Biogeosciences Discuss., 7, C5145–C5148, 2011
www.biogeosciences-discuss.net/7/C5145/2011/


**Biogeosciences Discussions**

# *Interactive comment on* "Effects of $CO_2$-induced changes in seawater carbonate chemistry speciation on *Coccolithus braarudii*: a conceptual model of coccolithophorid sensitivities" *by* S. A. Krug et al.

**S. A. Krug et al.**

skrug@ifm-geomar.de

Received and published: 24 February 2011

1) The titel: Why "CO2-induced"? The method of manipulation was acid / base. Is the term "conceptual model" appropriate here? It sounds a bit over the top. The respective part of the discussion is, in my opinion, exactly that, a part of the discussion.

We adopted a new title: Effects of changes in carbonate chemistry speciation on Coccolithus braarudii: a discussion on coccolithophorid sensitivities

2) Page 8766, line 24: The strain resides in the Roscoff Culture Collection now. Please



provide the RCC-code and the correct URL.

done

3) Why was NSW used in the first experiment and ASW in the second?

Technical reasons: Unfortunately, no natural seawater was available for the second experiment

4 a) Page 8768, line 19-25: Why were cell densities determined by means of Coulter Counter in the first experiment and light microscopy in the second? Do the two methods yield comparable results?

Again technical reasons: Unfortunately the Coulter Counter was not available for the second experiment. Similar results of cellular PIC and POC production rates at the overlapping pCO2 range indicate direct comparability

4 b) Why was growth rate calculated from two datapoints as opposed to exponential regression including say four or more datapoints? The background of this question is the observation that initial cell densities in dilute batch cultures are often not very accurate and, moreover, the growth curve of C. braarudii might include a lag-phase.

Determinations of cell abundance are available only for the beginning and end of the experimental period. As the initial cell abundance was based on cell counts of the inoculum (with comparatively high cell densities), we are confident that the data are reliable. We agree with the referee that this approach ignores the possibility of a lag phase. However, independent incubations in our lab with daily cell counts did not reveal a significant lag phase for the approach applied here

5) Page 8771, line 15-21: Although I do not generally disagree with that paragraph, there are two distinctions which need to be made. First, precipitation rate does not equal calcification rate. It is unknown whether precipitation rate is the rate-limiting factor of calcification rate. Second, the change in intracellular pH reported in Suffrian et al. (2010) is, if my guesswork is correct (the manuscript is obviously not at my

disposal), a mixed signal of all compartments. Such a signal does not allow drawing conclusions concerning coccolith vesicle pH. Therefore it cannot be inferred that the latter also changed. These caveats should be included in the discussion. There is no need to reject the overall conclusion (line 21), though.

We agree with the referee. To avoid confusion between precipitation and calcification rate and because the approach taken here determines calcification rate, we only refer to calcification rate in the revised manuscript. Indeed, changes in the intracellular pH reported by Suffrian (2010) can be perceived as a mixed signal of all compartments. It is therefore unknown whether a change in cytosolic pH also affect pH in the coccolith vesicle. However, even in case that coccolith vesicle pH is unaffected, the energetic costs of calcification may still be affected as the costs of proton transport across the vesicle and cell membrane will depend on proton gradients.

We will discuss these issues in more detail in a revised version of the manuscript

6) Page 8775: In the first experiment there are a number of treatments characterized by undersaturation of seawater wrt calcite. Undersaturation can lead to coccolith dissolution, which would, in turn, lead to underestimation of calcite production. This is not discussed at all. Please add a paragraph dealing with that issue.

We will address this point in a revised version of the manuscript as: Calcification rates decreased with increasing pCO2 as previously observed in Emiliania huxleyi and Gephyrocapsa oceanica, facing lowest production rates at the onset of CaCO3 undersaturation (see figure 1, Omega<1). The assumed incipient calcite dissolution could not be determined. For this reason calcite production rates reflect the net calcification for Omega<1.

7) Page 8776: The plots in Figure 1 are far too small. Please make sure that this does not happen again in the BG version of the manuscript.

done

---



Interactive comment on Biogeosciences Discuss., 7, 8763, 2010.