Biogeosciences Discuss., 7, C3837–C3842, 2010
www.biogeosciences-discuss.net/7/C3837/2010/

Biogeosciences
Discussions

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

Anonymous Referee #2

Received and published: 16 November 2010

General comments:

The manuscript by R. Gangstø et al. "Sensitivity of the marine carbonate cycle to atmospheric CO2" addresses an actual and important subject of the impact of ocean acidification on marine calcifying organisms. The quality of the methods presented in this work reflects a careful work underlying this study. The authors go beyond the standard approach used in most global biogeochemical models and include explicit formulation of aragonite cycle in the model PISCES. This, however, has been done already in their previous study (Gangstø et al., 2008). In the current manuscript, authors extend their earlier work by incorporating the model PISCES in a cost-efficient ocean circulation model Bern3D. This, compared to their previous work enabled them to perform long-term simulations, but without including the effects of changing circula-

tion and stratification as a result of climate change. In this manuscript the authors also present a very detailed evaluation of model results by comparing them to present-day observations – this is a rewarding effort.

Furthermore, this manuscript presents several (all very similar, except for the Lin1 scenario) parameterizations of calcification and discusses differences between them, as well as the differences in feedbacks on atmospheric CO2 that are also expectedly small. This, being one of the main conclusions of the paper, is not a novel finding, as the strength of CO2-calcification feedback has been investigated in other recent modeling studies, e.g. performed by Ridgwell et al. (2009) and Heinze (2004) which are also cited in this manuscript. The novel findings of the current study are poorly presented, and the paper misses a clear storyline. The title does not reflect the content of presented work.

On the technical side, although presentation and discussion of results are separated, "Results" section includes extensive discussion, whereas "Discussion and conclusions" section mostly repeats previously made points (see specific comments). The structure of the manuscript is confusing. For instance, within subsection 2.2.1., there are further sections 1. and 2; non-existing subsection is referred to in the text (i.e. section 4.2.5; see specific comments). Fig. 10 and fig. 14a show the same distribution of the saturation state for aragonite, except for extra isolines added on fig. 10 that do not appear on fig. 14 – this is clearly insufficient information for a separate figure. Also, I don't see a clear answer to one of the questions raised in Introduction about reversibility of changes in ocean chemistry.

Overall, scientific novelty of the current work is insubstantial and the presentation quality is poor. I recommend the authors to thoroughly read their manuscript and perhaps focus on the true originality of their work, such as for instance, how different groups of phyto- and zooplankton would respond to changes in aragonite and calcite production caused by ocean acidification. Authors may also want to consider adding more sensitivity studies to determine the array of feedbacks of carbonate cycle to perturbations.

In its current form, however, I do not recommend this manuscript for publication.

Specific comments:

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

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

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

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

p. 7036, l. 14: is Rp,c calcite production by nanophytoplankton?

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

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

p. 7038, l. 10: see comment 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.

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

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

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

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

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

p. 7043, l. 26-28: this will have implications for $CaCO_3$ production, this has to be discussed.

p. 7044, l. 4: largest differences in $CaCO_3$ production are in upwelling regions – this is not explained.

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

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.

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

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

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

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

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?

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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.

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

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.

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