

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

Reviewer 2

Review of Tedesco et al., “Frazil ice formation as a pathway for iron enrichment in Antarctic sea ice” submitted to the Cryosphere journal.

Using observations from an Antarctic sea-ice core (including measurements of snow and ice thickness, ice texture, temperature, salinity, and dissolved iron (dFe) concentrations) together with seawater temperature, salinity, and dFe data, laboratory experiments, and a newly developed dFe entrainment model, this study suggests that frazil ice formation in the Southern Ocean can efficiently capture and store dissolved iron from seawater. Upon melting, this iron is released into surface waters, where it can stimulate phytoplankton blooms, thereby linking sea-ice processes to ocean productivity and carbon uptake.

The topic of the manuscript is highly relevant as very little knowledge exists on frazil ice formation and iron entrainment and as such it is worth publishing. The new dFe entrainment model, however, is subject to uncertainty, as it depends on several empirical parameters and assumptions necessitated by the paucity of observational data for calibration and validation. I find some of the presentations and argumentation difficult to follow and would like the authors to take my comments into account to make it clearer what is going on.

We thank the reviewer for the positive assessment of the study. We agree that the model is necessarily subject to uncertainty because several processes remain poorly constrained observationally. In the revised manuscript, we will clarify the assumptions and parameter choices, make the sensitivity analyses more transparent, and improve the overall clarity of the modelling framework and results.

You state that you combine state-of-the-art field, laboratory, and model-based knowledge of frazil ice formation and consolidation into pack ice, particle scavenging, and resuspension to quantify dFe adsorption and entrainment into newly formed sea ice. However, there is no description of your laboratory experiment in your method. I would recommend you add more information of Figure 4 in the methods.

No new laboratory experiments were conducted as part of this study. Our intention was to indicate that the modelling framework integrates field observations from AWECS with process understanding and parameterizations derived from previously published laboratory and modelling studies. We agree that the original wording could be interpreted as referring to laboratory experiments performed in this study, and we will revise the text to make this distinction explicit.

How did you obtain the data?

We agree that the origin of the points shown in Fig. 4 should be explained more clearly. These points are not observational data. Each point was obtained from a separate model simulation in which the aggregation factor α was varied while the other model parameters were kept fixed. For each value of α , we recorded the time required for the simulated sea-ice dFe concentration to reach the observed

value at station 496. We will describe this procedure explicitly in the Methods and clarify it in the revised figure caption.

Why do you use such a poor fit for the data? Could you estimate some uncertainty in your model sensitivity by making different fits to the data?

We agree that the fitted curve in the original Fig. 4 was not particularly informative and could give the impression that the fit itself represents an additional source of model uncertainty. In the revised manuscript, we will remove the emphasis on the empirical fit and present the model-derived sensitivity directly. Uncertainty will instead be evaluated through sensitivity to physically relevant model parameters and assumptions, including the aggregation factor α , interaction time, frazil inventory, and turbulent layer depth.

With regard to absorption of iron into sea ice, you state that pFe is enriched in sea ice compared to seawater, and in your Figure 2 schematic representation of the key processes contributing to the high iron enrichment in Antarctic sea ice write that during winter, seawater supercooling leads to frazil ice formation, followed by the **adsorption** of dFe onto ice crystals. Could pFe not be the inoculum for frazil ice formation itself? Or could both things not happen? The pFe trapped in the frazil could then later enter the dissolved phase also.

We agree that both processes may occur. PFe can potentially be physically incorporated into frazil and grease ice during ice formation, while the operationally defined dFe pool may also interact directly with frazil crystal surfaces. Incorporated pFe could subsequently contribute to the dissolved Fe pool through dissolution or remineralization. However, the present model explicitly represents only the interaction between frazil crystals and the dFe pool. PFe incorporation and exchanges between particulate and dissolved Fe are not resolved. We will clarify this distinction in the revised manuscript and in Fig. 2, and identify pFe incorporation and pFe-dFe transformations as processes for future model development.

Specific comments:

Figure 2: You could consider adding a red dot in some of the frazil ice crystals in winter also. See my comment above.

In the revised Fig. 2, we will more explicitly show Fe associated with some frazil crystals during winter to illustrate that Fe-bearing material can become incorporated during frazil formation and aggregation. We will also revise the caption to clarify that the schematic represents Fe association with frazil crystals without implying that only one Fe phase or incorporation pathway is involved.

Figure 3: Could you plot the iron concentration data to this plot?

We thank the reviewer for the suggestion. However, the Fe measurements do not have the same vertical resolution as the Chl-a profiles shown in Fig. 3. dFe was measured in the bottom 10 cm section of the ice cores rather than as a vertically resolved profile. We will retain the dFe concentrations in Table 2 and clarify the sampling interval in the text and figure caption.

Figure 4: See comment above.

As noted above, Fig. 4 will be revised to clarify the origin of the model-generated points, remove the empirical fit, and present model sensitivity directly.

Table 1: Are data presented average of whole core measurements? - what is the difference from Table 2? Or the thickest ice corer?

We thank the reviewer for pointing out that the distinction between Tables 1 and 2 was not sufficiently clear. Table 1 reports surface seawater properties together with whole-core average physical properties of the sea-ice cores. Table 2 instead reports the physical and biogeochemical properties of the bottom 10 cm section used for the Fe analyses. We will revise the table captions and accompanying text to make this distinction more explicit.

Table 2: What are the “ * ” ? Estimated concentrations? If so – how did you do it? As I understand only corer 496 had iron concentration measurements.

We thank the reviewer for pointing out that the meaning of the asterisks was not sufficiently clear in the original table. Fe concentrations were measured in sea ice at all three stations. Station 496 was selected for the modelling because it was the only station consisting entirely of granular ice. In Table 2, * and ** indicate missing seawater dFe and pFe values, respectively, which were replaced by the corresponding seawater concentration measured at station 496 for calculation of the enrichment indices. We will clarify this explicitly in the revised table caption.

Table 3: Raising velocity of aggregates are missing “time” in their unit! Right now, only in μm , but should be $\mu\text{m}/\text{time}$? What is the time?

We thank the reviewer for identifying this error. The quantity reported is a velocity, and we will correct the units in Table 3 to m s^{-1} .

Line 215: Any data to back up supercooling of 0.040°C ? T,S profiles in the water column from the station seems to exist. Why not use data instead of assumed values?

We have revised the model so that frazil production is no longer calculated from a fixed supercooling value. Instead, the frazil inventory is constrained independently from the atmospheric surface energy balance over the period preceding sampling. We will therefore remove the prescribed 0.040°C supercooling from the reference simulation and clarify that the available temperature and salinity observations are used to characterize the station conditions rather than to impose a fixed supercooling magnitude.

Line 246-263: Could you include an estimate of uncertainty based on different fits of Fig 4?

As noted above, the points in Fig. 4 are generated directly from separate deterministic model simulations rather than from independent observational data. We therefore do not consider alternative empirical fits to provide a meaningful estimate of model uncertainty. In the revised manuscript, we will remove the emphasis on the fitted curve and instead quantify uncertainty through sensitivity analyses of the main physical and model parameters.

Line 313-318: Why not include an a sensitivity analysis of turbulence depth, frazil crystal size distribution, and supercooling intensity as they are also expected to strongly influence enrichment. It should not take long.

We agree that sensitivity to the main physical assumptions should be quantified. We have therefore performed sensitivity tests for turbulent layer depth and representative frazil crystal size while keeping the total frazil inventory fixed, and these results will be included in the revised manuscript. Over the tested turbulent layer range of approximately 22.5–34.5 m, the inferred aggregation factor α

changes only slightly, and varying the representative frazil crystal size while conserving total frazil volume has a similarly small effect.

The revised model no longer prescribes a constant supercooling intensity. Instead, frazil production is constrained independently from the atmospheric surface energy balance over the period preceding sampling. We therefore do not include a separate supercooling sensitivity experiment. We will, however, clarify that supercooling remains an unresolved process controlling frazil nucleation and growth and therefore represents an important source of uncertainty that should be better constrained in future observational and modelling studies.

Line 351: You need to include more information of the laboratory tests. Not possible to follow what you are doing when it is not included in the methods – that is mostly dealing with parametrization and assumptions in the model.

No laboratory experiments were conducted as part of the present study. The model parameterizations and process representations draw on previously published laboratory and modelling studies, together with the AWECS field observations. In the revised manuscript, we will revise the relevant text and Methods to explicitly identify which parameters and process formulations are taken from published laboratory studies and which quantities are constrained from field observations or prescribed as model assumptions.

We thank the reviewer for these constructive comments, which will help us clarify the origin of the model assumptions and improve the transparency of the modelling framework and sensitivity analysis.