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Discussions

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

Anonymous Referee #3

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I am sorry to say that I do not think the present contribution is suitable for inclusion in this special issue. It reads like a rough first draft of an "Ideas and Perspectives" paper, and needs a substantial rethink. Doing this will benefit the authors themselves as well as the broader Earth System Modelling community, but will take longer than is likely to be available for this issue.

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.

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.)

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, downscaling 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).

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?

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.

The choices for prioritization are unconvincing. N2O, 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 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.

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.

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