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Biogeosciences Discussions

Interactive comment on "From laboratory manipulations to earth system models: predicting pelagic calcification and its consequences" *by* A. Ridgwell et al.

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* For the main points raised by the Referee – see answer to overarching points (above).

* The referee also provides some helpful suggestions for minor edits and corrections, which we have addressed as follows:

> Abstract, p 3456, l 13: We have been much more careful in the use of which carbonate chemistry properties are explicitly mentioned (if any) and also have stated the respective relevant time-scale ('century').

> P 3457, l 21, and p 3458 l 10-14: We have explicitly addressed the Referee's point

in the Introduction and now discuss the time-scales under which the future-relevant relationships between carbonate chemistry parameters hold (as well as elsewhere in response to some of their other points, plus one of Referee 3).

> P 3458, l 29 – p 3459, l 1: We have re-formulated this paragraph along the lines suggested.

> P 3459, l 6-7: We have re-worded the start of this paragraph to help avoid the potential for confusion that the Referee identifies. In addressing subsequent comments, the remainder of the paragraph has also been re-written. With regards to the suggestion for a more complete over-view of the global carbon cycle in terms of glacial-interglacial changes and carbonate compensation – this has been added in addressing comments by Referee #3 (but appears in the 'Conclusions & perspectives' section and in the context of carbonate chemistry manipulations rather than in the Introduction).

> P 3459, l 9-12: We have now expanded on how the calcification reaction is expressed (and in response to a comment by Referee 2).

> P 3459, l 13-17: Agreed. We have adjusted the text discussing the role of calcification in modifying surface ocean carbonate properties and relationship of reduced calcification to enhanced CO2 uptake from the atmosphere and added the suggested reference.

> P 3461, l 20 and Fig. 1: We have made the changes requested, and explained the nature of the quoted strength of carbon-climate feedback (i.e., as an ocean-only carbon cycle analysis, it excludes terrestrial biospheric response, hence it is lower than the Referee was expecting).

> P 3461, l 20-22: We have duly revised the discussion in this section to include mention of other differences existing between models and relevant to estimations made of the strength of the CO2-calcification feedback. The suggestion regarding normalizing to initial CaCO3 export is very helpful and well made. We have now extended Figure 1

to include this recommended normalization and discuss the resulting spread in model predictions in the text. (Basically: initial CaCO3 export production is important because the larger the initial CaCO3 export, the larger the pCO2 impact for the same relative decrease in calcification rate.)

> P 3461, l 25-26: We are grateful for the reminder of the relevance of the work by Iliyna et al. [2009] – we have now cited this work. We have also re-written this section and moved it to the 'Conclusions & perspectives section' where it is better suited.

> P 3464, l 21, and Figure 2: We have made more explicit link in the text plus new discussion (see reply to Referee 2, point #6).

> P 3465, l 6, and Figure 3: We have removed this figure entire, partly to address a concern of Referee 4, but generally to avoid possible confusion.

> P 3565, l 17, eq. 6: There is actually no mistake here – the confusion is that in Eppley [1972], the growth rate data is presented as doublings per day, whereas elsewhere (e.g., Bissinger et al. [2008]) the units are per day. Thus there is in fact no disparity in the equations. However, we have revised the manuscript to clear up this evident source of confusion. Regarding the Referee's second point in this context – although we did include brief discussion of the potential drawbacks of the Eppley curve and did cite the Bissinger et al. [2008] paper in the original manuscript, we have now expended on this issue. We also now cite and discuss Behrenfeld and Falkowski [1997], although not the Limnolog. Oceanogr. 43(7), 1479-1491 reference provided by the Referee (which actually contained little critical discussion of the Eppley curve) but instead the 42(1), 1-20 Behrenfeld and Falkowski paper. We have also added the reference to Brush et al. [2002], although the inferences drawn in this are basically those in Moisan et al. [2002] which we already cite and discuss. In general, we have extensively reorganized this section and added new discussion and extensive caveats re. analogies possible with the Eppley curve.

> P 3465, l 25: Correction made.

> P 3466, l 9-27: We have revised the section in question and substantially caveated the currently limited justification/support for an Eppley curve like calcification response as requested.

> P 3468, l 4-6: We have removed the more 'speculative' paragraph including the section that the Referee finds potentially misinterpretable.

> P3468 l 24 – p 3469 l 1: We have removed the 'recommendation' aspect of our analysis and instead discuss model strategies in much more general terms in 'Conclusions and perspectives' as requested.

> P 3469, l 10: Text description and citation corrections made.

* Technical comments:

> P 3468, l 17: Typo corrected.

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