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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.

Anonymous Referee #1

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The manuscript by Krug et al. represents an interesting piece of original research. The response with respect to organic and inorganic carbon production of C. braarudii to carbonate system perturbation is described. The dataset adds to a published dataset by broadening the range of CO2. The experiments were thoroughly performed and the manuscript is concisely written. The hypothesis put forth in chapter 4.1 is reasonable and well argued. And, what is more, it is, although not easily, testable. There are a few points / questions I would like to see considered. These are the following: 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. 2) Page 8766, line 24: The strain resides in the Roscoff Culture Collection now. Please provide the RCC-code and the correct URL. 3) Why was NSW used in the first experiment and ASW in the second? 4) 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? 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. 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. 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. 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.

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