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Biogeosciences Discussions

Interactive comment on "The combined impact of CO_2-dependent parameterisations of Redfield and Rain ratios on ocean carbonate saturation" *by* K. F. Kvale et al.

L. Cotrim da Cunha (Editor)

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I'd like to thank the authors for posting their reply to the reviewers quite rapidly. The subject of this manuscript is highly relevant to current ocean/climate research, and the three reviewers have commented on this. However, my point of view after reading the first version manuscript and reading the three reviewers' comments is that the manuscript needs major revisions, and should be accepted for publication in Biogeosciences after a second assessment of the referees. Two very important issues highlighted in this initial discussion are:

1) All reviewers pointed out the need of model validation.

2) The manuscript should assess the model limitations.

As much as I could read from the author's reply to the reviewers and from the revised version of the manuscript, all issues have been addressed, but I would still suggest the authors the following:

a) It would really improve the manuscript if in section 2 (Model and experimental design) you would add more detail on the ocean biogeochemistry component of Mk3L. There is no need for repeating the detailed description as in the Matear & Hirst paper (1), but more details on how export production, sinking, remineralisation, for instance, would be helpful for the reader;

b) Another suggestion would be to include a table in section 2.3 (experimental design) showing the main differences between the four Mk3L model configurations analysed in this manuscript;

c) A question: which model configuration would you consider as "background"? I suppose, according to its name, it is the one called CONTROL. So why not compare the GLODAP data also with the CONTROL run in Figure 1?

d) A last comment on figure 1: I strongly suggest improving the graphics and adding the parameter names (DIC, alkalinity, calcite saturation, and nitrate) to each one of the panels;

e) Figures 2 and 4 captions are very difficult to understand, unless one has very carefully read section 3.1 (PIC and POC surface export) and 3.2 (carbonate saturation). In a manuscript, the figures should be used as a support to the text, and not the opposite. I think section 3 (Transient Simulations) could be re-written and called "Discussion" – otherwise the reader goes from model evaluation (or validation?) to section 3 then to conclusions without having a "true discussion" of the model results.

f) Summary: I think your model study "suggests", but not "demonstrates" the sensitivity (or not) of the interior ocean carbonate saturation to changes in elemental ratios, especially because later in the section you state this is a sensitivity study. Could you also add a sub-section where you explicitly assess the model limitations in this manuscript?

1. R. J. Matear, A. C. Hirst, Long-term changes in dissolved oxygen concentrations in the ocean caused by protracted global warming. Global Biogeochem. Cycles 17, (2003).

Interactive comment on Biogeosciences Discuss., 8, 6265, 2011.