

Response to reviewer's comments shown as blue text.

The reviewers and editor are thanked for their further time considering the text, especially over the holiday season. For clarity in responses below, lines refer to R1 as per the reviewer's comments. Revisions are annotated in the updated text (R2).

Reviewers' comments.

The authors clearly demonstrate their diligence in covering the scope of ice-ocean biogeochemical research. It is apparent that more effort should be devoted to describe the variability of these ecosystems and their responses to regional forcing, with particular emphasis on continued monitoring and automation, under a wider geographical lens (both hemispheres). The developing portrait of the ice-ocean interface may be obscured by shifts in baseline processes — therefore, the case-study approach here is particularly effective at capturing spatial and quasi-time variability, by comparing distinct regions and exchanging “time” for “place.” The calculations made in this review, I think, should motivate others to constrain the large uncertainties. Each layer of complexity presented has better informed the modeling community and I think this review will serve the scientific community well to think about multiple controls on primary production in dynamic coastal regions. The authors have whole-heartedly considered initial comments from the reviewers and made the appropriate changes. The review is formatted well and it is easy to extract the summarized findings. It is therefore my recommendation that the review be published in TC.

R: The reviewer is thanked for comments on the text.

[end of review]

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.

R: We acknowledge that there are areas where we disagree with the lines of argument raised by the reviewer. However, we do not think our newly-revised discussion is unbalanced. There are inevitably going to be a few places in such a broad text where we can always add more references and thank reviewers for suggestions accordingly. In some of the specific examples (see below) where it is claimed that the choice of references is unbalanced, our choices are very similar to that in other similar recent synthesis (e.g. the Wadham 2019 review cited, or the short related comments in Chapter 3 of the IPCC O&C report), thus if we are unbalanced we are no more so than in other comparable texts.

We understand the reviewer is passionate about seasonal shifts in freshwater nutrient chemistry, but these shifts aren't significant enough to be evident in the corresponding marine environment and are not really relevant to the topic of marine biogeochemistry/primary production (PP) (see comment below). The seasonal changes in freshwater nutrient composition are far less significant in terms of potential effects on biota that seasonality in many other variables/phenomena which

are, at most, lightly covered in the text. Therefore, we feel that it would be unbalanced to single out seasonality in freshwater nutrient content as a particularly important area to cover.

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.

R: The mechanism concerned here, freshwater-driven stratification, is not specific to certain regions, it’s generic to many coastal environments [see also the reviewer’s comment on temperature/stratification which we feel is incorrect, as it is well established that addition of cold, fresh meltwater into glacier fjords leads to strong stratification and thus very warm surface waters in summer, which is why these locations are considered at risk of HAB increases].

Contrary to the reviewer’s comment, the Joli et al., (2018) reference specifically explains the meltwater-driven mechanism that is associated with the distribution of HAB species in N Greenland and may lead to HAB expansion in the Arctic. This is the same mechanism that operates in non-glaciated catchments with freshwater discharge.

To clarify, we have reduced this former section, and we accept that it is the least well-covered in terms of existing Arctic literature. In the revised text, these comments are condensed to three paragraphs to summarise the few papers available and why it is an interesting hypothesis for future work (For clarity the deleted section is removed entirely in the tracked changes text and only shown once where added as 10.1).

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.

R: There are several updates to this trend, which generally show that PP (primary production) has been increasing over the past decade across most (almost all) Arctic basins (now added in text). These are broad regional surveys, they thus integrate areas of high PP and low PP within specific geographical regions. There is not necessarily a miss-match with any shelf region, but any “average” will invariably smooth hotspots of PP into a lower regional average, especially where

any sort of high PP region has sharp fronts with a lower PP region (e.g. at a shelf break) or a short bloom duration (most areas). We used the 2008 reference (some later work is available by the same group) because it is one of the few Pan-Arctic means available to quote and corresponds better to the dates of most PP measurements shown in section 3. This doesn't affect our subsequent discussion as it is a pan-Arctic phenomenon largely related to warming/increasing open water days. (Any more recent value would slightly increase the pan-Arctic mean and thus PP of each of the glacier fjords relative to an Arctic average would be lower).

Concerning the W Greenland shelf, the values discussed herein are in glaciated catchments and almost invariably inshore of the high productivity observed at the shelf edge, thus there is no contradiction with satellite records which generally cut off most glacier-fjord and inshore systems. Qualitatively, satellite data does match the high productivity measured (cited herein) in some areas of Disko Bay (among the furthest PP points away from the coastline in the data compilation), but is of limited use in most other glaciated 'catchments' because the scale is below that at which satellite data can resolve chl a.

Concerning satellite a logarithms, yes there is extensive debate about what this derived measurement actually represents and what fraction of chl a near the ocean surface (i.e. to what depth) satellite derived estimates penetrate. Never-the-less recent satellite work is extensively calibrated against in situ measurements and there are no other methods for looking at such broadscale PP shifts. The CDOM interference is confined to areas of extensive river plumes and attempts are made to calibrate for it, it doesn't affect glaciated catchments where freshwater leads to low CDOM. The main problems in glaciated regions are particle plumes and the difficulty resolving satellite data within fjord regions.

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.

R: It is not clear what the reviewer wants us to do here. There are 20+ papers that could be cited in this section, when making general points like this we have selected a range of literature to cover the breadth of the subject discussed with 2–3 given to complement each other here, for example, a site specific study covering a broad range of chemical properties, an Si piece with additional synthesis of all available macro-nutrient N/P/Si data, and a pan-Greenland Fe piece synthesizing all available trace metal data.

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".

Ammended.

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.

R: As stated in the text, this value refers to Kongsfjorden and is not from the Meire work which concerns Godthabsfjord.

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.*

R: We agree there is a clear distinction between long-term nutrient inventory change, and short-term nutrient availability change (i.e. a compound present at low concentrations in meltwater is adding to the ocean inventory on geological timescales, but meltwater decreases the short-term availability in regional surface waters due to dilution and/or stratification). Yet the papers concerned discuss fluxes in the context of inter-annual changes, not in terms of the geological ocean inventory.

If fluxes are not evident on annual timescales, they can't be discussed in the context of annual changes in the marine environment, and especially not in the context of fertilization, which is how the fluxes referred to are presented, discussed, and interpreted (as summarised in Wadham et al., 2019, in the literature in general and in the IPCC O&C). The larger picture of geological timescale shifts in ocean inventories is indeed interesting, but that is not discussed at all in the references cited and thus it is difficult to infer that this is the message the authors intended to convey.

We find that the comment 'dissolves in seawater over the source of several months to a year' is not quantitatively consistent with any case study we can find (see specific examples below). If the fluxes were occurring due to dissolution on annual timescales, they would still be evident in water masses transiting through glacier-fjord regions on these timescales.

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).

R: This is an important distinction, but the papers discussed herein are not discussing this effect, they are discussing short term annual fluxes, arguing that dissolution occurs in these short timescales, and the potential effects on short term biogeochemistry/productivity — which is what we remain critical of. If the total fluxes referred to are not meant to be interpreted as annual fluxes associated with runoff (which is how they are presented, cited and discussed in later work by the same/similar authors including the recent IPCC report), but instead as long-term geological fluxes into the ocean nutrient inventory, then they would for example also be inclusive of the benthic flux associated with dissolution of these particles (whereas the fluxes critiqued are presented by the same group/authors as being additional to the near-glacier benthic flux, which is inconsistent with the reviewer's comment and supports our interpretation of the data).

If a labile element is dissolving in seawater and adding to the dissolved nutrient inventory (i.e. PO₄/NO₃/Silicic acid), then the resulting dissolved nutrient will be present in seawater flowing away from the glacier, especially where this 'outflow' is subsurface and not subject to pronounced drawdown by productivity in surface waters (circulation through these regions occurs over a

similar timescale of a few months to a year). So any dissolved inventory, or calculation concerning the flux derived from the dissolved inventory (e.g. Fig 4) in outflowing glacially modified waters is entirely inclusive of any dissolution occurring over short time periods in the water column (or as a benthic flux into the same water mass). These dissolved nutrient inventories are not 'missing' a dissolution term, they are inclusive of it.

Math to prove the above point is unfortunately challenging to do accurately in most of the catchments discussed in the review paper, because we cannot accurately constrain the time it takes for a parcel of water to enter and leave each glacier-fjord system (the residence time), but there is one large Arctic catchment (unfortunately not one of these 5) this can be done reasonably accurately with. Work by Wilson & Straneo (2015), and others, constrains the residence time of water from the 79 North Glacier, one of the largest discharge sources in N Greenland, as about 4-11 months beneath the floating glacier ice tongue i.e. comparable to the timescale over which the reviewer suggests a large amount of dissolution should occur. This catchment is particularly useful to study in terms of abiotic processes as the vast majority (~85%) of freshwater outflow occurs at depth so any nutrients released from meltwater/particles are not subject to pronounced drawdown in the photic zone.

Full depth profiles of all nutrients are available for this catchment e.g.
<https://doi.pangaea.de/10.1594/PANGAEA.905347> and
<https://doi.pangaea.de/10.1594/PANGAEA.879197>

Looking at the change in Si concentrations contrasting inflowing Atlantic water, and outflowing glacially modified Atlantic water, from the 79 NG fjord system, the enrichment in Si is within uncertainties, i.e. there is no significant constant Si flux occurring from the 79NG system into the Atlantic after water has been beneath the glacier tongue for a timescale comparable to that over which it is suggested dissolution should be occurring. This is very difficult to reconcile with the argument that ASi is dissolving and releasing large quantities of dissolved Si, if this were the case most of the enrichment that could possibly occur from ASi dissolution and enrichment of Si should be evident. However, it is consistent with global biogeochemical models which do not prescribe, or require in order to match observations, a specific ice sheet Si source in the North Atlantic/Arctic.

This is not an isolated example. There are several well studied water masses in glacier fjords (Kongsfjorden is one example) where water enters in winter and then becomes trapped through summer behind sills in inner fjord environments. Such waters are then exposed to high particle loads through the year before being mixed again in autumn and should thereby accumulate dissolved nutrients from processes such as particle dissolution and benthic release. In Kongsfjorden, we cannot find any Si data for such trapped water masses above around 1-10 μM (after 3-6 months of exposure to high particle loads), again suggesting that the proposed net dissolution by Hawkins et al., is unrealistically high, or at least that other processes remove or counter act a net dissolution close to the glacier such that the distant effect of Si availability is limited. Likewise, full depth profiles for Godthabsfjord are available (from GEM, Fig. 4) and show that the net change in Si in deep waters flowing into the fjord is modest (about a +30% 'top up' of the inflowing Si over the time of its residence in the fjord-maybe a year or so- far short of what should be evident if the down-fjord Si export in surface waters/efflux from Greenland occurs primarily as a result of dissolution Hawkins et al.)

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).

R: It is not clear what the reviewer means by ‘has been demonstrated’, particle plumes are universally (as far as any literature we can find shows) a negative influence on primary production. The reactivity of the particles does not equate to a net positive effect on primary production, for example there is no doubt at all that large quantities of labile Fe are released from glacial outflows, but the primary effect of this material in these environments is to deprive phytoplankton of light and to cause death-by-ingestion to filter feeders, hence the high reactivity is irrelevant in the context of organisms exposed to this material.

Yes benthic fluxes are also important, but if the reviewer’s earlier comments are correct, that these fluxes are intended to represent long-term accumulation of nutrients in the ocean inventory, calculations with 100% dissolution of lithogenic material to produce annual fluxes are already inclusive of any fluxes arising from re-working on this material and not additional to it. Thus, we find it challenging to follow the reviewer’s argument and to reconcile it with the arguments as published.

The problem is not whether or not these fluxes are important, the problem is that fluxes arising from dissolution (or any other estuarine process) are already included in any calculation that derives the flux out of the ‘glacier system’ into the ocean based on observed marine concentrations, and that numbers derived from such calculations (see above examples) are far smaller than the annual fluxes presented.

L423-435: Nice paragraph!

L436: DOM should be “dissolved organic matter” not “dissolved organic materials”. Please correct.

R: ‘Material’ and ‘matter’ are used interchangeably in the literature. To clarify, we now use ‘dissolved organic matter’ throughout.

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.

R: These sentences lacked any references which was an oversight, we have now added appropriate literature. We do not understand the comment on Brown et al., here. This paragraph was written primarily by the glaciology co-authors and is a statement of relatively uncontroversial literature, Q-C relationships are investigated extensively in older literature and thus citing some older work here establishing the basis of Q-C relationships seems appropriate.

L487-491: This is quite disappointing and again emphasises 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).

R: The sentence 'of available..... catchments' refers to the several pages of discussion preceding, perhaps the phrasing was odd/unclear. We can rephrase 'a synthesis and analysis of available marine nutrient distributions [above]'. The freshwater work critiqued does not agree with marine literature because the marine work in question is inclusive of any dissolution occurring from particles, whereas the critiqued work argues that a far larger flux arising from the 'dissolution' of new particles should be added on top of the existing dissolved fluxes. Thus, the critiqued work proposes fluxes significantly higher than those which can be derived from the marine data discussed.

The papers in question (critiqued) specifically claim that dissolved nutrient fluxes from freshwater are generally under-estimated due to a lack of consideration of dissolution from labile particulates and that this may be linked to primary production on this spatio-temporal scale. Other work we can find does not make such specific claims.

Bhatia et al., do make comments concerning the effect of iron in the Atlantic and attempt to link discharge and bloom properties in parts of the Labrador Sea citing a strong correlation between peak bloom magnitude and annual discharge volume. Yet the bloom referred to occurs before meltwater release in the region/years concerned. Thus, a cause-and-effect relationship seems questionable. The Stevenson et al., reference is largely concerning the isotopic signature of Fe around Greenland and does not contain any discussion of primary production or effects in marine waters other than a comparison of the concentrations measured to other work.

The sentence as written in R1 was however incorrect and shouldn't have referred to Fe following changes made to R0 in response to the reviewer noting that Fe fluxes are subject to overlap when the uncertainty on estuarine mixing is accounted for in the same way consistently. It previously stated in R1 'macronutrient fluxes', but then also 'Fe and Si' at the end. It should have been amended to drop 'Fe' and the Hawkings et al 2014 Fe reference from the sentence. Thus the corrected sentence reads: "the recently emphasized hypothesis that macronutrient fluxes from glaciers into the ocean have been significantly underestimated (Hawkings et al., 2016, 2017; Wadham et al., 2016) is difficult to reconcile with a synthesis of available nutrient distributions in glaciated Arctic catchments, especially for Si (Fig. 4)'.

The Wadham/Hawkings papers referred to are the only references that we are aware of that specifically claim glacier-to-ocean fluxes have been generally under-estimated because of a hypothesized under-appreciation of labile particulate dissolution, that link meltwater (and the associated particle dissolution) to fertilization/productivity and imply that increasing meltwater/nutrient fluxes will be increasingly important for marine nutrient supply. Accordingly, they are also the only manuscripts cited as substantiating this point in other reviews/synthesis (including Wadham et al., 2019 and several points in Chapter 3 of the IPCC O&C). Thus, our interpretation, if unbalanced, is at least representative of the communities' and the respective authors.

These studies also show why it is important to look at 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.

R: In freshwater, but this is not relevant to broad-scale marine processes because this term is very small (Figure 10). Seasonal variation in freshwater endmembers has no discernible broad-scale effect on marine nutrient availability in these locations compared to other factors- particularly in light of the effects induced by stratification. The freshwater nutrient addition/dilution is so small that even increasing freshwater concentrations by an order of magnitude wouldn't significantly change PP within uncertainties (see Fig 10) and thus we disagree that seasonal variations in

meltwater composition should be discussed extensively. There may well be other reasons why such seasonal changes are important in a glaciology context, but not generally with respect to marine primary producers.

For Godthabsfjord for example, if we take a low and a high estimate for the N concentrations in freshwater discharge, (0.5-3 μM), and assume the variation is entirely due to seasonal shifts, and that the seasonal shift is maximised by a simple move between the two extreme N concentrations, the importance of freshwater for total N input compared to that required to support PP as per Meire et al 2017, would vary between maximum and minimum contributions of 1-7% to PP N uptake over the season. This can hardly be argued to be a significant feature. And this is oversimplistic as the stratifying effect of freshwater, which changes the water column structure and thus mixing and N supply, makes such a calculation meaningless.

Seasonality in general is of course important, but seasonality in light availability and properties, phytoplankton dynamics, subglacial discharge rate intensity and frequency, water column structure, feeding patterns and excretion related nutrient cycling, and the carbonate system are all much more significant than seasonal shifts in freshwater composition. It would be unbalanced to include a section on seasonal shifts in freshwater composition rather than any of these topics.

L643-644: Doesn't need to be solely during the meltwater season. Diffusive release of elements from benthic environments will happen year-round.

R: Thank you, we have re-phrased here to avoid misreading of this sentence.

L644: References should be Wehrmann et al. (2014).

R: Corrected.

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).

R: We are aware of work in prep (or possibly now in review) on both of these research topics, which we agree are interesting and relevant, but were unable to find much literature specifically addressing these topics in an Arctic glacier context. We have added a sentence raising each question at the end of this section as suggested.

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.

R: The Redfield ratio is challenging to apply in glaciated catchments, and yes this is largely because we don't have particularly extensive measurements of species like DON/DOP. Ratios are also subject to large uncertainties because many parameters (PO₄/NH₄) are often close to detection with small differences in the values themselves or how blanks are handled making a very large difference to any ratios, and similarly the effect of including/not including NH₄/DON may matter much more than in waters where the bioavailable N/P pools are more dominated by fewer species.

The cellular N:P ratio however (e.g. Ren et al., 2019) is a far less ambiguous indicator of whether either N or P is generally deficient because it reflects the actual status of organisms and doesn't

rely on assumptions about what aqueous labile/dissolved species should/shouldn't be included in any ratio calculation and this supports the idea that P isn't particularly in excess.

It's very difficult to comment further as there are few conclusive studies defining the relative importance of N/P to constraining PP in plume environments, and to some extent it may not simply matter as light limitation prevails to such an extent that neither nutrient supply is exhausted by phytoplankton during the short time period water spends in turbid glacier outflows.

L751-758: Citing van der Merwe et al. (2019) would also be appropriate in this section.

R: Relevant van der Merwe work added (an earlier paper by the same group is more appropriate for this specific point).

L841-861: I think it would also be appropriate to cite van der Merwe et al. (2019) here as well.

R: Added.

L859: How about "how particle bio-lability, ligands and estuarine mixing processes moderate the glacier-to-ocean Fe transfer"

R: 'Bio-lability' is not a well-defined term in a marine context, but it generally refers to the ease with which biology directly changes the lability of solid phases which we are not referring to here. In an Fe-flux context the most important term by far in terms of changing Fe species with respect to increasing the size of the bioavailable pool is likely the ligand-binding of Fe phases (Gledhill & Buck 2012) rather than the lability of the solid phases as these environments are saturated with labile Fe irrespective of the definition (e.g. Lippiatt work cited). We prefer the terms 'bioaccessibility' and 'bioavailability', which are better defined.

L936: suggest "minor source of total DOM to the fjord".

R: We don't see the difference here, as they are a minor source irrespective of how DOM/DOC is defined.

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.

R: There are 3 separate issues: 1) is how the total flux of DOC may change as glaciers retreat, 2) is how labile this material is, 3) is how the lability may change with retreat. Therefore, we prefer the sentences as written.

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.

R: The main process reducing concentration on this scale is dilution as evidenced by linearity in salinity plots. Lability and concentration are important for any nutrient, not just DOM/C. Even deriving labile OC rather than total OC, by any measure, glacier derived OC does not constitute a significant (or sometimes even measurable) OC enrichment in the marine environment. This can be seen in both chemical, uptake and bacterial distribution studies. The cited studies consider both the lability and the total flux and still conclude the impact is negligible or minor.

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.

R: The isotopic work quoted is consistent with OC budgets in both Young Sound and Kongsfjorden which reach similar conclusions (see above comment). Irrespective of how mixed the composition of OC in meltwater is, it does not constitute a large fraction of the OC utilized by bacteria in this location.

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).

R: 991 'is' rephrased 'can be', but we note in the Arctic/glacier context this and biological uptake are the prime degradation routes. The standing stock in offshore marine waters is not related to glacier discharge.

L995: It is appropriate to reference Wadham et al. (2016) here.

Added.

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).

Added.

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.

R: (Obsolete, this section is slimmed and no longer exists as a major component of the text).

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.

R: Perhaps the reviewer is not aware, but cold freshwater induces density stratification, which can result in warming of surface waters. Therefore, even if freshwater enters the Arctic at the ocean surface at temperatures close to zero, it can rapidly heat to warm temperatures in a surface low-salinity layer in a confined fjord (maybe 5-10°C, in a thin layer at the surface in the freshest water in Greenland's glacier fjords). Meltwater in surface layers in glacier fjords is therefore very warm and adding more meltwater further stabilizes surface waters and can lead to higher temperatures. This is well established and is a useful example of how freshwater properties alone cannot easily be used to make extensive comments on marine water column changes.

Hence the freshwater release is driving the temperature change (and thus potentially HAB susceptibility) in glacier fjords.

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 μM .

R: Thank you. We were not aware of this; references changed accordingly.