

Review of gmd-2021-58

The importance of turbulent ocean-sea ice nutrient exchanges for simulation of ice algal biomass and production with CICE6.1 and Icepack by Pedro Duarte, Philipp Assmy, Karley Campbell, and Arild Sundfjord

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1 Introduction

The main point of this manuscript is that most of sea-ice biogeochemistry models do not include a proper treatment of turbulent exchanges between the ocean and sea-ice brines. I commend the authors for raising this important issue and for testing the consequences on a dedicated numerical setup, but I would argue that their argumentation may lead to further misinterpretation. It is not a matter of neglecting the turbulent exchange at the sea-ice/ocean interface, but rather making a proper overall consideration of the underlying physical processes. Contrary to the authors' claim, this has indeed been taken into account in the published literature although the lack of direct explanations on the underlying assumptions may have contributed to misinterpretations. I think this is a very good manuscript that would deserve publication, since it is going to contribute to the development of more adequate parameterizations of sea-ice fluxes, especially in the likelihood of a nutrient-limited future Arctic Ocean, as pointed out by the authors in the introduction. I however think the manuscript would benefit from additional work on the current version to address the substantive concern detailed below.

2 General comment

The recommendation done by the authors is that nutrient exchanges (and by extension any material flux) at the bottom interface with sea ice should be consistent with the way heat and salt fluxes are parameterized. This is indeed a reasonable advice, which in my opinion it has been taken into account in the literature. There are some theoretical differences in the proposed approaches that need to be taken into consideration, and I would suggest the authors to include a slightly more extended explanation in their background analysis. It is true that a series of refinements have been added to the description of momentum, heat and salt exchanges in sea-ice dynamics. Models were initially considered isohaline, and only heat conduction was considered. The various simplifications were eventually reconsidered and expanded as new knowledge was available. I recognize

that a similar approach has not been taken for the nutrient and, more in general, material exchanges at the water interface. This led to a lack of synchronicity in the development of the physical and biogeochemical components.

The parametrization proposed by Cota et al. (1987) is basically a formulation of Fick's law of diffusion. This is valid everywhere it is possible to determine a diffusivity coefficient (molecular or turbulent). However, at the interface between ocean and sea ice, we deal with a combination of turbulent and advective flux due to the physical growth of sea ice.

Equation (2) from McPhee (2008) referenced by the authors is only one component of the full salt conservation equation at the interface (Fig. 6.3 and eq. 6.3 in McPhee, 2008, but written here as eq. 6.8 and using the same notation as used by the authors):

$$\alpha_S u^* (S_w - S_0) + w (S_i - S_0) = 0 \quad (1)$$

where S_0 is salinity at the ice-water interface, S_i is the brine salinity and S_w is water salinity in the far field (generally the mixed layer salinity)

Most of the earlier publications made the assumption (even if not explicitly stated) that salinity at the interface is equal to mixed-layer salinity, $S_0 = S_w$, and hence the first term vanishes and the second term becomes $w (S_i - S_w)$. The physical implication is that turbulent exchange is assumed to be quicker than any other process, the solute is vertically homogeneous in the mixed layer and hence salt flux is mainly regulated by the entrapment/release flux due to sea-ice growth

$$w = \frac{dh}{dt},$$

where h is sea-ice thickness.

Turbulence is less likely to occur within or through the pores of the brine channels because they are microscopic, usually smaller than the turbulent eddies found in the vicinity of the interface. Molecular diffusion should prevail. But turbulence, expressed here by the friction velocity and the non-dimensional scaling coefficient α_S does affect the actual concentration at the interface (S_0). This is the the concentration that would then be entrapped in the sea ice, as well as diffused at low Reynolds numbers (usually discarded). The authors use this equation to prescribe a nutrient flux (their eq. 4):

$$F_N = -\alpha_S u^* (N_w - N_i),$$

which implies that the solute concentration at the interface (what should be N_0 in accordance with McPhee's formulation) is now equivalent to the brine concentration ($N_0 = N_i$). Hence, the advective flux is neglected and the flux is fully regulated by water column turbulence (as if the interface with sea ice would be equivalent to the interface between two layers of water). The change of sign with respect to the salt equation is however unclear and should be explained. If the system of reference is oriented upwards, this would lead to a negative flux when the water concentration is higher than the concentration in the brines, but I stand to be corrected.

To my knowledge, there are no dedicated measurements that demonstrate which approximation is superior, hence the claim that the published models are neglecting an important flux should be reformulated. Table 1 (together with the introduction and part of the discussion) conveys a misleading signal, as if the works that did not include a specific parameterization of turbulent diffusion, did not have any flux at all. They instead incorporated the turbulent flux in the advective component. I also think the authors should more adequately address the difference between molecular and eddy diffusivity, and the way it has been employed in the literature. Models that resolve the bottom nutrient exchange through molecular diffusion (and neglecting the advective component) are indeed more likely to underestimate this flux as shown in the results. I would also suggest the authors to clarify the argument of changing the timescales by adding some more explanation. This is pertinent to the CICE implementation of nutrient diffusion, while it seems as a general approach used in the literature.

In summary, models should ideally resolve both terms of eq. (1) simultaneously, which is not possible without further assumptions, because there are two unknowns (S_i , S_0) and one single equation (as opposed to the T,S system described in eq. 6.8 by McPhee, in which it is possible to thermodynamically connect the two variables).

Having said this, there is a lot of merit in the results shown in this manuscript. They show the role of assuming full permeability of the ice-water interface, as if the brines would be covering the whole surface and be affected by turbulence as a layer of water. In this context, the role of α_S and the related time scale becomes dominant, as clearly shown by the authors in the result section (They also state that this parameter is usually different when sea ice is growing or melting, which is another indication of the importance of advective processes and the co-existence of the two). The claim that the published models underestimate bottom sea-ice algae production because they do not resolve the turbulent fluxes is not substantiated by the presented analysis, although the authors are clearly showing that the parameterization choices lead to a different evolution of sea-ice algae biomass.

3 Specific comments

- L58-59 I would suggest to report the units of ΔC and Δz separately, and not the units of the ratio
- L68-70 This sentence should be changed in light of the main comment above. It contributes to the lack of clarity that the authors are indeed trying to address. Diffusion and advection are two separate processes.
- L88-89 The symbol α is not the same in the text and in eq. (2)
- L105-122 It should be clarified that this implementation of the diffusion process and the difference in scales is due to the choices done in CICE
- L148-151 I would suggest the authors to include a (very) brief description of the simulation set-up carried out in Duarte et al., (2017), especially in terms of how

the nutrient far-field is prescribed.

- L153-155&179 Many other parameters were sequentially changed, and not always one at a time. It is thus difficult to appreciate the role of each one. I understand that one of the authors finding is that they had to artificially alter other parameters in order to supplement for the limited nutrient fluxes simulated by a molecular diffusion parameterization. I wonder if this could be presented in simpler terms without the many experiments shown in Table 2, which do not always contribute to the aim of this manuscript.
- L213 The α_s values should be presented in the text and not just quickly in the caption, and further discussed if possible. This becomes a crucial parameter as highlighted in Sec. 2. (please use a space for scientific notation for all the numbers in the table, e.g. $8.6 \cdot 10^{-5}$)
- L246-248 May I kindly request that the supplementary figures be prepared with experiments side by side as done in the manuscript? This would greatly aid the comparison.
- L249 “CICE tracers” should probably be “CICE diagnostics”
- L251 Figure 5 shows the direct consequence of the large change in diffusivity values. This figure does not appear to be fundamental and could be moved to the supplementary. A figure on the light limitation would instead be helpful, since this process is discussed in Sec. 4
- L262 I cannot see the magenta line
- L288 I think the authors mean “followed by silicate”
- L292-293 This sentence is unclear and I struggled to interpret it. Is it the standing stock at the end of the ice period? They appear quite similar to me.
- L299-301 This is also a direct consequence of the difference in magnitude. It is also not very visible. A comparison of the nutrient flux using the prescribed eq. (4) from the manuscript would have been more helpful
- L306-307 This does not explain why light is less limited on June 1st in Sym2 with less snow with respect to Sym1. Please clarify