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## Comment on the paper by Pedro Duarte et al.

Martin Vancoppenolle (Referee)

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Referee comment on "The importance of turbulent ocean-sea ice nutrient exchanges for simulation of ice algal biomass and production with CICE6.1 and Icepack 1.2" by Pedro Duarte et al., Geosci. Model Dev. Discuss., <https://doi.org/10.5194/gmd-2021-58-RC3>, 2021

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The paper by Duarte et al. on ice-ocean nutrient exchanges provides model evidence for a missing nutrient source late in the sea ice algal growth season in sea ice models. 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. Hereafter I try to provide constructive comments to improve the support for the key point to be made as well as to improve presentation and impact of the paper.

I do not make specific recommendations as to how to resist to the objections below as this could be done in various possible ways. I'm happy to discuss with the authors if they wish.

I also apologise for self-referencing, in the text below I tried to do it only when fully appropriate.

Best regards,

Martin Vancoppenolle

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### General comment

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**First**, the problem is not precisely posed, physically and mathematically. Currently, I am not sure whether the authors act on the nutrient flux below the ice (at the top of the the mixed layer, which is what McPhee et al are specifically referring to) or on the nutrient flux within the ice, right above the ice base, linked to brine dynamics and within the ice (which is what I'm afraid the authors acted on). Here, the authors should clarify how they treat the ice-ocean interface (upper mixed-layer, ocean-ice interface, ice bottom). Maybe a schematic would help to really focus the reader on what is changed in the model.

**Second**, the experimental setup is not well described, in particular the control conditions from which all simulations are compared to. The control simulation uses a diffusion scheme that is barely described, which I find problematic (in particular since there are two

different versions of that scheme in CICE, see Jeffery et al 2010). 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.

**Three**, the state of the art could be better described and the results could be more connected with the existing literature.

- Some forms of enhanced diffusion (including turbulent) approaches have been proposed in Vancoppenolle et al JGR 10 and Jeffery et al JGR 10, as a representation of brine convection during growth (Still the present paper is original, because of the focus on the melt period).
- In addition, diffusive approaches for brine convection were found less observationally supported than advective approaches (Thomas et al., TC 2020). Advective approaches have received wide theoretical support for gravity drainage of salt. I don't think this challenges the main hypothesis of the paper, but at least this would deserve to be provided with the context.
- Also, the physical description of the physical processes at stake can be improved. At this stage, the approach taken is a bit formal, and not very physical. Here is what I'm thinking of. Shear-induced convection in the water column is due to the fact that ice drifts (pack ice), or to the fact that there is a current under ice (landfast ice). Typically both ice drift and ocean current happen together and the relative ice-ocean velocity is the meaningful driver of the process. I think there has been some discussion of that process (thesis by J. Neufeld), also referred to as forced convection of brine in the literature. I remember I wrote a paragraph on this in the QSR review of 2013 (page 213), reviewing some works by the mushy layer community and by the Canadians in the 1980's.
- Since then, the group of CJ Mundy and J. Ehn have identified a nutrient source associated with tidal currents in fast ice (Dalman et al., Elementa 2019). I think what they refer to in that paper is very close to what the authors represent with a turbulent flux.

**Four**, what is original is, I believe, the extra nutrient source when ice does not grow anymore. I suspect NPP in the different experiments split exactly when the temperature gradient within the ice reverses, which switches off gravity drainage (brine convection). In this context, the extra nutrient source not only increases biological production, but also the length of the possible production period. This should be a bit explored and possibly recognized as a key point.

**Five**, whereas writing is quite decent, I think presentation can be improved. Figures are often blurred, some labels are hardly readable. Quite possibly there are too many of them (11) for the point to be made. The naming of experiments is not ideal. Sim1 could be referred as « CTL » to make clear that this is the control run. The other simulations could be grouped by process (RL\_TUR\_MIN, RL\_TUR\_MAX, ...). I think the names of the experiments could be worked at a bit more, to improve efficiency of the message, trying to isolate the different aspects treated in the paper (turbulent flux, biological tuning, ...). This would be easy to address and make the paper more accessible to a wider readership.

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**Specific comments**  
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**\* Introduction**

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

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

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.

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

l. 33-42 Here the reference to Notz and Worster (JGR 2009) might be worthwhile 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.

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.

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

l. 57 use subscripts for «  $F_c$  » and  $K_z$ . Use SI units throughout the paper ( $m^2/s$ ).

l. 49 if you refer to  $\hat{\rho}_C$ , then units should be  $mmol/m^3$ , if you refer to the product, then ok (but then writing needs to be corrected).

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).

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).

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.

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.

## **Section 2 Methods.**

This section would require some description of what happens both below and within the

ice (which transport equation, which formulation for diffusivity is used). The boundary condition at the ice-ocean interface should be given.

It is also particularly important to specify diffusivity, as there are possibly two options in CICE. As it reads, the manuscript implies that the default option is molecular diffusion, whereas all authors (including CICE developers) have clearly to date clearly established that the sole molecular diffusion is largely insufficient to deal with tracer transport within the ice (Untersteiner JGR 1968, Notz Worster 2009, Vancoppenolle et al JGR 2010, Jeffery et al JGR 2010, Thomas et al., 2021).

The dimensional equations looked a bit awkward. I would remove them, they do not help.

I would just keep the parallel to salt in this section (it is the important point), remove any reference to heat and momentum throughout the text. Possibly just mention at the beginning or the end, that heat (and momentum if you really want) can overall be treated similarly. The section would read better.

The use of subscripts should be done rigorously and precisely ( $D_m$ ) and of capital letters (e.g. kelvin degrees are capitalized). All symbols should be defined ( $w'$ ,  $S'$  are not defined).

I. 107-112. I felt your description of H was insufficient.

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.

**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.

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).

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

### **Section 2.3**

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

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

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.

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

### **Section 3.**

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

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

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.

#### **Section 4.**

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

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

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