Geosci. Model Dev. Discuss.,
https://doi.org/10.5194/gmd-2018-191-RC3, 2018

Geoscientific
Model Development
Discussions

GMDD

Interactive
comment

Interactive comment on "CSIB v1: a sea-ice biogeochemical model for the NEMO community ocean modelling framework" *by* Hakase Hayashida et al.

Anonymous Referee #3

Received and published: 4 October 2018

Geoscientific Model Development Discussions (Ms. No. gmd-2018-191)

Title:

CSIB v1: a sea-ice biogeochemical model for the NEMO community ocean modelling framework

Authors:

Hakase Hayashida, James R. Christian, Amber M. Holdsworth, Xianmin Hu, Adam H. Monahan, Eric Mortenson, Paul G. Myers, Olivier G. J. Riche, Tessa Sou, and Nadja S. Steiner

Printer-friendly version

Discussion paper

#Summary

The authors described a newly developed 3-D sea ice biogeochemical model embedded on the pan-Arctic NEMO and CanOE framework and then evaluated its basic performance with available validation datasets. Several sensitivity experiments were also performed to verify unknown parameter values such as shortwave absorption in the snow surface layer and light shading effect of ice algae. The paper was well written, and most parts of the presented analyses are quite reasonable. On the other hand, the target period of 1970s seems to be too old and short for model evaluation with reliable observational data. Computational cost should not be a primary reason because a simulation for more recent years can also be performed in the same manner. If there are another circumstances, please explain specifically.

#Major Comments

One of my major concerns is that the model target period for 1969-1979 is so old. Such an experiment is regarded as a spin-up one but is not usually chosen for main analyses due to lower accuracy of atmospheric forcing datasets and lack of field measurements. In addition, most results in the sensitivity experiments are seen only in 1979. Hence potential readers cannot judge whether the presented anomalies are typical or unique features. The authors may have insufficient computational resource. Even in that case, they can run the model for more recent years (e.g., from 2000 or 2010) and/or use decadal mean forcing (e.g., 1990s or 2000s). Then discussion with decadal changes would provide more interesting scientific findings. Is it impossible?

Second concern is that the authors show GPP for ice algae and NPP for phytoplankton. It is a little confusing. I know that some reasons are described in the manuscript and that their difference is minor. However, I expect that the authors rerun their model to produce NPP for ice algae or GPP for phytoplankton. It is important to reduce extra cares for potential readers throughout the manuscript.

#Detailed Comments

[Introduction]

>Line 6 in Page 2

Terminology of “mechanistic model” is unfamiliar, at least for me. “Numerical model” is more standard, right?

[Section 2]

>Section 2.3

What are “unresolved (eddy diffusion) motions of sea ice” described here. I know that eddy diffusion in ocean models represents sub-grid seawater exchange driven by mesoscale eddies. However, lateral exchange of (solid) sea ice packs due to eddy activity hardly occurs except a part of marginal ice zones. Some sea ice models adopt eddy diffusion only to damp numerical instability depending on advection schemes. Do the authors assume other physical processes in the central Arctic for this term? I find a sentence “Readers are referred to Vancoppenolle et al. (2012)”. But I appreciate that the authors explain more details, because a sensitivity experiment related with “eddy diffusion” term is presented in this manuscript.

>Section 2.3.3

Let me confirm whether ambient temperature controlling algal growth rate is kept at a freezing point of underlying seawater or not.

[Section 3]

>Line 21 in Page 18

Please correct a unit of Dupont’s PP.

>Line 1 in Page 20

What kinds of formulation or parameters are necessary to represent mat and stand contributions? Why did not the authors consider them in the present model?

[Section 4]

>Section 4.1 (EXP1 and 2)

It took a time for me to understand the purpose of these sensitivity experiments. Is it right that CORE-II provides only monthly mean data for snowfall and that DFS snowfall has daily mean time series only from 1979? Has daily climatology for 1979-2012 been used in the case of simulation before 1978? If yes, a simulation for more recent years does not cause this problem. Anyway, I recommend that the authors explain backgrounds for this analysis more clearly.

In addition, this subsection describes only snow depth comparison. How about impacts of forcing choice on ice algal PP?

>Section 4.2 (EXP3)

It may cause misleading that “Using these higher i0 reduces light limitation, and hence enhances ice algal primary production”, even though light penetration through snow column is overestimated. Since formulation and parameter values of light limitation term largely differ between models as described in the previous paragraph, light intensity at the skeletal layer is not always directly linked to ice algal PP. I recommend that the authors describe this part more carefully.

>Section 4.3 (EXP4)

Why is “space opening” necessary for new ice algal growth? It can also be considered that higher algal biomass causes higher PP as long as nutrient is available. Please explain a limitation factor.

As mentioned above, the authors should specify more detailed processes of “eddy diffusion” of sea-ice biogeochemical state variables. In addition, can individual impact of this eddy diffusion term be estimated?

Sea ice drift patterns also have large interannual variability depending on wind stress fields. Therefore, I am afraid that the presented anomalies in a single year are not representative in the pan-Arctic region.

>Section 4.4 (EXP5)

I do not understand that “the reduction in nutrient drawdown under regions of large ice algal biomass enhances nutrient advection into regions of low ice algal biomass”. Why is nutrient advection enhanced by biogeochemical processes?

Definition of bloom onset using bottom-ice PAR looks unnatural for me, because under-ice NPP is also calculated in the present model. The authors may try to compare with Castellani et al. (2017) more directly. But the target year is quite different between two studies so that this information is not so valuable.

[Section 5]

>Line 5 in Page 30

Again, validation in a single old year is insufficient to appeal the model performance, especially in terms of sea ice volume and extent.

[Figure]

>Spatial maps

I recommend that names of major countries or cities are overlaid.

>Time Series

Values of vertical axis should be smart (e.g., every 1 or 10).

>Figure 3

Magenta contours are hardly distinguishable from red contours.

>Figure 4

Is total number of vertical layer same between NAA1 and NAA6 versions (i.e., 46 layers)? This figure is confusing. How about replacing vertical axis to water depths?

>Figure 6

Although area total amounts are shown in Figure 5, why are only GPP and NPP values in this figure plotted using "per unit area"? I think that the pan-Arctic average value is meaningless because of large spatial variability.

>Figure 10

Please insert a zero line in (b). And I am wondering why the 1-m average minus the 12-m average shows negative for most periods.

>Figure 16

Contrast of a color bar in (c) seems to be weak.

[Table]

>Table 3

Unit of eddy diffusivity should be m^2/s.

Interactive comment on Geosci. Model Dev. Discuss., https://doi.org/10.5194/gmd-2018-191, 2018.