Biogeosciences Discuss.,
doi:10.5194/bg-2016-289-AC3, 2016

Biogeosciences Discussions

Interactive comment on "Ideas and Perspectives: Climate-Relevant Marine Biologically-Driven Mechanisms in Earth System Models" *by* Inga Hense et al.

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Received and published: 6 October 2016

We thank the reviewer for his/her constructive and helpful comments. Below, please find our point-by-point response (in blue color).

- I can understand the underlying rationale of their selection and I agree with the proposed mechanisms, although I would stress the fact that the processes that are being listed under each mechanism may not be exhaustive.

  We agree that the processes listed under each mechanism are not complete. Depending on the level of detail in the description of the mechanisms, the number

  of processes varies. We hope that we found a consistent way to include the most contributing processes for each mechanism.

- Another point of relevance is the role played by coastal processes. The authors seem to suggest that coastal parametrizations of nutrient supply may be sufficient to capture the most relevant aspects of the identified feedback-related mechanisms (this is for instance implemented in PISCES, both for macro and micro-nutrients). However, the next generation of ESMs is moving towards higher resolutions, which would imply a better resolution of coastal processes and benthic-pelagic interactions. This would probably enhance the vertical supply in coastal regions, requiring a continuous readjustment of the parametrizations. I wonder if this is what the authors are suggesting as the way forward.

  We completely agree that other biologically-driven mechanisms may become important, too, if for instance the spatial model resolution is increased. However, we are confident that the general framework with *classes of mechanisms* will persist even if it turns out that more *individual mechanisms* need to be included. We will address this issue in a revised version. Concerning technical issues, we have no easy solution to that, since they are not our focus. One approach could be to adjust the paramterizations to the respective resolution.

- ": without going into ..." This is a main explanation for the choice of the processes. I think it would deserve some more words, as for instance some feedbacks may not be known in general or not completely known to the authors.

  It is unclear to us what the reviewer means but we will rephrase this sentence to make sure that our statement is understood.

- The initial sentence implies that the functional group approach may not be

adequate for the description of the system. I cannot see the authors finding any substantial counter-argument against this approach in the rest of the manuscript. On the contrary, it seems like they are even suggesting a more lumped approach, as for instance with the use of bulk zooplankton and phytoplankton.

It was not our attempt to criticize that PFTs are introduced. We only regret that new PFTs are included based on their role in biogeochemical cycles, specifically, the carbon cycle alone. We will rephrase this part to make sure that this is not misunderstood.

- The role of dissolved organic matter in the biogeochemical pump has been thoroughly demonstrated in the field (Hansell et al., 2009, 2012; Kim et al., 2011) and in ESMs (Patara et al., 2012; Letscher et al., 2015). Particulate organic is only one pathway of carbon cycling, and likely to be mostly reduced in the anticipated more stratified conditions of a high CO2 ocean.

We assume that the reviewer is referring to the paragraph where we describe the classical biological carbon pump and sinking of organic matter. We agree that the *microbial carbon pump* is another way to sequester carbon over long time periods. However, to the best of our knowledge, there is no evaluation of the relevance of this pump for contemporary climate change. Even a large pool, like the marine RDOM, will have little impact on the climate system on time scales of some hundreds of years unless an imbalance between sources and sinks evolves. However, there are no indications or estimates for that, yet. We thus decided to only discuss this aspect in a revised version of the manuscript without proposing this mechanism to include into ESMs.

- There are actually two forms of calcium carbonate with different dissolution rates used by different organisms. If the emphasis is on alkalinity, then both forms of

CaCO3 (and the relative biological pathways) should be considered or at least indicated why neglected.

We fully agree that calcite and aragonite and the key organisms involved in the alkalinity dynamics need to be distinguished *if* the focus lies on the marine carbon cycle, as also pointed out in the reply to reviewer #1. Among the calcifiers, coccolithophores, however, are the most important group and mainly responsible for the vertical gradient in alkalinity. Other calcifying organism groups have been shown to be regionally important or are indeed assumed to be highly relevant for aragonite but only marginally for climate dynamics. From a climate perspective, the gain to represent calcifiers by more than one key group might be relatively small unless regional ESMs are applied; we are not aware of any study showing the added value with respect to climate relevance. Most importantly, the vertical alkalinity gradient needs to be generated; the carbonate chemistry should be represented in ESMs. With one additional key group, the calcifiers, represented by coccolithophores these basic features of the alkalinity pump will be achieved. We will extend the discussion to clarify this issue in a revised version of the manuscript.

- There are some inaccuracies in this table. The authors are invited to check the work by Laufkotter et al. (2015, their Table 1 and 2) to get a complete overview of the various processes implemented in the current generation of ESMs. In particular, TOPAZ has parametrized zooplankton and PELAGOS is the only model with separate bacterioplankton and dynamic DOM cycling (not included in zooplankton). PELAGOS was used in CMIP5 in the CMCC-CESM model.

Apparently, our table is not very clear. For PELAGOS, we lumped bacterio- and zooplankton together to avoid introducing an extra column - that is why we added a footnote. We have again checked all the entries and better arranged them to

include a revised version of the table.

- It is a bit dismissive to state that the regions of low oxygen concentration are confined to the shelf seas. It has been demonstrated that the open-ocean oxygen minimum zones (e.g. Stramma et al., 2008) are expanding and the biogeochemical cycles of these gases may be enhanced in a warmer ocean (Wright et al., 2012).

  We will completely revise this part and we will separately discuss the role of the marine biota in N2O-production and other greenhouse gases.

- The inclusion of cyanobacteria would imply the inclusion of another pathway in the nitrogen cycle because these organisms are N-fixers. This should be clearly indicated and the possible interactions with M2 should be provided.

  That is true, also other organism groups are involved in mechanisms relevant for the Earth system which indirectly may affect again the climate. But here we clearly want to concentrate on first order effects, otherwise it is inconsistent with the other mechanisms or the list of things that need to be considered will become too long. We will explain our focus more clearly in a revised version.

- I would tend to disagree that the buoyancy status is one major discriminant in underwater light attenuation by phytoplankton. The current representation of light absorption in ESM is rather crude and since most of the models listed in Table 1 have now a variable Chl:C ratio, it may be necessary to consider the specific attenuation of chl in various functional groups as this may affect the size of the oligotrophic regions of the subtropical gyres.

  A variable C:Chl-ratio is certainly a big step but insufficient keeping in mind that surface buoyant cyanobacteria (and other surface buoyant organisms) can build

up condensed surface mats in the upper 1-2 m with consequences for the heat distribution. This feature cannot be reproduced by only taking into account a variable C:Chl-ratio.

Interactive comment on Biogeosciences Discuss., doi:10.5194/bg-2016-289, 2016.