Dear Marcello Vichi,

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 into the revised manuscript. We follow the structure of your document divided in **Introduction**, **General comment,** and **Specific comments**. We also considered the corrections you made in a second file.

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

1. **Introduction**

Here you show your disagreement with the idea that most of sea-ice biogeochemistry models do not include a proper treatment of turbulent exchanges between the ocean and the sea-ice brines. Under the **General comment** you further develop on this opinion. Our understanding is that this is your main critique of our manuscript, and we will focus our responses mostly on this aspect.

It seems to us that part of your disagreement results from the lack of clarity of some of our statements. In fact, we felt the same after reading the comments from the second referee (Martin Vancoppnelolle). Therefore, we implemented several changes in the revised manuscript in order to try to make it clearer.

We would like to emphasize that we are focusing on turbulent exchanges between the ocean and the sea-ice that are independent of ice growth/melting and of brine drainage and that are driven by current velocity shear. Whereas sea-ice growth or melting should not be confounded at all with our focus, since they imply bulk exchanges, brine drainage may well be confounded with the turbulent exchanges emphasized in our manuscript, since it may also be associated with turbulence (e.g. Jeffery et al., 2011). Please refer the synthesis at the penultimate paragraph of the Introduction section that we reproduce here after changes introduced in the revised manuscript (the bold type is just to emphasize the processes that are our focus):

“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**.”

As a result, the processes listed here include but are not limited to those mentioned in your review, as our focus is rather on the exchanges taking place irrespective of brine drainage and of ice growth/melting. We tried to better emphasize this idea in the revised manuscript. In the abstract we also added some text to avoid the implication that other sea-ice biogeochemistry models do not include any exchange processes. We changed the sentence:

“We hypothesize that biogeochemical models which do not consider such turbulent nutrient exchanges between the ocean and the sea-ice underestimate bottom-ice algal production.”

To:

“We hypothesize that biogeochemical models which do not consider such turbulent nutrient exchanges between the ocean and the sea-ice, **despite considering brine drainage and bulk exchanges through ice freezing/melting**, **may** underestimate bottom-ice algal production.”

We made compatible changes in the penultimate paragraph of the Introduction section, that now reads as:

“We hypothesize that models which do not consider **the role of current velocity shear on turbulent nutrient exchanges between the ocean and the sea-ice** may underestimate bottom-ice algal production.”

We also made other changes in the Introduction also related with comments from the other referee an in line with the need to clarify better our approach.

2. **General comment**

In the first paragraph, you seem to agree with one of our main points that **nutrient exchanges at the sea ice bottom interface should be consistent with the way heat and salt fluxes are parameterized**. In the next paragraphs you use McPhee’s formulation (presented as equation 1) to analyze further the various fluxes involved in nutrient exchanges and the underlying physical processes. Please note that we focused only on the first term of McPhee’s equation – the one correspondent to turbulent exchanges – whereas you focus on both terms. Please refer the last sentence in the first paragraph of the Introduction. We added a sentence to the last paragraph of the Introduction section to emphasize this focus:

“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).”

After presenting McPhee’s equation, you write that most of the earlier publications assumed that interface and far-field salinities are equal and, therefore, the term which is the focus of our study vanishes. If you mean “model publications” we argue that, in fact, several models (listed in Table 1) consider diffusion exchanges based on concentration differences between the ocean and the bottom ice (it seems that for practical reasons bottom ice concentrations are used as a proxy for interface concentration). The point we emphasize in our study is that by doing so, they do not treat it as a turbulent process consistently with the calculation of momentum or heat exchanges. This aspect is emphasized in the manuscript. Please refer to the first two lines of the Abstract and the penultimate paragraph of the Introduction. As a side note we emphasize several corrections made to Table 1 regarding the diffusion approaches used in various models.

When discussing the second term of McPhee’s equation you imply that when the first term is discarded, salt and, presumably, nutrient fluxes become mostly regulated by entrapment/release due to ice growth/melting, gravity drainage and percolation (these last two were added in your Correction comments). Whatever processes are included here, they depend on $w \neq 0$, i.e., ice must be either growing or melting or there must be some brine drainage. The processes we focus on, occur irrespective of ice growth or melting or brine drainage. So, if ice thickness is not changing and brine is not moving, you still have bottom exchanges of nutrients as long as gradients exist between the bottom ice and the water. It is the role of these and only these fluxes we are addressing here. Please refer the last paragraph of the Introduction.

Regarding the comment on the minus sign used in manuscript equation 4, we added an explanation in the revised manuscript, immediately after presenting the equation - the minus sign follows the CICE and Icepack convention of considering negative the upward fluxes.

You noted the apparent absence of dedicated measurements to demonstrate which approach is superior. We are not aware of such measurements either. However, the studies by Cota et al. (1987) and by Dalman et al. (2019) (cited in our manuscript) provide evidence for the possible importance of turbulent exchanges induced by current velocity shear, that are the focus of our study, in supplying nutrients to the bottom ice and stimulating algal growth. In fact, we are planning to experimentally address the bottom ice nutrient diffusion processes within the scope of a recently approved Research Council of Norway project called BREATH, under the lead of Karley Campbell (one of the co-authors of this manuscript).

You mention that we should more adequately address the difference between molecular and eddy diffusivity, and the way it has been employed in the literature. Perhaps we misunderstood you comment but it seems to us this is already done in lines 51-66 of the preprinted manuscript and also in Table 1. However, we also added a new paragraph to the end of section 2.1 about molecular and turbulent diffusion and comparing their magnitudes. Moreover, in the revised manuscript we refer to other forms of brine exchanges that also may imply turbulent diffusion but that are not induced by velocity shear – refer the antepenultimate and penultimate paragraphs of the Introduction.

You suggested that we should clarify the argument of changing the timescales by adding some more explanation. As we understood you meant that we should explain that what we did is pertinent to the CICE implementation of nutrient diffusion but may not be applicable to other models. In line with this interpretation, we added some text at the end of section 2.1 to emphasize the specificity of this “time scale approach” to the CICE model. Moreover, and in line with comments from the other referee, we added the transport equation to 2.1 and detailed the part of the equation that we changed.

Regarding your comment about our claim that the published models underestimate bottom sea-ice algae production because they do not resolve the turbulent fluxes induced by velocity shear, we merely found evidence supporting such hypothesis. But, as you noted and we commented above, we lack experimental evidence to support our conclusions, despite the works of Cota et al. (1987) and Dalman et al. (2019), suggesting that turbulence may indeed enhance bottom ice algal growth by increasing nutrient fluxes. Therefore, in the revised manuscript version we limited the perceived certainty of such claims. In the abstract we replaced “underestimate” by “may underestimate” and we added a sentence emphasizing the need for experimental evidence. Moreover, we added two sentences to the first paragraph of the Discussion that now became:

“The results obtained in this study support the initial hypothesis, showing that replacing molecular with turbulent diffusion at the interface between the ocean and the sea ice, formulated in a way consistent with momentum and heat exchanges, leads to a reduction in nutrient limitation that supports a significant increase in ice algal net primary production and Chl a biomass accumulation in the bottom ice layers, when production is understood to be nutrient limited. Therefore, our results are in line with empirical evidence provided by Cota et al. (1987) and Dalman et al. (2019) but, to the best of our knowledge, experimental evidence from properly dedicated experiments is still lacking to test our hypothesis. Moreover, our results do not imply necessarily that experiments carried out with other sea-ice models would render the same trends.”

In line with these changes we also did some changes in the first sentence of the Conclusions which now became:

"Considering the role of velocity shear on turbulent nutrient exchanges at the interface between the ocean and the ice in a sea-ice biogeochemical sub-model, leads to a reduction in nutrient limitation and a significant increase in ice algal net…"

3. Specific comments (referee comments in italics)

L58-59 I would suggest to report the units of ΔC and Δz separately, and not the units of the ratio

Answer: Done as suggested.

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.

Answer:
The sentences mentioned by the referee were reproduced above after rewriting for the revised manuscript. We hope that the changes done in this part of the manuscript (penultimate paragraph of the Introduction) make the whole meaning of our sentences clearer.

L88-89 The symbol α is not the same in the text and in eq. (2)

Answer: Corrected.

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

Answer: Done as suggested. Please see the changes we did in 2.1.

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.

Answer: Please note that in the first sentences under "2.3 Model simulations" we provide a general description of the model setup. At the last paragraph of this section, we give details on model forcing, including nutrient forcing. Following your advice, we added some words to this paragraph, specifying how water column forcing was considered in the model, namely, that ocean forcing is based on measurements carried out within the surface 2 meters. In the revised manuscript this last paragraph became the first one, following some critics from the other referee regarding the organization of this section.

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.

Answer: We did considerable changes in Table 2 to make it more readable. The relatively large number of simulations spanning two ice types and considering several parameter changes are not easy to follow. We hope these changes in Table 2 make it easier to understand the logic behind the various modeling experiments.

L213

The αs 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 10^{-5}).

Answer: We added the αs values to the text in section 2.1, 3rd paragraph. We also corrected all "powers" that had a multiplication sign.

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.

Answer: The organization of the figures in the manuscript and in supplementary info follows the same sequence and organization so it is not clear what is meant regarding the preparation of the figures with the experiments side by side. Figures either include experiments 1, 2, 3, 4 and 5 or experiments 6, 7, 8 and 9, in the case of the Refrozen lead and both in the paper and in Supplementary info. Sequences and organization for second year ice simulations are similar.

L249 "CICE tracers" should probably be "CICE diagnostics"

Answer: Corrected as suggested.

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.

Answer: We prefer keeping the present organization. Please note that we show in Figure 3 the main limiting factor results for the refrozen lead, which is Si. We also would like to keep Figure 5 in the paper because it shows results directly reflecting the usage of different diffusion approaches/parameters, which is one of the main topics of this study.

L262 I cannot see the magenta line

Answer: Parts of the magenta line are visible but most of it is under the green line. We added some text to the figure captions explaining this.

L288 I think the authors mean "followed by silicate".

Answer: Corrected as suggested.

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.

Answer: The sentences read as:

“Maximum Chl a values predicted for SYI are between two and three orders of magnitude lower than those predicted for the RL (Figs. 2 and 7). However, standing stocks for the former are larger than those for the latter, considering both observational and model data (Figs. 1b and 6).”

We changed this sentence to:

“Maximal Chl a concentration predicted for the RL_Sim1 and RL_Sim5 simulations - those closer to observations - are two orders of magnitude higher than those predicted for SYI (Fig. 2a and e *versus* Fig. 7). However, standing stocks predicted for RL_Sim1 and RL_Sim5 simulations are smaller than for SYI simulations, as confirmed by the observations (Figs. 1b and 6).”

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.

Answer: Please note that we are showing only diffusivity to emphasize differences that are only due to the used formulation (refer 2.1 and the many changes done in this section following suggestions from you and the second referee). If we plotted instead nutrient fluxes, differences between both graphs would have resulted also from the nutrient gradients in both simulations.

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.

Answer: In fact, light is more limiting in SYI Sim2 on June 1st, especially for the simulation with less snow (limiting light values lower => more limitation). The reason for this is the higher chlorophyll concentration in SYI Sim2 (please compare Fig. 11g and h), resulting from less Si limitation, and blocking light more efficiently – in the CICE model sea ice optical properties are influenced by chlorophyll concentration.