# Review of "How does glacial discharge affect marine biogeochemistry and primary production in the Arctic?" by Hopwood et al.

I thank Hopwood and co-authors for their detailed responses to my first review. Rather than responding to these comments I've concentrated on addressing the revised manuscript. As per my previous review, I think this manuscript is timely and very well written. The revised version is a clear improvement on the original manuscript, and I thank the authors for taking on board some of my points. I still have a general comment that I would like to be addressed in the next version of the manuscript and have some specific points that I think need to be addressed below. I would like to reiterate that I do not disagree with the authors main points, nor with their more general overarching conclusions. However, I would still like to see some more balance discussion and use of the literature, and I do not think it is particularly helpful to use some references purely to argue when hypotheses appear to be wrong when there are many opportunities for them to be used appropriately elsewhere.

My main general point concerns section 9.0. I understand why the authors want to include it and it is an interesting hypothesis that deserves future attention, but I do not think the current evidence supports its inclusion as a separate chapter over and above section 10.0 (Insights into the long-term effects of glacier-retreat). Many of the "known" factors that help create HABs are somewhat specific to certain regions with most of the literature cited in this case from Patagonia (and do not apply to locations with high glacial coverage even if some HAB species are present). I know of no known location where there is a link between changes in glacial meltwater supply and HABs (yet - I'm sure the authors will correct me here if I'm wrong), and the additional references given do not really help to reinforce the main point of the chapter. I understand that the literature on this is uncontroversial when it comes to Patagonian fjords (with a strong emphasis on non-glacially fed fjords), but I do not fully accept a tentative link above and beyond non-glacial locations where hydrological regimes are changing (i.e. probably many locations) and leading to stratification change. There are other variables at play in Patagonian fjords that might not be applicable to Arctic fjords (e.g. nutrient loading for aquaculture as one example). This review is on the Arctic so a section that relies heavily on Patagonian literature feels like a stretch as a separate section.

Specific comments:
L167-170: I'm curious how this estimate of pan-Arctic primary production compares with more recent estimates (if they exist). As the authors will be aware, estimates from ocean colour are dependent on the algorithm used (Lewis et al., 2016), especially in the Polar regions and when CDOM is sometimes very high (e.g. near Arctic rivers). The annual primary productivity looks very high on the west Greenland shelf in Pabi et al. (2008), which does not necessarily match with some observations (including those referenced). Do the authors have any knowledge of this? Perhaps this is still the best estimate.
L231: This is quite a good example of the point in my first paragraph (using references in a balanced manner). Yde et al. (2014) is a good paper and I wouldn't argue for not including it, but this is the only location in which it is referenced. There are other papers

that are referenced later in the text that describe the chemical composition of glacial meltwaters around Greenland just as well that could be used here but aren't.
L270-272: Generally agree (especially in locations like Patagonia where overdeepening has lead to many proglacial lake forming with glacial retreat) but is dependent on multiple factors are at play including bedrock topography, bedrock composition, hard/soft beds, previous climatological scenarios etc... Suggest "may decline" instead of "declines".
L370: There is probably a simple explanation to this, but where does the value of ~5.9 uM come from in Meire et al. (2016)? Table 1 in that paper suggests the meltwater river values are more like 30 uM.
L399-400: I think this might be missing some nuance that I suspect is mainly a disagreement with what can be considered a meaningful flux (which I don't really want to get into too much as we'll be going around in circles). As the authors know the argument in the e.g. "glaciological literature" that fluxes may be underappreciated is based upon very large fluxes of labile particulates that aren't necessarily reactive on timescales observed (e.g. the demonstration in Hawkings et al. 2017, that a large proportion of ASi dissolves in seawater over the course of several months to a year). I think the "larger picture" is based upon additions to marine nutrient inventories rather than direct fertilisation of marine phytoplankton. For example, the current estimates of silica budgets (e.g. Treguer and De La Rocha, 2013 and Frings et al., 2016) include reactive riverine particles as part of the Si inventory (albeit poorly understood and constrained), not necessarily because they contribute to direct diatom fertilization, but more because they add to the marine Si inventory. The authors touch on this later in the manuscript by reference some of the benthic literature, but it's still a potentially important distinction (again the authors might not agree with me).
L410-415: These articles argue that they may have a positive effect based largely around the reactivity of the particles, which has been demonstrated. The idea that particles are not important when they are sedimented is not strictly accurate either. Benthic reprocessing also includes diffusive fluxes from sediments (e.g. Frings (2017) and Wehrmann et al. (2014) estimates of diffusive Si and Fe/Mn fluxes from sediments are substantial).
L423-435: Nice paragraph!
L436: DOM should be "dissolved organic matter" not "dissolved organic materials". Please correct.
L475-480: Again, there is probably more relevant literature to cite in this context (i.e. seasonal variation in nutrient concentrations from Arctic glaciers), and this is the only location where Brown et al. (1994; a study on an Alpine glacier) is referenced. As I mentioned, it is unfortunate there is not more balance in the referencing here.
L487-491: This is quite disappointing and again emphases my point of lack of balance. I don't really think the point is substantiated here either. A synthesis "of available nutrient distributions in glaciated Arctic catchments, especially for Si and Fe" is a sentence that requires some context. The Fe and Si concentrations in these publications actually agree relatively well with others (in the context of "dissolved" concentrations), so it is perplexing why they are singled out here for criticism when some of these other studies also emphasise large fluxes of e.g. Fe (Bhatia et al. 2013, Stevenson et al., 2017). These studies also show why it is important to look as seasonal datasets due to large

variability in elemental concentrations over a melt season, yet this is not reference at all in the above discussion which is unfortunate. Please also see my point in L410-415.
L643-644: Doesn't need to be solely during the meltwater season. Diffusive release of elements from benthic environments will happen year-round.
L644: References should be Wehrmann et al. (2014).
Section 5.2: I think this section may benefit from some additional discussion on e.g. the role of Fe oxyhydroxides on the export of C to depth (i.e. the "rusty carbon sink"), and on possible adsorption of PO4 to particles (this was briefly mentioned in Cape et al., 2019).
L721-723: Perhaps in non-glacial rivers yes, but the N:P ratio in glacial rivers is much lower than typical Redfield ratio (16:1) if using end members in Table 3 (more like 7:1). Related to the point, but to me this suggests that DON contributes a significant amount of "biolabile" N, contribution from NH4, and/or adsorption of PO4 to particles (and removal from the fjord surface) are all possible. I think this also provides some nice context as to why these environments are likely to be N limited.
L751-758: Citing van der Merwe et al. (2019) would also be appropriate in this section.
L841-861: I think it would also be appropriate to cite van der Merwe et al. (2019) here as well.
L859: How about "how particle bio-lability, ligands and estuarine mixing processes moderate the glacier-to-ocean Fe transfer"
L936: suggest "minor source of total DOM to the fjord".
L943-947: I think there's a bit of repetition here. It is sufficient to say that glacial DOM appears to be more labile than non-glacial DOM because of the high proportion of aliphatic or protein-like compounds (e.g. Barker et al., 2013, Dubnick et al., 2010, Hood et al., 2009, Pautler et al., 2012) commonly associated with microbial activity.
L970-972: I think saying it is diluted and consumed is more appropriate. DOM is relatively unique in that the concentration doesn't really matter (to a degree) – the composition is most important. Even if it dilutes marine waters, it's (glacial meltwater) highly biolabile nature means it is likely to be relatively important for heterotrophs.
L973-976: This is complex and a this is perhaps a generalisation considering the complex mixture of organic compounds present and the relatively high salinity of these sampling locations.
L990-992: Only some may be photodegraded so this is an over generalisation. There is a standing stock of DON in the ocean surface that is pretty recalcitrant are resistant to biological utilisation (it is favourable for nitrogen fixation for example; Letscher et al., 2013).
L995: It is appropriate to reference Wadham et al. (2016) here.
L1002: Appropriate to reference Hawkings et al. (2016) here because it is one of the only papers that has and discusses values for DOP in glacial meltwater rivers (low to negligible concentrations).
L1048-1050: The previous references refer to catchments with barely any glacial meltwater input contribution to discharge and this should be made clear here as this sentence does not make that clear.
L1055: It is slightly misleading to say that freshwater runoff in these scenarios includes glacial meltwater. Please change to "non-glacial freshwater sources". As I noted in my previous review, meltwater discharge in Patagonia is not currently decreasing, as

opposed to non-glacial precipitation driven catchments where precipitation does appear to be undergoing a long-term decline.
L1083-1085: Completely agree that glacial discharge affects stratification, but doesn't it also supply very cold water to the surface. The argument of an increased prevalence of HABs in Patagonian fjords and the Alaskan case study (Vandersea et al., 2018) is that warming of the surface also plays an important role. I therefore don't fully understand the link between increasing glacial meltwater freshwater discharge in the Arctic, a warming fjord surface layer (despite the increasing flux of cold, very fresh water) and HABs.

Table 3: Lawson et al. (2014a) do not produce their own DON data. The concentrations used here are from the Wadham et al. (2016) reference that was "in review" when this article was published. This concentration estimate was likely based off a mean seasonal concentration rather than the discharge weighted concentrations given in Wadham et al. (2016) of 1.7 uM.

New references cited:

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