

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

Anonymous Referee #1

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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 Ω) 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

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constrained 1-D model is sufficient or even necessary to reach this conclusion? 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 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?

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? 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)?

Discussion section is anemic. Model results are presented but not discussed. For example, why does the model assuming regulation by Ω predict higher carbon fluxes? Why is the model regulated by Ω 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?

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

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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. After a comparison of both papers, one can start to decipher the contribution of this paper:

1) Ω as a regulating mechanism: the results are for all intent and purposes identical to the model with CO_3^{2-} , presented in Bernard et al. (2008). This result is predictable. This paper is on carbon flux from the surface ocean. Ca^{2+} 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. CaCO_3 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 CO_2 , but later decide to use the Matlab scripts of Zeebe and Wolf-Gladrow (2003) to calculate the exact (i.e. not approximated) CO_2 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 λ is derived.

Nomenclature throughout the manuscript is poorly defined, or not defined at all. Terms

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

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.

- “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.

- The last 3 sentences of the abstract are unclear: “models assuming a regulation by CO_3^{2-} or Ω predicted much higher carbon fluxes. . . models controlled by CO_2 or HCO_3^- led to increased carbon fluxes”. How is this different from the Ω model?

5341: “It is only recently that CCM, implying intra or extracellular carbonic anhydrase enzymes, . . .”. CCM imply more than carbonic anhydrase activity. Active transporters of CO_2 and HCO_3^- have been identified.

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

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

5343: $r(.)$ the meaning of “(.)” was unclear until reading Bernard et al. 2008. Please clarify.

Identify new terms in equation 5, with units.

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?

5345: define more clearly k_Q , the subsistence quota. Why is “k” used? It is not a rate or a half-saturation constant? Why not call it Q_{subs} , which would be more intuitive?

k_2 needs to be defined.

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 influence the behavior of the various models?

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

5347: equation 17: Shouldn't the right hand side of the equation be divided by 2?

Line 11 $r = D/[CA$, remove “[

5349: “whose concentration are, respectively, S_{1,0}, S_{2,0}, and D₀”. Should read “whose concentration are, respectively, S_{1,0}, D₀, and S_{2,0}”.

5350: Where K_d ... is the exchange rate ... and K_{sed} is the sedimentation rate. K_d and K_{sed} are not rates, but rate constants.

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

International standard unit for salinity is unitless.

Line 19: “. . .during the declining phase ($h_1=0.1$)”. Shouldn't it be h_2 instead of h_1 ?

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

5353: r_m is not defined.

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?

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

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.

Line 13: “(see the 45% PIC drop in the CO₃²⁻ model)”. Should read Ω model.

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

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 steady-state concentrations in the graph. Also show PIC/POC and POC/PON ratios over the 20 days.

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