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Biogeosciences Discussions

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Interactive Comment

Interactive comment on "Challenges and opportunities to reduce uncertainty in projections of future atmospheric CO_2: a combined marine and terrestrial biosphere perspective." *by* D. Dalmonech et al.

Anonymous Referee #2

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This review is rather unusual and thus new in the sense that it covers both ocean biogeochemical models and land biosphere models. To my knowledge, this is the first review which tries to review the way the whole carbon system is modeled in Earth System Models. The problem is that while being a strength, it is also a weakness of the paper. Everything is being adressed in the paper whithout a clear message or a clear focus. Thus for each component taken individually, the review is rather incomplete or even weak. Many aspects have been covered in previous more specialized reviews (at least for the ocean). The authors should have tried to restrict the review to some very specific aspects of the carbon cycle models which are critical both in the marine and land systems. Primary Productivity could be a good candidate or the degradation of organic matter.

The other problem is that this paper adresses too many questions: the uncertainties in the ocean models, in the land models, in the data, in the way models are compared to the data, and so on ... Too many topics are reviewed and at the end, none are correctly and properly reviewed leaving the impression that the paper is only listing a long long long series of problems and issues. This looks more like a shopping list, and most of the items in this shopping list have been already identified and discussed in previous papers (for the ocean, cf Hood et al., 2006, doney et al., 2009, doney et al., 2004, Allen and Polimene 2011, etc ...).

Finally, to conclude on this general comment, I would say that by trying to adress too many aspects, the paper is quite confused. It's hard to find a clear message and clear recommendations. Thus, my advice would be to identify a limited list of specific points and concentrate on that list. Since the originality of the paper is to cover both the ocean and the land, these points should be of importance for both systems.

Specific comments:

Most of my specific comments are on the ocean component. Being an ocean biogeochemist, I don't have a sufficient expertise to properly evaluate the land part of the review.

The whole section 2.1 is quite weak and even exhibits mistakes.

Export production is not only due to the sedimentation of dead biomass. The sinking particles are a mixture of dead biomass, faecal pellets, aggregates including coagulated colloidal organic materials and living cells, etc ... A significant part of the export is also due to the subduction of dissolved organic matter. Finally, it is also clear that vertical diurnal migrations by zooplankton and mucronekton can contribute to up to a

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quarter of total export of organic carbon to the interior of the ocean.

Line 20, page 2088 : marine production is limited also by iron, and potentially by other micro-nutrients as well.

line 7, page 2089: this sentence is not very clear.

For the discussion on the temperature effect, I agree that a major unknown is the acclimation and/or adaptation of the marine organisms to the increase in temperature. Currently, all models basically assume that PP is increasing with temperature and that the ecosystem instantaneously perfectly adapts to the changing conditions which is far from being demonstrated by the lab experiments and field experiments. Making a different assumptions in models (for instance, a limit to the plasticity of the organisms or a limited connectivity between the different provinces) would certainly lead to different predictions. I'm not aware of any studies that tried to do that (even if Pahlow et al., 2005 tried something on that topic).

However, there are also many other unknowns related to the temperature effect such as the bacterial production, the fate of the organic matter, etc ...

The discussion on the Nitrogen cycle is extremely weak and too short. The change in oxygen is a topic by itself as there is no clear consensus on the future evolution of O2 in the ocean (see Bopp et al., 2013). This point is definitely a major uncertainty in the ocean part of the ESM projections. And the O2 content is not only important for the nitrogen cycle but also for the fate of the organic matter, the ecosystem structures, etc ... And all the feedbacks between the oxygen content and the carbon cycle, biological activity, ... are far fom being clearly understood.

Line 5, page 2091: this statement is quite obvious in the way it is written.

Last paragraph of section 2.2: some missing pieces of information in table 1 are available in the literature. For instance, TOPAZ and PISCES both use the Q10 formulation for temperature effect on phytoplankton growth.

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Section 4.2.2

The list fo datasets is rather incomplete. Ocean biogeochemical modelers very commonly use other datasets such as the World Ocean Atlas, sediment traps datasets, GLODAP atlases, and so on to evaluate or constrain the models. Furthermore, the authors mention in the table PP but they discuss in the text chlorophyll. For me, chlorophyll from satellite is a much more interesting and robust product than PP. PP includes not only the uncertainties related to chlorophyll estimations but also the uncertainties of the algorithm used to compute PP (see for instance the PPAR papers on that topic).

Section 6.3

The section on the compensating errors is extremely important I think and could have be a much more important topic in the paper. I would say this is really a key issue in modeling. And this question is critical for the model construction, the model evaluation, the model-data comparison, the selection of the parameter values, etc ... This is mentionned several times in the paper. For instance, the paper by Friedrichs et al (2009) quoted in section 2.2 is a good example of a study which covers that topic for ocean biogeochemical models. One problem is that many datasets used to evaluate the models give informations on the stocks, biomasses but not on the rates or the uncertainties are so large on the rate estimates that basically, theses estimates represent only very constraints. Table 4 for the ocean but also the land biosphere is a good example of that problem. And the stocks can very often be reproduced with completely different solutions for the rates (several papers quoted in this study illustrate that issue). Thus, this point critical according to me and would have merited a larger discussion.

Interactive comment on Biogeosciences Discuss., 11, 2083, 2014.