

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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General comments:

This paper addresses the response of pelagic calcification to changes in seawater pH from a modeling point of view. This is certainly a timely topic. The authors point towards the seemingly contradictory results yielded by manipulation experiments focusing on the calcification response of coccolithophores. By analogy to the temperature dependence of phytoplankton growth, a unified approach for net ecosystem calcification is proposed. This would provide a straightforward way to parameterize calcification in large scale ocean biogeochemical models.

C374

The temperature dependence of phytoplankton growth – the Eppley curve – results from the superposition of growth rate curves for individual phytoplankton species, all of which display an optimum curve with respect to temperature. The thought experiment relating optimum curves and initial conditions of manipulation experiments to the sign of change with respect to a given variable is clear and convincing. The problem with calcification and changes in carbonate chemistry (e.g. the saturation state) stems from the fact that today only one study has reported such an optimum curve and only for one of the coccolithophore species studied. This is fully acknowledged by the authors. An alternative approach would be to replace calcification in the context of whole organism physiology. We know already that the response of calcification in *Ehux* to changes in pH is modulated by nutrient stress and the light regime. A better fundamental understanding of calcification and its connection to photosynthesis in phytoplankton could ultimately provide a process parameterization based on physiological principles.

To conclude, the thought model build around the analogy between the Eppley curve describing phytoplankton growth as a function of temperature and net ecosystem calcification as a function of carbonate chemistry is valuable and interesting. It allows for a gradual response of calcifiers to changes in carbonate chemistry by mimicking a shift from vulnerable to more resistant species. It is at this stage however only a thought model, as the authors acknowledge themselves. In the past similar models transformed into hard grounded facts with time – and not with data – by the mere fact of citation. There is not much that we can about it, if only stressing that further experimental evidence is needed to validate or falsify the model.

Specific comments:

1. pH dependent calcification: this rises the question of the primary control on calcification and of the carbonate species utilized (carbonate or bicarbonate ion). Organisms are expected to calcify as long as the saturation state is favorable (supersaturation). The degree of saturation with respect to calcium carbonate is determined by the ion concentration product of calcium times carbonate. An increase in calcium concen-

C375

tration might compensate for a decrease in carbonate ion levels (and pH). I suggest replacing 'pH' or 'pCO₂' by 'saturation state'.

2. Equation (1): The net chemical reaction for creating carbonate minerals is a short-cut. The inorganic precipitation consumes carbonate ion. Please refer to Zeebe and Wolf-Gladrow (2001) for an improved description of reactions involved.

3. Line 1: "Ridgwell et al. (2007b) utilized established abiotic precipitation thermodynamics as the basis for their description of marine carbonate production" – please change to "abiotic precipitation kinetics".

4. Review of published modeling studies: the studies diverge with respect of the scenario selected for atmospheric CO₂ increase. In Gehlen et al (2007) for example atmospheric CO₂ increased from the pre-industrial level to 4 times that concentration over a time span of 140 years. This corresponds to an increase of 1% per year, nearly twice the rate typical for an IPCC business-as-usual emission scenario. It would be preferable to compare results of individual studies by selecting a common atmospheric target pCO₂, rather than at year 2100. Despite the discrepancies between model studies, the results converge, at least to my understanding. This is not surprising, because the underlying parameterizations rely on a common limited set of observations. Ridgwell et al. (2007) came to a similar conclusion: "Although other model differences such as saturation dependent water column dissolution of carbonates (Gehlen et al., 2007) prevent straight-forward inter-comparison, the spread across the 3 different models and associated parameterizations is no more than a factor of 2 in estimating the potential enhancement of CO₂ uptake".

5. p. 3461, L16: to my understanding, the studies agree in terms of order of magnitude of the calcification feedback and with respect to their basic conclusion that on a century time-scale the CO₂-calcification feedback is second order compared to the solubility pump and fossil fuel emissions. This is also a conclusion of the present paper (abstract, L21). While we can not exclude a future upward (or downward) correction of estimates

C376

of the CO₂-calcification feedback, this is unlikely to modify the main conclusion. The main challenge ahead is the quantification of impacts of future climate change and ocean acidification at the ecosystem level. We have to admit that neither of the models used during the studies quoted by Ridgwell et al. has the needed skills.

6. Section 3 presents a most valuable overview of published manipulation studies. Table 1 is most appreciated. The message behind Fig. 2 is not clear to me. Can you make the link between the individual pictures, variable calcification characteristics and environmental conditions (e.g. carbonate chemistry)? I agree that "we should regard *E. huxleyi* as a diverse assemblage of genotypes with highly variable calcification characteristics and ecological adaptations" (p. 3464, L21), but I don't see how this is underlined by Fig. 2

7. An 'Eppley Curve' for pelagic calcifiers: p. 3465, L25: please moderate the statement 'global models invariably utilize' by replacing invariably by 'widely'. In its present version the text gives the impression that the Eppley curve is commonly accepted as the most suitable model for representing the temperature dependence of phytoplankton growth. This is however not the case and the wide usage of the Eppley curve is matter of debate. Its validity for future projections in a warming ocean was for example recently questioned by Schmittner et al. (2008) (Future changes in climate, ocean circulation, ecosystems, and biogeochemical cycling simulated for a business-as-usual CO₂ emission scenario until year 4000 AD, *Global Biogeochem. Cycles*, 22, GB1013, doi:10.1029/2007GB002953). Ridgwell and colleagues should address the potential shortcomings of using the Eppley curve and present a summary of current discussions. Part of the discussion will touch on fundamental issues equally valid for the proposed calcification parameterization.

The reference Bissinger et al. (2008) is missing.

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C377