

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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Received and published: 5 May 2009

General comments

I review this manuscript from the viewpoint of a biogeochemical climate modeller. I am not a biologist and therefore have to leave a part of the detailed evaluation to respective specialists.

The manuscript nicely summarises the ambiguous situation of marine biogeochemical modellers when it comes to the simulation of the pelagic biocalcification as a function of pH (or pCO₂ in seawater). Especially the Tables 1 and 2 are a useful compilation. Further, the thought model of a “unified calcification response curve” can provide a

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valuable framework or benchmark for further refinements of the calcification parameterisation in large scale biogeochemical ocean models. Also the perspective for reconciling the partly apparently contradictory results on pH dependent biocalcification will add to a constructive discussion of this issue.

On the other hand, the study provokes several questions:

Is the proposed average CO₂/pH-calcification curve really scientifically justified at this stage of knowledge, should it rather be the end point than the starting point of the currently ongoing collaborative research projects on ocean acidification such as the European EPOCA project and respective programmes in other continents?

Does it really help, if all modellers implement the same curve for pH dependent calcification in their models in the same way as many modellers – not all (!) – use the “Eppley” curve for temperature dependent phytoplankton growth?

Do we come closer to the “truth” in future predictions of the calcification feedback through a “unified calcification response curve”?

I have severe doubts, that we learn much from using such a curve. In the worst case, its use could lead to the impression that we can understand and quantify more than we are actually able to do. Therefore, I suggest that the authors caution potential model developers and users of such a “preliminary curve”. The authors describe, that to date only one single experiment describes a clear “calcification optimum” (the work by Langer et al., 2006). Further, also the “Eppley” curve for the T-growth relationship has underwent some modifications and critical appraisals over time. The authors must also provide a more detailed discussion on whether the Eppley curve is justified indeed.

The manuscript includes a wealth of information and as a “mini-review” of the current difficulty in simulating pH-dependent calcification behaviour in large scale models it does a decent job. On the other hand, one may argue, that also the original Eppley curve may not be as useful or representative as its widespread application suggests.

A critical consideration of this issue needs to be included in the article. I suggest to either reduce the weight of the “Eppley curve discussion” (section 4) by boiling it down to a few sentences stressing the uncertainties in such an approach or even delete it, or, alternatively, section 4 could be extended with (a) a more critical appraisal of the Eppley curve as such, (b) a discussion of respective CO₂/pH-calcification formulations with potential benefits (e.g. for model intercomparison studies where a unified could be helpful) or dangers (oversimplification, not really knowledge based at this stage).

Specific comments:

Abstract, p 3456, l 13: In principle the calcification response has to be considered with respect to two inorganic carbon cycle values simultaneously, as pCO₂ and pH generally cannot be replaced by each other (the marine inorganic carbon system is unambiguously determined, if 2 of its variables are fixed, such as Alk and DIC, or pH and HCO₃⁻ etc.). I know that the authors are well aware of this and many expert readers will take this automatically into account. Nevertheless it should be mentioned for the readers not so familiar with inorganic carbon cycling.

P 3457, l 21, and p 3458 l 10-14: Again, it is not 100% correct to use the dependency of calcification rates to CO₂ and pH in the same way. In principle a change in pH would be thinkable in the ocean without a change in CO₂ (though this is probably not a critical issue on the timescales considered here).

P 3458, l 29 – p 3459, l 1: I think that we are still far away from “reconciling experimental observations and making future predictions”. There is no proof available, at best some very selected evidence, that the proposed CO₂/pH-calcification curve may go into the correct direction. Future scenarios using such a curve will remain scenarios (or sensitivity experiments) and by no means predictions. I suggest to formulate this more carefully, perhaps simply by using “projections” instead of “predictions”.

P 3459, l 6-7: The statement that CaCO₃ affects the climate system may appear contradictory to the reader in view of abstract l 21-24, where it is stated the calcification

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feedback on century time scale is small. It may help to expand a bit on the role of CaCO₃ in regulating natural CO₂. Then, next to precipitation of CaCO₃ also dissolution should be mentioned, the glacial rain ratio as well as coral reef hypotheses, as well as the mechanism of CaCO₃ compensation as a stabilising factor in the system on long time scales.

P 3459, l 9-12: Eq. (1) is nice in the way that it illustrates the CO₂ as an end product of the CaCO₃ precipitation formally, but it is only one of possible net reactions on CaCO₃ formation. See for example Wolf-Gladrow et al., 2007, *Marine Chemistry* 106, 287–300, their equations (33) and (34) and the text thereafter. The text should be modified in order to not give the reader the impression that eq. (1) as such is always true.

P 3459, l 13-17: Considering an approximate steady state of global CaCO₃ production at the preindustrial, no changes in circulation and ocean biology, and an increase of atmospheric CO₂, then the CaCO₃ production would not act as a “brake for the transfer of fossil fuel CO₂ from the atmosphere to the ocean”. Also less outgassing is a sink. Only a *change* of calcification (not the “process of calcification” as such) would act as a modifier of atmospheric CO₂. And then fossil fuel CO₂ molecules cannot be discriminated with “preindustrial” CO₂ molecules. Consequently, the statement, that reducing calcification would accelerate fossil fuel CO₂ uptake from the atmosphere is neither right nor wrong – simply less CaCO₃ production causes an increase in ocean alkalinity and a lowering of surface ocean pCO₂ and hence atmospheric CO₂ (whether the CO₂ is preindustrial or comes from fossil fuel emissions is not detectable).

P 3461, l 20 and Fig. 1: Figure 1 would be strengthened through citing the annual emission rate (replace “current” by a specific year) more precisely and give a reference. The climate-CO₂ feedback given after Cao et al., 2009 (I could not find out where the value of around 38 PgC comes from by scanning through the paper) looks small as compared to the values given by Friedlingstein et al. (2006, *Journal of Climate*, 19, 3337-3353, see e.g. their Figure 2f), which seem to be 3-5 times higher.

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P 3461, I 20-22: The following could be mentioned here: The models do not only differ in the CO₂/pH-calcification feedback, but also regarding, the interaction with changes in circulation (specifically upwelling rates and stratification), changes and couplings to the Si cycle, and simply the strength of the CaCO₃ export production in the control scenario. One could possibly normalise the CO₂ feedback of the various models with the absolute baseline CaCO₃ production rates. This would nicely add to Figure 1 and the review-type summary given in the text.

P 3461, I 25-26: I agree, that more and improved experiments are needed on the CO₂/pH-calcification dependence, but the integrated real ocean response may be different and is much more difficult to detect. Determination of a large scale decrease in calcification from alkalinity field measurements is likely to be unambiguously detectable only after ca.30 years from now (see Iliyna et al., 2009, *Global Biogeochemical Cycles*, 23, GB1008, doi:10.1029/2008GB003278). Therefore, even with more experimental work we will be in the dark for a long time concerning the integrated effect of CO₂/pH on calcifiers.

P 3464, I 21, and Figure 2: The different pictures in Figure 2 need more explanation. As it stands neither figure caption, nor the text, nor the photographs themselves are revealing. What do you want to explain through Figure 2?

P 3465, I 6, and Figure 3: I would find Figure 3 easier to read if it would be duplicated (a) once for T-phytoplankton growth, and (b) for CO₂/pH-calcification. But this may well be a matter of taste.

P 3565, I 17, eq. 6: Equation (6) is here presented as “the” Eppley curve. In this form, the equation has been cited in different publications. It looks, as if also the authors here took this form of the equation as it approximately Figure 2 in Eppley (1972) and also the authors’ Figure 4 (which has been adopted probably from that Figure 2). However, if one takes Eppley’s equation in his (1972) paper, then the first factor would be 0.85 and not 0.59 (convert Eppley’s equation (1) $\log_{10} \mu = 0.0275 T -$

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0.070 into natural logarithm etc.). Other authors have modified the Eppley curve, e.g. Brush, M.J., et al., *Marine Ecology Progress Series*, 238, 31-45 (2002), or Bissinger, J.E. et al., *Limnol. Oceanogr.*, 53(2), 487-493 (2008). Also a fairly critical view of the Eppley curve, and a series of other T-growth relationships can be found in Behrenfeld and Falkowski, *Limnol. Oceanogr.* 43(7), 1479-1491. I suggest that the authors describe in much more detail the validity of the Eppley curve, different versions of it, and potential problems associated with it.

P 3465, I 25: Not all global models use the Eppley curve for T-growth. Replace “invariably utilize” by “is widely used” or something like this.

P 3466, I 9 -27: The authors describe that only one single experiment (Langer et al., 2006) supports the CO₂/pH-calcification their relationship. Though one can hypothesize of postulate such a relationship, it is at this stage very unsure on whether it will hold in future. There is a danger, that the suggested relationship will be taken over by many modellers, just because it would be “citable”, but at this stage, the relationship is not really confirmed to exist according to what I read. Therefore, many misleading modelling studies could result from an uncritical use of the relationship.

P 3468, I 4-6: The statement could be misinterpreted as it stands. Do we indeed have enough knowledge to make such a statement in view of the sparse experiments available?

P3468 I 24 – p 3469 I 1: The sentence summarises the Eppley type curve for CO₂/pH-calcification nicely and the authors are refreshingly open here: “*This leads us to a recommendation for the form of the parameterization to be used in future ocean carbon cycle modelling, although the observational data needed to constrain the steepness of the CO₂-calcification response and hence the parameter values in this equation are currently insufficient.*” This is exactly the point. Ocean acidification researchers worldwide are trying to find the specification of such a relationship. But the statement as such does not add to new knowledge and neither does seem the use of a not yet

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confirmed Eppley type CO₂/pH-calcification curve.

P 3469, I 10: The averaged emission value of $7.2 \pm 0.3 \text{ GtC yr}^{-1}$ is valid for the time span 2000 to 2005. For the reference, instead of IPCC (2007), the following should be used (see IPCC AR4): Denman, K.L., G. Brasseur, A. Chidthaisong, P. Ciais, P.M. Cox, R.E. Dickinson, D. Hauglustaine, C. Heinze, E. Holland, D. Jacob, U. Lohmann, S. Ramachandran, P.L. da Silva Dias, S.C. Wofsy and X. Zhang, 2007: Couplings Between Changes in the Climate System and Biogeochemistry. In: Climate Change 2007: *The Physical Science Basis. Contribution of Working Group I to the Fourth Assessment Report of the Intergovernmental Panel on Climate Change* [Solomon, S., D. Qin, M. Manning, Z. Chen, M. Marquis, K.B. Averyt, M. Tignor and H.L. Miller (eds.)]. Cambridge University Press, Cambridge, United Kingdom and New York, NY, USA.

Technical comments:

P 3468, I 17: Typo (should be “chemistry”).

Interactive comment on Biogeosciences Discuss., 6, 3455, 2009.