Review of gmd-2021-58-revision

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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August 2021

1 General comment

I would like to thank the authors for the extended and comprehensive answers to my comments. Many of my remarks have been addressed, and the current version is more clear in explaining that this paper is meant to explore the role of turbulent fluxes in the overall exchange of nutrients at the sea ice-ocean interface. I think this can be further ameliorated, especially in reducing the confronting tone used when referring to the previous literature. I have made a few comments in this regard at the end.

The arguments made in this paper are specifically oriented to and implemented in the CiCE+Icepack model. However, they are presented in a more conceptual way (Sec. 2.1 title is indeed "Concepts), which is meant to be rigorous and understandable by all scientists and not just by those familiar with the model. I commend this approach because there is a need for clarity in this regard, but I am not yet fully convinced that this revision addresses my concerns. I am actually more confused than before in what kind of turbulent flux the authors are addressing. My initial understanding was that the authors parameterised the turbulent flux at the interface with the seawater side. This ambiguity has also been expressed by Dr Vancoppenolle, who requested to add the description of the nutrient transport within the sea ice (he thus thought the diffusion within the ice was somehow addressed in the paper). It is clear that both reviewers interpreted the work differently. I'm afraid the submitted revision still requires some work to achieve an adequate degree of clarity.

Let's start from the turbulent flux (eq. 2 in the revision), which I understand now it is meant to address only one component of the total salt flux at the interface (the Reynolds decomposition):

$$\langle w'S' \rangle \simeq \alpha_S u^* (S_w - S_0) \tag{1}$$

The left hand side represents the averaged co-variance of the turbulent fluctuations, and not the the interface “vertical velocity and salinity” as erroneously written in the revised manuscript. The right hand side is the approximated form of the turbulent flux through the use of a boundary layer “friction velocity”, where, as now properly indicated by the authors, S_0 is salinity within this ice-water interface, and S_w is water salinity in the far field (generally mixed layer salinity). The authors state that this method can be applied to the nutrient flux at the same interface. Note that this is an extension of the concept, not in agreement with McPhee, 2008 as stated at line 106, since McPhee does not make this argument for nutrients. Also, there is no mentioning in McPhee (2008) that the nutrient concentration at the interface (which should be N_0, in accordance with McPhee’s notation) is now equivalent to the nutrient concentration in the brines ($N_0 = N_i$) inside the sea ice:

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

(thanks for clarifying the sign). This would require some further explanation; but here comes my (new) confusion. From line 110 onward, the description of the concept is focused on the interface **within** the sea ice, the biologically active layer where brines are connected. This is what distance h in eq. (5) is said to be. I am very puzzled as to how the friction velocity can be applied within the brines region. To my knowledge this timescale indicates replenishment at the interface with the water, where the the nutrient concentration is N_0,and the distance should be related to that turbulent boundary layer represented through the friction velocity. I am lost if the authors state that this distance is a sea-ice layer! I did ask the authors to clarify this timescale in my earlier review, but this was not specifically addressed (unless I missed it). The timescale indicated in eq. (6-7) is an expression of the turbulence within the brines, and hence it should be made very clear what the authors mean when they say “comparable” (line L113). I agree with the argument of comparing the timescales to understand diffusion through the interface, but this cannot be made using the brine height as a reference distance in both timescales since the processes are occurring at the two different sides of the same interface.

What I find most confusing is the fact that eq (8) is valid within the brines, and the authors decided to change the diffusive term in a quite disputable way (that is, using a friction velocity that is only defined at the ocean water boundary). Maybe some clarity would come if the author would explain the relationship between N and N_i used in their equation (4). Nowhere in the text it is stated that this is only applied at the boundary (I see the vertical gradients in eq. 9). There may be an argument in doing this, which is by prescribing the turbulent flux at the boundary. In that case, it would be plausible to equate the Reynolds terms in the brines with the ones in the ocean, because that turbulence-driven flux is the same (this is why they are dimensionally the same quantity). But I do not see this done in the revised text.

I would thus disagree with the statement done at lines 132-134. I am therefore concerned that some of the changes reported by the authors may be due to inappropriate handling of the equations, which would lead to a code implementation that is not justified by the underlying mathematical concepts. If the authors have changed eq (8) with eq. (9) in the computation of the sea ice nutrient concentration within the sea ice, they have thus assumed that turbulent diffusivity **within the brines** (and not just at the boundary) is identical to the one in the ocean boundary layer. I am not surprised that the resulting nutrient concentration in the sympagic environment is therefore much increased. I would prefer to suspend any other judgment on the presented results until this issue is clarified.

Here follows a few specific comments linked to my argument expressed above.

L33-35 I would suggest the authors to further clarify this concept making clear that nutrient exchange is a combination of processes. One option would be to move the sentence that is now at lines 67- to here. The common interpretation of a process should come before the models and their parameterizations that approximate the real process to the best of their knowledge. The authors instead start by saying what models do, instead of saying what the nutrient exchange process at the sea ice-ocean interface entails.

L60-62 It is not clear whether Δz is in the sea ice or in the ocean. I think this is the crucial point that I am addressing in the general comment.

L63 The expression "calculate tracer diffusion" is unclear. As suggested earlier, this should be one way of parameterizing the diffusion term in the overall mass-balance equation describing exchanges at the interface

L86-88 The parametrization proposed by Cota et al. (1987) is a formulation of Fick's law of diffusion. Nutrient exchanges are also due to diffusive processes. Cota's formulation is the boundary condition of any diffusive process modeled using a parabolic differential equation.

L71-74 It may be just a language issue, but this sentence seems to imply that nutrient availability in the sea-ice is mainly controlled by diffusive process. As recognized by the authors in their answer, this is just one of the components of nutrient exchange at the interface. Enhanced turbulent at the sea-ice bottom has the capacity to alleviate nutrient limitation in the absence of ice growth or melt.

L76-77 This is a rather bold statement. Is there any evidence that the relative change in the stock/rate associated to sea-ice primary production (that is only a fraction of the global ocean carbon flux) would lead to climatic feedback?

L79 I would suggest the authors to leave out the momentum flux, which is not parameterized the same way as heat and salinity (although based on the same arguments of Reynolds averaging).

L99 This parameterization is from McPhee et al (2008). The fact that it is implemented in CICE is secondary.

L120-121 I am not familiar with the Icepack notation, but I would suggest to use z as the coordinate variable for the vertical rather than x.