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Interactive comment on "Review Article: How does glacier discharge affect marine biogeochemistry and primary production in the Arctic?" *by* Mark J. Hopwood et al.

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This review was comprehensive in its scope of biogeochemical impacts of freshwater discharge in the cryosphere. Using multiple case studies of Arctic fjords, Hopwood et al. capture the range of biogeochemical settings, and in doing so, identify and summarize multiple drivers for diverse phytoplankton response. The review was written with a broad audience in mind, with detailed discussions and patient explanations. The figures aided the discussion and were generally appropriate to the text. I support the publication of this much-needed review pending the appropriate revisions are made.I feel the authors were diligent in their discussion of state-of-knowledge and take a conservative stance when estimating fluxes of dissolved nutrients. I support this approach. However, I am cautious about language which aims to describe ecosystem function as similar in both the Arctic and Antarctic. Few studies exist which focus on the ice-ocean interface (within 1km of marine-terminating glaciers) in the Antarctic. The geochemical gradients are intense here and logistically more challenging to study. It is apparent to me that the Antarctic lacks a robust assessment of the fjords, and so the authors should acknowledge that comparatively less is known about the Antarctic.

R: We thank the reviewer for detailed comments on the text. It was not our intention to apply similarities between Arctic and Antarctic systems, or to draw extensive comparisons between the two in general as there are obviously important biogeochemical differences in the marine context between the two. We add some brief discussion of this at appropriate points in the text e.g. lines 544 'These differences may explain why some Antarctic glacier-fjords have significantly higher chlorophyll and biomass than any of the Arctic glacier-fjord systems considered herein (Mascioni et al., 2019). However, we note a general lack of seasonal and interannual data for Antarctic glacier fjord systems precluding a comprehensive inter-comparison of these different systems.'

I think the authors should include in their discussion mention of katabatic wind events and the efficiency at which they mix the upper water column, and the result this would have on export of the surface layer and upwell subsurface sources (see Lundesgaard et al. 2019). Katabatic wind events are important interactions between the atmosphere and ice sheets.

R: We can add further details around this. We expand a brief mention of wind events in 'Fjords as critical zones for glacier-ocean interactions' to read: "energetic shelf forcing (i.e., from coastal/katabatic winds and coastally-trapped waves) can result in rapid exchange over synoptic timescales (Straneo et al., 2010; Jackson et al., 2014; Moffat, 2014) and similarly also affect productivity (Meire et al., 2016b). Katabatic winds are common features of glaciated fjords. Down-fjord wind events facilitate the removal of low salinity surface waters and ice from glacier fjords, and the inflow of warmer, saline waters at depth (Johnson et al., 2011). The frequency, direction and intensity of wind events throughout the year thus add further complexity to the effect that fjord geometry potentially has on fjord-shelf exchange processes (Cushman-Roisin et al., 1994; Spall et al., 2017)"

Lastly, I am pleased with the discussion about new approaches being required to address these highly dynamic ecosystems. Namely, higher resolution (in space and time) studies are needed to understand how this system function and will respond to climate forcing. Specific comments: L281-282: I do not think we have a well-constrained estimate for the Antarctic. Subglacial discharge is one of the critical fluxes discussed in this review. Recent attention has been given to the subglacial environment and I think it is worth mentioning the uncertainty which surrounds this source. There are biotic and abiotic factors which influence the quality and quantity of iron released to the ocean. Weathering rates are controlled in part by regional geology, but also the microbial communities (namely, chemolithoautotrophs) and exposure to oxygen may be important controls. (Wadham et al. 2010; Tranter, Skidmore, and Wadham 2005) Further, it is nearly impossible to differentiate the effects of tidal uplift, sediment resuspension, glacial calving and subsequent scouring of the sediments at the glacier terminus from purely subglacial discharge. Our understanding of these effects would be greatly increased if measurements were made proximal to cold-based, low velocity marine-terminating glaciers. We can then begin to pick apart the contributions of these different processes.

R: Agreed, we have corrected this statement 'around the Arctic' (rather than 'globally')

L288: Seasonal variation may be an important theme for future directions, both in the Arctic and Antarctic. The authors make this note. Without the aid of the ocean modeling community, we do not yet know how subglacial discharge responds to climate forcing.

R: Agreed, we can add a comment to this effect in the text. (new lines 495): 'Yet determining the extent to which these events affect fjord-scale mixing, biogeochemistry and how these rates change in response to climate forcing' My general feeling is that while the comparisons may be obvious, there are important functional differences between the Antarctic and Arctic. And so supporting information should be appropriate. For example L203-206 has two well-known studies of particulate iron in the Antarctic (Gerringa et al., 2012, and Annett et al., 2017). The authors may choose to mention this is an important question in general for particle-enriched iron sources. (Fitzsimmons et al. 2017)

R: Yes there are certainly differences, whilst we do not wish to extensively discuss Antarctic systems (precisely because they are so different), we flag the differences elsewhere and instead re-phrase this sentence to refer exclusively to Arctic studies.

L192-195: I think it is important here to discuss the potential for dissolved-particle exchange, facilitated by undersaturated organic ligands or by dissolution in the guts of zooplankton. (Gledhill and Buck 2012; Barbeau et al. 1996)

R: We can add a few lines discussing this in addition to our already present notes to the complexity of the Fe cycle. New lines 255: 'Furthermore, the mechanisms that promote transfer of particulate Fe into bioavailable dissolved phases, such as ligand mediated dissolution (Thuroczy et al., 2012) and biological activity (Schmidt et al., 2011); and the scavenging processes that return dissolved Fe to the particulate phase are both poorly characterized(Tagliabue et al., 2016).'

L452-455: This discussion is accurate, however nutrient stoichiometry (both supply and demand) is what drives primary production and selects for specific phytoplankton taxa, especially in enriched environments. In the instance of diatoms, the N:Fe ratio is a good predictor of iron limitation, where a threshold describes the point at which diatoms begin to grow sub-optimally. The application of geochemical proxies (N:Fe, Siex) for nutrient stress should be applied where such data exists (see King and Barbeau 2007; Hogle et al. 2018).

R: We can add some general comments on nutrient stoichiometry here, it is perhaps

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a little detailed to specifically address the subtleties of Fe-diatom limitation, but in general terms the issue can be discussed as an influence on taxonomic groups; 'Although proximal limiting nutrient availability controls total primary production, organic carbon and nutrient stoichiometry has specific effects on the predominance of different phytoplankton and bacterial groups (Egge and Aksnes, 1992; Egge and Heimdal, 1994; Thingstad et al., 2008).'

L511-512: This is indicative of Fe-limitation of the phytoplankton community, which is dominated by diatoms during the sampled summer growth season. Please indicate this is a log-transformed ratio.

R: Explicitly stated.

L496: "islands occur within" The phytoplankton community must meet several requirements for a pronounced increase in growth to occur. They must be physiologically adapted to use glacially derived iron sources. It is unknown the degree to which phytoplankton in the Antarctic use colloidal iron, which would require biotic and abiotic processes to transform it in to a bioavailable form (ie organic complexation, dissolution, photoreduction). I challenge the simplistic view of HNLCs and acknowledge this to be a grand question of our time.

R: Yes, there is no doubt to us that the biological utilization of labile particulates is something under-studied and we can add a few sentences to raise the complexity of biological Fe uptake here in general terms. We note that budgeting exercises show that only a few percent of the 'sedimentary' Fe added downstream of such island plumes has to be solubilized to explain observed primary production. Exactly what this fraction is not particularly clear yet. Also there are other confounding factors such as light limitation, 'However, even in these HNLC waters there are also other concurrent factors that mitigate the effect of glacially derived Fe in nearshore waters where light limitation from near-surface particle plumes may offset the positive effect of Fe-fertilization'

L584-596: This is a great discussion on the uncertainties which remain largely in marine iron biogeochemistry.

L672: ": : :additional subsidies of labile carbon: : :" R: Ammended.

L731: Our data for Antarctica is spares, and biased towards summer growth periods. We have little information about the community dynamics throughout the ice-free growth season. L742: We see the same in Antarctic fjords, but lack an early Spring diatom bloom. Instead, flagellates dominate the fjords. A pronounced diatom bloom and sedimentation event spans 2 weeks, and overall production falls dramatically early-Fall.

R: Agreed, we have noted now the data deficiency when discussing differences between Arctic and Antarctic systems (562): 'However, we note a general lack of seasonal and interannual data for Antarctic glacier fjord systems precluding a comprehensive intercomparison of these different systems.'

L758: "of Patagonia" R: Corrected.

L792: It is becoming more apparent that fjords in the Antarctic are highly productive relative to their Arctic counterparts. Primary production in the fjords rivals that of the Fe-limited shelf regions during the summer. Indeed, we find that organic carbon export is greatest in the inner-fjord environment (unpublished). This is more evidence of the differences in behavior between the Arctic and Antarctic.

R: Yes, as above, we have added some very simple comments aiming to clearly distinguish our case studies from the Antarctic and avoid any possible inference that Arctic and Antarctic glacier fjords can be considered as similar with respect to marine primary producers.

L819-822: How then do we reconcile the expansion of the icesheets and the decreased availability of sediments eroded by wind? R: With difficulty on regional scales, but on global scales this is not implausible. High latitude dust sources may be significant locally, but globally are minor compared to dust from the world's low latitude dessert regions. Changes in 'global' dust signals may therefore be highly sensitive to what happens at low latitudes and relatively insensitive to what happens around the world's IceSheets/glaciers.

L862-863, 865: Autonomous gliders with optical backscatter and seawater sampling capabilities would we a great way to begin to address this. I agree!

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