Rebuttal "Brief communication: Intercomparison study reveals pathways for improving the representation of sea-ice biogeochemistry in models" by Tedesco et al., 2025

Reviewer #1

The manuscript presents an intercomparison between 6 one-dimensional sea-ice biogeochemical models. Models were forced with the same atmospheric and oceanic data from a time series of observations from a refrozen lead and validated with associated chlorophyll *a*, nitrate, and silicate data. Models were first run with their default parameterizations, and then teams were asked to tune their models to best match the observations. The authors found that with tuning, most models were able to reproduce chlorophyll *a* timing and concentration but not so much nutrients. Model teams also differed in their tuning choices.

I think the exercise of model intercomparison is valuable and this is a timely moment to do one for sea-ice BGC, as the number of models has been increasing. I also think the exercise is well-structured and shows significant effort on the part of many people in independently running and tuning the models with the same set of forcings (not a given in other model intercomparisons) in order to reveal a process understanding.

We thank Reviewer #1 for their thoughtful and supportive evaluation of our manuscript. We appreciate the recognition of the significant collaborative effort involved in coordinating and executing this model intercomparison, as well as the timeliness and relevance of such an exercise in the context of a growing number of sea-ice biogeochemical models. One of our main objectives was indeed to promote transparency and comparability across modelling approaches, and we are encouraged that the reviewer acknowledges the value of running and tuning the models under a common experimental framework.

As the manuscript is currently written, however, it is difficult for a reader outside of the group to gain much insight or understanding from the exercise. I know this is meant to be a "brief communication", but I think it deserves a far more detailed paper, for which the changes would probably be considered major revisions.

We thank the reviewer for their comment. We will try to provide as many details as possible in the revision while staying within the constraints of the Brief Communication format, which we still believe is the most appropriate choice for this exercise because the primary goal is to present key results and insights from the intercomparison in a concise and accessible way, serving as a first step to highlight critical model differences and priorities for future development. A more extensive discussion of model-specific processes and tuning strategies will be better suited to follow-up publications or supplementary material. Please, see also related comments below.

1.) My main concern surrounds the brief treatment of A) describing what the models are doing

We thank the reviewer for their comment. Since all six models are based on previously published and independently developed frameworks, we chose to summarise only their major structural and biogeochemical features in Table 1. However, to enhance clarity and accessibility for readers less familiar with these models, we will include brief descriptions of each model in the Supplementary Material in the revised version of the manuscript.

and B) usefully synthesizing the outcomes. The manuscript sets up its goal as wanting to gain increased process understanding (rather than simply comparing model outputs, which is more the goal of intercomparisons like Watanabe et al. 2019 that they cite) but I did not feel like it went beyond listing individual model tuning in a way that informed my understanding of the sea-ice ecosystem.

We apologise for the lack of clarity regarding our objectives (lines 62-67 and 73-76). Our primary goal in this intercomparison was not to advance new process understanding of the sea-ice ecosystem per se, but rather to improve our understanding of how different models represent key processes and respond to a shared set of boundary conditions. In particular, we aimed to identify where model behaviour diverges, how tuning strategies vary, and what this reveals about current modelling approaches. We will revise the manuscript to better reflect this objective and to clarify how our synthesis highlights model-specific differences and common challenges in simulating sea-ice biogeochemistry.

What were key parameter values before/after tuning? Table 2/the list in L178-182 seems like it should be the heart of the Discussion but I didn't know why modeling teams chose to make those tuning adjustments, whether any choices were specific to model design or previous parameterization, or how model outcomes might be tied to their design. E.g., Can anything be said about models that are BL vs. DE or quota vs. Redfield? Does increased model complexity improve match to observations? Were the tuning changes still within reasonable values for BGC processes, or do they suggest that model physics might be off? What would this work mean for future sea-ice researchers, especially those adjacent to the author group?

We thank the reviewer for this thoughtful and constructive comment. We agree that a deeper synthesis of the tuning outcomes, model structures, and their implications would strengthen the manuscript. In the revised version, we will expand the Discussion to address the following points:

- We will clarify the reasoning behind each model team's parameter adjustments
- We will include a comparative discussion of how different model design choices, such as BL vs. DE formulations, or quota-based vs. Redfield-type stoichiometry, may have influenced both tuning strategies and performance relative to observations.
- We will confirm that the chosen parameter values remained within plausible bounds for sea-ice BGC processes and briefly discuss whether any compensatory tuning may point to mismatches in the physical environment.
- We will enhance the utility and transparency of the intercomparison for the broader community.

Lastly, depending on the direction of Discussion, it may be useful to include more model description than currently exists in the Methods. I understand the challenge of summarizing 6 different models, but sometimes there is a place for including key equations.

We agree that providing additional model details could improve the manuscript's clarity and usefulness to readers. While space constraints limit the extent of methodological detail we can include in the main text, we will add a concise summary of key equations (e.g., primary production, nutrient uptake, and light attenuation) in the Supplementary Material. This will complement the structural descriptions already in Table 1 and the expanded model summaries we are preparing in response to earlier comments.

2.) I would like to see physical variables from both the N-ICE observations and the models (those without prescribed physics). Even though the focus here is on BGC, the ice environment is critical for light and nutrient dynamics and thus for understanding sea-ice algal growth. Please consider adding another plot and adding to the Results and Discussion accordingly. Current places in the writing where the physics were alluded to but could use more backing were L129 and L143-144.

We agree that including physical variables is important for better contextualising the biogeochemical model performance. In the revised manuscript, we will add a figure comparing relevant physical variables from the N-ICE2015 observations with outputs from the models that simulate their own physics. We will also revise the relevant sections of the Results and Discussion to incorporate this comparison and reflect more clearly on how physical variability may have influenced biogeochemical outcomes across models.

3.) It is near impossible to make sense of the nutrient validation when there is only one time point, which the authors themselves acknowledge (L169-172). Have the authors looked into other time series, such as those from Green Edge, CASES (Cape Bathurst), Resolute Bay, etc.?

We agree with the reviewer that a more extended nutrient time series would improve the robustness of model validation. At the time of the experiment, the N-ICE2015 dataset was the only available time series that provided physical and biogeochemical observations at sufficient temporal resolution for all the variables needed to support this intercomparison. However, we acknowledge the value of additional datasets and, also to this end, we plan a second phase of this project.

The writing itself is generally good and clear. Here are a few line-by-line comments:

L8. Tromso is misspelled.

We thank the reviewer for spotting this typo. It will be corrected in the revised manuscript.

L37-38. Please consider adding a citation for the claim of significant effects throughout trophic levels.

We thank the reviewer for this suggestion. In the revised manuscript, we will add citations to support the statement, such as Post et al. (2013), who provide a comprehensive synthesis of

the ecological consequences of Arctic sea-ice decline, including cascading impacts throughout marine food webs:

Post, E., et al. (2013). Ecological consequences of sea-ice decline. Science, 341(6145), 519–524. https://doi.org/10.1126/science.1235225

L51. Please specify that this is maximum algal growth rate.

We thank the reviewer for this suggestion. We will add " algal" to the revised manuscript.

L67. I feel that for the last sentence of the introduction, this places a lot of emphasis on temporal variability, when your results are also about magnitude. Please consider revising.

We thank the reviewer for this helpful observation. In the revised manuscript, we will revise the final sentence of the Introduction, emphasising that the intercomparison explores differences in both the timing and magnitude of simulated sea-ice biogeochemical processes.

L124. Is 83 to 83N correct? If so, please include more details about the drift trajectory

We thank the reviewer for spotting this typo. The correct drift trajectory was from 83°N to 80°N. This will be corrected in the revised manuscript, and we will also include a map of the drift track in the Supplementary Material to provide additional context for the study setup.

L164-166. This sentence is relatively redundant for the information that it conveys. Perhaps trim to "Most models exhibited deviations in either phenology or bloom magnitude."

We thank the reviewer for their suggestion. We will modify the text accordingly in the revised manuscript.

L194-196. This sentence confused me. Something seems off with the "show to disagree" verb?

We thank the reviewer for their comment. We will revise the sentence in the following way: *"Overall, it remains unclear which element primarily limited algal growth, as the models either differ structurally—by excluding the element of interest—or apply different parameterisations."*

L220-221. This statement ("challenges encountering in simulating a refrozen lead") seems important to understanding the models-observations comparison, but it was never discussed before the Conclusion. Please consider treating this in greater detail in the Discussion.

We thank the reviewer for their suggestion. We agree that the unique challenges associated with simulating a refrozen lead warrant more detailed discussion. In the revised manuscript, we will expand the Discussion section to explicitly address these factors and their implications for the observed discrepancies between model outputs and field observations.

L230. This is a minor point, but please consider a more common word than "auspicabile"

We thank the reviewer for their suggestion. We will modify the sentence in the following way in the revised manuscript: *"A Phase 2 of the intercomparison would be highly valuable, potentially extending the study to the habitat variability characteristic of Antarctic sea ice."*

Figure 1. Please report n for the observations and clarify whether replicates are from different ice cores or technical replicates from the same core.

We thank the reviewer for their suggestion. In the revised manuscript, we will report the number of observations (n) for each variable shown in Figure 1 and clarify replicates in the caption.

Table 2. For SIESTA tuning strategy, what does “possibility to keep position” mean?
We thank the reviewer for their comment. It means that algae can actively move against brine movements. We will clarify this in Table 2 of the revised manuscript.