Geoscientific

Model Development

Discussions

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comment

Interactive comment on "BFM17 v1.0: Reduced-Order Biogeochemical Flux Model for Upper Ocean Biophysical Simulations" *by* Katherine M. Smith et al.

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Response to Reviewer 1

We appreciate the constructive and insightful comments from the reviewer. A number of changes have been made to the text at the reviewer's request and we feel that the revised paper is now substantially clearer and stronger as a result. Detailed replies to each of the reviewer's points (in italics) are provided below.

This manuscript concerns the development of a biogeochemical model of open-ocean ecosystem dynamics called BFM17. The authors make the case that the model is

"complex and flexible enough to capture open-ocean ecosystem dynamics, but reduced enough to incorporate into highly resolved numerical simulations with limited additional computational cost." Furthermore, they provide validation of the model in 0D and 1D mode with measured data from an oligotrophic open ocean station. In general, I agree with the claims of the authors. My view is that reduced complexity models such as this one are important for process studies, so I welcome the initiative of the authors to develop one more option for biogeochemical modelers. It would be preferable to have a validation also in a 3D setting, which is the one that the model was developed for but, since the model equations use standard formulations for the source and sink terms, I believe that the validation presented in the manuscript is sufficient. Overall, I think the model has the potential to become a valuable addition to the tools available to study ocean biogeochemistry processes. With models such as these, only time can tell if it will fulfill this promise.

Comments:

My only remark is that, like similar papers, little time is dedicated to describe the way the parameter values are found. I find it peculiar that since, as everybody in this business knows, model parameters are paramount to getting decent model results, hardly no one takes the time to explain how they got to the values they are using. Thus, I urge the authors to add a little more detail on this matter, because it will certainly help other that may want to use the BFM17 model in their work.

We agree with the reviewer and we will provide an explanation of how the parameter values were determined in the revised manuscript. Briefly, the values used in this paper were based largely on the values established by Vichi *et al.* (2003, 2007, 2013) for BFM56. We are also currently pursuing computational optimization of these parameters to minimize model error when compared to the BATS observational data; this future work will be mentioned in the Conclusions section

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of the manuscript.

Please check the reference style in line 347.

Thank you for catching this mistake. The correct reference style will be used in the revised manuscript.

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