Biogeosciences Discussions

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Review - Seasonal dispersal of fjord meltwaters as an important source of iron to coastal Antarctic phytoplankton

Anonymous Referee #1

Referee comment on "Seasonal dispersal of fjord meltwaters as an important source of iron and manganese to coastal Antarctic phytoplankton" by Kiefer Forsch et al., Biogeosciences Discuss., https://doi.org/10.5194/bg-2021-79-RC1, 2021

Forsch et al., provide an unusually comprehensive study surveying the distribution of Fe and Mn in an Antarctic fjord, which has been the site of ongoing work by the FjordEco project. In addition to conducting profiles of the water column, the authors report sedimentary work, some analysis of ice samples, ligands, and some regional model work to comment in more detail on sources of Fe/Mn. The Fe:Mn ratio is also used to provide insight into the relative importance of different trace metal sources. Overall this is quite a novel study, there are few studies reporting depth profiles of these elements, which limit primary production across much of the Southern Ocean, in Antarctic coastal areas. The combination of data makes the study unique and a valuable addition to the literature. The visiting of the same site in two seasons is particularly valuable for an Antarctic fieldsite.

As written at present it is however quite long and in places I think more suitable for a Marine Chemistry readership, I think the text could be shortened a little. Some of the extrapolations from model work and calculations based on only a few melted ice samples could be trimmed a fair bit. This would strengthen the scientific arguments presented, cut out the parts of the discussion where large uncertainties remain and not much informative can be said, and make the text more readable.

This is a minor critique however, and overall the text is a strong addition to the field.

Comments/corrections by line

Title: Why not 'Fe and Mn'?

15 ‘of bioavailable Fe’ why not just ‘Fe’?

23 Ocean or atmospheric temperature?

28-29 This isn’t strictly speaking correct, there isn’t a simple relationship between increasing phytoplankton productivity and increasing carbon export because carbon export efficiency varies markedly between regimes (Henson et al., 2019), it would be more precise to say that Fe addition to Fe-limited regions increases primary production and potentially carbon export.

45 'and reduced macronutrient supply'. It isn't turbidity that does this on broad scales – although there may be a very small phosphate sink onto Fe-rich particles – it's strong stratification that leads to very low productivity in some Arctic fjords (Holding et al., 2019). Rephrase.

48 There's also the question of chemistry and factors that control Fe stability which you develop later. If discharge increases into a region which already has nM concentrations of dFe, is it possible to increase dFe and lateral dFe fluxes further? (Lippiatt et al., 2010) among other more recent references hints that dFe may be saturated in some near-shore, in this case Alaskan, regions which implies that increasing deposition of dFe or labile particles inshore wouldn't changes lateral dFe fluxes. More recent GEOTRACES work also comments on the competition between oxyhydroxide surfaces and ligands to bind Fe such that in these high turbidity environments undersaturation may be driven by increasing particle (and Fe) loads (Ardiningsih et al., 2021).

54 (I'm not a glaciologist) Cold-based - is this the correct term? My understanding from the literature was that cold-based and warm-based terms are not used to refer to submarine ice, only to land-based ice (e.g. see entry in Encyclopedia of Snow, Ice and Glaciers). I think a different term is required when comparing submerged ice faces that are/are not subject to submarine melt.

59 Note sure what 'minimal alteration' means in this context? You mean elsewhere there is also melting of the ice terminus – but I thought this was usually a very minor component of total freshwater discharge even in warmer catchments so I'm not sure it's much of a critical difference?

61 anoxia- is this always, or only sometimes the case?

78 "prior to significant glacier retreat" does not read well without a sentence explaining that this is(?) forecast/anticipated at this location. Also, I assume, you should specific prior to retreat associated with recent climate change?

88 Two two cruises

91 Neko Harbor – I would not know where this is without a dot on the chart or a lat/long

Fig 1. I struggled to read the text on the right hand side of this figure, maybe improve the contrast of strengthen the outline

106 N_2 and it would be better to state the specific grade e.g. 99.99%.

115 and filtered prior to analysis, repeated

120 Is there a specific reason for mentioning GP16, reads a little odd?

133 Q- is quartz distilled? Selectively means you varied the concentration factor?

138 It would be better to say what this was e.g.. x nM dFe Pacific surface seawater

144 Define LOD

167 0.2% is ambiguous, M concentrations would be better

168 solution of solution

181 If these are average values, why ~? Surely they are exact. Are they meant to imply the gradient is subject to high uncertainty? If so, maybe show the uncertainty.

188 Reference format

207 ambiguous: ,for deeper layers, is 84.6 m and the minimum thickness, for surface layers, is 0.5 m (?)

208 It might help some readers not familiar with the region if the Straits were labeled on Fig. 1

214 'captured' Maybe 'represented'

215 'These new freshwater sources include also surface runoff and local melt of glacial ice' repetition

232-235 I don't think you need this.

240 What increased Si are you referring to? The innermost station looks depleted and the water column from 0-10 km on fig. looks like high Si-high NO3. A 'new' Si signal would, I assume, show an excess of Si over NO3 if originating from bedrock related sources– you can test this by plotting Si* and considering the origin of the watermasses. Is this Si high relative to NO3, and if not is the different macronutrient concentration in this watermass related to seasonal inflow/outflow and the sluggish turnover beneath the sill?

Fig 2 It would be useful to see where these stations are in order for the reader to be easily able to interpret the trends. Can you, for example, overlay the transect line on figure 1. What drives the 1 station with really low chl a in Dec, is this real, it looks suspicious/erroneous as plotted?

277 You can presumably calculate the upper limit though, if you assume all freshwater required to balance MWf came from this one point source (obviously thereby easily an upper limit), you would get a discharge of <1 m3 s-1 (correct?) which means this is unlikely to be driving considerable circulation.

299 Is this decrease significant?

325 But you measured TdFe? So why not just 1 sentence comparing TdFe values?

360 Yes, these seem extremely high, I'm not sure if many prior values are published, the only ones I'm aware of are Al in (Menzel Barraqueta et al., 2018) who report much lower levels for Al. I note however that the authors' elemental ratios do seem sensible, so it looks like it just happened to be the case that the ice collected had a high sediment load, do you know (roughly?) what this was?

Table 1 The significant figures here could be reduced a bit, it doesn't really make sense to report decimal places for the high concentrations as written for example.

374 Details of statistical test

Table 2 Check sig. figs. A few values are either rounded or missing .0

452 value_unit consistency

478 "we note that the icebergs within Andvord were predominantly "clean" ice" How do you know this? And does this mean you intentionally sampled some ice which was

sediment-rich when selecting the ice endmember samples?

493 “Average” means a mean? (I think in this context it’s important to stress the mean/median values are likely very different)

501 ‘might’ can probably be removed here, it’s obvious from your data scavenging does occur, as it does everywhere else.

515 “It seems reasonable…” repetition of the last few sentences

518 “(82-86% of TpFe, 61-64% of TpMn)” It’s not clear at a glance what measurement the % refers to as a fraction of TpFe/Mn

526 delete ‘as’

527 As above, is it generally correct to state the subglacial environment was certainly anoxic, or does this vary with location? Do you have evidence specifically in this region that it is anoxic?

533 I think you need to state what this (8 nM dFe) is ‘low’ compared to (subglacial dFe?), in a marine context it’s very high

534 You need to state here what you’re assuming the freshwater content is, basal ice? These sentences I think are speculative, if you look at any freshwater studies trying to quantify dFe (granted, there are no extensive surveys of freshwater dFe in runoff along the WAP that I am aware of, or similarly for subglacial discharge) the range is huge, so an obvious caveat is that you don’t really know exactly what the freshwater concentration corresponding to these marine values is/was – and even if you did, it would likely vary so much in time and space that this variation would preclude any direct calculations concerning the exact weighted concentration most appropriate for this calculation (e.g. see the (Zhang et al., 2015) you already cite). If you really want to deduce a freshwater concentration, I think you really must try to present it also with an estimate of the (high) uncertainty.

535 I’m not sure this is surprising, if you look at any studies (either field or lab-based) looking at dFe behavior, you invariably see strong removal at salinities even fractionally above zero (<1) practically immediately (within minutes), so I think it would be correct to say all available data suggests a universal trend in dFe removal on this scale.

537-540 I’m not sure there is presently evidence to support this, either that dFe concentrations change with glacier type/scale. I haven’t looked at this in detail, and this is hard to deduce as there’s obviously lots to think about in terms of what concentrations to compare and other confounding factors. In terms of the plume, I think the concentrations here are very similar to those reported for much larger discharge Greenland catchments e.g. (Hopwood et al., 2016; Kanna et al., 2020).

I also find this a little confusing (it is clearer after reading the next few paragraphs) as it reads as if the (Death et al., 2014) study is quoting a value of 3-30 uM for the plume, whereas I think this actually refers to zero salinity. I agree, that unless a model manages to formulate the rapid scavenging/removal occurring on very small scales particularly well -most models simply can’t do that on this scale because this is subgrid for another other than a regional model- that these values are too high to do what they are being designed to do, but I think the phrasing here could be clearer.

586 are upper limit

598 Be more specific with what your oxidation rate is referring to, dissolved Fe(II) and dissolved Mn(II)?

600 I recently read another pre-print concerning Mn and Fe trends in a similar environment which you may find interesting (https://www.essoar.org/doi/10.1002/essoar.10506252.1)

Figure 8 How many glacial ice samples are you plotting here? Is there enough data to do this robustly?

620 Is this an increase considering the uncertainty on the values?

626 This seems speculative "are the target for ligand-mediated mineral dissolution and perhaps microbial uptake"

639 Is there a specific reason for a comparison to the California Current transition zone? I don't this discussion adds much, yes there is a huge excess of NO_3 pretty much everywhere across the region, and from a ratio perspective, much of this NO_3 will remain following complete dFe drawdown (which is confirmed by time series at bases in the region showing NO_3 very rarely approaches low concentrations) – but microbes are still experiencing a high dFe concentration throughout much of the year, so I don't think it's the case that they are Fe-limited in term of their growth during the growth season (or did I misunderstand something here?)

654 Raiswell, correct reference? This statement is perhaps is a little too specific, you could comment that the detail of ligand concentration/binding strength is not explicitly represented in most models.

656 I think the earlier (Lippiatt et al., 2010) work argues this.

659 "associated feedbacks on climate" this is a big step

678 Are there fjords with strong katabatic winds in the Amundsen Sea?

686-890 Values like this derived from a hypothetical meltwater endmember need to be flagged as 'rough' or have some uncertainty quantified.

693 – I moved this comment having written it earlier in the text – how do you know the meltwater fractions you calculate are all associated with meltwater from this fjord? Presumably it is not, on its own, the major source of meltwater to the region, so other sources, likely some outside your model boxes, are producing meltwater which is then laterally transferred through your region? I think this caveat needs to be explained as at present in many places the text reads as if your fjord was the major source of meltwater (and thus dFe) to the region.

696 "or, alternatively, that the glacial end member concentration is too low" Not sure I see the logic here, only if meltwater had to be 100% of the dFe supply? But we know, as shown in the text, there are multiple sources, so this doesn't make sense

705-715 This presumably supports the earlier caveat about where meltwater comes from, that the Bay studied is not a/the sole major meltwater source, so the meltwater observed in/around the Bay is coming from multiple places not captured in the model set up?

785 "leading to enhanced productivity and sedimentation of carbon" You don't show this herein.

Conclusion – This is quite long and I think would be sharper if cut. The new calculations are interesting but might sit better in the main text.

852 cause melting (warm-based). (?)

889 I'm not sure you can make a conservative estimate of dFe from TdFe, is there a simple relationship between the two? I would say a 'rough' estimate of 10%.

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