

Point-to-point Responses to Reviewers - v0 BGC Letters

Reviewer 1

The manuscript is well written and generally well organized, but there are many statements with no references. The authors put a lot of emphasize on frazil ice formation and where and when the frazil ice forms? I'm fully aware that the spatial extension of frazil ice formation is needed to estimate the importance of this mechanism in percent. At present state it would be better to focus on the modelling and how this model actually is working, and what is actually modelled?

We thank the reviewer for this comment. In the revised manuscript, we will shorten and consolidate the background discussion while retaining the information required to motivate the Antarctic scale extrapolation. We will also add references where statements are currently insufficiently supported. The revised text will make it more clear that the model is intended as a process-based proof of concept rather than a fully calibrated predictive model.

I'm not a modelling person, but it appears that the authors have to make a lot of assumptions to run the model.

Several model components are necessarily based on assumptions because direct winter observations of frazil formation, turbulence, supercooling, frazil crystal size distributions, and Fe incorporation are extremely limited. In the revised manuscript, we will make these assumptions more transparent and distinguish more clearly between field-constrained quantities, literature-based parameterizations, prescribed assumptions, and model-calculated variables. We will also state more explicitly that the purpose of the model is to test whether frazil-mediated scavenging can plausibly reproduce the magnitude of observed dFe enrichment, rather than to provide a fully constrained predictive simulation of Antarctic sea-ice Fe dynamics.

Further, it's not convincingly explained how frazil crystals select for dissolved iron and not any of the dissolved macro nutrients?

We do not intend to imply that frazil crystals selectively take up Fe in the sense of biological or thermodynamic selectivity. Rather, Fe and macronutrients differ in their physicochemical forms in seawater. Macronutrients such as nitrate, phosphate, and silicate are predominantly present as dissolved ions and generally follow brine rejection during freezing, whereas a substantial fraction of operationally defined dFe occurs in colloidal and organically complexed forms that can interact with particle and ice surfaces. In the revised manuscript, we will clarify this distinction and add supporting references when needed.

Is the dissolved iron adsorbed to the particulate matter, and then what is the difference between dissolved and particulate matter apart from different size classes? This should be explained in more detail.

The model represents the direct interaction between frazil crystals and the operationally defined dFe pool using an effective size for collision calculations. PFe is treated as a separate pool and is not explicitly simulated in the present model. In the revised manuscript, we will clarify that the 0.1 μm diameter assigned to dFe is an effective numerical representation motivated by the predominance of

colloidal Fe within the operational dFe fraction, and does not imply that all dFe consists of discrete spherical particles of this size.

There is a Fig. 1 where the processes are explained, but there is no explaining text? What is complexation and what is L and L' in Fig. 1? The differentiation between dissolved and particulate iron is quite unclear and should be improved.

In the revised manuscript, we will substantially simplify the figure and expand its caption. dFe and pFe will be explicitly distinguished using the operational 0.2 μm size cutoff, and the principal forms shown within the dissolved pool will be identified as Fe(III), Fe(II), organically complexed Fe (FeL), and organic Fe-binding ligands (L). We will also clarify the processes of complexation, photochemical/biological reduction, oxidation, reduction, and remineralization. The notation L', which is not essential to the present study and introduces unnecessary complexity, will be removed. The revised caption will also make clear that the figure provides general biogeochemical context and that the present model does not explicitly resolve the full Fe cycle represented in the schematic.

L19: Frazil ice formation also in the Arctic

We agree and will revise the wording to clarify that frazil ice formation occurs in both polar regions, while the present study focuses specifically on its role in Antarctic sea ice.

L22: should be upper 10 m of the water column

Agreed. We will revise the text to read "the upper 10 m of the water column."

L29: Several? Please specify and give references

In the revised manuscript, we will specify the major Fe-dependent processes and add supporting references. The sentence will be revised to state that Fe plays key roles in photosynthetic electron transport, chlorophyll synthesis, respiration, and nitrogen assimilation.

L31: What forms can be labile? Specify and give a reference.

In the revised manuscript, we will replace this wording with a more explicit description of its potential contribution to bioavailable Fe and provide a supporting reference. We will clarify that particulate Fe is generally less immediately available for biological uptake, although part of this pool can become bioavailable through dissolution and remineralization.

L32: There are more up to date references than 1990?

We will retain Martin et al. (1990) as the original demonstration of Fe limitation in Southern Ocean waters and add Ryan-Keogh et al. (2023), which provides recent evidence of widespread and increasing Fe stress in Southern Ocean phytoplankton. We will also revise the wording to avoid implying that low Fe availability completely prevents phytoplankton growth.

L34: Resuspension? That's only in coastal and very shallow shelf waters. The reference de Jong (2013) also adds wind-blow dust as a source of iron. Resuspension can only have limited spatial effect of the release of iron.

In the revised manuscript, we will distinguish more clearly between broadly distributed Fe sources and regionally important coastal and shelf sources. Sediment resuspension will therefore be described as an important regional source over continental shelves and in coastal waters rather than

as a major Southern Ocean wide surface water source. We will also explicitly include atmospheric deposition as a source of Fe and revise the associated references accordingly.

L47: The figure needs to be explained in much more detail. What are L and L'? Text at external inputs is difficult to read!

Please, see our reply above to Fig.1's comment. Furthermore, the readability of the external input labels will also be improved.

L51 is also enriched? Explain in more detail!

In the revised manuscript, we will specify that enrichment refers to concentrations in sea ice relative to the underlying seawater, and we will distinguish more clearly among the different variables. We will explain that Chl-a, POC, and pFe can all show strong enrichment in sea ice, particularly near the ice-water interface.

L49–54 No references here?

In the revised manuscript, we will add references to Janssens et al. (2016), Lannuzel et al. (2010), and Henley et al. (2023). We will also revise the wording slightly to distinguish more clearly between direct observations and interpretation.

Janssens, J., Meiners, K., Tison, J., Dieckmann, G., Delille, B., & Lannuzel, D.: Incorporation of iron and organic matter into young Antarctic sea ice during its initial growth stages, *Elementa: Science of the Anthropocene*, 4, 000123, 2016.

Lannuzel, D., Schoemann, V., de Jong, J., Pasquer, B., van der Merwe, P., Masson, F., Tison, J.-L., and Bowie, A.: Distribution of dissolved iron in Antarctic sea ice: Spatial, seasonal, and inter-annual variability, *J. Geophys. Res.*, 115, G03022, 2010.

Henley, S., Cozzi, S., Fripiat, F., Lannuzel, D., Nomura, D., Thomas, D. N., Meiners, K., Vancoppenolle, M., Arrigo, K. R., Stefels, J., Van Leeuwe, M. V., Moreau, S., Jones, E. M., Fransson, A., Chierici, M., Delille, B. : Macronutrient biogeochemistry in Antarctic land-fast sea ice: Insights from a circumpolar data compilation, *Marine Chemistry*, 104324, 257, 2023.

L69 barrier to particle incorporation. Any reference?

In the revised manuscript, we will add references to Ackley and Sullivan (1994) and Takahashi et al. (2025), supporting the lower efficiency of particle incorporation in columnar/congelation ice compared with granular/frazil ice. We will also revise the wording so that “barrier” is not interpreted as complete exclusion of particles, but rather as comparatively less efficient incorporation.

Ackley, S., & Sullivan, C.: Physical controls on the development and characteristics of Antarctic sea ice biological communities - A review and synthesis, *Deep Sea Research Part I*, 1583-1604, 1994.

Takahashi, K. D., Ito, M., Nojiro, N., Nomura, D., Sano, M., Aoki, S., Wongpan, P., Williams, G. D., Yamamoto-Kawai, M., Tamura, T., Moteki, M., Hirawake, T., & Makabe, R.: Algal enrichment process for newly formed sea ice in the Dalton polynya off East Antarctica during the late summer–early autumn, *Elem Sci Anth.*, 13 (1): 00038, 2025.

L71 Not only winds and waves also currents!

We agree and will revise the text to clarify that turbulent conditions conducive to frazil formation can be generated by winds, waves, and ocean currents.

L78–80 I suggest to move this section up below L53 which deals with incorporation in the sea ice.

We agree that the flow of this part of the Introduction can be improved. In the revised manuscript, we will reorganize these paragraphs so that the discussion of Fe incorporation mechanisms follows more directly from the observational evidence of enrichment. This will also help separate the discussion of physicochemical Fe interactions from the subsequent description of frazil and congelation ice growth modes.

L86 what is sediment mobilization

“Sediment mobilization” is the terminology used in the description of the PISCES biogeochemical model in Person et al. (2020), where it refers to the sedimentary Fe source represented in the model. To avoid ambiguity, in the revised manuscript we will replace this wording with “sedimentary Fe”.

L90 more? Compared to what?

In the revised manuscript, we will clarify that dFe is generally more readily available for phytoplankton uptake than particulate Fe, while acknowledging that bioavailability depends on Fe speciation.

L96 where is resuspension part of the model?

Bottom resuspension is not active in the present test case because the AWECS station used for the model application is located in deep water, so the computed turbulent layer does not interact with the seafloor. However, the model framework explicitly notes that bottom resuspension would need to be considered where the turbulence depth approaches the water column depth or where bottom currents are strong. This is described later in the Methods (L390-392). In the revised manuscript, we will clarify this distinction earlier so that resuspension is not interpreted as a process explicitly represented in the present simulation.

L116 Move figure 3 here where you present the data.

We agree. This is a presentation issue, and in the revised manuscript we will move Fig. 3, but also all other figures and tables, closer to their first citation.

L120 I’m not sure that a reference to a data set on Zenodo classifies for a reference, and here that Fe tends to follow Chl-a distribution in the ice core? It’s shown in the Lannuzel et al. 2016 paper.

The Zenodo dataset is intended primarily as a data resource and we agree is not the most appropriate reference to support the interpretation of the vertical relationship between Fe and Chl-a in sea ice. In the revised manuscript, we will cite the relevant peer reviewed literature, including Lannuzel et al. (2016), for this statement and retain the Zenodo dataset citation only where the underlying data compilation itself is referred to. We will also revise the wording to make clear that the Chl-a profiles are used as qualitative context for the likely vertical distribution of Fe, rather than as a direct proxy for unmeasured dFe concentrations.

L124 Why are macronutrients not shown? Could be in supplm.?

We agree that, because the contrasting behaviour of Fe and macronutrients provides useful evidence that the observed Fe enrichment is not simply a conservative freezing effect, the macronutrient observations could be shown. In the revised manuscript, we will include the available macronutrient data in the Supplementary Material and refer to them in the Results. We will also revise the text to state more clearly that most dissolved macronutrients behaved predominantly conservatively with salinity during sea-ice formation, while ammonium was an exception and showed enrichment that may reflect remineralization.

L129 Any reference on the statement?

In the revised manuscript, we will replace the statement that POC was “mainly composed of detritus” with a more cautious interpretation based directly on our calculations. The revised text will state that algal carbon represented only a minor fraction of measured POC, implying a substantial non-algal organic matter component, and that together with low Chl-a and predominantly conservative macronutrient behaviour this supports an important role for physical incorporation processes during early sea-ice formation.

L132 The figure is a conceptual model and not a result. Maybe combine Fig. 1 and Fig. 2? Further, is it snow coming from the clouds? A source of iron? A bit confusing.

Fig. 2 is a conceptual schematic, not a model result. The figure was already first cited in the Introduction, where the model concept is introduced, but in a revised manuscript we will place the figure itself directly in that section so that its conceptual role is immediately clear. We prefer to retain Figs. 1 and 2 separately because they serve distinct purposes: Fig. 1 provides the broader Southern Ocean Fe cycle context, whereas Fig. 2 illustrates the specific frazil-mediated processes represented in the model. The snowfall shown in Fig. 2 does not represent an Fe source. It refers to the initial crystal seeding used to initialize the frazil size classes, corresponding to one hour of snowfall following Smedsrud (2002), as described at L220-222. We agree that this was not sufficiently clear and will clarify this explicitly in the figure caption and in the text.

L133 followed by adsorption. Yes, maybe but this processes is absolutely essential and need further and more explanation.

We agree that this central process requires clearer explanation. We will also revise the terminology. The model does not explicitly resolve molecular adsorption kinetics or surface chemistry, rather, it calculates encounters between frazil crystals and the operational dFe pool and represents successful attachment through an aggregation efficiency factor (α). In the revised manuscript, we will therefore distinguish more carefully between collision, aggregation, and adsorption, and explain how α parameterizes the probability that an encounter results in Fe-bearing material becoming associated with a frazil crystal. We will revise both the model description and Fig. 2 caption accordingly.

L140 rising? In the sense of increasing or upward vertical velocity?

By “rising” we mean the upward vertical motion of positively buoyant frazil crystals and frazil-Fe aggregates toward the ocean surface. In the revised manuscript, we will clarify this terminology and only use “rise velocity” where appropriate.

L141 much like snowflakes? Please specify what that means?

We agree that the comparison to snowflakes can be seen as too informal. In the revised manuscript, we will remove this analogy and describe the crystal morphology directly.

L144 Any references?

In the revised manuscript, we will add references: Sherwood (2000) and Smedsrud (2002) (already included in the manuscript), and the new reference, MacFarlane et al. (2014). Those references support the simplified representation of frazil crystals as disks in frazil ice and particle aggregation models. We will also revise the text to clarify that this is an idealized modelling assumption adopted because natural frazil crystals can have more complex morphologies and available in situ observations remain limited.

MacFarlane, V., Loewen, M., and Hicks, F.: Laboratory measurements of the rise velocity of frazil ice particles, *Cold Regions Science and Technology*, 106, 120-130, 2014.

L147 75% of the dissolved fraction and 25% of dFe's is in the soluble form. A bit confusing as the dissolved fraction is soluble or what?

In the revised manuscript, we will make it more clear that dFe is operationally defined as the fraction $<0.2\ \mu\text{m}$, and that this dissolved pool can be further divided into colloidal and soluble fractions. Following Lannuzel et al. (2014), approximately 75% of the dFe pool is considered colloidal ($0.02\text{--}0.2\ \mu\text{m}$) and approximately 25% soluble ($<0.02\ \mu\text{m}$). We will also revise the text accordingly and make clear that the $0.1\ \mu\text{m}$ diameter used in the model is an effective numerical representation of the operational dFe pool for collision calculations, rather than a literal particle size for all dissolved Fe.

L147 on page 1 its' $<0.2\ \mu\text{m}$?

We agree that this was unclear. We will clarify the operational size definitions and use them consistently throughout the manuscript. Dissolved Fe (dFe) is defined as the fraction passing a $0.2\ \mu\text{m}$ filter, and this pool includes soluble Fe ($<0.02\ \mu\text{m}$) and colloidal Fe (approximately $0.02\text{--}0.2\ \mu\text{m}$). We will revise the wording at L147 accordingly to avoid ambiguity at the nominal filter cut-off.

L154, yes, but what model?

We thank the reviewer for pointing this out. The original wording was imprecise. The turbulence depth was not calculated using a full standard $k\text{--}\epsilon$ turbulence closure, but with a simplified one-dimensional turbulence parameterization in which turbulent kinetic energy and dissipation decrease with depth from wind-forced surface values. The base of the turbulent layer is defined as the depth at which both quantities fall below prescribed threshold values. We will revise the manuscript to describe the approach accurately and provide the relevant model assumptions and parameter values in the Methods.

L156 Do the numbers refer to the bottom section of the ice? The bottom 10 cm was collected for analyses as mentioned some where. In that case I'm surprised that sea ice temperatures are that low?

The values reported in Table 1 refer to average physical properties of the full sea-ice core, not specifically to the bottom 10 cm. In contrast, the biogeochemical measurements reported in Table 2, including dFe, were made on the bottom 10 cm section of the cores. We will clarify this distinction in the table captions.

L169-179 this could be shortened and condensed.

In the revised manuscript, we will shorten this paragraph and retain only the assumptions directly relevant to the model application. We will clarify the adopted grease ice composition, the assumption of a well-mixed turbulent surface layer, and the treatment of frazil accumulation into grease ice, while moving or removing more general descriptive material that is not required to understand the simulations.

L181 I checked some of the enrichment index numbers in table 2 and get 306 for Chl-a > 10 μ m and not 366 in core 488, and only 5912 in core 496. Check again. I get same number of dFe enrichments. Check again.

We thank the reviewer for checking these values. After reexamining the table, we found that the enrichment index calculations themselves were performed using the original full precision concentration values and are correct. The apparent mismatch arises because several seawater and sea ice concentrations were displayed in the table rounded to only two decimal places, so recalculation from the displayed values does not reproduce the reported enrichment indices exactly. We will revise the table to show additional decimal places. In doing so, we also identified one typographical error in the seawater dFe value at station 489, which should be 0.33 rather than 0.20 and will be corrected. We will also take this opportunity to revise the table layout to align with the journal's formatting requirements.

L207 When crystal structure is important I suggest to enlarge these images and replace them with the chl-a graph.

We agree that the ice texture images should be more prominent. However, we will retain the associated Chl-a profiles in the same figure because their juxtaposition is intentional and helps illustrate the correspondence between ice texture and the vertical distribution of biomass. However, in the revised manuscript, we will enlarge Fig. 3 to occupy the full available page width so that both the ice texture images and the accompanying Chl-a profiles are clearer.

L232 ocean current velocities of 0.7 m s⁻¹ is a very strong current, and this factor should have been discussed in more detail considering the turbulence levels and frazil ice formations.

We will clarify that this value is used only in the grease ice thickness parameterization of Smedsrud (2011), which is retained as an independent consistency check on the frazil inventory. The reference frazil production is now constrained separately from the atmospheric surface energy balance and therefore does not depend on the assumed ocean current velocity. We will also make clear that horizontal currents are not explicitly resolved in the one-dimensional frazil-dFe model.

L236 Yes, but how much below? Give a number.

We agree and will quantify this explicitly. During the three weeks preceding sampling (3-23 June), ERA5 2 m air temperature averaged -16.6°C , ranging from -24.9 to -3.6°C . Air temperature remained below 0°C throughout the entire period.

L242. From where do you get the data points Fig. 4? Aggregation factor (?) on the x-axis. It does not look like alpha.

The points were generated from separate model simulations in which the aggregation factor α was varied while the other model parameters were held constant. For each value of α , we recorded the time required for simulated sea-ice dFe to reach the observed concentration of 1.93 nmol l^{-1} . We will clarify this explicitly in the Methods and figure caption.

L258 larger crystals? How larger? Give a number.

In the revised manuscript, we will clarify the frazil crystal size range explicitly using the size classes adopted in the model.

L265-282 A very long and extended discussion about frazil ice formation. Consider to condense and shorten. Further I would start the discussion with the modelling results, which is what this paper is about.

In the revised manuscript, we will substantially condense the general discussion of frazil ice formation and reorganize the Discussion so that it begins with the main modelling result. The physical characteristics of frazil formation will then be discussed only if needed to interpret the simulated dFe enrichment and its uncertainties.

L299 Where do I see that the model reached this values?

In the revised manuscript, the modelled dFe concentration used for the Antarctic scale estimate will be shown explicitly in the revised model results figure, together with the corresponding model assumptions and simulation time. We will also revise the text so that the reported concentration can be directly traced to the model output rather than appearing only in the Discussion.

L305-306. Maybe I missed it, but why are there different concentrations of iron in the wasters between the years? It's 3.2 Gg both years 2023 and 2002. Is there any difference?

The two water column dFe inventories are different, but this difference was obscured by excessive rounding in the original manuscript. Using the same assumed seawater dFe concentration of 0.38 nmol l^{-1} , the estimated dFe inventory in the upper 10 m is approximately 3.22 Gg for 2023 and 3.52 Gg for 2014, reflecting the different sea ice-covered areas used for the two years. We will correct the reported values and clarify the basis of the calculation in the revised manuscript.

L315 enrichment. Any reference here?

We will add references supporting the role of turbulence, frazil crystal size and supercooling in controlling frazil formation and particle-crystal interactions, and revise the wording so that their influence on Fe enrichment is presented as a model-based expectation rather than a directly observed relationship.

L317 multi-parameter. Yes, but specify the parameters! Targeted field campaigns. Yes, what samples should be collected and what should be measured to adequately run an improved model?

We will make this statement more specific. In the revised manuscript, we will identify the principal parameters that require sensitivity testing, including the aggregation factor α , frazil crystal size distribution, turbulent layer depth and intensity, frazil residence time, and supercooling. We will also specify the field measurements needed to constrain these processes, namely observations of frazil crystal size and concentration, turbulence and mixed layer structure, surface energy fluxes, and simultaneous dFe measurements in seawater, frazil/grease ice and newly consolidated sea ice.

L320-333 this section is a bit out of focus and related to more general conditions.

In the revised manuscript, we will substantially condense the discussion of future Antarctic climate and retain only the implications directly relevant to frazil-mediated Fe enrichment. Specifically, we will focus on how changes in sea-ice duration, surface cooling and turbulent frazil formation may alter the opportunity for this mechanism to operate, while avoiding a broader discussion of regional atmospheric circulation and polynya dynamics that is beyond the scope of the present study.

L340 and particles. What particles? PFe?

The term “particles” was misleading because the present model does not explicitly simulate pFe. Rather, it represents collisions between frazil crystals and the operationally defined dFe pool using an effective size for the collision calculations. We will revise the wording to make clear that pFe is discussed only as complementary observational evidence and is not included as a modelled pool.

L342. Remineralization. This needs to be clarified and discussed in more detail or at least give a reference.

We will clarify that particulate Fe may contribute to the dissolved pool through processes including dissolution and microbial remineralization of Fe associated with organic matter. We will revise the wording accordingly.

L348-353. Very long. Try to condense.

In the revised manuscript, we will condense this passage substantially and retain only the points directly relevant to interpreting the frazil-mediated Fe enrichment mechanism and its uncertainties.

L357-361. Also condense if possible. It's quite general statements.

We will condense the concluding discussion to avoid repeating general statements about model limitations and future research needs. The revised text will focus specifically on the main remaining uncertainties in frazil-dFe interactions and on the observations required to constrain them.

The manuscript needs some revisions and clarifications before accepted for publication. I acknowledge the authors courage to carry out this preliminary work based on a very limited field data-set with many assumptions to run the model.

We thank the reviewer for these constructive comments, which will help us improve the focus and clarity of the manuscript. We will substantially streamline the Discussion and will refocus it on the modelling results, the proposed frazil-mediated dFe enrichment mechanism, and its main uncertainties.