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Biogeosciences Discussions

Interactive comment on "Calcium carbonate production response to future ocean warming and acidification" *by* A. J. Pinsonneault et al.

Anonymous Referee #2

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General comments:

The manuscript addresses important scientific questions of how ocean acidification and climate change will impact marine calcification and what feedback will these impacts have on Earth's climate. Until now, only a few modeling studies have included the sensitivity of CaCO3 production to increasing CO2. The authors go further and include the effects of warming the grow rates of phytoplankton as well. Hence, the manuscript contains novel concepts, and the subject would clearly fall within the scope of BG.

There are, however, serious deficiencies regarding material presentation and critical assessment of results. The authors attempt to encompass the different feedback mechanisms of ocean carbonate system on climate, but omit citing fundamental publications on the topic, such as for instance the studies by Broecker, Archer, Sundquist. More-

over, studies that have been mentioned are cited inaccurately and the author's names are often misspelled – an inaccuracy which could have easily been avoided if an appropriate citation tool had been used.

The tool used in this study, the UVic ESCM, is well documented. The paper focuses on calcification and addresses the dynamics of ocean carbonate system on millennial time-scales. Thus, model assumptions and parameterizations of processes, relevant for the scope of the paper and the time-scales in question, have to be given in details. This includes parameterization of dissolution in the water column and in the sediments and how changes induced by ocean acidification are incorporated in these parameterizations. Furthermore, authors should discuss potential effects of processes that are not included in the model, i.e. the ballast effect and changes in dissolution due to ocean acidification.

Presentation of results, their evaluation, and discussion are extremely concise. The increase in CaCO3 production reported in the manuscript is simply a consequence of the increased primary production in a warmer ocean projected in UVic ESCM. The authors need to critically discuss this result, given that most other models predict a decline in global biological production in a warmer and more stratified ocean. Likewise, decreasing CaCO3 production projected in simulations with the saturation state dependence in this study has to be discussed in the context of previous publications.

I also found the paper hard to read: Sentences are often unnecessarily too long and unclearly written. I suggest the authors to carefully edit the text of the manuscript.

In summary, I recommend this manuscript for resubmission after major revisions including the points raised above. I also provide specific comments to improve the manuscript.

Specific comments:

p. 11865, l. 21: "many areas in the water column" doesn't make sense

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p. 11865, l. 25: Recent laboratory and mesocosm studies show that calcifying organisms are sensitive to a wider number of parameters of the ocean carbon system, for instance the seawater pH.

p. 11866, l. 6: Previous studies cited in this sentence show that this negative feedback is rather small.

p. 11866, l. 9: Gangstø is misspelled.

p. 11866, l. 9-11: A comprehensive summary of different responses of organisms to ocean acidification is given in Gattuso and Hansson, 2011.

p. 11866, l. 18-21: A model intercomparison study by Steinacher et al., BG (2010) showed that many models predict a decrease in biological production due to reduced nutrient supply in a warmer and more stratified ocean. Is this effect included in the model? This has to be mentioned and discussed.

p. 11866, l. 21: Explain how an increase in temperature would result in a more rapid CaCO3 production.

p. 11866, l. 26: remove “up”; the term “DIC” has not been introduced yet.

p. 11867, l. 18-13: The sentence starting with “This would lead . . .” is rather incomprehensible and needs to be re-phrased.

p. 11868, l. 5: Ilyina is misspelled.

p. 11870, l. 11-13: It is not clear how water-column dissolution is described in the model. What was the dissolution rate?

p. 11870, l. 26-28: Similar to the point above, how carbonate dissolution in the sediment is treated in the model? If sediment dissolution is independent of the saturation state, how does the model incorporate fossil fuel neutralization? This process is relevant on longer-time scales.

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p. 11872, l. 6: The temperature effect on CaCO3 production should be explained. Ideally, experiments should be complemented by another model run in which neither temperature effect, nor dependency on the saturation state are included.

p. 11873, l. 15: by '"latitudinal dependence" did you mean latitudinal variations?

p. 11873, l. 17-18: There is no doubt in the modeling community that observations are crucial in constraining the models! The statement needs to be re-phrased.

p. 11873, l. 20: "the control model", add "run"

p. 11873, l. 20: What observations have been used?

p. 11873, l. 23-24: It is not explained how high sensitivity to saturation state is responsible for a weaker agreement between modeled and observed alkalinity?

p. 11873, l. 26-27: Is alkalinity conserved in the model runs without the effects of climate change and ocean acidification?

p. 11874, l. 23: "an increase of ∼20%", over what time period?

p. 11875, l. 26-27: The increase in CaCO3 production needs to be clarified and discussed in comparison with projections from other models.

p. 11876, l. 10-11: Other modeling studies, e.g. Gehlen et al. (2007), Gangstø et al. (2011), Ilyina et al. (2009), Ridgwell et al. (2006) provide different estimates of the decrease in CaCO3 production. This has to be mentioned.

p. 11876, l. 23-25: The usage of potential alkalinity to improve process understanding needs clarification. A definition of potential alkalinity is necessary for non-specialists.

p. 11877, l. 17-19: I suspect that including saturation-dependent dissolution of carbonate sediments in the model would also effect the burial rates. This issue has to be brought up in the discussion of sediments dynamics.

Interactive comment on Biogeosciences Discuss., 8, 11863, 2011.