

# ***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 #2**

Received and published: 23 February 2018

### General comments

The ms by Yakubov et al. presents a new transport model for coupled numerical simulations of sympagic, pelagic and benthic biogeochemistry. The model development itself is innovative and relevant to the scientific community. However, I have several concerns regarding the very limited description provided for the models that were used, the very little results presented, the total absence of discussion and the poor conclusions. Although GMD journal focuses on novel model developments, I believe that the material provided by the authors so far is more suitable for a report than for a publication in

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GMD. However, I am sure the handling editor is the one that best can evaluate this.

I have suggested several corrections already but, generally, I believe the manuscript would benefit from being reviewed by an English native speaker or being grammar checked.

Please use standard international units as much as possible throughout the ms.

When referring to parameters/forcing shown in Tables, please refer to them throughout the ms. E.g. on page 8, Line 1, for kf Line 7-8, for kscatter, Aice, Asnow, and ksnow etc.

In the Introduction, I would suggest you have a look at Vancoppenolle and Tedesco, Numerical Models of Sea Ice Biogeochemistry in Sea Ice 3rd Edition edited by D.K. Thomas, 2017, and refer to it as needed since it is a comprehensive work on the most novel part of your work, i.e., the inclusion of a sea ice biogeochemical module.

In Section 3 there is a very brief description of the models (i.e., less than 20 lines), and it is overall not sufficient to make it clear to the reader what the chosen modules are about. In fact, I found Sect. 3 very confusing. Any info on growth limiting factors is for example missing. There is no description of physiological/ecological differences between the communities in different habitats. On top of the additional text, some scheme might also be good to provide. Also, there is no info on the initial/boundary conditions used, i.e. the reader has no idea where Fig. 3 comes from.

Section 4 presents and describes way too little model results in my opinion and no sensitivity to any of the many choices that for sure the authors had to make. I don't think this model is anyhow validated at this point and/or ready to be used for ice/water/sediment studies. If the authors are confident it is, then much more supporting results should be presented and discussed. There is currently no discussion at all in this work.

Specific comments

Title - To be consistent with the rest of the title, you should replace "Ice-Pelagic-Benthic"

with “Sympagic-Pelagic-Benthic”, and thus “IPBM” with “SPBM”.

- “Simulation” or “simulations”?

- I don’t really have a preference for version numbering systems, but I’d rather name this first version as “1.0” than “0.1”. Just a suggestion, up to the authors.

## Abstract

Line 1. “Aquatic” or “Marine”? I suppose it is “Marine” what you want to refer to.

Line 1. Why “especially in polar regions”? I would rephrase as: “interact in shallow areas of the polar regions with”, or something like that.

Line 2. I’d suggest removing “layers”.

Line 3 and further. Same comment as in the title about replacing “Ice” with “Sympagic” for consistency, and thus the acronym “IPBM” with “SPBM”.

Line 5. I’d suggest removing “(reaction terms)”.

Line 6. “partly coupled” in which way? Do you mean “coupled to both”?

Line 9. What “main variables”? Which model are you referring to? Please, specify.

Line 9-11. Since the model setup explicitly stresses a benthic component, the reader is perhaps expecting a comment also on that in this sentence (on top of the others for sympagic and pelagic dynamics).

## Introduction

### Page 1

Line 13. Is there a need of “marine” between “Arctic” and “ecosystem”?

Line 13. Please, consider replacing “induced” with “driven”.

Line 14. Do you refer here only to CCSM? Or more generally to ESM? Please, correct

in case.

Line 15. Please, change “global mean” with “mean global”.

Line 15. This reference might be updated.

Line 17. Please, place a reference at the end of the sentence.

Line 17-18. This sentence could be much better rephrased to emphasize the role that numerical models may play in the understanding of future changes of the Arctic Ocean ecosystems.

Line 19. This sentence applies to all model developments. I'd suggest modifying as “of this region, such as the seasonal to permanent ice cover and the presence of shelf areas. Thus, the model should preferably combine. . .”, or something similar.

Page 2

Line 1. Please, place a reference at the end of the sentence.

Line 3. “2012”, recent? Please, also consider overall estimates of sea ice algal production and related parameters that can be found in Arrigo, K., Sea ice as a habitat for primary producers, in Sea Ice 3rd edition, edited by D.K. Thomas, 2017 (see e.g. Tables 14.1,14.3,14.5 and 14.7).

Line 4-5. Again, I suggest having a look also at other references in the same chapter just mentioned in my previous comment.

Line 5. Correct reference style and update reference to 2017.

Line 5. Please, place “algal “between “sea ice” and “production”, and please, replace “from” with something like “accounting for”.

Line 5-6. Please, also here add e.g. “primary” before “productivity”.

Line 6. Please, specify “in a study area of Greenland” after “community”.

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Line 11. “Pervasive”?

Line 11. “localized”? or “local”?

Line 11. Please, replace “the most” with “an”.

Line 20. Please, place a comma after “Also”.

Line 21. Please, remove the comma after “pelagic”.

Line 21-22. Repetition of “correct”. Please, rephrase.

Line 22-23. Please, change “structured in the vertical” with e.g. “vertically structured”.

Line 28-30. Please remove parenthesis before “such” and at the end of the sentence for better readability.

Line 31. Please, place a comma before and after “i.e.”.

Line 33. Missing reference after “MOM”.

Page 3

Line 16. Something wrong with line editing.

Line 16-20. It is a good practice to specify variables’ names in order of appearance. Please, correct accordingly.

Line 22. Please, replace “between in” with e.g. “within”.

Page 4

Eq. 4. Please, consider replacing “IceGrowth” with e.g.  $\text{d}z/\text{d}t$ . Also, if this is in  $\text{cm s}^{-1}$ , I see a problem with units since  $z$  is in  $\text{m}$ .

Line 8. Where does “72” come from? Does it need a reference? Or is a tuned parameter? Please, specify.

Line 14. Where does “3” come from? Does it need a reference? Or is a tuned param-

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eter? Please, specify. I don't think sea-ice diatoms have to have a sedimentation rate. They are able to move and stick within sea ice and this has been proven now for a long time (see references e.g. in Arrigo et al, 2017, reference above)

Page 5

Line 1. Please, correct reference style.

Line 20. Should "width" be replaced with "thickness"?

Page 6

Line 1. Please, correct reference style. Also, why do solutes have a sinking velocity?

Alg.1 Is this the same "IceGrowth" as earlier in the ms? To me, it looks like something different.

Page 7

Line 13. Something wrong with line editing.

Line 15. Please replace "depths" with "depth z in the water column, which" and specify according to which scheme (with reference, possibly). Also, consider replacing "propagate" with e.g. "compute".

Line 17. Please, remove one "is".

Page 8

Alg. 2 If this refers to melting, I suppose that the sign for "IceGrowth" should be rather negative than positive, right?

Line 1-12. Please refer to related Tables for parameters such as kf, kscatter, etc.

Page 9

Line 1. Same comment as above.

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Line 4-5. Please, explain this further.

Line 6-8. So, which option was used here? If both according to ice presence, then please, rephrase to make it more clear.

Line 10-11. Not clear the sentence in brackets. Should the comma before salinity be replaced by an “and”?

Line 13-15. Bad wording, please, rephrase with e.g. “More accurate estimates of downwelling shortwave radiation and PAR can also be read from..”

Line 16-20. Not clear, please rephrase and correct writing style.

Page 10

Line 2. Please, remove double brackets.

Line 4. I wonder why “non-bacterial” since ERSEM does have a bacterial component. Also considering that you did consider bacteria in the sea ice. How do bacteria end up in the ice if they are not in seawater? And please notice that strictly speaking, plankton definition does include bacteria (i.e., bacterioplankton).

Line 4-5. Please, replace “conception of the functional groups” with “concept of functional group (..), which”. And please, reformulate better the rest of the sentence to make it clear what a functional group describes.

Line 6-8. Why was this choice made? For technical reasons? For lack of observations? Or? Please, explain also the consequences/limitations due to this choice either here or later in the ms.

Line 16. I would call it “site” rather than “point”. What is also the ROMS grid point? Is it one of ROMS test case study? If so, please, rephrase. Also, a location map of the site with also bathymetry would be a nice addition.

Line 17. 1984 is a curious number, any explanation for this choice?

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Line 20-21. I don't think this is the way that the problem should be faced. What was too high? The snow depth provided by the physical model? How high? Did you compare with observations? Sea ice is rarely snow-free and ice algae can certainly grow under thin snow. If the model was not able to produce any growth, then perhaps you should review algal growth parameterizations rather than removing snow from the model, which has e.g. the effect to anticipate unrealistically the bloom timing and even causing photoinhibition, as shown by both field experiments (e.g. Campbell et al., 2014) and modelling work (e.g. Tedesco and Vichi, 2014).

Campbell, K., Mundy, C.J., Barber, D., Gosselin, M., Characterizing the sea ice algae chlorophyll a-snow depth relationship over Arctic spring melt using transmitted irradiance, *Journal of Marine Systems* (2014), doi: 10.1016/j.jmarsys.2014.01.008

Tedesco, L., Vichi, M., Sea ice biogeochemistry: a guide for modellers. *PLOS ONE*, (2014), doi: 10.1371/journal.pone.0089217.

Page 11

Fig. 3,4,5. Please, invert the scale of the y-axes, i.e. sea ice thickness should be represented in the y-axes, as normally done for sea-ice plotting.

Fig. 3. Labels of Chl-a concentrations should be changed for better readability in the upper two panels, i.e. 0.8 rather than  $8.00 \times 10^{-1}$ , etc.

What is the yellow band at the end of February in panel c)? What is the sense of presenting Chl-a in the deep if there seem to be only a "spike" in the data?

Line 1. How do you define it as a "bloom"? Also considering the very little number, I'd rather call it a "small growth".

Line 2-4. I don't agree. Sea ice algal Chl-a can have a tremendous spatial and temporal variability (see e.g. the very many Chl-a values reported in Table 14.1 of Arrigo et al, 2017 (reference above)). One cannot compare model results just like this; especially for a 1D model development when the reader does expect that the site has been

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chosen also for the availability of observations/measurements to compare the model with. Also, 0.8 mg Chl m<sup>-3</sup> is certainly among the lowest concentrations ever seen, not very typical.

Line 4. Could this be just “export” rather than “deep Chl-a maximum”? And what about the non-diatom Chl-a?

Line 7. As far as I know values in Fig. 4a are way too low. Do you have any reference to provide here? Any measurement?

Line 11. Is really "oxygen transported down"? Please, rephrase.

Line 12. This is not correct. Not all brine channels have very high salinity. Their values vary both vertically and seasonally. Please, rephrase.

Line 13. What is the carbonate system constant?

Line 15. You would need to place here at least a reference.

Line 17. “habitats” would stand better than “media”. Please, consider rewording.

Page 12

Fig. 4. Labels of O<sub>2</sub> concentrations should be changed for better readability, i.e. 70.0 rather than 7.00 x10<sup>1</sup>, etc.

Fig. 5. Labels of pH should be changed for better readability, i.e. 11.0 rather than 1.10 x10<sup>1</sup>, etc.

Page 13

Line 1-3. The multilayer approach advantages’ were not discussed at all in this work. The same applies to BBL and multilayer sediments.

Line 4. Please, remove “of”.

Line 5-6. I am not sure this is a scientifically sound approach. E.g., not every model

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might fit any habitat. And this could be the case for sea ice algae that did not grow under snow. In general, the approach the authors chose has not been discussed properly in my opinion. If the authors are confident this is a scientifically sound approach, they should add a discussion about it.

Line 7-8. What the authors suggest for further work, i.e., a "validation to a particular case", I believe it is something missing from their own work in here.

Appendix A. Why the author choose such a simple sea-ice model? A discussion on this choice should be added.

Line 23. Duarte et al., 2015 is a wrong reference here.

Page 14

Line 1. Duarte et al., 2015 is a wrong reference here.

Page 16

Table C5. Where do those values come from?

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Interactive comment on Geosci. Model Dev. Discuss., <https://doi.org/10.5194/gmd-2017-299>, 2017.

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