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Biogeosciences Discussions

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.

O. Bernard et al.

olivier.bernard@inria.fr

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The authors are grateful to referee 1 for his detailed review and his interesting and helpful comments which highly contributed to improve the manuscript quality.

In this paper, the authors derive a 1-D model to study the impact of various photosynthetic and calcifying regulating mechanisms on carbon export during an Emiliana huxleyi bloom. The main conclusion of the paper is that better constraining the inorganic carbon species controlling photosynthesis and calcification is important in predicting the change in export flux of coccolithophorids with an increase in CO2. Assumptions about the regulating mechanism (CO2, HCO3-, or) lead to variations in carbon fluxes of

the same order of magnitude as a doubling of CO2. The authors argue that the model presented "highlights a phenomenon that will take place in more detailed models". The export model, as acknowledged by the authors, is based on poorly understood processes. This leads the reader to question whether an inadequately constrained 1-D model is sufficient or even necessary to reach this conclusion?

First we want to emphasize our original modeling approach. We did not limit our work, as it is generally the case, to design a model, and analyze its simulation result. This classical modeling path would have lead to a more classical (and easier) publication. Our goal was really to represent as much as possible the scientific questioning in the model, and thus, as e.g. for the IPCC models, use a set of models to assess our prediction capability from the state of the art.

As a consequence, an outcome of this work is that the sensitivity of the model response to pCO2 is lower than that to other mechanisms and parameter uncertainty. It is a result by itself, which was not predictable a priori, and the prediction of the range of uncertainty is, to our point of view, a new and key result.

Finally, we do not agree that the proposed models can be described by "an inadequately constrained 1-D model". We did our best to show that the proposed models, which are the first ones to couple biology and carbonate system kinetics, have a higher predicting capacity than the other models quoted in the paper. However, we made it clearer that the model we designed is valid only during the bloom period, and in that sense it is simpler than existing modeling embedded in ecosystem models. However, this is, to our knowledge, the first model which couples the biological dynamics including growth and calcification and the inorganic carbon dynamics. We do believe that the coccolithophorids dynamics is, during the short bloom period, realistically represented.

These aspects are now emphasized in the discussion.

In the discussion, the authors quote Riebesell (2004) that "it seems impossible at this point to provide any reliable forecast of large-scale and long-term biological responses

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to global environmental change". Is the modeling exercise presented in this paper necessary to demonstrate that a better understanding of the factors regulating photosynthesis and calcification is important in predicting the response of coccolithophorids to increasing CO2?

Once again, our conclusion is much stronger in the sense that our models quantify the prediction sensitivity to the underlying regulating factors. Without such an approach it is not possible to a priori predict which phenomenon will show the strongest impact on predictions.

The most significant conclusion of the manuscript, that the assumption of the regulating mechanism leads to variations in carbon fluxes of the same order as a doubling of CO2 needs to be further evaluated with a sensitivity analysis. How is this conclusion influenced by the model's assumptions?

In the new version of this paper, we have included a sensitivity analysis to model parameters. Now our predictions result of 9000 Monte Carlo simulations from parameters following gaussian distributions. This approach contributes to even better assess the prediction uncertainties which are consequences of the scientific questioning from to date state of the art.

For example, in the mesocosm experiments of Riebesell et al. (2007), the carbon to nitrogen uptake, DOC production, and TEP production increase under high CO2, which would favor carbon export (TEP enhances aggregation). How would this mechanism influence the conclusions that regulating mechanism is as important as a doubling of CO2? How reliable is the assumption that the export (sedimentation) is a first-order kinetic function of the POC concentration in the mixed layer (equation 28)? As POC increases, wouldn't aggregation increase (non linear function)?

First, the use of the Droop model allows us to better represent the PN/POC ratio and its effect on growth. The export computation is based on the modeling of De La Rocha and Pahlow (2006, 2007) and Ridgwell et al. (2007). Of course these models could

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be improved, and the suggestions are interesting. But since they are not supported by enough quantitative measurements we prefer to stick to existing works.

Discussion section is anemic. Model results are presented but not discussed. For example, why does the model assuming regulation by predict higher carbon fluxes? Why is the model regulated by enhanced by the depletion in DIC? Is it because the bloom leads to an increase in pH, favoring CO32-, and therefore carbon flux? If so, is a 1-D model necessary to predict such behavior? If not, what is the mechanism?

We did our best to improve the discussion section. In fact, the answer to these questions was already in the paper, but in the result section. These points are now better presented and discussed.

Furthermore, a significant proportion of the modeling component of this study is a reiteration of Bernard et al. (2008). In fact, some sentences and paragraphs, in the introduction and elsewhere, are copied verbatim (e.g. paragraph 25 of section 5340, "Since the pioneer works: : :"). The authors do not need to repeat the derivation of Bernard et al. (2008) and can simply refer to derivations and present the final equations. Because some of the equations are identical to the ones presented in Bernard et al. (2008), it is unclear which components of the model presented in this paper are innovative.

In the first version of the paper we wanted the paper to be self-contained, this is why a fraction of the biological model explanation was repeated (it was however simplified). In this new version, we have minimized the duplications from Bernard et al. (2008). We made clearer that the contribution is not the biological model (except maybe for the regulation by the calcite saturation state), but its embedding in a mixed layer modeling in order to simulate a bloom of E.hux. This part has been rewritten so that the new modeling part (the paper in Ecol.Modelling was dealing with chemostat experiments!) appears now much more clearly.

After a comparison of both papers, one can start to decipher the contribution of this pa-

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per: 1) as a regulating mechanism: the results are for all intent and purposes identical to the model with CO32-, presented in Bernard et al. (2008). This result is predictable. This paper is on carbon flux from the surface ocean. Ca2+ concentration in the ocean is unlikely to be affected by a bloom. 2) Incorporation of equations derived in Bernard into a 1-D model of POC and PIC export from the surface ocean. Equations 21-27 of this manuscript are identical to equations 30-34 of Bernard et al. (2008) with the addition of: a. CaCO3 dissolution term for PIC and DIC pools b. Sedimentation term (which is the 1-D model addition to the previous model) for the POC and PIC pools c. Respiration term for the POC and DIC pools. d. In the 1-D model, growth is now a function of the light intensity in the mixed layer. All 3 new rates are simple first-order kinetic functions of their respective pools. Some of the equations derived seem to lead to cul-de-sac, i.e., they are not being used later in the manuscript. For the most part, section 2.2. is copied from Bernard et al (2008). For example, the authors derive equations 15-20 for an approximation of CO2, but later decide to use the Matlab scripts of Zeebe and Wolf-Gladrow (2003) to calculate the exact (i.e. not approximated) CO2 concentration. Some of the terms, such as v(r) are not defined at all. The reader must go to Bernard et al. (2008) to figure out what v(r) is. Same observation for equations 12-14: it is unclear why lambda is derived.

In addition to the previous answer, we think it is important to present the basics of the carbonate system. This is an innovative aspect from a modeling point of view;; so far we are not aware of any in situ model representing the dynamics of biology coupled with the dynamics of the carbonate system. To do this we had to slightly modify the scripts of Zeebe and Wolf-Gladrow (2003) (and we need the lambda term). Now this is better explained, and much more synthetic.

Nomenclature throughout the manuscript is poorly defined, or not defined at all. Terms should be defined within the manuscript, and units should also be included in parentheses, when they are first presented in addition to the table. For example, it is not clear from r that it is normalized to the carbon pool until one refers to table 2.

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We have improved the nomenclature throughout the paper. However, it is NOT the standard in modeling works to give units for all the parameters (otherwise it becomes difficult to read).

Q is defined as "the internal nitrogen quota". Based on equation 22, it seems that the authors mean "internal inorganic nitrogen quota". Is this correct?

We did not deeply recall the Droop modeling since it is rather classical. The (classical) definition of the quota Q is "the internal nitrogen quota" and NOT "internal inorganic nitrogen quota".

By convention, rate constants should not be capitalized (equilibrium constants are capitalized). The gas transfer coefficient "KL" should not be capitalized. Same is true for exchange rate constant through thermocline and the sedimentation rate constant.

To our knowledge (see the quoted works) there is no such official convention, and practice can be highly variable depending on the communities. We tried to follow the reviewer recommendation, but then the notations turn out to become different from Bernard et al. 2008, which may disturb the reader.

Minor comments: Abstract The conclusions of the study need to be clarified in the abstract: - "Indeed recent experiments, performed under nitrogen limitation, : : :". Which experiments are the authors alluding to? If Riebesell et al. (2000) vs. Iglesias-Rodriguez et al. 2008), weren't experiments performed in a batch exponential growth? Regarding the controversy, the authors should also cite the more recent exchange in Science between the 2 groups.

Of course this was a confusing sentence. Abstract has been rewritten. More references have been cited.

- "We designed models to account for various scenarii of calcification and photosynthesis regulation in chemostat cultures of : : :". Please clarify that these models for chemostat cultures were derived in a previous study.

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It was indeed very ambiguous, and it is now clear.

- The last 3 sentences of the abstract are unclear: "models assuming a regulation by CO32- or predicted much higher carbon fluxes: : : models controlled by CO2 or HCO3- led to increased carbon fluxes". How is this different from the model?

It has been clarified

5341: "It is only recently that CCM, implying intra or extracellular carbonic anhydrase enzymes,: : :". CCM imply more than carbonic anhydrase activity. Active transporters of CO2 and HCO3- have been identified.

This aspect is better discussed.

Line 16: what are the 12 models? Please elaborate.

More details are now given.

5342: line 9: "regulating the inorganic carbon uptake." Regulating the inorganic carbon uptake of photosynthesis and calcification?

This has been modified.

5343: r(.) the meaning of "(.)" was unclear until reading Bernard et al. 2008. Please clarify. Identify new terms in equation 5, with units.

To avoid confusion, these more rigorous notations were given up .

5344: "and there is consensus to admit that CO2 would be the substrate for photosynthesis while HCO3- would be the substrate for calcification". Show citations. Also, could the CO2 released during calcification be a substrate for photosynthesis? How would this mechanism influence the conclusions of the study?

Citations have been added. Actually, this is what automatically happens in the model: when a mole of inorganic carbon is consumed (whatever the species), the chemical equilibria are displaced. Thereby, because of the alkalinity consumption, it leads to

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more available CO2, which may stimulate (or not) the corresponding model. As a consequence, the uncertainty is displaced towards the regulating factor nature. This is now a point in the discussion

5345: define more clearly kQ, the subsistence quota. Why is "k" used? It is not a rate or a half-saturation constant? Why not call it Qsubs, which would be more intuitive?

This is classical notion based on Droop model. The notation is also quite standard. We however used another notation to better fit the BGS community.

k2 needs to be defined.

Done

Is the ratio X/N (POC/PON, or C/N) influenced by growth rate and light intensity? It is well established that the C/N ratio is a function of growth rate and light levels. Is this taken into account in the model, as this will influenced the behavior of the various models?

This is exactly what the Droop model does: growth rate is a function of C/N (=1/Q) ratio.

10, p. 5346: "The total alkalinity (TA) is defined by (see : : :)". It is not defined but approximated by equation 13.

OK

5347: equation 17: Shouldn't the right hand side of the equation be divided by 2? Line 11 r = D/[CA , remove "[".

This part has been deleted

5349: "whose concentration are, respectively, S1,0, S2,0, and D0". Should read "whose concentration are, respectively, S1,0, D0, and S2,0".

Thank you for remarking this mistake.

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5350: Where Kd : : : is the exchange rate : : : and Ksed is the sedimentation rate. Kd and Ksed are not rates, but rate constants.

You are right, but this abuse of language is very common in most of the modeling papers. Looking through the literature, we could not find papers mentioning “maximum growth rate constant” or “mortality rate constant”. We therefore did not modify this point, to avoid confusion.

5352: isn’t equation 29 the average PIC flux and not the Total PIC flux?

Yes, thank you.

International standard unit for salinity is unitless.

OK, but it is not ambiguous with the unit.

Line 19: “: : :during the declining phase (h1=0.1)”. Shouldn’t it be h2 instead of h1?

Yes, thanks.

“dt” in the integrals should be “dt”. By convention, the greek letter "tau" is for residence time.

The classical convention is that tau is the variable in the integral. Anyway, this has been changed to be more easily understood by the BGS community.

5353: rm is not defined.

This is now done, thank you.

5354: line 7: DIC is not presented in Figure 2. Shouldn’t it be Figure 4? Last line of paragraph: Shouldn’t it be Figure 2 instead of Figure 4 this time?

You were right, sorry for this confusion.

5355: Line 9: “As indicated by the coefficient of variation: : :”. Coefficient of variation is the standard deviation normalized to the mean for comparison of populations with

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different means. Why not simply compare the concentrations to figure out the impact of a doubling of CO2 on concentrations?

This was a misleading statement which has been corrected

Line 9: ": : : all models predict a two-fold difference in the final concentrations: : :" Which concentrations? The HCO3- model does not show a two-fold change.

This has been more extensively (and clearly) discussed.

Line 13: "(see the 45

Corrected

Table 2 is barely readable. Increase the size of the fonts in the table.

Table 2 has been made for the final BGS format, this is why in the BGS-discussion form it turns out to be very small, we tried to improve this.

Figures 2 to 7 should be combined into 1 large multi-panel figure, or at least 2 multi-panel figures for easier comparison of results at 380 and 760 ppm CO2. Show steadys-tate concentrations in the graph. Also show PIC/POC and POC/PON ratios over the 20 days.

New graphs are now presented, to more clearly compare the simulations within different pCO2.

Please also note the supplement to this comment: http://www.biogeosciences-discuss.net/6/C3670/2009/bgd-6-C3670-2009-supplement.pdf

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