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Geoscientific Model Development Discussions

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

A. Randelhoff (Referee)

achim.randelhoff@takuvik.ulaval.ca

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Hayashida et al. report here on their implementation of a sea ice biogeochemical model into the NEMO framework. The strength of NEMO is the ability to compare different submodules, and thus to isolate the overall impact of very specific parameterizations and submodels. This is an important step in refining existing models and towards singling out future research directions in this ambitious field of mechanistic modelling of (Arctic) ocean biogeochemistry.

I found the paper generally well-written and laid out clearly; find some suggested improvements below.

Major issues:

Section 3.1: I am not entirely convinced by your analysis. The "break points" you see in the time series appear to be quite arbitrary; they probably make sense to you based on your familiarity with models and previous studies, but a reader might want to see a statistical analysis that supports your claims. That being said, I am not entirely sure to what extent you need these spinup times for your later analysis; you are probably able to carry through the rest of your paper without most of the claims put forward in this section. If they are important in their own right, I would recommend considering a more rigorous presentation.

The paper should probably also explain more generally why it focuses on the period 1969-79 for spin-up and 1979 for all experiments, especially since e.g. snowfall climatology (and likely all other data) are much scarcer for that period than later ones. The Arctic also looked very different in 1979 from what it is today, so you should give a good reason if you expect that your comparisons from the 1970s are relevant today as well.

Minor comments and suggestions:

General:

Some of your figures have colorbars that are of very little use (examples: Fig. 8, Fig. 14b+c, Fig. 16c, ...) because they are scaled linearly and some extreme regions mask the variability over most of the map. Assuming you produce your figures using matplotlib, you could look into https://matplotlib.org/users/colormapnorms.html.

You mix present and past tense occasionally (e.g. Section 3.3.1; p20 line 9ff; ...).

Specific:

p1

15 spell out LIM2/3, PISCES

p2

23 "horizontal transport of ...": Technically correct (here as elsewhere), but it might be more intuitive to explain that you mean transport with sea ice drift.

27 You may want to quickly mention in this section why you develop another sea ice BGC model after several others already exist. I assume it is because the NEMO framework allows model intercomparison, your overarching goal, and it did not have any yet?

p3

Tab. 1: Specify that i0 is for the uppermost layer, be it snow or ice or both (which I think it is based on p25, l.14, as Maykut & Untersteiner had apparently measured it for snow-free surfaces, but correct me if I am wrong). Mention briefly (here or in the text) what you mean by "ice algal skeletal layer".

9ff. " a thin layer...": Is this synonymous with the "scattering layer" of sea ice optics? What happens when there is less than 10 cm of snow+ice? Does this fraction penetrate only below 10 cm, after which different attenuation coefficients are applied, or is transmissivity below this "thin layer" set to 1?

11 Two times "this" is repetitive

p4

Fig. 1: Again, is this "surface thin layer" the same as the scattering layer? In this schematic, you could also indicate if there is additional attenuation below this "surface thin layer".

9 I think I understand what you are saying, but in my mind the point of mechanistic modelling is exactly to include more and more parameterizations. Is PISCES' performance not good enough to justify the extra computational costs it would imply as compared to CanOE?

15-16 I can live with the sentence but found it a tad vague, probably because I did not understand what you mean by "state of the ecosystem". In terms of nutrient budget

modelling, part of the ecosystem *is* the sulfur cycle so the inverse statement is a tautology.

p5

11-12 43% in units of W/mˆ2? I also assume you mean "43% of the downwelling shortwave radiation reaching the sea surface"

p6

Fig. 2: "stoichiometry": stoichiometries

p7

4 Plase explain what "[e]ddy diffusion tendencies" are. How are they computed (explicitly)?

10 By "concentrations", I assume you mean sea ice bulk, not brine concentrations. Since salinity gets a special treatment, are there problems of mass conservation during ice freezing because of changes in ratios of nutrients to salinity?

11 Add "the concentration (X) of any ..."

18-19 "minimum biomass threshold": I assume this threshold is also arbitrary, not based on field measurements?

29 You say you use the molecular diffusive exchange of nutrients, but my impression is your model would not resolve the molecular sublayer (a few mm from the ice-ocean interface). Without having checked I assume you use combined turbulent-molecular diffusion coefficients, but you may want to include the right references here, especially since such coefficients have never been measured (as far as I know) for tracers other than momentum, heat, and salinity.

30 Flooding due to negative sea ice freeboard does not count towards flushing?

p8

2-3 "designed to be the most realistic ...": "realistic" is as such a bit vague and you might want to rephrase as something like "thought to be most realistic among all choices considered in this paper".

p10

1 "initialized to arbitrarily low values": I do not understand what "arbitrarily low" means.

5 "while the other two boundaries (along North America and Eurasia) were assumed to be closed": So how was riverine (freshwater) input distributed into the ocean?

13-14 "... was neglected, and therefore ... not addressed": This is a tautology. Is there a reason why? Lack of data? Too small? Too hard?

p11

Tab. 3: Typo in units of rn_ahtrc_0, should probably be m2s-1.

6 "modified": How? Were only the data quality flags adjusted?

10 So just to be sure; You used the 1979 snowfall for all years 69-78, but for all the other variables you use the 69-78 time series data?

15-16 You may want to consider additionally archiving the current version (e.g. using a doi) upon publication.

18-19 "were adjusted to improve": How adjusted, and by what measure did you check that they "improved" sea ice volume etc.? Maybe insert reference to later if this is part of the discussion of the model runs.

19-20 "were adjusted to simulate reasonable": Same as in the previous sentence.

p12

18 Repetition: "the the Pan-Arctic"

p13

7 "diagnose potential drifts": Unclear to me. In addition, how do you separate this (effects such as potential mass non-conservation in some tracers) from the inherent "spin-up dynamics" (meaning the adjustment from some relatively arbitrary initial condition to a state that is permitted by model dynamics)?

p15

4ff. What is the reason for not simply masking the respective regions from the PIOMAS dataset in order to compare your model outputs across the same regions?

28 Is the occurence of such thick ice off Siberia in PIOMAS discussed in any of the literature about PIOMAS? I am mostly asking out of curiosity, I agree with your conclusion that it is likely an artifact.

p18

13 "confined to shelf regions" (excluding the Barents Sea)

29 space between 'Figure' and '8d'

29 "in both qualitative and quantitative ...": I feel "in quantitative ..." is enough already as it entails the other.

p20

Section 3.4: "subsurface chlorophyll a maximum": Maxima at around 5–10 m are hardly comparable to the severals tens of meters usually found in the Arctic; splitting by Atlantic/Pacific sectors might be worthwhile here due to their very different hydrographies, just as extending the profiles in the plots deeper (to e.g. 40 m).

Fig. 9 gives the impression that the surface mixed layer is not deep enough (compared to what I think observations would show), hence surface mixing might be too weak in the model. Could this also be the reason for strong DMS gradients in the upper 10-15 meters?

Do you think including under-water PAR irradiance in Fig. 9 could help with the interpretation of the results?

24ff. I am not sure what these gradients in DMS tell me. You state "DMS flux would be underestimated", but can you spell out what exactly is being underestimated? The real flux, the modelled flux? Is the DMS flux calculation currently based on the 1-m concentration or the 12-m concentration, and why does the formula not use the one that would be appropriate for your model? How many percent would the underestimation be? 15% under- (over-?) estimation in the surface ocean DMS concentration as such does not sound so bad, but the effect on the flux probably depends on the air concentration.

p22

8 "mm dˆ(-1)": Does this mean amount of meltwater equivalent, snow...?

p23

5 "extremely-low": extremely low

14ff. So if these model-internal parameters can have such a big and non-intuitive effect on accumulated snow depth, why do you tune/adjust/modify the input snowfall dataset instead of the model parameters? (I also kept wondering, what happens to the thickness of the snow cover during ice dynamics (i.e. convergence)? Is total snow mass being conserved?)

Section 4.2: Briefly remind the reader what i0 is.

p25

14 "This value" referring to Castellani et al.'s 0.3?

p26

Section 4.3: I am a bit confused as to what you mean by "neglecting" advection/eddy

diffusion. What happens instead when ice concentration changes from or to zero in a grid cell? Are sea ice BGC parameters somehow reset, do they pick up from where they were last time, whenever ice re-appears? I am especially thinking of nutrient and biomass budgets. Are these still being conserved?

Again, note my earlier reservation about calling this "advection", you may want to specify that you are talking about moving sea ice, and hence moving BGC variables around with the ice (I think).

Section 4.4: Which parameter(s) is/are being modified concretely to "neglect" "the shading"?

p28

31ff I think you can be more explicit here: What I understand is that ice algae shading can affect pelagic bloom *timing*, but will not affect annual pan-Arctic NPP. Regarding "patchiness of ice algal distribution": If you mean patchiness at the subgrid scale then I do not think your model accounts for this anyway; so perhaps it should not be part of the argument.

p29

Fig.15: I think including ice+snow transmissivity, or snow depth, or lead fraction, or something else of the sort should be included here to separate the rise in under-ice PAR into the two factors "increasing sun angle" and "more transparent ice cover" as the season progresses.

p30

4 "were necessary to properly simulate": I am unsure what "properly" means here.

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