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Interactive comment on "The combined impact of CO_2-dependent parameterisations of Redfield and Rain ratios on ocean carbonate saturation" *by* K. F. Kvale et al.

Anonymous Referee #3

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General comments:

This paper discusses the results of a sensitivity study, where POC and PIC export are synchronously modified using pCO2-sensitive parameterisations. It confirms previous findings by Boudreau et al. 2010, that the ocean interior will respond strongly to such changes in biological processes, whereas the surface ocean will not. While I think the general idea of the paper is interesting, I have some reservation about the paper's form and methodology, and about its publication in Biogeosciences. In its present form, the paper is merely discussing the results of a 'computer game' rather than giving insight about potential changes in ocean biogeochemistry, and there is not much new when compared to Boudreau et al. (2010). Hence, perhaps this paper is better suited to being published elsewhere in a lower impact journal. The model is not validated, parameterisations are used that are unsuitable for global use, and only two key processes are modified, whereas the there are many others likely to be affected by increased ocean pCO2. Hence, I cannot recommend publication in Biogeosciences unless the paper undergoes major revisions.

Specific comments:

A - Model validation:

In the present version of the paper, we are not presented any evidence that would make us believe that the model results are realistic, even before the modifications on the export parameterisations are carried out. What is the model's annual mean primary production, how well does the export compare with current estimates and how well does it simulate calcification in the different ocean basins? Furthermore, can the authors convince us that their nutrient patterns make sense, and do they tell us how their results are linked to inaccuracies in the simulations of the before mentioned quantities? How well is the carbonate saturation simulated, when some components of the carbonate system are compared to e.g. GloDAP DIC and alkalinity? I don't think the reader can make any sense of the relative changes observed between control simulations and the different sensitivity tests, unless s/he is provided with a general sense on how good this model is performing.

B – Use of parameterisations:

I have strong reservations about the use of Riebesell et al. (2007) for a global study like this. The bizarre (and unjustified) equation (5) was probably derived from the result of one mesocosm study with unadapted organisms in a community dominated by diatoms, a high latitude community which lived in a Norwegian fjord, so not even in an open ocean environment. How can the authors just take this parameterisation without even cautioning the reader against its use, except for saying ' that we are dealing with an incomplete sampling of biodiversity'. In fact, I think that using this stoichiometry for tropical regions, where much of the deviation from the control run is found, does not make sense, since diatom-dominated communities are uncommon at these latitudes, except for the coastal regions, hence the findings in Riebesell et al. (2007) are not applicable to these waters. A proper snythesis of all observational an laboratory evidence on the effects of ocean acidification on stiochiometry whould have been necessary, taking also into account naturally acidic systems, such as in the Med Sea.

C – Model suitability

Since this model only includes a very limited amount of biology, I ask myself whether or not it is really suited to study the differential impact of modifications in so-called biological processes. Given that we do not know anything about the model tracers after a very poor model description (and validation), we are left to doubt whether or not there even is some degree of biological realism. Many global ocean ecosystem models now include complex nutrient dynamics with completely decoupled nutrient dynamics, but this model does not seem to include any of this complexity. Furthermore, the second trophic levels (zooplankton) are also likely to be affected by changes in ocean pH, and they calcify, too and generate most POC. No information is given on zooplankton here, and neither is its role discussed in the conclusion section (e.g. Gangsto et al. 2009). Without any detailed information on the model tracers, this study turns into a mere computer game, as I mentioned above, and we may as well read the box model study by Boudreau et al. 2010. Furthermore, this study does not only lack complexity in the simulation of the model ecosystem. Since this model does not even include a proper particle representation with aggregation processes etc. (the mesocosm experiment cited in this study also found significant changes in TEP production), I strongly question whether or not changes in export can really be discussed using this model. However, this is nothing the authors can do anything about. However, they can thoroughly revise their model description.

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D – Documentation of purpose of study, inclusion of relevant references

This paper fails to give a background on the purpose of the study, and fails to include many important references on calcification, the study of physiological changes in ocean biota under sea water acidification and other climate-related impacts on marine ecosystems. It totally fails to discuss the impact of changes in ocean temperature on particle export and the remineralisation loop, which are very likely to modulate the projected changes in this model. The manuscript remains unacceptably vague when literature is cited or important findings are discussed, and does not include a discussion of other effects (impact of ocean acidification on the N cycle, for example, changes in stratification and nutrient availability). The authors mention that they only want to show that biogeochemical feedbacks are important, but again then the entire study turns into a computer game, and we can as well read Boudreau et al. (2010).

Minor comments:

Abstract:

Rewrite entirely, as vague and we cannot judge what you find, since you do not mention here what kind of parameterisation you have used.

“This non-linear effect has..” explain why?

“linear and non-linear effects” Where do these come from?

Introduction:

First paragraph: Be clear and always indicate the direction of observed change, e.g. avoir “vary” “altering” and “adjustments” and give direction of the feedback.

“availability of carbonate”: Not true that equation (2) tells you about the “availability”. This is simply an equilibrium equation.

P 6267, L12: “reduction...” cite appropriate reference.

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P6267, L 16: “which could impact...” How?

P6267, L19: “truncated nutrient”.. explain.

P 6267, L 21: “long term” What is long term?

P 6267, L 23: “ In the studies mentioned above” Which studies? And why the low latitudes? Be precise.

P 6269, L1: Riebesell et al. 2007, This study worked with “shocked“, i.e. unadapted communities of coastal organisms. How can you justify it’s use for the global, open ocean and for century-scale change?

Equation (5): Define those numbers you use there. Where do they come from (2, 700)? Did they once have units – “pCO2” should be given as “pCO2/[uatm]”, since I assume F should be unitless.

P 6269, L 4: “the same scaling” How is this justified?

P 6269, L19: Spell Gangsto correctly.

P6269, L21 ff: “does not account for particle aggregation....” How does this impact your results? Come back to this in your Conclusions, detail the limitations of your model there.

P6270: What about the potential impact of temperature changes on POC/PIC export? Discuss this somewhere!

Results:

General: Expand more on this “linear” versus “non-linear” effect you see, and why this difference is important to you?Does the “linear” case mean that the POC effect dominates over the “PIC” effect, and vice versa for the “non linear case”? If so, discuss why, what and where. Furthermore, give the relative size of “linear” and “non-linear” effect (and clean up Summary and conclusion, so that the reader knows why this distinction is important to you).

P6272: “striking pattern..” not so striking to me, since the color scale is difficult to see.

Fig. 3: Changes scales is plots. We can harldy see the effects.

P 6273: “Tropical regions...” there I don’t think Riebesell et al. (2007) is applicable, since the communities are dominated by (non-calcifying) picophytoplankton, which were not dominant in the mesocosm study. Reflect on this in your manuscript.

Summary and conclusions:

P6274: What about temperature effects, see above.

P6274: What about the effect of ocean acidification on calcifying organisms and their distribution, and on the nitrogen cycle? How will this influence your export effects?

P 6274: “Insensitivity of PIC export to Omega...” why is this? Do you believe it? If so, justify.

Interactive comment on Biogeosciences Discuss., 8, 6265, 2011.