Biogeosciences Discuss.,
doi:10.5194/bg-2016-405-RC2, 2016

Biogeosciences
Discussions

Interactive comment on "A data-model synthesis to explain variability in calcification observed during a CO_2 perturbation mesocosm experiment" *by* Shubham Krishna and Markus Schartau

Anonymous Referee #2

Received and published: 21 November 2016

In this paper, a mathematical model is applied to a dataset consisting of 9 mesocosm experiments performed to elucidate the impact of CO2 enrichment on ecosystem development. It is shown that independent from the CO2 enrichment, the main distinction between the 9 mesocosms is in the degree in which there is calcification. Whereas this was already known from the literature, the model shows this to be mainly caused by the initial, physiological conditions of the coccoliths. These factors, not under experimental control, mask the possible impact of CO2 concentrations on ecosystem development, which was the original aim of the experimental study. In addition, the model results suggest that in the high CO2 scenario, there is impact of CO2 on the ecosystem to an extent that is not included by the model. As the model describes the effects of CO2

only on the relative amount of PIC produced, the deviance should be caused by other factors, yet unknown.

This is the second paper that uses a model on these data, but the emphasis of the current manuscript is different, the model used is also different, and has shown its worth in previous modelling studies.

Apart from the inevitable typo and small errors, I found the manuscript well written. The model equations are described at great length, and a rigorous data assimilation approach is taken, making the modelling results credible.

However, in its current state it is much too long which obscures the main messages. Due to the overkill of figures, tables, and text, it is also not simple make a good review – thus I decided to give mainly comments on how I would restructure the paper. Detailed comments can then be given on a second -shorter- version of the paper.

(1) I miss the ecological implications of the findings that have surfaced thanks to the model. For instance, the conclusions should not simply rephrase the modeling aspects, but discuss the ecological implications of the modeling study, or the consequences for future setups of such mesocosm studies.

(2) as a way to reduce the paper in size, I would remove all figures starting from Fig. 11 – and also significantly reduce (or remove) the corresponding text.

(3) I would also remove the lengthy CNfact discussion (and the corresponding figures 6 and 7, which are difficult to interpret).

In conclusion, I would suggest this manuscript to be acceptable but only after a strong reduction in size.

Some detailed comments.

P6 Line 24; symbol theta used but not explained.

Equation 13: I would have expected to see zooplankton excretion here.

p. 13-14 I do not understand how the three simulated mesocosms can be used to estimate data variances (Ri) for DIC, as this differs for each mesocosm?

P 15 L2-3. As far as I understand, the proposal distribution is adapted by the AMH algorithm Thus, the Hessian is used only as the initial proposal.

P1 L18; I do not understand what is meant with "optimal" mass flux estimates, as there are two completely different optimalisations: one for the data assimilation (DA), one for the optimal resource allocation in phytoplankton

Figures:

Fig. 2. Why is the Chl content not added here?

Fig. 3-4. Cumulative plots are difficult to interpret. I would prefer to see the actual probability distributions instead. Also, the figs 3 and 4 could be combined, if the three calcification scenarios for each parameter would be put in the same figure rather than in 3 of them.

Fig 8-10. The main difference is in between low and high calcification scenario – Fig 9. Could be removed.

Appendices: A7: the uptake rates are in mol N/mol C/day and not in mol N/molC. Similar for A8.

Table A1 – mean irradiance in W/m2/d -> should be W/m2

Interactive comment on Biogeosciences Discuss., doi:10.5194/bg-2016-405, 2016.