

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

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Reviewer 1

OVERALL COMMENTS:

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

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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 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!

We thank the reviewer for their excellent and constructive suggestions on how to improve our manuscript. In order to improve the overall flow for the reader, the large sections in the manuscript were broken down into subsections. Citations of key papers regarding ocean acidification and the CO₂-calcification feedback (e.g. by D. Archer, A. Ridgwell, E.T. Sundquist) were added in the introduction and the misquotations cited papers were corrected. Additional figures were also included in order to expand on the presentation of model results and the discussion section of the manuscript was expanded to better assess our results in comparison to existing studies in the literature.

SPECIFIC COMMENTS:

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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.

The text in the introduction was revised to improve both phrasing and clarity and the introduction was broken into subsections to improve the structure. Additional citations were added (e.g. Archer, 1997, 1998; Archer & Maier-Reimer, 1994) and poorly quoted or misinterpreted citations were either removed or corrected.

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

Phrasing changed to refer to the Ca²⁺ ion concentration relative to that of the CO₃⁻ ion concentration as opposed to residence time in the ocean.

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

Corresponding timescale of 100-400 years added.

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;

This section was rewritten to focus on empirical acidification-calcification studies (e.g. Delille et al., 2005; Engel et al., 2005; Iglesias-Rodriguez et al., 2008). Mention of modeling studies was separated and mentioned strictly in the context that they base their parameterization of the CO₂-calcification feedback on the results of laboratory and mesocosm studies.

11867 : rain ratio changes, impact on sediment compartment and associated feedbacks 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),

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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.

Section was re-written for clarity and relevant citations (Archer, 2005; Archer & Maier-Reimer, 1994; Archer et al., 1997, 1998; Gehlen et al., 2008; Ridgwell, 2003; Ridgwell et al., 2007) were added. Presentation of ballast hypothesis was also expanded upon relative to uncoupled PIC and POC export and time scales regarding changes in deep ocean chemistry and sediment dynamics were included.

L11867, L25: provides an illustration of misquotation: Gangstø 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*.”

This section was re-written to correct this misquotation. Comparison of the Andersson et al. (2006) and Gangstø et al. (2008) was replaced with a comparison of Heinze (2004) and Gehlen et al. (2007). This comparison illustrates how basing model parameterizations of the CO₂-calcification feedback on data from different empirical studies, when there is such variability in the calcification response to acidification between these studies, can be a source of uncertainty in model predictions.

L11868, L2: “... other modeling studies ...”: relevant studies to be cited and discussed are Ridgwell et al. (2007); Ridgwell et al. (2009);

Citations added and discussed.

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)

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provide a comprehensive synthesis of modelling studies quantifying ocean acidification impacts of marine biogeochemical cycles.

Reworded to specify longer-term studies specifically addressing the uncertainty in the CO₂-calcification feedback parameterization as opposed to any long-term studies using models with deep-sea sediment components. Therefore, aside from Ridgwell et al. (2007), these additional citations do not apply and were not added.

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.

A paragraph was added to the model description section distinguishing the primary experimental study by Eppley (1972) and its secondary use in the UVic Earth System Climate Model. Eppley (1972) showed that phytoplankton growth rates increase exponentially with temperature so, in the model, maximum rates of phytoplankton growth, fast nutrient recycling, zooplankton excretion rates, and remineralization of detritus are all assumed to depend exponentially with temperature.

Calcium carbonate production: by what mechanism does zooplankton mortality contribute to Pr(CaCO₃)?

Foraminifera, which are zooplankton, are important calcifying organisms that contribute about half to global calcification (the other half is coccolithophores). Since we do not explicitly model CaCO₃ concentration but assume the production is instantaneously dissolved in the water column it is appropriate to treat part of the zooplankton loss as transfer of CaCO₃ from the surface to the deep ocean. A sentence to this effect was added to the model description section.

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

Yes, the pore layer is the bioturbated layer. This was clarified in the text.

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. Gangstø et al. (2008) evaluated the potential contribution of aragonite production to the pelagic CaCO₃ budget.

The reference to coastal processes was removed from the text. Gangstø et al. (2008) confirms the results of Fabry (1990) that ~90% of pelagic calcification is in the form of calcite so this citation was added in support of our focus on this CaCO₃ polymorph.

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.

We are aware of this limitation and chose this initial development phase of the UVic ESCM's ocean ecosystem/biogeochemical component to focus on CaCO₃ production. The next development phase will involve updating the water column CaCO₃ dissolution kinetics as well but that is outside the scope of this paper.

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?

In all spin-up simulations weathering input of alkalinity to the ocean and marine sediment burial of alkalinity were kept in balance. In the transient simulations terrestrial weathering was fixed at the preindustrial rate at the end of the spin-up simulation and sediments were allowed to respond to changes in ocean chemistry and biological production. This has been clarified in the text.

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11872, L16: values of Kmax 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 Michealis-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;

We acknowledge that using Kmax values ranging from 0.07 to 20 is not realistic and were only used to cover a range of calcification responses. We will, however, consider the physiological meaning of the Michealis-Menten curve in future stages of the model development.

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).

The temporal window has been clarified in the presentation of results throughout this section. Results focus on years 2100 and 3500 but not year 2300. Please refer to next comment response regarding extending the model assessment at the end of spin-up (model-data comparison).

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

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observations should be added.

The basic biology, ocean chemistry, and sediment fields in the UVic ESCM have been already validated elsewhere (Schmittner et al., 2008 and Eby et al., 2009). Though we have made a modification to the model's parameterization of the CO₂-calcification feedback, Figures 2-4 include comparisons of contemporary model output and observations and serve to validate this modification. We feel that extending the model assessment at the end of the spin-up simulations would not serve to further validate the UVic ESCM. Furthermore, this would less well represent the data and would be somewhat confusing to the readers since we would not only have to differentiate between model suites (S suite vs. M suite) and configurations (runs 0-5) but also spin-up and transient runs.

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

Yes, this figure represents potential alkalinity rather than alkalinity. Potential alkalinity is the alkalinity corrected for biological activity and normalized for salinity.

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. Gangstø 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.

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Throughout the discussion section the time frame for the presented results has been clarified (e.g. 2100 vs 3500). The results section was further broken down into subsections for the sake of clarity. Additionally, we added a figure displaying surface ocean calcite saturation state as a function of latitude and time for control (S0 and M0) and extreme (S5 and M5) scenarios (new Figure 6) and a figure of the evolution of the volume of undersaturated ocean waters for all simulations (new Figure 9).

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 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 discussion section was expanded to better critically assess our results in the context of previous studies. This expansion includes a discussion of the primary production response of different Earth System Models (Steinacher et al., 2010 vs. Schmittner et al., 2008 and our study) and a caveats subsection where the lack of iron limitation in the UVic ESCM and its potential impact on the results presented in this paper is mentioned.

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

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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.

The evolution of the biological and chemistry fields is now discussed in the context of relevant older studies with the time scales clearly stated for each individual process.

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.

The conclusion was shortened to include only the main findings.

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"?

Total cumulative CO₂ emissions in the SRES A2 scenario equal 2166 Pg C. In run S5, total ocean C was 272 Pg C greater than control (see Table 2). This difference in ocean carbon equates to ~13% of total carbon emissions. This was clarified in the conclusion.

Reviewer 2

OVERALL 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 growth 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

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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.

We thank the reviewer for their very constructive suggestions on how to improve our manuscript. In order to improve the overall flow for the reader, main sections in the manuscript were broken down into subsections and sentence structure was simplified where possible. Furthermore, the suggested key publications (e.g. studies by

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Broecker, Archer, and Sundquist) were added, inaccurate citations were corrected, and all citations in the text were replaced using citation software to correct any misspelling of author names. Model parameterizations, such as CaCO₃ dissolution in the water column and in sediments, were clarified and discussed in greater detail in the model description section. Additionally, the discussion section was expanded to better critically assess our results in comparison to previous studies and the discussion of omitted processes such as saturation state dependent CaCO₃ dissolution and the mineral ballasting were added to the text.

SPECIFIC COMMENTS:

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

The phrasing was revised to read “. . . a greater extent of both the surface oceans and waters at depth, particularly at high latitudes, will become undersaturated. . .”

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.

The direct impact of pH changes on ocean biota was added to the text and (Hinga, 2002) was cited.

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

We are aware that studies have suggested that the CO₂-calcification feedback is small. This is mentioned following the presentation of modelling studies that assess this feedback in the introduction with citations of Gangstø et al., 2011; Ridgwell et al., 2007b; Ridgwell et al., 2009 and Zondervan et al., 2001. However, despite this feedback being small, it still contributes uncertainty to climate model predictions and merits further study.

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

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Spelling corrected throughout the text.

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

This section was rewritten to focus on the uncertainty in model parameterizations of the CO₂-calcification feedback rather than on the species specific responses to ocean acidification. A detailed discussion of the biological responses to acidification is covered elsewhere (e.g. Doney et al., 2009 and Secretariat of the Convention on Biological Diversity, 2009) and is outside the scope of this paper.

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.

Yes, reduced nutrient supply in a warmer and more stratified ocean is included in the model (see Schmittner et al., 2005). The different primary production response seen in the Steinacher et al. (2010) study and that using the UVic Earth System Climate Model is now mentioned in the introduction and discussed in the discussion section.

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

This is explained in the model description section of the methodology and a detailed discussion of temperature-dependent phytoplankton growth rates has been added to the discussion section. Essentially, phytoplankton growth rates have been shown to increase exponentially with temperature (Eppley, 1972, Bissinger et al., 2008). Bacterial and zooplankton respiration rates also depend strongly on temperature (Kirchman & Rich, 1997, Pomeroy & Wiebe, 2001, Sarmiento et al., 2010, White et al., 1991). In the model maximum rates of phytoplankton growth, fast nutrient recycling, excretion rates of zooplankton, and remineralization of detritus are all assumed to depend exponen-

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tially on temperature (Schmittner et al., 2008). As CaCO₃ production in the model is parameterized as a fixed ratio of nondiazotrophic detrital production, CaCO₃ production rates tend to increase along with detrital production rates in response to increasing sea surface temperatures (Schmittner et al., 2008).

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

DIC is introduced in the first paragraph of the introduction. However, “taken up” was nevertheless changed to “sequestered”.

p. 11867, l. 8-13: The sentence starting with “This would lead. . .” is rather incomprehensible and needs to be re-phrased.

This entire section was re-written for the sake of clarity.

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

Spelling was corrected throughout the text.

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

Dissolution of CaCO₃ in the water column is calculated by assuming instantaneous sinking of the vertically integrated production. This was clarified in the text through the addition of the equation for the dissolution parameterization in the model description section. The dissolution rate varies as a function of this equation.

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.

Sediment CaCO₃ dissolution is simulated using a model of CaCO₃ dissolution kinetics coupled to the diffusion and pH reaction of carbonate species present in the sediment pore water (Archer, 1991; Archer, 1996; Emerson & Bender, 1981). As such, sediment

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CaCO₃ dissolution is dependent on the pH (and, hence, saturation state) of the overlying water. A decrease in deep ocean pH through the invasion of anthropogenic CO₂ would cause the dissolution of sedimentary CaCO₃ and add alkalinity to the ocean. This has been clarified in the text.

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.

The temperature effect on CaCO₃ production has been clarified in both the model description and discussion sections. Control runs S0 and M0 are runs where CaCO₃ production is independent of calcite saturation state. However, the temperature-effect on phytoplankton growth rates, and consequently primary production and calcification, is deeply integrated into the UVic ESCM and removing it would require significant restructuring of the UVic ESCM's ecosystem/biogeochemical component. As such, additional simulations with no temperature dependence are not currently possible.

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

Yes, the rain ratio varies by latitude due to differences in ocean temperature and biological production.

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.

This statement was not stating that observations were not crucial in constraining models but rather that the rain ratio observations were of limited use in constraining the model configurations in this study because the errors and differences between the observational estimates were typically larger than the differences between our different model configurations. The statement was rephrased for clarity.

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

The word "model" was replaced with the word "run".

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

The GLODAP observations from (Key et al., 2004) were used. This has been clarified in the text and figure caption.

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?

Model configurations with high sensitivity of CaCO₃ production to saturation state increase the modeled alkalinity gradients due to a greater difference in rain ratio between high and low latitudes. This increased difference in alkalinity removal efficiencies between high and low latitudes makes the agreement with observations worse. This has been clarified in the text.

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

Yes, alkalinity is conserved in the spin-up runs for all model configurations because alkalinity input from terrestrial weathering and alkalinity removal by sediment burial are kept in balance. In the transient runs, alkalinity ceases to be conserved because the terrestrial weathering rate is fixed at the preindustrial value and sediment CaCO₃ burial is allowed to fluctuate with changes in ocean chemistry and biological production. This has been clarified in the text.

p. 11874, l. 23: "an increase of _20%", over what time period?

Time period is 1800 – 2550 (750 years). This is now specified in the text.

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

The discussion section was expanded to discuss the changes in CaCO₃ production predicted by our model in comparison with the projections of other studies (e.g. Heinze, 2005, Gehlen et al., 2007, and Ridgwell et al., 2007).

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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.

Please see response to previous comment. It should be noted, however, that Ilyina et al. (2009) focused on changes in ocean alkalinity rather than CaCO₃ production. They did, however, report CaCO₃ production decreases in the tropical and subtropical oceans but our study focuses on global CaCO₃ production. As such our CaCO₃ production results were not compared to those of Ilyina et al. (2009).

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.

A definition of potential alkalinity was added to the text in the results section. As potential alkalinity is ocean alkalinity corrected for biological production and normalized for salinity, potential alkalinity is only affected by CaCO₃ production and dissolution. As such, it can be used to assess the relative importance of the temperature versus the acidification effect on ocean alkalinity. This has been clarified in the text.

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.

Dissolution of CaCO₃ sediments in the model are already dependent on pH (and, hence, saturation state). This was clarified in the text.

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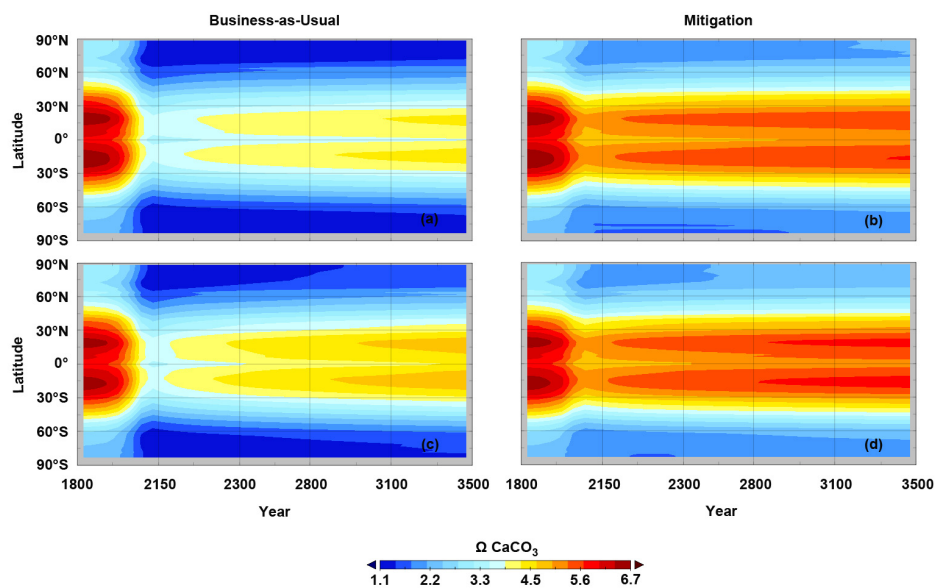


Fig. 1. Evolution of sea surface calcite saturation state over time as a function of latitude for (a) run S0, (b) run M0, (c) run S5, and (d) run M5 respectively.

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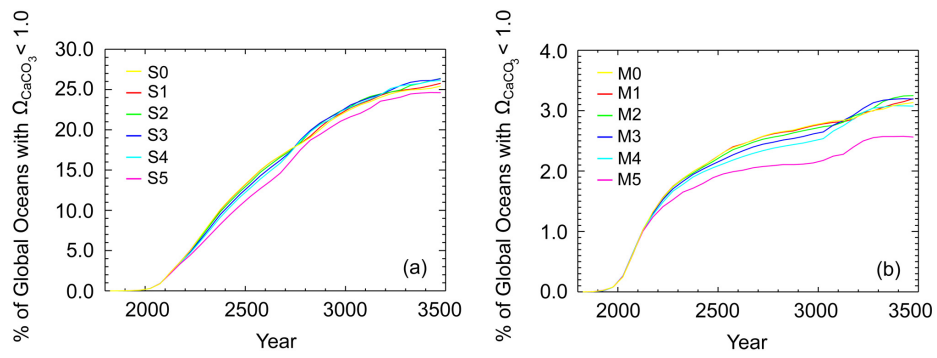


Fig. 2. Plot of the change in the percent of global oceans undersaturated with respect to calcite relative to preindustrial under (a) a “business-as-usual” CO2 emissions scenario (Suite S) and (b) under a “mi