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The Cryosphere Discussions

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 article is timely and presents an opportunity for the authors to summarise the current "state of play" and highlight potential future research direction in the Arctic with regard to ice-ocean biogeochemical interaction. I broadly agree with most of the main points raised and the authors touch on most of the key research areas. The review is well written, the figures largely appropriate (apart from some points below) and I'm supportive of its publication after revision. However, I have suggestions for improvement. When and where some of the literature quoted is relatively selective and the way it is contextualised in certain circumstances misses nuance. One major omission is

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a discussion of particulate fluxes (both as part of nutrient budgets, and importance in ballasting and C burial) and indirect processing of glacial inputs (related to particulate inputs; i.e. benthic recycling and/or burial). Given the context of these environments (dominated by inputs of products of physical weathering), and the existence of literature in other glacially influenced regions (e.g. Laura Wehrmann's and associated groups ongoing work in Svalbard; e.g. Wehrmann et al., 2014), this could have been an opportunity to start a balanced discussion. This is an oversight, especially for a review article, and given recent interest in particulate fluxes (not just in glacial locations), even if the authors do not think these flux terms are important. As a previous reviewer indicated, there is also a need to discuss and incorporate more recent publications (i.e. Hendry et al., 2019, but also Seifert et al., 2019, Wadham et al., 2019 amongst some others I suggest below), and some key papers have been omitted or not referenced where they should have been. I have some major reservations about section 8, which feels incredibly speculative, and think it should be toned down and incorporated into section 9 in a much reduced form. Specific comments to be addressed are below.

L65: Calcium carbonate is not an ion. This should be corrected to "inorganic salts. . .".

L64-68: These plumes also carry large quantities of reactive particular material, including labile particulate nutrients. Whatever you think of their ultimate fate (which can be discussed) I think this is important to note as it is an important characteristic of glacial meltwaters. In this context I'm sure the authors will be aware of the literature (some suggestions for inclusion are Hendry et al., 2019, Seifert et al., 2019, Jeandel and Oelkers, 2015, Grimm et al., 2019, Schoenfelt et al 2017, Morgan et al., 2014, Eiriksdottir et al., 2015).

Figure 1: I'd like to see the quality of this figure improved before publication. As the first figure and a key map of study areas it's also a little too basic at present.

Section 3: I find the referencing in the first paragraph curious. Although by no means do I think that the authors should be referencing some work ahead of others, the first

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reference of a particular group's work is page 8, where it's critiqued, despite the number of publications from this group that are suitable for referencing before (in this context). The authors discuss the need for seasonal datasets to contextual flux information, yet there are already several studies currently available that contain temporal datasets over several months and several years of monitoring for hydro chemical parameters, macronutrients and Fe. The concentrations used on Table 2 are from some of these studies, and are discharge weighted mean concentrations derived from a seasonal dataset (the only DWM concentrations in Greenlandic meltwaters that I know to exist at present). There is certainly a debate that can be had with regard to the particulate nutrient inputs (which the authors should deal with in a more balanced manner), but I do not fully understand why other aspects of those papers have been overlooked. These might not be datasets that span whole melt seasons (typically early May to early September), but they are the longest available at the moment and should be acknowledged as such. I would like to see the current literature discussed in a more nuanced way in the next version of the manuscript.

L163: Semantics but I think this should be "dissolved macronutrients". Again, the role of particulate macronutrients can be critiqued, but this is an important distinction to make. Glacial meltwaters have high concentrations of particulate nutrients (save N), and low concentrations of dissolved nutrients, and it's important to highlight that whatever you think of the eventual fate.

L164-166: There is a push here to emphasise that the PO4 concentrations in glacial meltwaters are particularly low. I'm not arguing against this (they are compared to some marine waters), but the PO4 concentrations in glacial meltwaters from large catchments (see Leverett Glacier) are similar to the global river mean (0.32 μM; Meybeck, 1982), and also similar to (or exceeding) PO4 concentrations in Arctic rivers (0.03-0.76 μM). Further, the annual yields (normalised to catchment area) are very high (see Table 4 in Hawkings et al., 2016). Again, not all this information may be needed in the context of the review, but it's important to not single out glacial inputs as being particularly nutrient deplete as is currently done.

L167: This needs a reference and some contextual information. See point above.

L178: I'm not sure if I'd call these measurements "extensive" given they are from two small glaciers in a fjord with many meltwater inputs (the major inputs coming from much larger tidewater glaciers). The references given for studies of Svalbard meltwaters also have listed LoD for PO4 is 5 ppb (0.16 μM), and a limit of quantification likely even higher (although not mentioned) making those figures difficult to compare to the fjord measurements when the LoD is typically better.

L188-189: As above point, I don't really understand the referencing here. There are other appropriate studies that emphasise the existence of reactive particulate Fe that should be referenced here (Bhatia et al., 2013, Schroth et al., 2012, Schroth et al., 2014, Hawkings et al., 2014, Hawkings et al., 2018).

L198-199: Low nM concentrations are still fairly significant in a marine context, especially when ~100 km from the main inputs at the head of the fjord. Surface open ocean waters and even some coastal systems are typically <0.5 nM and often much lower (Johnson, Gordon, & Coale, 1997; Tagliabue et al., 2017). These concentrations would usually be considered very high for marine systems - an important point worth making I think.

L205-206: Schroth et al. (2014) should be referenced in this context as well.

L211-212: What about biological uptake?

L222-230: The first assertion in this paragraph (that Si is generally released from the particulate phase over a salinity gradient) is based on one referenced paper (Windom et al., 1991). Other review articles on estuarine environments (e.g. review article of Statham, 2012) and many other estuarine papers (e.g. Edmond et al., 1985, Burton et al., 1970, Cloern et al., 2017, Bell, 1994, Raguenau et al., 2002 to list a few) note that conservative behaviour, or in some circumstances reverse weathering and/or adsorp-

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tion/other removal processes have been observed, especially in similar high sediment, deltaic environments (Treguer et al., 2013, Kamatani et al., 1984), apart from when strong benthic Si fluxes have been inferred (e.g. Eyre and Balls, 1999). I'm not saying dissolution of particulate material is not important in other systems, but the authors need more than one reference to support this generalisation.

L230-234: I welcome balanced debate, however, it's disappointing the review makes no mention of incubation experiments performed in this study, which show release of DSi from particulate material to seawater over a period of 30 days in samples that weren't treated to remove ASi. This doesn't necessarily mean DSi is released in the fjord surface, but it's worth consideration especially given the recent findings of Hendry et al. (2019) and Gruber et al. (2019) among others. The former shows strong evidence bottom water modification for example. The benthic environment is currently ignored and the lack of discussion of this is an oversight.

L242-254 (and Figure 3): I don't disagree with most of the interpretation here, but given that some of the low salinity end members are not dissimilar to Hawkings et al. (2017) (where there are no high salinity end members) it seems curious that the authors explain this by lack of data and complexity of fjord systems in these instances. Simply drawing linear regressions through points in Figure 3 is also misleading and doesn't tell the whole story that is being shown in each dataset. e.g. if you drew a linear regression through the Bowdoin Fjord plots at the same salinities (<10) then it would look very different. It's generally inappropriate to draw a regression line beyond where the data points lie and I'd like to see this corrected for relevant fjords. It would be better to use a GAM model to fit the surface data in Figure 3 and the authors should consider doing so (and not plotting beyond the dataset). In addition it would advantageous to indicate which samples on this figure are taken at the surface and which are taken at depth to avoid confusion. "Leverett" should be Søndre Strømfjord.

L252-253: Worth pointing out this is from a small land terminating glacier. Although there's a lot of debate, larger glaciers seem to export meltwaters with comparatively higher dissolved silica concentrations (Wadham et al., 2010). Pedantic, but I'm also not too keen on the term "surface discharge", as it could indicate any meltwater entering the fjord via surface rivers. Surpaglacial meltwater would be a better term. Most supraglacial meltwater is also routed to the glacier bed (and the subglacial drainage system), so I would think this is unlikely to be a large contributor. By "ice melt" I assume the reference is to iceberg melt?

L261-264: Discussion of Hendry et al. (2019) would be useful here. I think the wording misses nuances given flux estimates of Si for ice sheets did not exist before Meire et al. (2016) and Hawkings et al. (2017), and so were considered zero in biogeochemical models and estimates of the global silica cycle. I would consider no estimate an underestimate. Table 2: I was not aware that Lawson et al. (2014) measured dissolved organic nitrogen (DON). The discharge weighted DON concentrations of Wadham et al. (2016) need to be included here (1.7 μM). No mention has been made of NH4 concentrations. They are minor but should be discussed for completeness. I think some discussion of methodology with regard to Fe concentrations would also be appropriate here. As the authors know, it is complicated to simply compare concentrations of Fe where measurements are conducted via different methodologies, for example size fractionation (<0.2 μm, <0.45 μm), and filter type (e.g. PES, PVDF, PC), without noting as such. Polycarbonate (PC) filters (as used in Statham et al., 2008) are particularly problematic as the effective pore size of them reduces sharply upon filtration of even small amounts of sample, especially in highly turbid waters (see Shiller, 2003, for some discussion of this). Further, it is also worth considering representative glacier sample collection. This should be discussed in terms of future research direction. For example, from what I can ascertain, the glaciers samples in Hopwood et al. (2016) that form the Fe concentration estimate in Table 2 are all ~1-2 km2, are not ice sheet catchments, and represent insignificant inputs into the fjord. It's questionable how representative a ~1-2 km2 glacial catchment is in the context of an ice sheet.

L281-286: This is not strictly true, as the authors comment later on L319-323. The

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flux differences for Fe in particular are due to an arbitrarily applied fjord removal in the papers. The 11-fold flux difference between Stevenson et al. (2017) and Hawkings et al. (2014) is due entirely to the application of an arbitrary fjord removal factor - the flux-at-gate (i.e. the flux from the river into the fjord) are very similar between the studies (note the ∼90% removal is also discussed in Hawkings et al., 2014, and an estimate of flux after removal given).

L288-293: As several points above. A point is made that seasonal datasets are needed, yet the only publications with datasets >2 months in length have been omitted in the referencing. This needs to be rectified.

L298-302: I understand why the authors want to make this point. In defence of these studies, the flux calculations are made at the "gate" and therefore represent a first order estimate for inputs into the fjord (which is how elemental fluxes from rivers are almost universally calculated). The elemental estimates for ice sheet fluxes (previously assumed to be inconsequential) are also some of the first, so in that context glacial estimates were underestimated (as they weren't estimated at all before). As is touched upon, the largest flux term in the papers cited is the particulate loading (and the particulate fluxes from the ice sheet are massive – estimated at 8% of global sediment fluxes to the ocean; Overeem et al., 2017), which clearly isn't observed in the surface samples and on the timescales the authors discuss. It is a shame the fate of these particulates are not discussed in a balanced manner, and only somewhat negatively, if at all.

L303-307: I think this should come earlier – after line 283.

L319-323: As the authors mention elsewhere, turbidity is important in suppressing surface productivity (via light limitation), which should be mentioned here. Discussion of new work by Seifert et al. (2019) should also be discussed in the context of carbon removal.

L417-421: This is the first reference to any benthic processes occurring. This is an oversight of the current manuscript, and this deserves discussion. Studies in both polar regions have investigated benthic recycling and diagenetic processes and the authors should discuss this as well (see Wehrmann et al., 2014, Henkel et al., 2018, Buongiorno et al., 2019).

L449: Can be a few 10s km where turbulent plume is observed and can be spatially variable with time (Tedstone et al., 2012, Hudson et al., 2014).

L456-458: This is one of several reasons why the limiting nutrients are likely differ. What about riverine inputs, dust inputs etc. . .?

L471-472: This is slightly misleading. The coastal regions of Antarctica have low Fe concentrations but there are now several studies highlighting the potential importance of glacial inputs. Further Figure 5 misses out PFe concentrations from Marsay et al. which are consistently >1 nM. Further the authors in this study comment that measurements come following 2 months of intense primary productivity (i.e. these are not traditionally limiting waters, but a productive coastal ecosystem).

L552: This is a rather low estimate of DFe from a grounding line and there is very little information available on concentration estimates. I'm not quite sure how the authors came to this value from Marsay et al. (2017), so it would be useful to provide a sentence to elaborate.

L584-596: Completely agree and pertinent point to make given we know almost nothing about ligand binding in glacial fjords (and very little in estuaries more generally). However, I think the perspective here is mainly focused on the idea of bioavailability in "open ocean" waters, which is almost certainly controlled by ligand binding (what this does to the bioavailability I think is still poorly understood given the wide range and complexity of metal stabilising ligands). An increasing number of studies (Kranzler et al. 2011, 2016, Shoenfelt et al. 2017, Grimm et al. 2019) are demonstrating the importance of accessing Fe from particulate pools yet there is very little discussion of this. Surely in coastal areas the particulate pool is likely to be very important given the high

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concentrations (Schroth et al., 2014) and is almost as poorly understood as the ligand pool? Some balanced discussion of this is important.

L608-609: I don't know what other bedrock types the author's think are likely, but carbonate and silicate bedrock broadly covers them all.

L639: Some repetition here. Please rephrase.

L620-643 p1: Linked again to my point about lack of discussion on the importance of benthic cycling, I think there should be some discussion of the potential role of alkalinity production in sedimentary environments (e.g. via denitrification and sulfate reduction).

L620-643 p2: Some contextualisation is needed here. To be my knowledge (and I am definitively not an expert in this) but there tends to be a conservative decline in alkalinity in most estuarine settings (see Cai et al., 2010 and Thomas et al. 2009 for example), so this is not unique. The trend of decreasing alkalinity with increasing freshwater is therefore not particularly surprising in the context of freshwater-saltwater continuum environments as a whole. I agree that monitoring these changes with increasing meltwater discharge will be an important future undertaking.

L620-643 p3: I think some additional detail in this section would be useful for readers. Could the authors also consider an alternative scenario whereby glacial meltwater have low pCO2 and high pH as glacial meltwater tend to be elevated in pH and corresponding low pCO2 (Tranter et al. 1993, Sharp and Tranter, 2017)? For example, there is currently no mention of the conclusions of Meire et al. (2015), which shows glacial melt water associated with low pCO2 regions of the fjord. Also see recent studies by Pilcher et al. (2018) and St Pierre et al. (2019).

L649: What is meant by a DOM concentration? Do you mean DOC concentration?

L689-695: All of the samples in the Holding et al. (2016) bar one are taken from salinities above 34 therefore it's not particularly surprising a clear signature of glacial DOC is observed in bacteria here. Additionally, there is no mention of a glacial DOM in algae, some of which are likely to be mixotrophic as commented in this manuscript (i.e. the interpretation is not straightforward). In this context I really don't think you can consider the Holding et al. estimate of ∼11% of bacterial OC in marine waters to be from glacial DOM as minor. It is worth mentioning that other studies (e.g. Fellman et al., 2015, Hagvar et al., 2016) much closer to glacial inputs have found assimilation of glacial DOM into food webs. This is much debated, but one part of the story is that glacial DOM is highly bioavailable (as observed by a number of studies) and is therefore likely consumed very close to the glacier front.

L695: Paulsen et al. (2018) isn't the correct reference to use in this context and is slightly misleading. This study shows that bioavailability is influenced by glacial meltwater inputs not that it is a minor component of bacterial consumption.

Section 8: This whole section feels extremely speculative to me and is not actually correlated to real world observations, nor with any observations from the Arctic. Most of the literature cited provides tenuous links with the only evidence that I can see based on the observation that HABs occur in Patagonia and that there are glaciers in Patagonia as well (but not in the same locations at the HABs). The main study cited (Leon-Munoz et al., 2018) was conducted in fjords with very little or no glacial cover, and contains no reference to glaciers, or meltwater inputs. I'm not against the inclusion of some points from this section into the next section (long-term effects of glacier retreat), as it's important to form hypotheses for testing (especially when anticipating future change), but it needs to be significantly toned down, reduced and explicit mentioned that the hypotheses are speculative.

L749-750: I disagree with some of the glaciological interpretation in this paragraph. The study cited (Bliss et al., 2014) is a modelling study to predict future meltwater runoff terms, with no observed data presented (yes future estimates of mass change and runoff are given and are useful, but that is not how this study was cited). This is especially problematic in Patagonia, where there is a relative dearth of data to use for model inputs/validation. There is no evidence to suggest that glacial runoff is in long term decline in this region. The opposite is actually likely to be true with regard to the Patagonian Ice Fields (see recent studies of Forresta et al., 2018, Richter et al., 2019, Li et al., 2019), which are currently the largest contributor to sea level rise per unit area in the world. Glacial meltwater runoff is not intricately linked to precipitation as per non-glacial rivers, but reduced precipitation is likely to amplify mass balance losses.

L773: As above – this paper does not demonstrate this and cannot be used in this context. Section 9: I like this section, but some alterations are needed based on my previous comments above.

L829-830: I don't disagree but references needed here to substantiate point.

Figure 9: Nice looking figure, but I'd really like to see more balance in the interpretation of the literature represented in it. One major omission (again I'm going back to it) is any lack of benthic feedback. "Sedimentation and Carbon(/nutrient) [burial]" is seen as a one way process here, which is unlikely to be true (see works by Wehrmann amongst many others).

L945-947: Recommend updating these figures with new data available in Mouginot et al. (2019).

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