Title: Challenges and Opportunities to reduce uncertainty in projections of future atmospheric CO2: a combined marine and terrestrial biosphere perspective.
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Reply to RC2

In bold key comments of the reviewer are reported. The reply follows.

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.

We appreciate that the reviewer understands the vastness of the effort to put together this review.

The problem is that while being a strength, it is also a weakness of the paper. Everything is being addressed in the paper without a clear message or a clear focus. 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. 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.

We realize that given the ground covered in the manuscript, we had to be short on many specific aspects. We accept that the necessary brevity of some of the ground covered meant that we could not do fully justice to each individual topic. We could remove some aspects of the first two sections as e.g. the population dynamics in both marine and terrestrial ecosystems. However, one of the aim of the first sections was to report key processes in ocean and land that

are shown, via modeling experiments, to be crucial in modeling the net carbon fluxes between biosphere and atmosphere and their projections, and it is not straight forward to reduce the list of important processes to few that are common (and important) in both systems.
Beyond the sections about process uncertainties, the other sections of the manuscript cover aspects that are important for both terrestrial and marine systems, e.g. uncertainties in satellite datasets, metrics, model dependencies and how these affect the applications of methodologies for constraining future projections.
While we do believe that our review raises a couple of important points typically missed in current benchmarking activities, the difficulty in rewriting the review such that it covers the really important points for both marine and terrestrial system in a length that is still feasible for a journal paper and not a book has led us to the conclusion to not pursue a revision of this manuscript.

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.

We agree with the reviewer that the ms could be reorganized focusing on compensating errors. Yet, the manuscript was not meant to address issues related specifically to model development

and evaluation and, as the reviewer noted, the issue of compensating errors is mentioned several times within the several sections.

Clearly, a better model can lead to better predictions. However there is the need to achieve a better interpretation/use of the models spread in the projections of atmospheric CO_2 and get most of the benefit offered by current, although imperfect, models. In this context we found it more valuable to focus on current limits and uncertainties to the process of constrain/reduction of the spread of atmospheric CO_2, considering the available metrics, data and methodologies.

The data problem, as underlined by the reviewer, is also related to what we can measure and what we can model, and the correspondence between what is measured and what is modeled (one key example is soil respiration) rather than stock vs rates only. This topic would deserve more investigation, but in a different manuscript.