by how much the DIC inventories differ, since the atmosphere is an unlimited source to the ocean in the spinups, as I understood. These numbers could be added in Table 2.

Figure 8: I cannot see any grey or white lines.

Figure 11: Please add bar on the left showing the respective percentages from the Glodap data

Figure 13: maybe you could add a panel showing the percent amount of undersaturated water, which would nicely indicate the persisting impact of OA at greater depth

Caption for Figure 1 (below): PIC:POC ratio (full lines) as function of Omega as used in the manuscript (Fig 1.). The dashed lines show the respective sensitivities, d(PIC:POC)/dOmega.

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

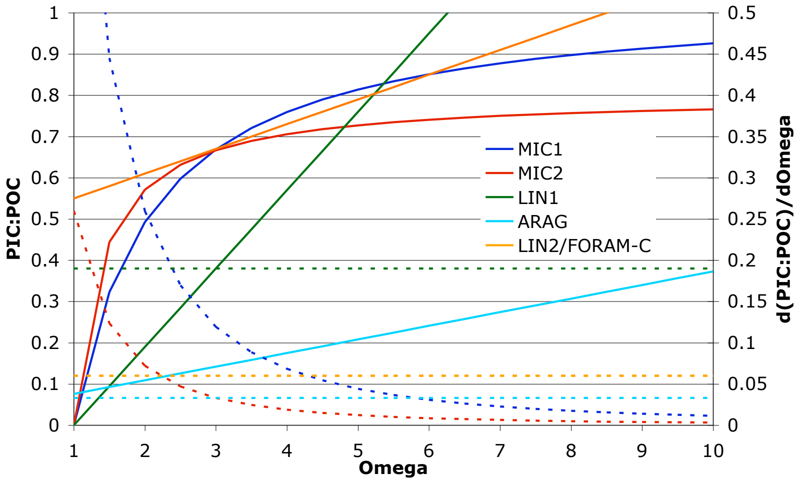

Fig. 1.