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Geoscientific Model Development Discussions

Interactive comment on "A 1-Dimensional Ice-Pelagic-Benthic transport model (IPBM) v0.1: Coupled simulation of ice, water column, and sediment biogeochemistry" *by* Shamil Yakubov et al.

Anonymous Referee #1

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This paper describes a coupled physical-biogeochemical model for the sea-ice, water column and sediments. In the following lines I include only some general comments whereas in the text I did a number of detailed comments using the Adobe Acrobat editing tools. Whilst some comments are just minor suggestions of changes in a few sentences, others concern doubts I had about some model details. Regarding these last ones, I would emphasize comments and questions I introduce in page 6, about equations 18 and 19, and in page 10, about Algorithms 1 and 2. These comments do not imply that the authors did something wrong that requires correction, but are just

to suggest that the authors have a second look at a few things, answer to my queries and, possibly, improve the way those things are explained in the text. I think this paper presents a potentially valuable contribution to current research on modeling sea-ice, water column and sediments. However, I also think that the results presented herein are not enough for a proper evaluation of model performance. In the Abstract authors write that "The test run showed reasonable results for all main variables", whereas in the paper under the Results section (lines 20-21) they explain that "For demonstration purposes the snow depth was set to zero since the ROMS values were too high to allow ice algae growth during the melting season". This seems to contradict the vague sentence about model performance. What do you mean when you say that ROMS predicted too much snow? How much is too much? Was the problem with ROMS snow forecast or with your model parameters? Without a sensitivity analysis and more test runs for different places/years with some comparison to observations it is difficult to evaluate model performance. It also helps to plot the limiting factors during the simulations so that one may evaluate if, for example, algal growth is being limited by the "expectable" variables. This may help detecting some logical inconsistencies. I think that a final decision about the publication of this paper depends on what the main goals here are and this is not very clear from the Introduction. If the goal(s) is/are merely to describe and demonstrate the application of a technology, linking different models and coupling sea-ice, water and sediments and, if such accomplishment is well in line with the goals of the journal (I guess the editor is the right person to judge this), I think that the authors did the job and that the paper may be accepted for publication after some corrections/clarifications (assuming they have an answer for all the queries inserted in the text and mentioned above). Otherwise, if the purpose is to present a ready to use model, I suppose that more work is required to evaluate the model properly before the work is published. It seems to me that the main achievement here was the coupling of the three environments and the implementation of the vertical transport routines. Presently, there are quite sophisticated sea-ice models that make a bit obsolete the way sea-ice physics was handled in this work, based on a simplistic description of brine transport and diffusion, partly taken from literature published more than 20 years ago (Arrigo et al., 1993). I personally think that it is better to implement biogeochemistry in already available sea-ice models that handle the transport equation within the ice and between the ice and the water, whilst they also calculate brine transport in more physically sound manners (e.g. the Los Alamos Sea Ice Model). This does not imply that simplified physics should not be used when the focus in on biogeochemistry and when, for example, different biogeochemical parameterizations are being tested. However, I do think that efforts should be done to construct biogeochemical models that may be coupled to already available sea-ice models in line with the modularity that the authors emphasize in the text. I wonder if the authors have something to add about the possibility of coupling their modeling with already available sea-ice physical models. I am not qualified to say much about the quality of the English used in the manuscript but it seems to me quite good and, generally, easy to understand.

Please also note the supplement to this comment:
https://www.geosci-model-dev-discuss.net/gmd-2017-299/gmd-2017-299-RC1-supplement.pdf

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