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Biogeosciences Discussions

Interactive comment on "Significant long-term increase of fossil fuel CO_2 uptake from reduced marine calcification" *by* A. Ridgwell et al.

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In my view, a periodical such as Biogeology Journal has a dual task: to be a communication medium for specialists and to provide information to a more generally interested audience. It therefore has to find an acceptable compromise between detail of information and readability. I myself have been involved in the biogeochemistry of pelagic calcification, but since my retirement I did not follow the literature in any detail. So, one may classify me with the interested audience, although with special interest. This means that I shall review the paper from that point of view, and also that other referees must be invited to comment on the nitty-gritty of modeling, etc.

I begin with a general comment on the content of the paper. As I see it, carbonate production compensates the steady inflow of bicarbonate and calcium into the oceans

by rivers and hydrothermal activity, so that on geological time scales the influx equals the outflux. *One first question would be if the rates of these flows are relevant over the timescale considered in this paper, and if the model should include this aspect*?

Secondly, the response of the ocean to the inflow is indirect, as physico-chemical precipitation is largely suppressed, so that virtually all carbonate production is biological. The result is a tremendous overproduction (I remember it is in the order of 5 times). Now, for the maintenance of chemical equilibrium the overproduced carbonate has to be redissolved. Much of this happens at the CCD, of course, but before this physical chemical mechanism can set in in deeper waters, there is large scale *biological* redissolution, by grazers, endoliths and so on, much of it in the oversaturated, photic zone. The CO2 feedback may be described in this context as a newly discovered mechanism whereby carbonate (over)production is suppressed even before the stuff can be redissolved. *The question then is, what difference this feedback makes to overall CO2 absorption?*

So, from my simple perspective, your feedback is like a storm in a glass of water. But maybe I am wrong. This is so nice about this journal: let's have it up for discussion!

Now about the style of the paper. Even if I don't agree with the content, it certainly is interesting and well-written, but for an amateur like myself the authors are insufficiently detached from their specialization (see above). The actual subject is a minor component of an extremely complex puzzle, and I recommend that the introduction is extended to include a brief overview of marine carbonate biogeochemistry and the specific problem addressed in this paper. For instance, it is only at the end of the paper that we hear about the ballast aspect for the organic pump, while the problem of how carbonate production may vary with the response of ecosystem composition to increasing atmospheric CO2 is only marginally addressed (e.g. in the Riebesell experiment). Furthermore, you have insufficiently distinguished the CO2-feedback this paper is about and carbonate dissolution (see above). Also, the emphasis is on the pelagic carbonates and it is not clear to what extent the important benthic carbonates

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Interactive Comment

are included in the modeling.

I have similar problems with section 2. Why do I have to wait for the end of the paper to find an explanation (hardly a definition) of η? Why aren't you a little more helpful with explaining k ('an overall scaling constant'). Why do you explain only one page later why Ω has to be subtracted by 1? What are the 54 members of p. 4? Why do you use the Ω for calcite while we have an aragonite ocean? In fact, the writing of the mathematics and modeling reads like a bit sloppy to me. I suggest that you go through the paper again and make it more user-friendly.

I hope to see this now in the discussion forum.

Peter Westbroek

Interactive comment on Biogeosciences Discuss., 3, 1763, 2006.