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Biogeosciences
Discussions

Interactive comment on "A mathematical modelling of bloom of the coccolithophore *Emiliania huxleyi* in a mesocosm experiment" *by* P. Joassin et al.

Anonymous Referee #4

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General comments

A model is presented that is used to simulate the development of E. huxleyi blooms in a mesocosm experiment. The experiment in question (published in Delille et al. 2005 and Engel et al. 2005) is interesting because the dynamics of coccolithophores are so poorly understood, and because the data set includes various biogeochemical measurements of interest including TEP and viral lysis. In general, I believe that the onward march towards ever increasing complexity in biogeochemical models is best served by setting up models for specific scenarios with associated high quality data, as is the case here.

I do however have serious reservations about the work, most importantly: (1) Modelling is about using simulation to help provide insight into the dynamics of systems, not merely reproducing observations; this study leans much too much toward the latter. The last two concluding sentences of the abstract exemplify this: "The model represented carbon, nitrogen, and phosphorus fluxes observed in the mesocosms. Modelled profiles of algal biomass and final concentrations of DIC are in agreement with experimental observations." So what? Readers will want to learn about biogeochemistry, rather than simply being told that a model matches observations. The main text of the manuscript is largely in the same vein. (2) The model includes only a single phytoplankton state variable, for E. huxleyi, and no state variables for grazers. Is this really an acceptable representation of the ecosystem in the mesocosms? I doubt it (for details, see below). Of course, just because an apparently good fit with data was achieved, this does not in itself justify the chosen model structure. Many model parameters were tuned, and I would have been surprised if a reasonable fit had not been achieved. (3) The grammar is highly variable, being generally poor throughout (and exceptionally poor in the early sections). Even the title is grammatically incorrect, e.g. it should be something like: "Mathematical modelling of a bloom of the coccolithophore Emiliania huxleyi in a mesocosm experiment".

Specific comments

Regarding the conclusions reached by the authors, these are not entirely absent. The do note that E. huxleyi production and extracellular release are critical processes. Not entirely surprising, given that the ecosystem model focuses solely on E. huxleyi. On p. 809 there is: "An accurate representation of the calcification ... is obtained when formulating this process as a function of algal primary production rather than biomass as usually formulated in other models." Did the authors try a formulation based on biomass, and show that this does not work? And how confident are they in their result given that so many parameters were fitted? Finally, many modellers assume that the production of $CaCO_3$ is a fixed fraction of primary production, and so I think that their statement is inaccurate anyway - I do not see their approach as being novel. The Discussion as a whole is just a ramble, rather than focusing on what we can learn from the model bout the biogeochemical cycling associated with E. huxleyi (in a general context). The fact that there are very few cited references in this section only serves to emphasise the fact that it fails to adequately put the findings of this work in context of biogeochemical cycling of the topic as a whole, along with its associated literature.

On page 792 there is: "The development of phytoplankton groups other than Emiliania huxleyi only occurred in the beginning of the experiment in some mesocosms but had minor impact on nutrients consumption (Delille et al., 2005; Engel et al., 2005). Therefore the model will ignore the reproduction of groups other than Emiliania huxleyi" [note that I am making not attempt to improve incorrect grammar in the quotes]. In Delille et al there is: "A succession of distinct phytoplankton assemblages took place in the course of the experiment. The assemblage was first dominated by Synechococcus sp and nanoflagellates ...". If one looks carefully at the data, it seems that there is very little activity in the bags associated with E. huxleyi before day 10, production being dominated by other phytoplankton. Yet the authors choose to model the first 10 days using a model that includes only E. huxleyi. This cannot be justified. If the authors must pursue their approach of a single phytoplankton state variable as E. huxleyi, then they should initialise the model for day 10 of the experiment and run from there. The results prior to that day are entirely irrelevant to this study.

Regarding the role of zooplankton, there is (page 792): "The grazing of phytoplankton cells by zooplankton was negligible in comparison to the export of phytoplankton cells due to sedimentation. Consequently, the model does not include any grazing term." I find this statement and assumption unjustified. If one looks at Table 1 of Engel et al., where there is a complete list of the measurements undertaken, there is no mention of zooplankton or grazing. So on what basis are the authors making their bold statement? Surely micrograzers were present in the mesocosms? Indeed, in Engel et al. there is: "Particle concentrations during the course of the blooms was likely affected by ...

secondary processes such as grazing or particle sinking". Grazing is such an important process in marine systems, and I can see no justification whatsoever for leaving it out of the model.

Given that a major overhaul is required, including restructuring the model to include zooplankton, as well as a complete overhaul of the text, I am not going to go through the text and make minor comments. These major concerns need to be addressed first. However there is one other thing that struck me. Why is there no comparison of modelled primary production with data? This is standard practice for modelling studies. The data are for example presented in Figure 2 of Delille et al. (2005). So let's see how the model bears up in comparison.

Interactive comment on Biogeosciences Discuss., 5, 787, 2008.