

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

Anonymous Referee #1

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The manuscript by Pinsonneault and coworkers addresses the evolution of marine pelagic calcium carbonate production under future conditions of ocean warming and acidification. To my best knowledge, previous modeling studies focused on impacts of ocean acidification on future carbonate production, but excluded ocean warming. From this perspective, the present manuscript provides an important and needed addition to the field of ocean acidification research. While ocean acidification is commonly accepted to have a negative impact on calcification, warming will increase phytoplankton growth rates. The ultimate response of carbonate production will thus depend on the relative strength of both of these processes. That is pelagic calcification by phytoplankton might increase in a warming ocean in response to enhanced growth rates, despite decreasing calcification rates. This hypothesis is illustrated and confirmed in Fig.6. Pinsonneault et al. address this problem by a series of sensitivity studies with an Earth
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system model of intermediate complexity and for two IPCC AR4 CO₂ emission scenarios. The simulation strategy is well described and acceptable. The results complement our current understanding. They contribute to evaluate uncertainties in the strength of the combined warming-calcification feedback to atmospheric CO₂ in climate projections. While this study is of value and should be published, I can't recommend it in its present form for publication in Biogeosciences.

My main concern with this paper is the lack of critical assessment of results. It reads in large sections as a report rather than a scientific publication. The authors cite previous studies in the introduction, but make little use of results by others to discuss their own findings. Also of concern is the fact that important papers are not cited, while others are misquoted. The presentation of model output is very selective and does not allow the reader to fully/critically appreciate simulation results. I provide suggestions and comments below for each section. I hope that the authors will find them useful while preparing their study for resubmission. I strongly encourage the authors to resubmit!

Detailed comments:

Introduction:

This section is long and lacks structure. It is in part badly phrased and confusing. Relevant citations are missing and previous studies are sometimes badly quoted or misinterpreted.

11865, L18-19: please clarify statement: “since the residence time ...”;

L20-24: statements like “will become undersaturated with respect to calcite ...” need to be completed by adding the corresponding time scale;

L25 & 11866 to L11: please cite experimental studies in support of acidification impacts on calcification rates rather than a mix of experimental and modeling work; this section would benefit from being restructured;

11867 : rain ratio changes, impact on sediment compartment and associated feed-

backs to ocean alkalinity cycle: needs to be clarified; relevant citations are missing: Gehlen et al. (2008): short term response of carbonate sediments to anthropogenic CO₂ invasion; Ridgwell (2003): discussion of rain ratio in the context of the ballast model for particulate organic C fluxes; Archer (2005), Archer and Maier-Reimer (1994), Archer et al. (1997) and (1998), Ridgwell et al. (2007): assessment of calcium carbonate compensation on long time scales. Again, time scales should be clearly identified when discussing potential sediment feedbacks.

L11866 provides an illustration of misquotation: Gangsto et al. (2008) assess the contribution of pelagic aragonite production through pteropods, while Andersson et al. (2006) evaluate changes in benthic carbonate production in a box model for the coastal ocean. It is thus wrong to state that "Much of this variability is attributed to the calcifying species represented in the model, most often the coccolithophore *Emiliana huxleyi*."

L2: "... other modeling studies ...": relevant studies to be cited and discussed are Ridgwell et al. (2007); Ridgwell et al. (2009);

L13: concerning "longer-term studies using models with deep-sea sediment components": please refer to Archer (2005), Archer and Maier-Reimer (1994), Archer et al., (1997) and (1998), Ridgwell et al. (2007), Tschumi et al. (2011).

Gehlen et al. (2011) provide a comprehensive synthesis of modelling studies quantifying ocean acidification impacts of marine biogeochemical cycles.

Model description

Please note that Eppley (1972) published an empirical relationship relating phytoplankton growth and temperature. To my knowledge it does not discuss organic matter turnover rates. The Eppley-type dependency is used in the model presented by Schmittner et al. (2008). Please distinguish between the primary experimental study (and publication) and the secondary use in models.

Calcium carbonate production: by what mechanism does zooplankton mortality con-

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tribute to Pr(CaCO₃)?

11870, L25: what is the 'pore layer'? Do you mean 'bioturbated layer'?

11871, L7-L11: "Since the model does not properly resolve coastal processes where aragonite is produced ...": wrong, please check V. Fabry's older studies on the contribution of aragonite to particulate inorganic C fluxes and production. Gangsto et al. (2008) evaluated the potential contribution of aragonite production to the pelagic CaCO₃ budget.

L15: no saturation state dependency of calcium carbonate dissolution: this is surprising: dissolution kinetics of calcium carbonate are well understood compared to the production term. Please consider updating dissolution kinetics.

Model experimental methodology

spin-up strategy: model configurations were integrated 10'000 years: did the authors verify the global carbonate budget? Was it in balance, did weathering input equate sediment burial of alkalinity?

11872, L16: values of K_{max} ranging from 0,07 to 20: does such a range make sense? I understand that the authors wanted to cover a variety of shapes of the CaCO₃ production versus saturation relationship, but the Michaelis-Menten curve has a physiological meaning. I'm not asking to redo the sensitivity analysis by replacing the case 5 by a linear dependency, but rather to consider the primary significance of a curve based on process understanding prior to parameter adjustment for future model sensitivity studies;

Results

The presentation of results has to be improved by extending the model assessment at the end of spin-up (model-data comparison) and by clearly structuring scenario results with respect to the temporal window considered (2100 vs 2300).

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Spin-up

I would appreciate a more detailed assessment of how different or similar relevant model fields were at the end of the spin-up for the different CaCO₃ production terms. This could be done by displaying mean values and standard deviation, along with the extreme cases 0 and 5. The model output – data comparison should be completed by adding the depth of saturation horizon of calcite. Finally, since the model includes a sediment compartment, a comparison between modeled sediment composition and observations should be added.

Figure 3: Are you sure that you're displaying 'potential' alkalinity? Not alkalinity??

It is not always easy for the reader to follow whether the presentation of results is referring to year 2100 or to the final year of model experiments. The long term evolution of dissolved ocean properties needs to be discussed with greater care. How do alkalinity and saturation state evolve for the various cases? A figure displaying saturation state of surface ocean waters with respect to calcite as a function of latitude and time would be very informative. Similarly, the evolution of the volume of undersaturated waters with time should be included. Gangsto et al. (2011) and Steinacher et al. (2009) provide nice examples of how to present complex information on changes in carbonate chemistry in a condensed and easy to interpret way. Please highlight your results up to 2100 by presenting (1) presenting differences between scenarios and parameterizations; (2) impacts of alkalinity and saturation fields (surface and 3D); address the evolution of biogeochemical fields between 2100 and 3500 in terms of long-lasting impacts and legacy of CO₂ emissions in addition to calcification parameterizations.

Discussion

The discussion is weak. I encourage the authors to put their work into context by critically comparing results to previous studies. For example, the response of the UVic model in terms of primary production is quite unique when compared to other Earth System Model studies (e.g. Steinacher et al., 2010). Could it be that the strong

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temperature-sensitivity in your model compensates at first order for a simplified representation of ocean biogeochemistry? Most models include an explicit iron cycle. One could hypothesize that the lack on Fe limitation of PP in the UVic is in part compensated by a strong temperature dependency of phytoplankton growth and production. I'm not suggesting that some models are better than others. It is however important to comment such important model differences, especially since the temperature response is essential to the outcome of this study.

The evolution of the marine carbonate cycle, changes in CaCO₃ production and burial, feedbacks to atmospheric CO₂ and temperature need to be addressed in the light of relevant older studies. Please be careful to distinguish time scales associated with different responses (e.g. CaCO₃ production, burial and sediment response). This will further highlight the contribution of the present paper to the field of ocean acidification research.

Conclusions

This section needs to be obviously rewritten too. Keep it short and restricted to the main findings. There is no need to reproduce a mini-summary/abstract.

A last question: 11878, L18: what do the 13% refer to? A 13% reduction of atmospheric CO₂ at the end of the simulation compared to S0, right? Or as stated in the abstract "13% of total C emission"?

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