

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

**Inga Hense et al.**

inga.hense@uni-hamburg.de

Received and published: 7 September 2016

We thank this Reviewer for his/her comments – we are reassured now that it is necessary and high time to stimulate a discussion about a more biological perspective on Earth System modeling. Below, please find our point-by-point response ([in blue color](#)).

- The present draft tries to cover too much and ultimately achieves little. It is concerned mainly with special pleading for more consideration of a few processes in which the authors have invested time and developed expertise, but does not make a strong case for why these processes, and not others, should be given more attention by the ESM community.

In our manuscript we clearly state that we are interested in the **biologically**-driven mechanisms that are relevant for the climate and involve feedbacks among the different Earth system components. We provide a general framework for including the most important of these mechanisms in ESMs. This framework can persist even if new biological climate-relevant mechanisms are discovered. We substantiate our choice of mechanisms based on previous studies.

- The English is poor. If at all possible the authors should enlist a colleague entirely fluent in English (preferably a native speaker) to help refine the MS before resubmission. In such a commentary (vs a primary scientific contribution) attention to the details of language is particularly important. (Note also that terms like 'albedo' and 'radiative forcing' are used in a naive fashion. The latter in particular is often contentious and needs to be used (if at all) in a manner consistent with its usage in the existing literature.)

We fully agree that a clear language is important in scientific communication. However, the reviewer does not specify where clarity is missing in the text. Concerning the terms "albedo" or "radiative forcing", we are well aware that these two terms are used differently in the different communities or studies (see e.g. Chung and Soden, Environ. Res. Lett 10(7) 2015). Yet, the reviewer does not elaborate in which way we use these two terms in a "naive fashion".

- The paper considers the need both for additional biological processes and more focus on the coastal zones, but does not make a strong case for either. Ocean circulation is taken for granted and the technical challenges of resolving the nearshore in global- scale models are not considered. The spatial resolution required to resolve ocean circulation e.g. in the North Sea is such that running models at global scale with this resolution is simply not possible. Nesting, down-

[Printer-friendly version](#)[Discussion paper](#)

scaling and adaptive grids are all approaches that can be used in complementary ways to fill information gaps, but there is no discussion in this commentary of the literature on these topics. Embedding in existing global scale circulation models models of biological processes that we know to be important in coastal zones achieves nothing (garbage in - garbage out).

Again this comment is very vague. We would like to stress that it is not our goal to discuss general deficiencies in ESMs like circulation patterns but rather to look at the representation of climate-relevant mechanisms induced by the marine biology. Consequently, we do not aim at reviewing or summarizing all issues related to the representation of coastal zones in global models and the approaches to overcome them. Instead, we present our view on future needs of ESMs with respect to the representation of marine biology relevant in climate simulations. Also, it is not our goal to address any technical issues.

- I am reminded of the commentary of Prof. Myles Allen in Nature 425: 242 (2003), who stated that the “challenge of probabilistic - or risk-based - climate forecasting is to start saying what changes can be ruled out as unlikely, rather than simply ruled in as possible”. The current contribution is not concerned with such forecasts, but I find this statement relevant and instructive. The manuscript offers up a shopping list of ocean biogeochemical processes that might be important for climate (ruled in as possible) but lacks clear direction in discussing which ones the authors think should be given priority. Their criticism of existing practice has a 'straw man' quality to it, e.g., on 1/13-15. Who exactly articulated such a strategy?

It is unclear to which part of the manuscript the reviewer is referring to with this comment and where on page 1 (l. 13-15) we criticize a strategy that has not been articulated; we also agree with the statement by Allen (2003) but don't see the

[Printer-friendly version](#)[Discussion paper](#)

point. First of all, we provide a general framework with classes of mechanisms (not processes - and we clearly distinguish processes and mechanisms, see p.1, line 21ff. and p.2, line 20ff.) – this framework is not a shopping list. Second, we consider **biogeochemical and biogeophysical** and not only biogeochemical mechanisms (we clearly distinguish these 2 types, p.4, line 33ff.). Please also note, that this is a manuscript submitted to the “Ideas and Perspectives” category, where personal opinions are not a reason for rejection.

- What are the criteria for a process to be considered globally important? In this essay there is no discussion of this question that could reasonably be described as quantitative. Processes may be locally important but average to zero globally; nonlinear rectification effects may be demonstrable but still second order at global scale.

We agree that in general there are aspects that are locally or regionally important but turn out to be less important on a global scale. Yet, in our manuscript, first of all we focus on climate-relevant and not necessarily globally important aspects. Second, we describe for each class of mechanisms in which way the climate system is affected and provide quantitative information. Unfortunately, the climate effects of the different mechanisms cannot be directly compared. This would require to implement the mechanisms and to carry out systematic model runs. However, it is out of the scope of this study since we aim at *communicating our view* on which biologically driven mechanisms are climate-relevant and need to be captured in ESMs.

- The choices for prioritization are unconvincing. N<sub>2</sub>O, for example, is more or less dismissed out of hand. The reasons given for dismissing it are incorrect (e.g., Dore et al. 1998 Nature 396: 63; Lueker et al 2003 10.1029/2002GL016615), and the literature that shows that it may be an important climate feedback (e.g., Jin and Gruber 2003 10.1029/2003GL018458) is not considered. DMS on the

[Printer-friendly version](#)[Discussion paper](#)

other hand is given pride of place as an important climatic driver, and more attention from Earth System Modellers is recommended, but recent literature suggesting that it is actually a second order effect (e.g., Quinn and Bates 2011 10.1038/nature10580) is ignored. The emphasis on the biophysical effects of changing ocean viscosity is quite perplexing. It may be true that this is an important climate driver that has been neglected. Or, more likely, it may prove to be an interesting (if rather esoteric) subject for research, but of negligible importance for climate. These authors make no effort to explain why they think it should be prioritized relative to the dozens of other possibilities.

First, we provide references of specific articles to emphasize the importance of the biogeophysical aspects and kindly ask the reviewer to read them. Second, we would be very interested in a few examples for “the dozens of other possibilities”. Third, we would like to stress that the framework, including the three classes of mechanisms, will persist even if new mechanisms are discovered or turn out to be climate-relevant. Fourth, concerning N<sub>2</sub>O, there is ongoing discussion about the climate relevance of marine sources. We admit that such discussions also have taken place with respect to DMS. To be consistent, we will include N<sub>2</sub>O in more detail in a revised version of the manuscript.

- One thing the authors could do is make a table of all of the processes they discuss, and rank their importance in terms of future model development by criteria that are clearly stated and applied consistently. This might lead them towards crafting a sound and credible contribution.

It is not our goal to rank the classes of mechanisms. We are more interested in providing new ideas and stimulate the discussion about the role of the marine biota in the climate system.

[Printer-friendly version](#)[Discussion paper](#)

[Printer-friendly version](#)

[Discussion paper](#)

