

Dear Dr. Marcello Vichi,

Thank you very much for the time you took to review our manuscript and for your comments. In the following text we will address your comments and explain how they were incorporated into the revised manuscript or used to clarify the points you addressed.

We focus first on your general comment and then on your specific comments. Please note that when indicating line numbers where changes were implemented in the revised manuscript, we refer the version without tracked changes.

With my best regards,

Tromsø, 9 November 2021

(Pedro Manuel da Silva Duarte, on behalf of all co-authors)

General comment

In the first paragraph of your review, you write that many of your previous remarks were addressed in our previous review but that the work may be ameliorated, especially, in reducing the confronting tone used when referring to the previous literature. We did not mean to be “provocative” when addressing the literature but solely wish to emphasize why this study is important to conduct. To avoid a confrontational ton, we have removed Table 1, where several examples of models specifying the parameterizations used to compute diffusion of tracers were originally listed. Accordingly, the sentence in the Introduction where we referred Table 1 has become (lines 60-63):

“The analysis of several models published over the last decades and their approaches to calculate tracer diffusion shows that some models do not consider this process or limit it to molecular diffusion. Other models consider turbulent exchanges parameterized as a function of the Rayleigh number, calculated from brine vertical density gradients. Only one of the sampled models (Mortenson et al., 2017) uses a parameterization based on friction velocity.”

Regarding your main concerns about the paper that are expressed in the paragraphs to follow, we begin with one of your sentences in the 2nd paragraph:

“My initial understanding was that the authors parameterized the turbulent flux at the interface with the seawater side.”

Your initial understanding was, and it still is correct. We parameterized the turbulent flux solely at the interface between the bottom ice and the ocean.

In the 3rd paragraph you commented on the way we defined the terms on the left hand-side of equation 1. Please note that in the original we refer not to the term as a whole but to each of its components. For the sake of clarity, we have changed the text according to your suggestion.

You commented that extending the usage of equation 4 (3 in the revised manuscript) is not in accordance with McPhee but it is merely an extension of his concept. We have changed the text accordingly (line 93): *“This is an extension of the concept used for heat and salt by McPhee (2008)”*

In the same paragraph you noted the imprecision in using N_i , instead of N_o , which was also corrected in the revised manuscript (line 92).

In the next lines of the 3rd paragraph as well as in the 4th paragraph you develop further on the idea that we seem to have applied equation 4 (3 in the revised manuscript) within the brines. However, we did not do so, in line with our statement above confirming that the turbulent flux was parameterized solely at the interface between the bottom ice and the ocean. This misunderstanding was perhaps the result from the use of h (the thickness of the biogeochemical grid in the CICE model) in equations 5, 6 and 7 (4, 5 and 6, respectively, in the revised manuscript). In the CICE model, the biogrid is vertically resolved in a number of layers. In our simulations we used 15 layers (please refer Table S3). Therefore, the existence of terms in the equations where the whole thickness of the biogrid (h) is used should not prevent the correct calculation of vertically resolved processes between the various layers, as well as between the bottom of the sea ice and the ocean. So, why then does this term show up? This happens because the biogrid is non-dimensional and the position of a point along the grid (described by x) is zero at the top of the biogrid and 1 at the ice-ocean interface. Therefore, in the CICE transport equation for the biogeochemical tracers the total thickness of the biogrid must always be multiplied by x or its differential to “convert” a relative distance to an “absolute” distance in meters. We added text explaining these details while citing the relevant references (please refer lines 96-116).

You commented that *“nowhere in the text it is stated that this is only applied at the boundary”*, referring to our turbulent diffusion parameterization. Please note that the following text to outline this approach is provided at the end of the Introduction (lines 69-71):

“To test the above hypothesis, we use a 1D vertically resolved model and contrast results using the default diffusion parameterization and a “turbulent” parameterization analogous to that of momentum and heat transfer, at the interface between the ocean and the sea ice, based on McPhee (2008).”

Please note also that before equation 10 in the previous version of the manuscript version, we wrote:

*“We rewrite the last term of 8 **for the bottom ice layer** as:”*

In the revised manuscript we “reinforced” this statement in lines 120-123.

The 4th paragraph of your general comments begins with the following sentence:

“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)”

Please note that equation 8 (7 in the revised manuscript) is valid within the brines until the sea ice bottom. However, this does not imply that it cannot include ice-ocean interface exchanges which in fact define the

bottom model boundary. Its last term includes molecular diffusion and mixed length diffusion which are calculated between each adjacent pair of layers and, in the last model layer, are calculated across the ocean-ice interface as well. We replaced the molecular diffusion component with turbulent diffusion only across the ice-ocean interface. We have removed equation 10 as it is no longer necessary after the following sentence we added in its place (lines 120-123):

“The last term of equation 7 includes the contribution of molecular diffusion that is calculated at the interface of all layers of the biogrid and at the interface of the last layer and the ocean. In the simulations using turbulent diffusion, we perform the same calculations, except that the molecular diffusion term $\frac{\varphi D_m}{h^2}$ is replaced with a turbulent diffusion term $\frac{\alpha_s u^}{h}$ at the interface between the last model layer and the ocean.”*

In summary, we hope that our changes to the manuscript and responses have clarified that: (i) we did not assume turbulent diffusion within the brines was identical to the one in the ocean boundary layer; (ii) that we did not inappropriately handle the equations.

We removed the example of turbulent diffusion based on Cota et al. (1987) because, after reading more carefully the cited paper, we realized that turbulent diffusion was used in that study to calculate nutrient replenishment of the mixed layer and not exchanges across the ocean-ice interface. This has no influence on results or discussion presented.

We also removed most of the references to momentum exchanges following your suggestion.

Specific comments

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.

Answer: Done as suggested. Please refer lines 25-28.

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.

Answer: As outlined above, we removed this equation in the revised manuscript and hopefully with the changes described before this misunderstanding is resolved.

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.

Answer: We rephrased this to “calculate tracer diffusion across the ice-ocean interface” (line 60).

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.

Answer: This example was removed from the paper for reasons explained above.

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.

Answer: We write “*when brine gravity drainage is limited*” which implies we acknowledge other mechanisms stressed in the first paragraphs of the Introduction (line 64).

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?

Answer: The sentence was removed.

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).

Answer: Done as suggested.

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

Answer: The authorship of McPhee is acknowledged in the sentence. The reason we include the equation is to emphasize that the approach we adapted for nutrients is already implemented for heat in the CICE model. This is to reinforce our arguments about using consistent approaches to heat and dissolved tracers.

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 .

Answer: z is the vertical distance; x is the relative distance. So, we think z is used as you suggest. Here we followed exactly the notation used in the cited references for the sake of clarity.

Dear Dr. Martin Vancoppenolle,

Thank you very much for the time you took to review our manuscript and for your encouraging comments.

Please note that in the revised version we removed the previous Table 1, following your suggestion. We made some changes in the manuscript following the comments from Dr. Marcello Vichi and tried to clarify better a few issues. These changes have no implications in the paper concepts, results or conclusions.

With my best regards,

Tromsø, 9 November 2021

(Pedro Manuel da Silva Duarte, on behalf of all co-authors)