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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.

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Since the anonymous Reviewer 1 comment doesn't have an explicit structure we divide it into pieces.

"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."

- Sorry for the misunderstanding, but this paper does not describe a physical-biogeochemical model, according to the name of the article and abstract it describes a transport model. This misunderstanding occurred maybe due to sentence in the line 25 page 2, to represent better the goals of the work we rephrased it to "Therefore, the goal of this work was to develop a model capable of simulating transport processes in all three domains simultaneously with a vertically resolved grid, while the biogeochemistry is provided by models of choice through the FABM".

- Here we will provide answers to the questions found in the attached file including the mentioned ones, minor suggestions are just fixed in the text:

"Why should hydrodynamic models provide such things for ice or sediments? I mean, for ice and sediments you need ice and sediment models. "

- Hydrodynamic models do not have to provide vertically resolved ice and sediment domains. But it may be useful to have one rather simple tool to simulate biogeochemical processes and fluxes in ice, water, and sediments together. Boundary layers (such as the sediment-water and water-ice interfaces) are areas with the active transformation of organic matter. Fluxes on these boundaries affect the ecological states within the different domains. When sediment (or ice) models are considered separately, boundary conditions are often either constant or simplified. When we use them together, interactions between the different domains may be more realistically simulated. For better representation of such benefits, we have expanded the results section. The sentence in question has been deleted from the Introduction because it is potentially misleading: SPBM is not a hydrodynamic model, rather it is an "offline" transport model that uses physical calculations from other models as inputs.

"Why a sedimentation rate for the diatoms in the ice, when they are known to be capable of active up and down motion? See for example: Aumack, C. F., A. R. Juhl, and C. Krembs (2014), Diatom vertical migration within land-fast Arctic sea ice, J. Marine Sys., 139, 496–504. Even if we consider diatoms not capable of significant motion in the ice why would they "sediment" in such a porous environment with their movements restricted by the brine channel network?"

- As all models our model uses some simplifications. The revised text reads: "To represent the ability of sea ice diatoms to maintain their vertical position relative to the skeletal layer (Arrigo et al., 1993) their vertical velocity is set to a constant but possibly layer-dependent value within the ice column, and zero on the interface between ice and water domains." So as you can see we do not consider diatoms to be incapable of motion. The vertical velocity parameter describes the ability of diatoms to move; this parameter is adjustable (can be positive, negative, and can be different for different layers).

"I have some doubts here. First, I wonder why a "sinking" velocity and, especially, a "burial" velocity for solutes. Why would solutes be "buried" or even "sink"? I understand that some solutes may precipitate, may be adsorbed and may be released to/from the sediment particles but these are different process. I also understand that solutes may be exchanged by diffusion and bioturbation and this last term is included in 18. However, it seems to me that "sinking" and "burial" are inappropriate and misleading terms for solutes. I suppose that "burial" should be used here only for particles but I would like to have authors feedback on this. Now, my second question is related with the usage of a "burial" velocity for the sediments as well. There is some discussion around this issue in the paper: "Meysman, F. J. R., Boudreau, B. P., and Middelburg, J. J.: Modeling reactive transport in sediments subject to bioturbation and compaction, Geochim. Cosmochim. Ac., 69, 3601–3617, doi:10.1016/j.gca.2005.01.004, 2005. I suppose that the "burial velocity" used by the authors corresponds to the sedimentation rate at the upper sediment layer, right?"

- It is about Eqns (18, 19). We changed "sinking velocity" to "burial velocity" here.

Both solutes and solids are "buried", because they both get sediment placed on top of them, such that their depths within the sediments increase over time. For solutes, the burial velocity is an advection velocity due our choice of the SWI as a reference frame (depth z has a fixed value at the SWI). In the frame of the SWI, the settling of sediment gives the bottom water velocity a very small component downward through the SWI (since it is porous, see Boudreau 1997, p34: "The burial component of porewater advection results from the sediment-water interface moving away from porewater as sediment accumulates"). To be strictly consistent, the solute burial velocity could be extended through the water column as an advection velocity, but the contribution of this to transport in the water column is safely negligible since the deep burial velocity is of order 10ˆ-10 m s-1 (Table C3). The burial velocity is depth dependent and generally different for solids vs. solutes. However, under the assumptions underlying Eqns 18, 19 (see Yakushev et al., 2017) both solute and solid burial velocities tend to the constant "deep burial velocity" parameter at deep depths (see Table C3, note that the "deep" was missing here and has been reinstated). The term "burial velocity" is widely used for both solutes and solids (e.g. Berner 1980, Boudreau 1997(2003)). Meysman et al. (2005) criticize the diversity of terminology for the velocities in the sediments (and the use of "rate" instead of "velocity", which we have avoided). However they do not explicitly propose a standardized term. They tend to use simply "velocity" or "advective velocity". In our view, "advective" would be misleading for the sediments because, for us, "advection" refers to motion due to being carried along by a moving fluid, and the solids in the sediments are not being carried downward by fluid (rather, they are being buried).

"As I understand this implies that you have ice layers with a constant ice thickness along the whole simulation, right? I mean if the spacing of the grid points does not vary...I wonder if this implies that you add/remove grid points only when the total ice growth/melt equals the thickness of the ice layers. It seems so, reading Algorithms 1 and 2 but I may have misunderstood something here. I guess a simple scheme would make it more clear."

- It is about the sentence “IPBM uses a fixed grid structure for the water column and sediments, and a time-dependent grid for the ice column where the number of grid points but not the spacing is varied” - line 14 page 6. Thank you for the comment, we modified an existing scheme to describe the grid structure a little bit better. Also, in fact, there is maybe an answer in the text for your question on line 19 page 6: “The ice column is discretized into layers of strictly constant thickness zs, and when the ice column grows or melts its total thickness can change only by multiples of zs”.

“In this and also in Algorithm 2, authors use IceGrowth as an integer and, as I understand, they mean the number of layers that are added/removed from the ice grid when the ice grows/melts. However, IceGrowth has been used before in equation 4 and expressed in cm sec-1. Therefore, I suggest using here and in Algorithm 2 a different name. Also, if s is the surface water layer index, I presume that Cs is the surface water layer concentration. If this is the case, then why is it influenced by ice freezing. I mean if part of the surface water layer was frozen, that should not change by itself the concentration of substances in that layer, unlike what happens when ice melts and mixes with the surface water layer, right? Perhaps I misunderstood something here but I leave here this comment for authors consideration. Furthermore, when you put IceGrowth in the modulus sign it implies by definition that the obtained value is always > 0. The same applies to Algorithm 2 of course. Therefore, and in accordance with the usage here of IceGrowth as an integer designating the number of layers added/removed to/from the ice (assuming I understood correctly its meaning...) I suppose you want to use its real value (positive in Algorithm 1 and negative in Algorithm 2) and not its modulus, right?”

- This question is about algorithms 1 and 2 on pages 7 and 8. IceGrowth is a wrong name, it was fixed. Thank you for mentioning it. Yes, Cs is the surface water layer concentration, description of this and all other subscripts are provided. Here we are using a grid and ice thicknesses from a hydrophysical model, and for simplicity, the water column is assumed to have constant depth (we use the time-independent mean water column depth from ROMS). So we can conclude that in our case ice appears out of

nowhere. This is an acceptable simplification because IPBM (now SPBM) treats transport and transformations of biogeochemical tracers, not of the water itself. However, we must conserve tracer mass, so when tracer mass is locked into newly-frozen ice it must be subtracted from the (constant-depth) water column. Concerning the modulus, we considered it according to its definition. It is true, the obtained value is always > 0, and as mentioned in the text “IceGrowth is the number of freezing (positive value) or melting (negative value) layers”.

“What about ice algae? How were they simulated? As phytoplankton?”

- It is about line 13 page 10. We have completely modified a description of the biogeochemical models used for the presentation of the model output and moved it to the test run section. Yes, ice algae are simulated as phytoplankton in the test case 1 (using the ERSEM “primary producer” module) and we have made this explicit in the revised text.

“The sea ice profile needs to be inverted. The variability in ice thickness is mostly at its lower boundary which is in contact with the seawater and that is (I suppose) where you show the bloom around August-September. Once you invert the the ice profile, I guess that Chl should be plotted along the lower part of the profile, i.e., the bottom ice.”

- It is about Fig. 3 page 11. When considering the 3 domains together this representation can be better since the most important areas are the boundaries and it can be more convenient to represent them as straight lines. We added more figures in the test run section to show it.

“How come do you have more Chl in March than later in summer when some of the bloom should sink to thee depths?”

- The same picture (Fig. 3 page 11). Apparently, these extremely low concentrations come here from the previous October bloom. It is a model output based on transport and ERSEM ecosystem formulation described earlier.

“I think that it is more correct to say that it is exchanged between the ice and the water by diffusion and bioturbation, and it decreases due to biogeochemical consumption within the sediments, right?"

- It is about a sentence "Below the SWI oxygen is transported downward by bioturbation and concentrations decrease with increasing depth" line 11 page 11. Thank you for mentioning it. We modified and renamed the results section and removed this sentence.

"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."

- We aimed to provide an efficient computational tool for flexible, offline simulations of coupled biogeochemistry in ice, water, and sediment domains. It was not our aim in this work to parameterize or validate a particular physical or biogeochemical model (new or old) for a particular site. This has been clairified in the revised text. That said, it is, of course, desirable that the test cases give realistic results given realistic forcing inputs. We agree that reducing the ROMS snow cover was an unfortunate fix, and we have avoided this in the revised manuscript. Instead, we have adapted the light

limitation parameter values for the ice diatoms (originally based on the ERSEM diatom parameterization). To our knowledge, these new parameter values are consistent with plausible a priori ranges for ice diatoms.

"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."

- Yes, the diffusion coefficients and brine volume parametrization based on Arrigo et al. (1993) are simple and empirical, but we believe that they do still offer a reasonable approximation, at least for young sea ice, and have therefore retained them as a default option in the revised code. Meanwhile, we have added the option of forcing SPBM with time-dependent profiles of brine volume, diffusive coefficients, and brine salinity, possibly from thermodynamic sea-ice models. The user can thus incorporate the best available information on sea ice physics (e.g. from the Los Alamos Sea Ice Model). This offline forcing approach neglects possible feedbacks from sea ice biogeochemistry on sea ice physics but to our knowledge, no strong feedbacks of this type have yet been demonstrated.

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