Interactive comment on "Carbon fixation prediction during a bloom of *Emiliania huxleyi* is highly sensitive to the assumed regulation mechanism" *by* O. Bernard et al.

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The paper has been deeply revisited in order to address the points raised by Referee 2.

In this paper the authors used a model to test different scenarios concerning the regulation of calcification and photosynthesis in Emilliania huxleyi. They considered either carbon dioxide, bicarbonate, carbonate or the calcite saturation state to be a regulating factor. They run the models at three different concentrations of atmospheric carbon dioxide and compared the carbon dynamics, including export within and between the models. The qualitative results of this paper directly follow

from DIC speciation chemistry: if production is made a function of CO_2 or HCO_3 then it increases with atmospheric CO_2 and if a function of CO_2 or Ω it decreases. Since sedimentation rate is constant in this model it follows production.

This is not so simple because the coccolith dissolution and export rate terms are CO_2 dependant. However, we indeed assume that, for the short considered period (20 days), sedimentation rate is constant.

The quantitative results of this paper are of no scientific interest, because they are not based on measured rates, do not include competition among phytoplankton groups and include no mechanisms that might have made the results depart from those predicted from DIC speciation.

We disagree with these statements: this model focuses on a bloom of E.huxleyi. It assumes that during the bloom (20 days), Emiliania is dominating the phytoplanktonic communities and predation is done at constant rate, which is a reasonable hypothesis at this time scale. The second phase (decline of the bloom), due to predation, leads to most of organic and inorganic carbon export. These points have been made clearer in this new version. We recall that it is, to our knowledge, the first model dynamically coupling DIC speciation and biological models (through pCO_2 and alkalinity changes) for photosynthesis and calcification. Finally, our models have been calibrated using data and parameter values from other measurement based modeling works.

Detailed comments

Proofreading by a native speaker would be highly desirable. Many sentences are difficult to understand, but on close reading suggest they were written to make foregone conclusions sound new.

English has been reread.

"Model simulations suggested that only the models where carbonate ion regulates calcification could reproduce the decrease in calcification rate after a pCO_2 doubling, hence refuting the general assumption of a regulation by HCO_3. Ǎň" This is a misleading statement, as the ongoing discussion in the literature is not at all concerned with carbonate chemistry – everybody accepts that carbonate decreases with an increase in pCO_2 AT CONSTANT ALKALINITY- but some experiments show an increase in calcification and others a decrease under these or similar circumstances (constant DIC). Thus, the issue is not whether calcification decreases with increase CO_2 once you've set calcification to respond to carbonate, but whether the latter is the case or not.

We are not completely sure to have understood the point raised by the reviewer: it is clear that alkalinity plays a key role to understand the complex behavior of the coupled coccolithophorid-carbonate systems. Since alkalinity is not constant because of calcification, we agree that it must, of course, be considered, and this is what we DID in our modeling (it plays a KEY role). The mentioned sentence was referring to the work of Bernard et al. (2008). In this study, the analysis which was performed took into account the alkalinity variation, and actually the statement was proven from a generic mathematical analysis, independent of parameter values.

Pg. 5341 and 5342: Paragraph more or less copied from Bernard et al. 2008

This part has been made more synthetic, but we wanted to keep the paper self-contained to make the reading easier.

Eq. 4 would be understandable if it were multiplied by α

If this equation is multiplied by α, then it is no more normalized by the photosynthesis

rate μ.

From Eq. 16 to 20, an approximation of the DIC speciation is presented, and then it is stated that this approximation wasn't used. Delete this section.

This has been made more synthetic. Since the Matlab code of Zeebe and Wolf-Gladrow (2003) had to be modified to account for the Ca^{++} variations, it was however important to provide details. It is now clearer.

The text at the end of 3.1 and Figure 1 suggest that there is no sinking POC, while Eq. 23 and Table 4 say there is.

It was indeed ambiguous. It has now been corrected.

The four models are calibrated to give the same behaviour at pCO_2=380 ppm and are then run at a lower pCO_2. Predictably, the four reference runs at present CO_2 then behave completely differently, so that it becomes very difficult to see what the relative effect of a pCO_2 doubling is.

As explained in the paper, one of the reasons for this is that pCO_2 is not at all constant in the water, especially during a bloom, and especially since the coccolithophorids bloom takes place after the diatom bloom which exhausted the inorganic carbon. This is now better explained and discussed in the discussion section.

By using growth rate to calibrate the model, the temporal dynamics of the four reference runs are very different.

What is important is that these models all predict the measured inorganic uptake for standard experiments with nowadays pCO_2. This is why the parameter $\bar{\mu}$ was used to tune the models in order to ensure the same measured baseline. We recall that $\bar{\mu}$ is

NOT the maximal growth rate (this is a Droop model parameter from which maximal growth rate can be deduced), and our calibration procedure ensures that all the model predict the same (measured) growth rate in standard seawater. As a consequence "growth rate (IS NOT USED) to calibrate the model ".This is now better explained in the paper.

Amongst others, this means that the peak in N:C occurs at different times, with repercussions for the carbon dynamics and the conclusions.

The exhaustion of the inorganic nitrogen appears indeed at different time (earlier for the models which are stimulated by the considered conditions). However, it always appears within the 20 considered days, and this has therefore no striking effect. *I very much doubt that the large differences that they find in the reference runs are robust. E.g., what would have been the results if they had used α or some other model variable to calibrate the model?*

In this new version of the paper, a Monte Carlo approach has been developed: the parameters are randomly taken from a distribution centered on the nominal parameter values. These 9000 simulations allow assessing the model accuracy and sensitivity to the set of parameters. This extended work confirms that the regulating variable has an influence comparable with pCO_2 effect.

Pg. 5356 line 9 and 10: There is no two-fold difference in the HCO_3 model run

Indeed, we referred to PIC and POC. The discussion has been extended, and this point is now clear.

Please also note the supplement to this comment:

C3705

http://www.biogeosciences-discuss.net/6/C3701/2009/bgd-6-C3701-2009-supplement.pdf

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