Response to Reviewer #1 on behalf of all coauthors

Seasonal dispersal of fjord meltwaters as an important source of iron and manganese to coastal Antarctic phytoplankton

I reviewed the original submission of this article. The authors have appropriately discussed and replied to two reviewers, both of whom found the original article interesting. I have no major comments on the revised version or on the authors' _replies. The manuscript is technically sound and a substantial, and very useful, study.

We thank Reviewer #1 again for helpful comments and recommended edits. These improvements make the message from this work clearer, precise, and compelling. Responses to reviewer comments are provided below in-line.

Minor technical edits suggested below:

30 primary production increases and potentially carbon export -suggest 'primary production, and potentially carbon export, increase' _

We have changed accordingly.

33 Delete 'existing' _

Deleted.

39 ACC – _defined? You don't use it much, suggest just writing it out

Antarctic Circumpolar Current is now written.

115 'In brief…' _Doesn't read well. I suggest you just move this sentence before the sampling, 'Bottles were pre-cleaned…' _and then the paragraph will read ok

Moved.

142 0.34 nM dFe

Specified, dFe.

144-150 No units on any concentration

Units are added.

193 Buck et al., ref format

Reference is fixed.

202 were previously applied

Fixed.

313 (**318**) Suggest splitting this sentence in two

Fixed.

323 Fig. 5 n6 ??

Fixed.

501 I agree generally, but you do not explicitly show Fe or Mn is limiting herein, suggest 'could stimulate' _

Changed.

623 (**628)** More convincing is the correlation between LpFe and dFe so suggest just referring to this directly. I suspect the 4% and 5% have relatively large standard deviations so wouldn't state it was 'remarkable'.

Comparison to LpFe is now stated. We have removed 'remarkable.'

They do have large standard deviations: 4±3% late Spring; 5±2% Fall.

801 small fraction of Fe from these particles

Fixed.

821 I understand this long term perspective, but potentially important to stress this is long term given the earlier comment on the potential for short-term increases, so maybe add a clause 'In the short term, increased Fe and or Mn supply may increase primary production…. But conversely, in the long term….' _

We've added this clause for distinguishing between short term responses to climate change, and longer term changes with changes in ice volume.

Supplement
40 mwf of

Fixed.

Response to Reviewer #2 on behalf of all coauthors

The authors have done a good job revising the manuscript and I only have a view remaining comments.

We thank Reviewer #2 for an additional round of careful review of the manuscript. The suggestions made here will improve the precision of the language and the overall clarity for readers, which is always of the greatest importance. We reply to each comment in-line.

125-133 I fully understand and respect the explanation that the ice sampling was opportunistic. However, stating 'Acute attention to cleanliness was applied….' does (at least to me) imply the authors were following best practice rather than doing the best with the tools available. To avoid setting a precedent for future research by others, please state explicitly the recommended tools (no steel tools, ceramic knive, etc).

Absolutely, we agree with this. A sentence of caution is added and a recommendation for alternatives is provided.

191 not my area of expertise, but a recent publication in this journal (doi.org/10.5194/bg-2021-134) indicates the used method 'causes overestimation of [L] and could result in a false distinction into more than one ligand group'. The current ms is not a methods paper, but maybe the implications of such bias should be discussed in this ms?

While overestimation is a potential issue with using these methods, the trends within the dataset, including the interpretations are sound including comparing to existing datasets using these methods. The Gerringa et al. (2021) reference applied these methods to model ligands, and not environmental samples. We have included a sentence to this effect in section 3.5 and the reference provided by the reviewer.

345 how was the plume defined (i.e. what where the criteria for 'in the plume' vs 'out of the plume')?

The plume was defined in lines 273 – 275: "subsurface neutrally-buoyant plume (~100m) characterized by a point source of relatively cold and particle-laden water emanating from the terminus of Bagshawe Glacier…".

485 I indeed suggested to move the sentence, but moving it to the current location has not improved the clarity of this section; at least I do not follow how the discussion concerning a negative relation with MWf and metal ratios in particles, leads to the conclusion meltwater is a significant source of Fe throughout the growth season.

Sentence removed for section clarity. In the late Spring, a negative relationship occurred because of intense removal of surface reservoir (winter reservoir) due to phytoplankton uptake and stratification of the surface (prevents surface replenishment). The point that meltwater is a significant source is solely based on the very high concentrations of terrigenous trace metals in meltwater (relative to seawater) and the supply, which is input directly to the euphotic zone.

508 is such a buoyant plume here observed? Was mentioned in the results but would be good to stress here again

Now explicitly stated.

525 'Cold-water glaciers are locations where the subglacial environment flows directly into the fjord with minimal mixing with seawater.' seems contradictive to line 510 stating that cold tongues 'entrain deep water masses'. could you clarify?

Clarified. Low meltwater production rates do not induce extensive overturning and turbulence. Both are essential for vertical currents and ambient seawater entrainment.

535-546 the sentences in this section seem somewhat disjointed (i.e. hard for a non-specialist to follow the narrative/rationale).
We have added some clarifying phrases to the text.

548-566 what is the connection between flocculation (1st paragraph), resuspension (2nd paragraph) and diffusion-based sediment fluxes (3rd paragraph), not very clear to me.

This section was focused on the role of sediments as a source to the water column, and that particulate material is derived from the subglacial plumes.

567 what are these results, the flux estimates? the 12 µmol m-2 d-1 is ~factor 4 lower than the estimate in the previous paragraph, is it implied these are similar or relatively different?

Clarified. We imply that the results are not surprising for a coastal setting with high dissolved oxygen because our estimated fluxes are higher than, but similar in magnitude to, the average for continental shelves.

633 I meant Ardiningsih et al., 2020 https://doi.org/10.1016/j.marchem.2020.103815) as a comparison to Arctic work

Yes, the wrong citation was listed. We've corrected this, although the 2021 (*Biogeosci.*) publication does have some relevance to this work.

We have added a sentence comparing the findings of these works in Antarctica and the Arctic: large source of undersaturated weaker ligands close to glaciers compared to adjacent open ocean stations.

661 sounds similar to interpretations for an Arctic system by Ardiningsih et al., 2020 (direct citation from abstract 'The lower ligand binding strength in the outflow results in a higher inorganic Fe concentration, [Fe´], which is more prone to precipitation and/or scavenging than Fe complexed with stronger ligands.'). Seems noteworthy to me to discuss such similarities.

And how does this section relate to the previous section 'In the Fall, ….. fraction of dFe and TDFe does not reflect a greater enrichment of particles.' Seems contradictive; first lower complexation capacity is not deemed to lead to partitioning to particles (i.e. scavenging) whereas here it is stated the DFe pool might be more subject to scavenging. Please clarify.

The dFe pool may be more subject to scavenging in the Fall, but we do not see this in the particulate Fe speciation data (near constant ratio of dFe:LpFe). We now make the point that the greater excess of weaker ligands in the Fall is due to a large source of these ligands and they are not saturated because there is a smaller increase in dFe concentrations. This contrasts with competition with particles, which might remove bound dFe from ligand complexes, but we don't see this.

662 Ardiningsih et al., 2020 can be updated to the published paper (rather than discussion paper)
671 export the surface layer sounds odd to me, do you mean it leads to mixing of surface and deeper waters (i.e. vertical export) or that fjord surface water advects offshore (i.e. horizontal export)?

Clarified by saying, "can laterally export the surface layer".

675-676 deepening of the mixed layer by 25 m and water upwells from 150 m depth. Can you clarify what happens where?

Water that is being upwelled would displace surface water by moving it laterally, whereas winds also add energy internal to a water parcel by increasing turbulence, thus increasing the depth of well-mixed surface water. Upwelling was found to occur at both Sill 3 and in the inner basin A. These are also locations where mixing increased, indicated by a deeper

mixed layer. Both of these processes had varying strengths at either location (Sill 3 and IBA).

690 what stations are referred to with 'both stations'?

Sill 3 and Gerlache Strait stations. Fixed.

703-708 I commented on this last time, and the response file gives an explanation, but the changes indicated in the response do not seem to be made in the text (same figure, no explanation the station was occupied before and after the wind event with different observed concentrations). Moreover, based on the supplemental table I do not get the average value of 4.14 after December 11 for the shallowest sampled depth at station S3, and given the range in values, I highly doubt the concentration difference is significant.

The explanation is now included in the text, although you are correct, the difference is not significant despite there being a higher concentration at S3 following the wind event (3.94 nM not 4.14 nM, miscalculated). Late Spring average 3.19±1.53 nM dFe; Fall average 3.94±1.92 nM dFe.

730 previous section was also (partially) on upwelling at station S3 which is not the glacier terminus so I suggest some rearranging/clarifying of the text as it is not always obvious which process is discussed in which section.

We have added some text to the manuscript to improve clarity.

748 quicker than what?

The deep dye. Clarified.

774-787 I like this section, but think you should at least mention that the actual 'effective' fluxes of Mn and Fe will be much lower due to non-conservative behavior of these elements.

We have added a sentence about the uncertainty of the effective fluxes due to scavenging and biological uptake during transport offshore.