Dear Martin Vancoppenolle,

Thank you very much for your constructive comments that we used to improve the manuscript. In the following text we will address your comments and explain how they were incorporated in the revised manuscript. We follow the structure of your document with some general introductory remarks followed by **General comment** and **Specific comments**.

The citations used below were also used in the paper. Therefore, you may find the corresponding references in the bibliography of the manuscript.

In your introductory remarks you write:
"Whereas I believe the authors make a key, nice and novel point, the means they currently use are not robust enough. One key problem is that the processes below and above the ice-ocean interface are not clearly distinguished and treated together."

We believe that this results from a lack of clarity in our previous manuscript version. Since you develop further on this topic along your comments, we will try to clarify better our approach in this response and in the revised manuscript.

General comment

Your first critique concerns the lack of clarity about whether we act on the nutrient flux below the ice/at the top of the mixed layer or within the ice. Whilst the model setup we used considers both nutrient fluxes **within** the ice and at the **interface** between the mixed layer and the bottom ice (in the words of McPhee "salt balance at the **interface**"), our experiments acted only upon the **interface** nutrient fluxes. Please refer to the first paragraph of 2.1 Concepts, where we did some changes in the revised manuscript to make the whole ideas clearer:

" Eq. (1) from Cota et al. (1987) provides the basis for our reasoning about nutrient exchanges between the ocean and the sea-ice bottom being based on a turbulent exchange process enhanced by current velocity shear, irrespective of other exchanges based on brine dynamics, ice melt and ice growth. These turbulent exchanges may be parameterized through the flux of a quantity at the interface between the ocean and the sea ice, calculated as the product of a scale velocity and the change in the quantity from the boundary to some reference level (McPhee, 2008):"

Please note that we assume that nutrient exchanges at the interface are comparable to salt fluxes and governed by the same physics. Given the physical discretization of the biogrid (the biogeochemical model grid used in CICE), these interface fluxes directly affect the properties of the ice bottom layer only. These effects "propagate" vertically through diffusion within the ice or brine exchanges.

We did some changes to the caption of Table 1 to specify better our focus. Now it reads as:

"**Table 1.** Model parameterizations used/proposed by different authors to compute diffusion of nutrients. The only example based on friction velocity is that of Mortenson et al. (2017). "None" is used when exchange processes depend solely on ice growth/melting**."**

As you may see in Table 1 contents and in the changes made elsewhere in the text, now we clearly acknowledge that brine dynamics, as treated in some models, may be seen as a turbulent diffusion process. Please see also the antepenultimate paragraph of the Introduction that now reads as:

"Table 1 summarizes several models published over the last decades and their approaches to the calculation of tracer diffusion. 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."

Please see also the penultimate paragraph of the Introduction:
"From this assessment one may divide the ocean-ice exchange processes of existing biogeochemical models into those related to: (i) entrapment during freezing; (ii) flushing and release during melting;(iii) brine gravity drainage, driven by density instability, parameterized as either a diffusive or a convective process; (iv) molecular diffusion; (v) turbulent diffusion at the interface between the ocean and the ice induced by velocity shear – the focus of this study. In the absence of ice growth and when brine gravity drainage is limited, diffusive nutrient exchanges between the ocean and the ice have the capacity to limit primary production…"

We did a clear distinction between the turbulent diffusion driven by velocity shear, which is the focus of our study, and other forms such as molecular diffusion or diffusion driven by hydrostatic instability.

Your second critique is about the experimental setup, and you criticize the lack of explanations about which diffusion scheme we use. You also cite Jeffery et al. (2010) about the two existing diffusion schemes. We are not aware of the paper you cited but we cite a paper from 2011 (Jeffery, N., Hunke, E. C., and Elliott, S. M.: Modeling the transport of passive tracers in sea ice, J. Geophys. Res.-Oceans, 116, Artn C07020, doi:10.1029/2010jc006527, 2011) where two diffusion schemes are described. We use the Mixed Length Diffusion scheme described in this paper. This is now explained in the revised manuscript. Please refer 2.1 and 2.3.

We agree that here the reader may become unclear about the details of our settings, and the separation between within ice and interface processes, in line with your concerns. Therefore, we reordered the topics presented in 2.3 and, in the third paragraph, where we describe model settings, we write:

"We ran simulations with the standard formulations for biogeochemical processes described in Jeffery et al. (2016) and settings described in Duarte et al. (2017), using mushy thermodynamics, vertically resolved biogeochemistry, and including: freezing, flushing, brine molecular and mixed length diffusion within the ice and at the interface between the ocean and the sea ice as nutrient exchange mechanisms (Jeffery et al., 2011, 2016)...We contrasted the above simulations against others that replaced brine molecular and mixed length diffusion of nutrients at the interface between the ocean and the sea ice with diffusion driven by current velocity shear (Table 2), calculated similar to heat and momentum exchanges, and following the parameterization described in McPhee et al. (2008) and detailed above (equations 2 – 10)"

Please note also major changes in 2.1.

You also wrote "The limitation factors could be described with a sentence or two and values of the key parameters could be given, as the results refer to those extensively".

In the revised manuscript, we detail the values of the parameters used in the calculation of diffusion driven by velocity shear. We also add more details about Mixed Length Diffusion, but this is merely repeating info that is given in Jeffery et al. (2011). Therefore, we avoided many details and cited the mentioned authors for more info. Please see 2.1.

Your third critique is about a better description of the state of the art and more connection between our results and the existing literature. We will address below each of the points you raised.

Your first point is about the existing forms of enhanced diffusion described in papers that are already cited in our manuscript (we repeat here that we are not aware of the paper you cite "Jeffery et al. (2010)" but we cite Jeffery et al. (2011) which describes enhanced molecular diffusion and mixing length diffusion as parameterizations of gravity drainage). Please refer Introduction, 2nd paragraph:

"More recently, other authors have integrated formulations based on hydrostatic instability of brine density profiles, to compute brine gravity drainage and tracer exchange between the ice and sea water, based on diffusive (Vancoppenolle et al., 2010; Jeffery et al., 2011)…"

We changed this sentence to specify "enhanced diffusion" and make it clearer what was done by the cited authors:

"More recently, other authors have integrated formulations of "enhanced diffusion" (Vancoppenolle et al., 2010; Jeffery et al., 2011) or convection (Turner et al., 2013), based on hydrostatic instability of brine density profiles, to compute brine gravity drainage and tracer exchange within the ice and between the ice and the sea water. Comparisons between salt dynamics in growing sea ice with salinity measurements showed that convective Rayleigh number-based parameterizations (e.g. Wells et al., 2011), such as the one by Turner et al. (2013), outperform diffusive and simple convective formulations (Thomas et al., 2020)."

In this first point you also mention **"Still the present paper is original, because of the focus on the melt period"**. In fact, we are not focusing only on the melting period. Please note that we write in the penultimate paragraph of the Introduction:

"In the absence of ice growth and when brine gravity drainage is limited, diffusive nutrient exchanges between the ocean and the ice have the capacity to limit primary production …"

Moreover, our simulations also cover periods of ice growth, in the case of the refrozen lead, and periods when ice thickness was quite stable, especially in the case of second year ice.

We hope that the changes made to the last paragraph of the Introduction will help clarifying better the focus of this study.

Your second point is about the experimental support for advective approaches for brine convection. Thank you very much for letting us know about the study by Thomas et al. (2020), providing evidence for the accuracy of Rayleigh number-based parameterizations for predicting sea ice bulk salinity. We added some text to the paper specifying these findings by Thomas et al. (2020). A last sentence was added to the second paragraph of the Introduction:

"Comparisons between salt dynamics in growing sea ice with salinity measurements showed that convective Rayleigh number-based parameterizations (e.g. Wells et al., 2011), such as the one by Turner et al. (2013), outperform diffusive and simple convective formulations (Thomas et al., 2020)."

Your third point is about the description of physical processes, suggesting there is room for improvement and emphasizing two aspects: relative ice-ocean velocity and forced convection of brine. In the revised manuscript, we added a new paragraph at the end of 2.1 (partly following critics from the other reviewer - Marcello Vichi) where we specify better some of the relevant physical processes. Here we also refer to **relative ice-ocean velocity** when talking about "stream" velocities. Moreover, we added a sentence about the work by Dalman et al. (2019) and a sentence about **forced convection** at the end of this new paragraph and following your suggestion.

In the fourth point you suggest that what is original in our work is the introduction of the extra nutrient source when ice is not growing. In fact, such an extra source exists also in models that consider molecular diffusion at sea-ice interface. The main point of our study is to evaluate the effect of velocity shear in diffusion in line with "forced convection", for example. Moreover, this nutrient source is acting always. You write that you "suspect that NPP in the different experiments split exactly when the temperature gradient within the ice reverses, which switches off gravity drainage (brine convection)". In fact, looking into Figure 1b (dashed lines), you may see that NPP splitting occurs before middle May in the refrozen lead simulations and that was when ice was still growing, and brine being produced. But you are absolutely right when you mention the increase in the production period. We added a few words about this to the second paragraph of the Discussion.

In the fifth point you criticize the quality of some figures and suggest renaming the simulation experiments. Regarding figure quality, we believe that the problem is only with the figures included in the pdf version due to their low resolution. We will provide high resolution figures at 300 dpi. Concerning simulation naming, we prefer keeping them as they are for the sake of simplicity. If we add some suffix linked to the type of the simulation as you suggest, and considering that RL_Sim6 – 9 repeat RL_Sim1-4, except for the starting date, we would have to had a number or something else to distinguish the former from the latter simulations and we prefer to keep naming as simple as possible – this becomes more practical when it comes to insert legends within the figures, for example. We hope that the considerable reworking of Table 2 will help the reader tracking the various simulation experiments.

Specific comments (referee comments in italiscs)

l. 28. I would use « released » not « published »

Answer: Done as suggested.

l. 30 I would use « exchange » not « diffusion » of tracers, since there can also be advection of nutrients at the ice base

Answer: Yes, but here we are explaining the focus of our work which is on the diffusion and not in the exchange in general.

l. 34 I think here the references of Vancoppenolle et al QSR 2013 and more probably Thomas et al (The cryosphere 2020) would be appropriate, as they review such approaches.

Answer: Done as suggested.

l. 35 I would use « brine transport » instead of diffusive and convective fluxes

Answer: Done as suggested.

l. 33-42 Here the reference to Notz and Worster (JGR 2009) might be worth to consult and invoke, as they provide the current state-of-the art for brine dynamics. For gravity drainage, Wells et al (GRL 2011) would probably be best in terms of physical understanding.

Answer: We added a reference to Wells in the lines you suggested, when writing about Rayleigh formulations. In these lines we focus on processes used to model nutrient exchanges. However, in the following paragraph we mention desalinization and the main processes associated with it. Therefore, following one of your suggestions we added here a citation to Notz and Worster that was missing.

l. 43-49 Here I think the writing reflects some confusion between processes in the ice and below. In the cited papers, Vancoppenolle and Turner use an infinite salt / heat reservoir assumption (constant mixed layer salinity/temperature) and therefore do not need to specify what happens in the water column and use McPhee formulas.

Answer: It seems to us that irrespective of considering or not what is happening in the water column and even assuming it as a constant and infinite reservoir, changes in the ice brine nutrients, as a result of biogeochemical processes, imply gradients at the sea-ice interface. These gradients should drive some exchanges.

l. 43-49 In a 1D context, I would not refer to momentum transfer (it is not very useful since in such a context).

Answer: You are right in that the focus of the paper is not on momentum transfer. However, momentum, heat and salt or nutrient transfers occur altogether at the sea-ice interface and that is why we would prefer keeping the emphasis on this communality.

l. 57 use subscripts for « Fc » and Kz. Use SI units throughout the paper (m2/s).

Answer: Done as suggested.

l. 49 if you refer to âˆ�C, then units should be mmol/m3, if you refer to the product, then ok (but then writing needs to be corrected).

Answer: This was corrected, also following a suggestion made by the other referee. Now we present separately the units, instead of the “product units” as before.

l. 60 I think the reason why it has not been used is because many authors have modelled sea ice only, and not the under-ice water reservoir of nutrients (except possibly Tedesco and Vichi).

Answer: You may well be right but, it seems to us that the relevance of the process does not depend on the way you consider the under-ice reservoir because changes in the sea ice will create gradients at some point, irrespective of the constancy of properties under the ice, and these gradients should drive fluxes.

l. 66-77. Here I think you should more precisely describe the ice-ocean interface between what happens below (shear/buoyancy-induced mixing, cfr. McPhee) and above the interface (brine circulation, cfr Jeffery 2010, Vancoppenolle 2010, Thomas et al 2021).

Answer: We changed this paragraph in the revised manuscript according to your comments.

l. 66-68. « Brine drainage » as you refer to it should read « gravity drainage ». Flushing is understood as a brine drainage mechanism, so this should be reworded. I'd recommend to invoke Notz and Worster JGR2009 or Vancoppenolle et al QSR2013, which provide reviews on salt and nutrient transport physics, in order to be more precise in terms of wording.

Answer: Corrected as suggested. The mentioned authors are cited before these lines when talking about each of these processes in more detail.

Table 1 is misleading I think because what is meant by diffusion is ambiguous. For instance, Vancoppenolle et al 2010 use a diffusion equation within the ice. Also, the CICE model uses a diffusion that is not only molecular in the ice (see Jeffery et al 2011) whereas the table suggests it does. I would refer to « ocean-ice nutrient exchanges » and split between what happens below / within the ice. Below the ice, you could separate between the models which include some ocean reservoir, and the others which do not and assume infinite ocean reservoir. Next, you could possibly specify what models do within the sea ice. I would also remove the title of the paper in the column (« associated model»), which I found mismatching and not very helpful. You could also remove the table, I'm not sure it is useful.

Answer: Please note that the caption to this table was changed in the revised manuscript. We hope that now it is clear that we are only focusing on diffusive processes taking place at the interface between the ocean and the sea ice.

Your specific comments about Section 2 Methods

Answer:

We did several changes in Sections 2.1 and 2.3 which we hope are in line with many of the aspects mentioned in your comments.

We removed the dimensional equations as suggested.

The separation between within ice processes and those at the interface was addressed before following your comments. Here we focus only on interface diffusion.

We commented before about the usage of diffusion parameterizations in CICE to model brine gravity drainage.

The nutrient boundary condition at the ice-ocean interface is the concentration at the upper mixed layer and this is now specified in the first paragraph of 2.3.

We did not remove references to heat in 2.2 because doing so would make it difficult to explain the relationship between the transfer coefficients for heat and for salt/nutrients and thereafter justify the values used in our study.

We corrected the subscripts and defined all symbols as suggested.

L. 111-112 why multiplying D by porosity and what are these matrix coefficients ? I think ambiguities would be relieved if you gave the diffusion equation.

Answer: We removed the sentence about the multiplication by porosity and we added many details and some equations to this section, including the diffusion equations.

Your specific comments about Section 2.2

Section 2.2. I think it is nice to provide an implementation section, but this one looked a bit detailed for a scientific paper, especially because in comparison the physical / numerical implementation is not enough detailed.

Answer: Please note that the numerical implementation is detailed in Jeffery et al. (2016), cited now in 2.1. We detailed the implementation for the sake of transparency and to make it easier for other users to reproduce what we did. However, if necessary, we may simplify this section.

Setting a minimum value for u corresponds to assumptions on the relative ice-ocean current (you are assuming ice moves with respect to seawater, and it might help to acknowledge that).*

Answer: We mentioned relative ice-ocean velocity in the new (last) paragraph added to 2.1

l. 138-141 I felt this a bit pointless in the context of the paper.

Answer: These are practicalities about how we implemented our changes in CICE and Icepack. Without giving them, it might be difficult for someone else to reproduce our simulations. However, as stated above we may reduce the detail here.

Your specific comments about Section 2.3

Overall the section would read better if field experiments, forcing, initial conditions and sensitivity experiments were better separated.

Answer: We did quite some changes in this section in line with your concerns.

I would also suggest to work on more talkative simulation names (see generic comments), and work in parallel for the two sites.

Answer: We answered above to a similar comment.

Table 2 is exhaustive but was quite painful to read. You can gain in communication efficiency by making it more compact, and simpler, and keeping the details elsewhere in the text.

Answer: Please note that we changed radically Table 2.

l. 152 What do you mean by brine freezing? What do you mean by molecular diffusion?

Answer: We did not mean to imply "brine freezing" and we rephrased the sentence in the revised manuscript. We also specified "within the ice and at the interface between the ocean and the sea ice as nutrient exchange mechanisms" which is accounted for in CICE (refer Jeffery et al. (2011 and 2016) listed in the bibliography).

Section 3

l. 236. « Top of the brine network ». Where is that?

Answer: We rephrased the sentence to (1st paragraph of the Results section):

“Concentrations in the layers located between the bottom and the top of the biogrid, defined by the vertical extent (brine height) of the brine network (green lines in the map plots) (Jeffery et al., 2011)”

We also add more explanations about the biogrid in 2.1

l. 241. Higher limitation can mean both things (use stronger limitation or higher limitation factor?).

Answer: Replaced with “stronger” as suggested.

l. 299. If you refer to such a thing as « interface diffusivity », you should clearly define what is meant there. Also, I would rather look at the nutrient flux at the interface, than at the diffusivity.

Answer: We changed the last paragraph of 3.1 to:

“Interface diffusivity (one of CICE diagnostic variables, corresponding to the diffusion coefficient between adjacent biogeochemical layers and between the bottom layers and the ocean) for simulations with turbulent exchanges ($\alpha_s u^*h$) are up to two orders of magnitude higher at the bottom (diffusivity between the bottom layer and the ocean) than for control simulations with only molecular diffusion (D_m) or D_m + the mixed length diffusion coefficient (D_{MLD}) (refer 2.1 and Fig. 5).”

We also changed the legend of Figure 5 to:

“**Figure 5.** Daily averaged results for the refrozen lead (RL) simulations 1-5: Simulated evolution of interface diffusivity as a function of time and depth in the ice (note the colour scale differences between the various panels). In (a) interface diffusivity corresponds only to the molecular diffusion coefficient (D_m) or to D_m + the mixed length diffusion coefficient (D_{MLD}). In the remaining panels and at the bottom layer it corresponds to the turbulent diffusion coefficient ($\alpha_s u^*h$) (refer 2.1). Ice thickness is given by the distance between the upper and the lower limits of the maps. The upper regions of the graphs, above the green line with zero values, are above the CICE biogrid and have no brine network. The magenta line, partly covered by the green line, represents sea level. Refer to Table 2 for details about model simulations.”

Please note that we are showing only diffusivity to emphasize differences that are only due to the used formulation (refer 2.1). If we plotted instead nutrient fluxes, differences between both graphs would have resulted also from the nutrient gradients in the different simulations.

Section 4.

« replacing molecular with turbulent diffusion ». I’m not sure this is the correct wording for what has been done (see general comment).

Answer: This sentence was now changed to:

“The results obtained in this study support the initial hypothesis, showing that considering the role of velocity shear on turbulent nutrient exchanges between the ocean and the sea ice, formulated in a way consistent with momentum and heat exchanges…”

These changes were done for consistency with the way our hypothesis was reformulated in the revised manuscript to avoid confounding different forms of diffusion.

Regarding silicate half-saturation, there are papers in the Antarctic that have found the same thing (Lim et al., JGR 2019).

Answer: This study is cited in the revised manuscript. Please refer 4th paragraph of the Discussion section.

l. 367. Delta-Eddington parameter -> insufficient detail of what is meant here.

Answer: We explain what this parameter represents, and we refer sources where more details can be found (Urrego-Blanco et al., 2016; Duarte et al., 2017). In the revised manuscript we also specified that the model positive bias in June mentioned in the 6th paragraph of the Discussion is a shortwave bias.