

We thank the Reviewers for their constructive comments that helped to improve the manuscript and strengthen the impact of our results. With the aim of addressing all the comments raised during the revision process, we made modifications to the code of SIMBA2 and to the parameters of both models. This was part of the tuning strategy to improve the comparison between models and observations. The structure of the manuscript remains basically unchanged, but the figures are different from the original manuscript. Moreover, we included in the text a section (2.5 Tuning) where we briefly introduce the tuning approach for all the case studies. In the supplementary materials we present a more detailed description of the tuning procedure.

The line numbers refer to the new version of the manuscript (to be provided at later stage).

## Rev1

Review: "Turbulence-Enhanced Nutrient Supply: A Key Driver of Algal Growth in the Arctic"

Castellani et al.

In this work, a new parametrization of ocean-ice nutrient fluxes is presented which combines molecular diffusion and turbulent exchange, with a transition between these two processes dependent on the under-ice flow regime. Authors tested this parametrization with 3 cases studies, comparing simulations with observations. With a sensitivity analysis, N fluxes across the ocean-ice interface are shown to vary strongly with bottom ice roughness. This research concludes that turbulent exchange of nutrients allows for higher ice algal growth than with molecular diffusion alone, and therefore sea ice biogeochemical models should include turbulent exchanges of nutrients.

The manuscript is well written and the interpretation of results and the main conclusions are supported by the model experiments. Some issues should be addressed, although these likely do not impact the main conclusions.

Specific comments:

1. Different values for parameter  $\alpha_s$  are used in the 2 models, resulting in SIMBA having an order of magnitude lower turbulent flux. (Maybe numerical instabilities in SIMBA could be addressed by using a smaller time step?). Almost no information is given on this parameter, how uncertain is it?

This difference in parameters would explain the differences in N flux between the 2 models (e.g. Figures 4 and 5), but no mention of this anywhere. This makes any comparison between the 2 models difficult to interpret, and authors should at least discuss the implications of this difference in parameters.

Values for  $\alpha_s$  from observations vary between  $8.6 \times 10^{-5}$  and 0.006 (McPhee et al., 2008). Thus, originally both Icepack and SIMBA2 used parameter values that were in the

range suggested. However, during the tuning procedure we tested different values in both CICE + Icepack and SIMBAv2.0. The final results that we present in the revised manuscript are obtained with the same value of  $\alpha_s$  for both Icepack and SIMBA2 for the MOSAiC case. However, for the NICE and Resolute case the values are different between the models (see Section 2.5 Tuning). Moreover, we account for the effect of stratification during melting by reducing the  $\alpha_s$  coefficient by  $\sim 1$  order of magnitude. This approach follows McPhee, (2008) and is based on the ratio between the two extreme values that he observed: 0.006 and  $8.5 \times 10^{-5}$ . With this approach, we most likely introduce an underestimation of the turbulence in the case of MOSAiC that already uses a small value for the  $\alpha_s$  coefficient, making our conclusions more conservative. The new Figures 5 and 6 present the fluxes computed with the exchange coefficient of  $8.6 \times 10^{-5}$  for both models (the fluxes are now presented per day and not per second anymore). The fluxes from the 2 models are now comparable in magnitude. Fluxes are a bit larger in CICE + Icepack due to the fact that CICE + Icepack builds up more algae than SIMBAv2.0. Moreover, fluxes are influenced by concentration differences between the brine and the water under the sea ice. Since Icepack and SIMBAv2.0 use different vertical resolutions and different numerical schemes, some small differences are expected.

This is now discussed at lines 311-315.

2. For MOSAiC, the fact that the sea ice and snow thickness data doesn't correspond to the same location than the ice algal Chl and ice N data location makes the assembled dataset maybe not very well suited to assess a new parametrization. But, there is at ice thickness data from ice cores (points in Figure 2c?), and it compares well with observations from the ice buoy data used for forcing (line in Figure 2c ?). Moreover, it appears that snow thickness data was recorded from the cores for ice N (see <https://doi.org/10.1594/PANGAEA.971385>). Why was this data not used? Authors should at least use this data to compare with the simulated snow depth.

Indeed we thank the reviewer for pointing to these inconsistencies in the paper. First of all there was a mistake in the explanation: Icepack is forced with meteorological (including precipitation) and oceanography data, whereas SIMBA2 was originally forced with ice thickness and snow thickness computed by Icepack (for the revised version we now use snow depth from observations). In Figure 3d of the revised manuscript we show average snow height and ice thickness from samples collected at the coring site (dots) and simulated values (lines) with CICE + Icepack. Despite the general model good agreement with observations for both snow and ice thickness, there is a small overestimation of snow height that was originally the reason why algae did not start growing early enough to match observations. Regarding the forcing data, these were collected in different stations located in a range of about 15km. The forcing data used in the present study following Fengguan et al. (2024) consist of an average of atmospheric data from Met City installed at the central observatory, and the three Atmospheric surface Flux Stations (ASFS) located in the distributed network, and average oceanic data from 8 CTD buoys. We thus believe that such data are representative of the conditions at the MOSAiC sampling area, which includes the

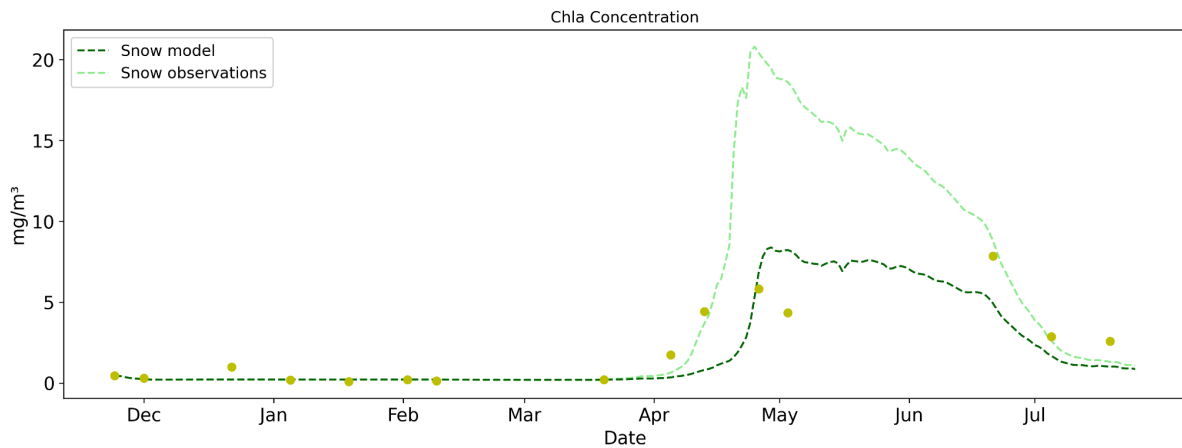
coring site. Data from the T63 buoys were used for comparison of ice thickness and snow thickness, but are not shown here. We corrected and reformulated the text from this paragraphs (see new Forcing paragraphs, as well as lines 171-172, 332-335).

The reasons for the positive bias in snow accumulation can be manifold: Icepack is not able to represent small scale snow redistribution due to wind forcing for example. Moreover, in the month of April there has been deformation ongoing which created rafting and ridging at the coring site, which caused redistribution of ice and snow. Icepack, as most 1D sea ice models, does not consider such dynamic processes. This is now discussed in lines 372-381.

Despite the limitations we have, regarding available data and/or model shortcomings in reproducing some observations, we would like to emphasize that our main goal does not entirely depend on fitting the model to observations but rather on the comparison of model results under different parameterizations. Therefore, observations are primarily necessary for forcing and/or to provide a realistic context to run the simulations.

3. Simulations reproduce poorly the timing of measured ice algae Chl for the MOSAIC and Resolute case studies, and as a consequence the timing of N uptake and maximum ocean-ice N flux is also not well simulated. This has little influence on some results (e.g. the sensitivity of the N flux to the roughness parameter  $h_s$ ) but it limits the assessment of the proposed parametrization. The authors base their confidence in the new parametrization and the need for turbulent exchange in part from the fact that simulations with turbulent exchange reach the observed Chl levels. But as the timing is off, other factors could be at play, with ice algae growing later when light is higher responsible for high growth.

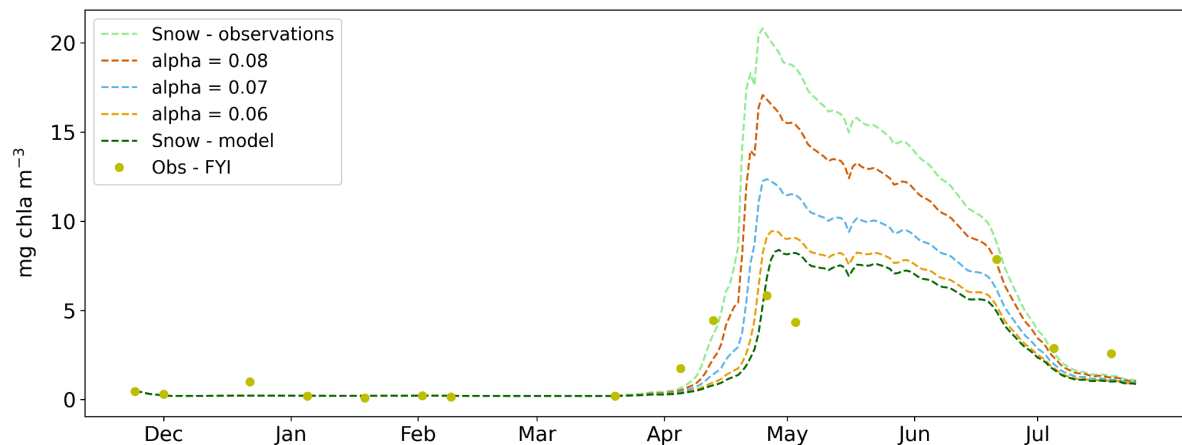
The timing of algal bloom in the case of MOSAIC is poorly represented because the model CICE + Icepack accumulates too much snow compared to observations. In order to first test this hypothesis and then to overcome this issue, we proceeded as follows: we first ran SIMBA2 with interpolated snow observations instead of snow thickness produced by CICE + Icepack. In this case (as it is now shown in the new Figure 5 of the manuscript) the initial growth represented by the model is in much better agreement with observations, also supporting our initial hypothesis (see Figure A1 in this document, we took the case of  $h_s = 0.0025$ ). We then tuned the PI parameter  $\alpha$  (the initial slope of the Production-Light function) in SIMBA2 to be able to reproduce the same onset of growth even with the snow simulated by CICE + Icepack. This is shown in Figure A2 (for the case of  $h_s = 0.0025$ ). We did not change how CICE + Icepack calculates snow thickness based on precipitation because, as mentioned above, other processes responsible for snow redistribution may have impacted the snow depth values measured at the coring site, rather than thermodynamic processes.



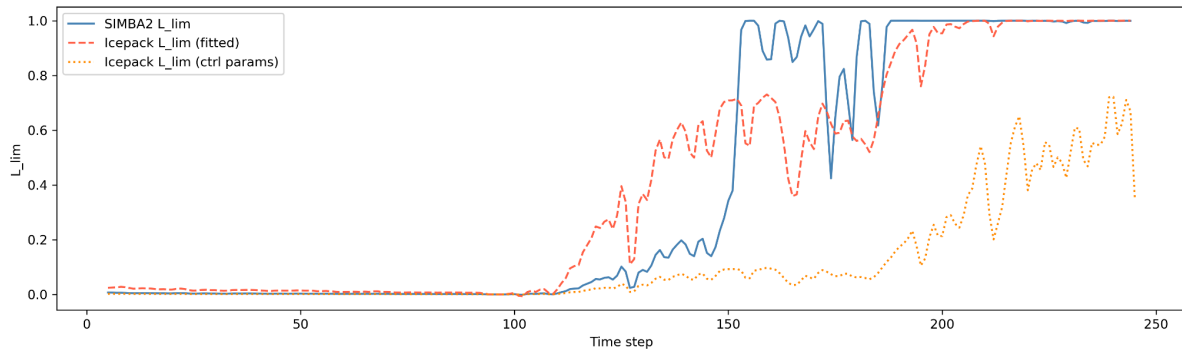
**Figure A1.** Algal growth simulated by SIMBA2 in the case of  $h_s=0.0025$  comparing the case when the model is forced by observations (light green line) and when the model is forced by the snow thickness computed by CICE + Icepack (dark green line).

However, there is no direct correspondence between the way SIMBA2 and CICE + Icepack calculate light limitation -SIMBA2 utilizes a Jassby-Platt formulation (Jassby and Platt, 1976), whereas CICE + Icepack uses the Platt et al. (1980) formulation with photoinhibition. Even if  $\alpha$  represents in both models the initial slope of the PI curve, the formulations and the units of  $\alpha$  are different. We thus tuned the values of  $\alpha$  in Icepack so that they match the PI curve in SIMBA2 (Figure A3). The simulations for the MOSAiC case are then carried out with the tuned parameters (new Figure 5 in the manuscript, and Table 2 with parameters). It has to be noted that by using a larger  $\alpha$  value the onset of algal growth happens earlier in the season, but the model also accumulates more biomass. All this is now included in section 2.5 of the new version of the manuscript, in the supplementary material and it is discussed in lines 218-220.

It must be emphasized though that any tuning efforts are difficult to support during the period of maximal growth (between April and June) due to the sparsity of observations.



**Figure A2.** Tuning of the alpha parameter in SIMBA2 when using snow thickness calculated by CICE + Icepack in order to match algal growth with the case of model forced by observations.



**Figure A3.** Fitting of the two formulations for light limitation from Icepack and SIMBA2.

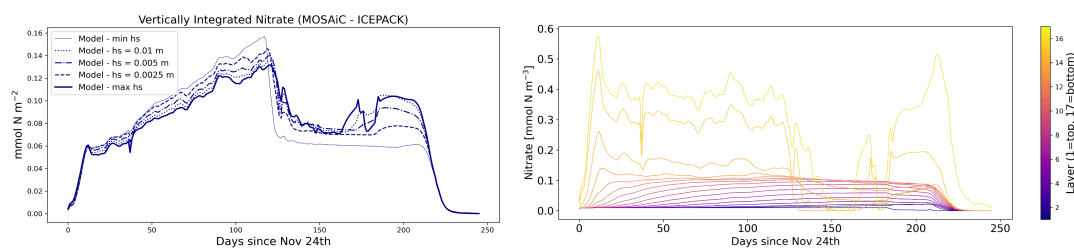
Regarding the Resolute case, the final tuned parameters are also presented in Table 2.

I think this study could be strengthened by a minor tuning of parameters, likely the photosynthesis parameter alpha, to adjust the timing of blooms. Authors state (L218) that they do not tune parameters "to ensure compatibility", but it isn't clear why and what compatibility the authors want. There are little comparisons between the different test cases and comparisons between the 2 models is problematic (see point 1). Also, the ice algae in the different sites are likely very different as they grow in different conditions, so it does not make sense to use the same photosynthesis parameters. If the purpose of using observations is to evaluate the new parametrization, I would think that simulations should be as close as possible to observations.

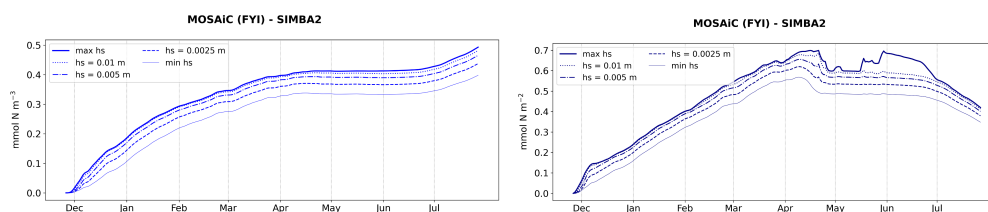
Agreed (see also our answers above). Following the suggestion of the Reviewer we tuned the alpha parameter for the MOSAiC and Resolute case. However, this did not completely solve the issue of matching observations in the case of MOSAiC, where observations are very sparse, and the physics of the system has proven difficult to reproduce. Indeed, besides the snow redistribution, the presence of a fresh water layer under the ice isolated the ice bottom (Smith et al., 2023, 2025) thus reducing the exchange of nutrients between sea water and ice bottom. These points are now presented and discussed in the manuscript in lines 342-345. This also shows that tuning can on one hand improve the comparison with observations, however it can do so by compensating for other shortcomings of the model or observations. This is also now mentioned in lines 381-383.

4. The discussion states (L275) that in the MOSAiC case, ice N equilibrates with ocean N but in the CICE simulation, ice N equilibrates to a lower value because the vertical resolution causes CICE to take longer to equilibrate. However, this explanation does not make sense to me: (1) N flux in CICE is greater than in SIMBA, from a greater alpha\_s parameter value; (2) it has over 6+ month to equilibrate, but actually no trend is visible in ice N from December to May. The initial uptake of N is fast and ice N seems to reach an equilibrium in less than half a month. So it is not clear why ice N is lower for CICE.

We understand that this point was confusing, mainly because the fluxes were of different magnitudes in the two models. Now this part has been corrected and the fluxes are calculated with the same  $\alpha_s$  parameter. It has to be noted that in both models nutrients that are fluxed into the ice are then transferred upwards. However, the two models are very different in nature (i.e., Icepack is vertically resolved and calculates tracer diffusion across its vertical layers and also brine convection, whereas SIMBA2 has only bottom algae and the transport of nutrients upwards is only due to remapping during ice growth/melt). The trend in nutrients is visible in the vertically integrated and vertically resolved time series for Icepack (Figure A4) and in the concentration of nutrients in the upper layers of the ice in SIMBA2. It has to be noted that in SIMBA2 the nutrients are simply stored in the above layer and are not used for any biological production. This also explains why SIMBA2 accumulates more nutrients. The bulk concentration at the bottom layer depends on the brine fraction. Icepack calculates brine fraction based on thermodynamics, whereas SIMBA2 assumes a permeable bottom layer ( $\phi = 0.75$ , value typical of skeletal layer). Such differences are also responsible for differences in bulk concentrations, with values in SIMBA2 much closer to sea water. This sentence has been revised (see lines 312-315 and 319-324 in the manuscript).



**Figure A4.** Icepack: Vertically integrated nitrate in the permeable part of the ice column (left panel) and nitrate concentration in each vertical layer (right panel) where layer numbers start with 1 at the top and go increasing towards the bottom.



**Figure A5.** SIMBA2: concentration of nutrients in the upper reservoir (left panel) and vertically integrated (right panel).

It would be interesting to show ocean N concentrations along with ice N concentrations, for comparisons.

Agreed, nutrients concentration in water for the MOSAiC case is shown in Figure 3 (bottom panel). For the Resolute and N-ICE2015 simulations, we refer to Mortenson et al. (2018) and Duarte et al. (2017), respectively.

5. Parametrization: A few details on the description of the parametrization could be added for clarity.

- what is the formula for a and b (equation 3)? Agreed, equations included (eq. 5 and 6 in the manuscript). We also included a figure in the supplementary material showing how a and b vary as a function of the Reynolds number.

- where does the relation  $hs=30 z$  (L93) come from? This isn't used in the rest and no information seems to be given on what this surface roughness parameter corresponds to or if it is used by the models. The relation comes from Shirasawa and Ingram (1991). However, since as the reviewer pointed out this relation is not used further in the manuscript, we removed it.

6. Giving N fluxes in per day might be more insightful, e.g. for comparison with the stock of nutrients in ice. Considering the values given L177, the N flux would be 0.108 mmol m<sup>-2</sup> d<sup>-1</sup>, N in bottom ice is 1 to 3 mmol m<sup>-3</sup> (figure 2) and you consider a 10cm bottom ice layer so an areal concentration of 0.1 to 0.3 mmol m<sup>-2</sup>. This indicates that the N flux replenishes the nutrient stock in 1 to 3 days. Agreed, fluxes in Figure 5 and 6 (and corresponding Figures in the supplementary material) are now shown as d<sup>-1</sup> rather than s<sup>-1</sup>.

Technical corrections:

abstract L15 reference to brine drainage affecting model simulations doesn't appear in main text. This sentence does not appear anymore in the abstract

L25 " After the onset of algal bloom" missing an or the Agreed. Corrected by adding 'an'

L46 missing a verb Agreed. Corrected by adding 'is'

L82 Is this value of the drag coefficient correct? In the given reference (Hunke et al., 2015) it is 0.00536 and typical values reported elsewhere are between  $1 \cdot 10^{-3}$  and  $24 \cdot 10^{-3}$  (Table 1 in Lu et al. 2011, <https://doi.org/10.1029/2010JC006878>). Also consider adding the formula used for friction velocity. This was a typo in the text, the value used is indeed  $5.36 \times 10^{-3}$ . It is now corrected in the new version of the manuscript.

Figure 1 caption typo: "Soncpetual" Corrected

Figure 2 line 3 "Starts in panel b" Probably a typo, should be panel c? are referring to initial conditions used to simulate ice thickness here? This Figure and the related caption have been modified.

L265 What is laminar layer? This was a mistake and it was changed to "molecular layer".

L325 " chl a max specific growth rate" I think it should be the maximum chl-specific growth

rate Agreed

L345 typo "sued" Agreed

## Rev2

Global Model Development (Ms. No. egosphere-2025-5384)

Title: Turbulence-Driven Nutrient Supply Sustains Algal Growth in the Arctic: A Modeling Approach

Authors: Giulia Castellani, Karley Campbell, Sebastien Moreau, and Pedro Duarte

### #Summary

The authors incorporated a new parameterization to calculate sea ice–ocean nutrient exchanges and examined its impact on Arctic ice–algal growth in three regions using two numerical models. The presented overall theory and story are basically reasonable, while I still have some concerns before recommendation for publication in the Global Model Development. Besides, more editorial improvement is also necessary.

### #Major Comments

I guess that sea ice freezing/melting also directly influence nutrient fluxes. For example, meltwater discharge directly flushes out nutrients from sea ice column and enhances stratification just beneath ice–ocean interface. The stronger stratification may restrict turbulence even under the same flow condition. Is any relationship between the simulated ice–ocean nutrient exchanges and sea ice freezing/melting considered in these simulations and analyses? Is any contrastive feature between freezing and melting phases detected? I expect more clear description and discussion on this issue.

Both models include exchange of nutrients due to growth and melt of ice at the bottom. All the exchanges are taken into account, including melting/freezing related exchanges. Particularly, Icepack also simulates vertical exchange of nutrients through brine drainage (see equation 3 in Duarte et al., 2022). The parameterization is used independently of the season. However, we do account for melting by lowering the  $\alpha_s$  exchange coefficient when the ice is melting (this approach follows McPhee, 2008). It has to be noted that processes related to melt would most likely affect the amount of nutrients that reach the under-ice layer (the boundary layer). So any melting process would rather affect the concentration under the ice that we use as a nutrient pool. In part this is considered since we use observations of nutrient concentration to force the model, which should reflect such effects. This is now better described in section 2.5 (Tuning) and at lines 180-182.

The authors indicated that the turbulent flux in addition to molecular diffusion has a significant impact on nutrient supply from ocean surface to ice–algal habitat. Sensitivity experiments regarding ice–bottom roughness supported their implication. My concern is potential strategies to represent spatiotemporal variability in ice–bottom roughness when this process is incorporated into traditional 3D sea ice–ocean models. I expect that the authors will provide a realistic approach for advanced 3D modeling.

This is indeed a very interesting topic. The simplest strategy for large scale models is probably to rely on  $h_s$  as a fixed parameter (a sort of realistic average) or to determine the value of such parameter based on ice age/type. One possibility for large scale applications could be to use information on deformation processes (e.g., formation of ridges) and retrieve values of bottom roughness based on deformation. However, not all models simulate ridging. Moreover, the scale of ridges may not be relevant for the scale of the molecular sublayer. If on one hand the parameterization presented here is a step towards more realism in representing nutrient transfer, targeted observations are needed to better relate such processes to ice deformation. This is now discussed at lines 303-308.

#Detailed Comments

= Title =

I suggest that “Algal Growth in the Arctic” will be reworded to “Ice-Algal Growth in the Arctic Ocean”, because there are algae living in the Arctic lakes. I guess that target of GMD covers wide research communities (not only oceanographers). [Agreed](#)

= Abstract =

>Lines 3, 4, and many others

I suggest to use “bottom” instead of “surface” throughout the manuscript, because the latter generally reminds of “top”. [Agreed, changed here and everywhere else in the new version of the manuscript](#)

= 1. Introduction =

>Line 57

Should “Bottom algae” be “Bottom Algae” to stand for “SIMBA”? [Yes, corrected.](#)

= 2. Methodology =

>Line 81: possible instabilities

Is this “numerical instability” (not turbulent instability)? [Yes, indeed SIMBA2 had numerical instabilities when using a larger value for the exchange coefficient. This is because SIMBA2 was running on a daily time step for the physics, which was too large. We changed this by allowing SIMBA2 to run on a smaller time step. We however changed the value of the](#)

exchange coefficient since what CICE + Icepack used was too large, so now both models run for what is suggested by McPhee (2008) for summer:  $8.6 \times 10^{-5}$ . So this line has been removed.

>Line 102 and others

The link of “https://https:” should be corrected. **Agreed**

>Lines 135–137 and Table 1

I suggest to add a spatial map showing these locations (e.g., cruise tracks). **Agreed, now this is figure 1 in the new version of the manuscript**

>Line 140

This exact name of MOSAiC should be introduced at its initial sentence. **Agreed. In the new version of the manuscript we introduce the acronym of the three case studies in the Introduction (see lines 60-61).**

>Line 158 and Table 1

“x” should be inserted between “0.6” and “ $10^{-3}$ ”. **Agreed**

>Figure 2

“Hi” and “Hs” should be “ice thickness” and “snow depth”, respectively.

Specifically, “hs” is already used as a different meaning. **Agreed, see new version of Figure 2**

“Starts in panel b)” should be “Dots in Panel c)”. **This Figure and the related caption have been modified**

= 3. Results =

>Line 170

“Section” should be inserted before “2.1”. **Agreed**

>Figure 3 and others

I suggest to relocate a unit (e.g.,  $\text{m s}^{-1}$ ) above a left axis such as Figure 2. **We followed the reviewer's comment in some of the figures where this change did improve how the figures would look like, but not in all of them.**

>Line 123: “during ice growth and melt, flushing”

I have same concern as my first major comment for this sentence. [See answer to general comment above](#)

= 4. Discussion =

Lines 239–243

The sentence of “This may be ...” is too long. I suggest to separate it. [Agreed.](#)

>Line 242– 243

Since Watanabe et al. (2019) is a model-intercomparison paper, it is better to cite Watanabe et al. (2015) in this context. Besides, macro-algae are also considered, but not a main focus in that study. Uptake of seawater nutrients is dominant only for specific condition (i.e., larger biomass). [Agreed.](#)

>Line 249

“McPhee, 2008, e.g.” should be “e.g., McPhee, 2008”. [Agreed](#)

>Line 252

“(Long et al., 2012)” should be “Long et al. (2012)”. [Agreed](#)

>Line 273

“MOSaiC” should be “MOSAiC” [Agreed](#)

>Line 339

“a” of “Chl” should be italic. [Agreed](#)

>Line 343

I suggest to rephrase “with thick snow light limitation” by “light limitation owing to thick snow”. [This sentence does not appear anymore in the revised version of the manuscript.](#)

>Line 349

“, preprint” should be “(Preprint)” like other parts. [This paper is now published and not a preprint anymore, so the citation has been changed here and everywhere else in the text.](#)

= 5. Conclusions =

>Lines 364–366

The sentence of “We argue” is complex. I suggest to revise it. [Agreed](#)

= Appendix A =

>Line 372

“Bottom algae” should be “Bottom Algae” to stand for “SIMBA”. [Agreed](#)

>Line 386

Should “k\_CN” be “k\_N”? [Agreed](#)

= References =

There are many strange repetition such as “<https://doi.org/https://doi.org/>”.

I suggest to recheck details of all reference lists.

>Line 626

Reference of Tedesco et al. (Preprint) should include its journal name.

[Agreed](#), we thoroughly checked the references (those repetition were caused by a formatting issue with Bibtex) and also updated those that are now published and not in preprint anymore.

## **References**

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