The reviewer is thanked for detailed and thorough comments on the text. A point-by-point response to issues raised follows. As some general issues are raised across multiple comments, a few key topics are discussed here first which we will refer back to.

(a) Linking glacier particulate nutrient fluxes to primary production

The issue of links between glacier-derived particles and marine primary production in environments downstream of glaciers has been raised extensively in several recent papers around Greenland (Hawkings et al., 2016, 2017; Wadham et al., 2019), implying that glacier-derived particles contain 'bioavailable' nutrients and are contributing to 'high productivity' in meltwater affected waters. But where is a positive link between glacier-derived particles and Arctic marine primary production (PP) established?

For example, looking at the recent (Wadham et al., 2019) review, which focuses extensively on glacier particulate fluxes where discharge enters the ocean (i.e. near zero salinity) and does a good job of bringing together geochemical work in this field, the justification for the conceptual link between glacier-derived particles and Arctic marine PP is summarised in the following paragraph, which underpins the large emphasis on these fluxes in recent manuscripts and their link to the global C cycle:

"Observations of heightened biological activity in marine waters surrounding glaciers has been noted as early as 1938, when brown zones (representing turbid melt plumes) in front of tidewater glaciers were noted to be particularly productive regions for biota[70,71], with more recent research reinforcing this connection[10,72]."

But the references in this paragraph do not support this statement with respect to primary producers — those in the Arctic show the exact opposite. The Lydersen et al. (2014) reference states that glacier-associated particle plumes downstream of Arctic glaciers negatively affect marine primary production. The argument presented in Lydersen et al. (2014) is that secondary production (not primary production) is high around glacier plumes in Arctic fjords because microbial biota are 'stunned' by the stressful, turbulent conditions in these plumes, which then make easy prey for higher-level organisms. This is consistent with all other literature in the study location (Kongsfjorden — a site we already discuss extensively). Primary production measurements, and extensive chl a data, show these 'brown zones' are low PP environments (e.g., Hop et al., 2002). The Meire et al.(2017) reference similarly does not support the statement, as the lowest primary production in the region studied (W Greenland) is associated with heavy particle inputs. This paper attributes the increased primary production in some glacier fjords solely to upwelling, not freshwater runoff and/or associated particles— which is suggested to have no clear significant positive effect on productivity assessed in terms of higher trophic levels. The other references referred to (and references therein) either discuss Antarctic catchments (where a fertilizing effect arises from Fe, although in early references this is not extensively discussed correctly as it wasn't widely accepted until the 1980/90s that Fe-limitation was extensive across the Southern Ocean), or the upwelling effect. As acknowledged later in the review, there is no explicit evidence of a link between glacier derived particles and increased marine PP around Greenland. Yet data is available to assess this, so we should perhaps use it herein.

It is challenging to produce an Arctic 'average' PP for reference, because PP primarily occurs during intense bloom periods, and thus annual PP averages for any specific area are biased low compared to typical PP during any bloom period. Furthermore, observations in glacier fjords are generally temporally biased towards bloom periods. We can however define two rough reference PP values.

One, a low threshold for 'high' PP (as it integrates high and low PP time periods) is a simple pan-Arctic mean for the period March–September (420 mg C m^{-2} day^{-1}) (Pabi et al., 2008) (which we already reference in the original text). This provides a very low benchmark for assessing 'high' PP for an individual PP measurement. A more meaningful comparison, because the limited primary production data downstream of glaciers is generally biased towards summer, and because fjords are shelf environments, is to look at the range of shelf PP across the Arctic in May–August (360–1500 mg C m^{-2} day^{-1}) (Pabi et al., 2008) (Figure 1).

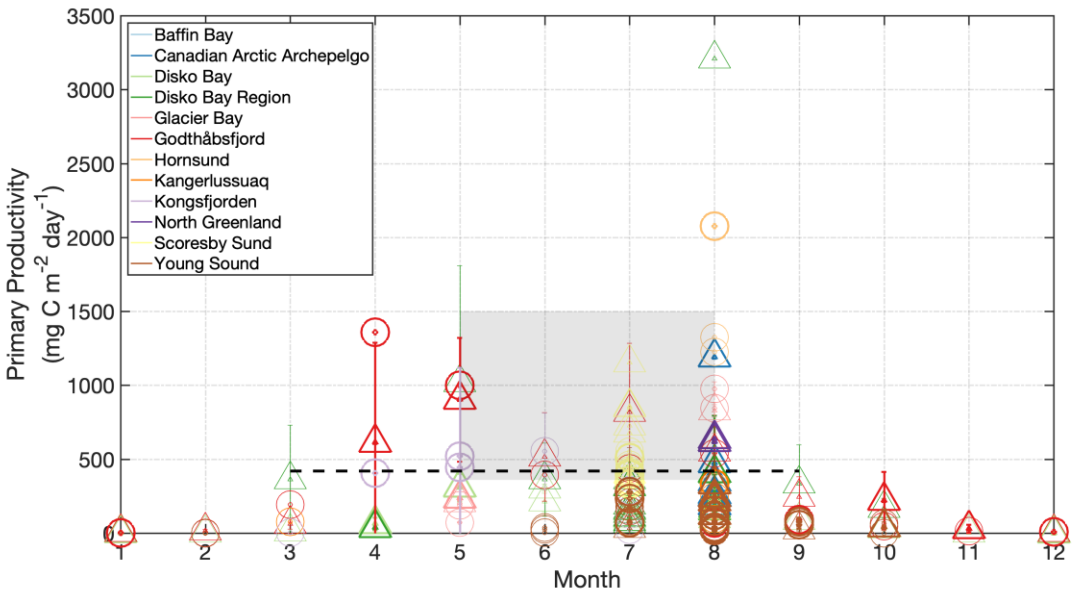


Figure Discussion 1: All available Arctic primary production data by month for regions with glacial influence. Data from (Harrison et al., 1982; Hodal et al., 2012; Holding et al., 2019; Hop et al., 2002; Iversen and Seuthe, 2011; Jensen et al., 1999; Juul-Pedersen et al., 2015; Levinsen and Nielsen, 2002; Lund-Hansen et al., 2018; Meire et al., 2017; Nielsen, 1999; van de Poll et al., 2018; Rysgaard et al., 1999; Seifert et al., 2019; Smoła et al., 2017; TG and Hansen, 1995). Circles represent glacier-fjords, triangles sites beyond glacier-fjords, bold shapes are <80 km from a marine-terminating glacier. Error bars are standard deviations for stations where multiple measurements were made in the same both. Hashed line is the pan-Arctic mean primary production (March-September). Shaded area is the pan-Arctic shelf range of primary production for May-August.

We summarise this by categorising data according to whether PP was determined close to a marine-terminating glacier, or within a glacier-fjord producing the following statistics for the peak of the discharge season (July-September) (Table 1).

Category	Mean primary production (± standard deviation) mg C m^{-2} day^{-1}	n	Data from
(I) Marine-terminating glacier influence, non-fjord	817 ± 892	10	Disko Bay, Scoresby Sund, Glacier Bay, North Greenland, Canadian Arctic Archipelago
(II) Marine-terminating glacier influence, glacier fjord	480 ± 403	33	Godthåbsfjord, Kongsfjorden, Scoresby Sund, Glacier Bay, Hornsund,
(III) No marine terminating glacier influence, non-fjord	305 ± 268	29	Godthåbsfjord, Young Sound, Scoresby Sund, Disko Bay,

			Canadian Arctic Archipelago
(IV) No marine terminating glacier influence, glacier fjord	108 ± 88	40	Godthåbsfjord, Young Sound, Kangerlussuaq

Table 1. Primary production from studies conducted in glaciated Arctic regions and pooled according to whether <80 km of marine-terminating glacier ('marine-terminating glacier influence'), and whether within a glacier fjord. Data sources as per Figure 2. n = number of data points, where studies report primary production measurements at the same station for the same month in multiple years (e.g. Juul-Pedersen et al., 2015) a single mean is used in the data compilation (i.e. n=1 irrespective of the historical extent of the time series).

P values	I	II	III
IV	<0.001	0.001	0.153
III	0.001	0.211	
II	0.071		

Table 2. P values for inter-group comparisons (for Table 1)

It is apparent that glaciated regions are low productivity environments during the meltwater season with the exception of sites close to marine-terminating glaciers. Higher PP is generally found over the shelf, beyond the zone over which extensive sedimentation has occurred. Inner-fjord environments, with strong freshwater glacier plumes where glacier particles remain largely in suspension, are almost universally low PP (Lund-Hansen et al., 2018; Meire et al., 2017), and include the lowest Arctic coastal PP values measured (Holding et al., 2019). Inter-fjord variability is inevitably an issue and hence why we opted for the structure of the review considering the best-studied cases. But we note that even in the fjord region where the most extensive comments have been written about the potential positive fertilizing effects of particles downstream of glaciers (Kangerlussuaq (e.g. Hawkings et al., 2015, 2017; Wadham et al., 2016)), we see low PP during the meltwater season (Lund-Hansen et al., 2018) which is explicitly linked to the negative effects of glacier-derived particle plumes. Over the spatial range where these particles are visibly present in the water column (i.e. prior to extensive sedimentation improving water clarity), the photic zone is diminished, nutrient uptake is reduced, and chlorophyll a and primary production are low (Murray et al., 2015). Irrespective of their geochemistry, the general effect of glacier particle plumes is a suppression of marine primary production on this spatio-temporal scale, so we are unclear why such emphasis is being placed on the importance on a potential fertilizing effect of these plumes.

If Arctic glacier fjords were included as an ecological province alongside shelf/open waters/etc. (e.g. as per Pabi et al., 2008), they would (according to literature data), be the lowest PP province in the Arctic. We therefore disagree with some of the reviewers' comments on this topic, including those alluding to particulate fluxes summarised by (Wadham et al., 2019). This is problematic from the perspective of writing a review. It would be difficult to focus extensively on the geochemistry of glacier-derived particles across the Arctic and how they could fuel marine primary production, only to go on to show that the major effect of these particles determined from oceanographic data is light limitation (or other, less well characterised, negative effects).

(b) The effect of stratification and the influence on flux gate calculations

Similarly, some recent studies, as summarised by (Wadham et al., 2019), have argued that the nutrient flux from Greenland is larger than currently considered in the oceanographic community and have taken a freshwater orientated approach, typically multiplying the concentration of labile particulate phases in freshwater by freshwater discharge volume to define a flux into the marine environment. For inter-catchment comparisons this is perfectly valid. Yet concerning the net change

in PP, the only change that matters is the net change in nutrient availability in the surface mixed layer or photic zone over the growth season. This is not the same thing as the flux flowing out of a glacier precisely because freshwater changes the depth of the surface mixed layer, increases stratification, and generally decreases the depth of the photic zone. This is a well-recognised problem in the Arctic as it affects all freshwater nutrient 'sources' (McClelland et al., 2011a).

[Freshwater volume × freshwater concentration] will always produce a positive flux of any nutrient, but the net change in the availability of that summertime nutrient supply downstream can (and often is) still negative because of the effect of freshwater stratifying marine waters. Therefore comments concerning how marine PP will change based on large freshwater derived fluxes alone [freshwater volume × freshwater concentrations] are usually misleading as they often do not capture the direction of change (dilution of macronutrients within surface waters and loss of vertical supply) concerning the availability of a nutrient to marine primary producers. A flux gate at the point of discharge into the ocean can therefore be used for inter-catchment comparisons, but it can rarely be used to make comments about changing Arctic marine primary production. We have therefore deliberately avoided commenting extensively on work which falls into this category.

We do not dispute that freshwater contains macronutrients, or that freshwater contains labile particulates, but all of the field evidence we can find shows unambiguously that these 'direct' inputs are associated with negative (or no) changes to Arctic marine primary production and negative (or very limited) changes to regional Arctic nutrient budgets. Any plausible exceptions (which are limited in the Arctic) and reasons for this are already highlighted in the text (alongside comments on stratification and light limitation and a brief contrast with the Southern Ocean where Fe-fuelled phytoplankton blooms downstream of glaciers are well documented).

(c) Calculating nutrient fluxes from particle dissolution

Glacier-to-ocean fluxes for non-conservative components (e.g. Fe and Si) are more challenging to determine compared to generally conservative components because of the pronounced changes that occur in concentration and speciation across the estuarine salinity gradient. There are several approaches to deducing the flux from a glacier we can find in the literature to date:

1. Measuring freshwater dissolved and particulate concentrations and then using an estimated removal or dissolution factor to account for the extent to which the dissolved and particle phases interact over the estuarine gradient (e.g. a removal factor for Fe, or a dissolution factor for amorphous Si).
2. Using an estuarine mixing diagram to deduce the estuarine endmember which, in simple terms, accounts for the net dissolution/removal occurring across the salinity gradient and produces an effective concentration that can be multiplied by freshwater discharge volume to produce a flux.
3. A water mass analysis to determine the relative enrichment of a parameter in glacially-modified waters (by contrasting properties in a water mass before and after the addition of glacial meltwater).

All of these approaches can be used to derive a flux into the ocean. Each has advantages and disadvantages, and is subject to different data requirements. (1) Requires zero salinity dissolved and particulate data, and some knowledge of how the dissolved and particulate phases equilibrate over the salinity gradient. This method is therefore more problematic for estuaries with multiple freshwater endmembers and for parameters where the estuarine behaviour of a chemical is spatially/temporally variable. (2) Requires data across the salinity gradient, works particularly well if

the non-conservative effects are confined to low-salinity values, but is more challenging to apply if multiple processes are occurring on different scales. (3) Can be conducted only with extensive water column profiles and a water mass analysis, with most of the endmembers well defined in terms of their chemical properties prior to mixing. This is the most conclusive approach in terms of determining the net input of any chemical into the ocean, but also has the highest data requirement.

Using Si as an example, we can illustrate the present problem in the literature with existing fluxes.

Taking the Kongsfjorden Si data (in Figure 2) as an example, where both estuarine and freshwater dissolved Si endmembers have been determined, approach (2) suggests an estuarine endmember of 6.4 µM. An enrichment factor of 13% can then be calculated for use with method (1) by comparing 6.4 µM to measured freshwater endmembers.

For the larger catchments discussed, the same approach (method 2) can be applied to derive estuarine endmembers of 25 µM (Godthabsfjord), 29.1 µM (Gulf of Alaska), 8.49 µM (Bowdoin) and 7.47 µM (Sermilik) (Table 3) (Brown et al., 2010; Cape et al., 2019; Kanna et al., 2018; Meire et al., 2016). The largest caveats with this method are that it may cause an overestimate in catchments where strong diatom drawdown of dissolved Si occurs in the higher-salinity end of an estuary or an under-estimate where strong drawdown occurs over the full salinity gradient, and that it is generally conducted only for surface waters (thus, if there are large differences in the dissolved Si behaviour between runoff entering at the fjord surface, and subglacial discharge entering sub-surface layers, these endmember values only apply to the surface runoff component).

The only complete application of method 3 we can find in the literature is for Sermilik (Cape et al., 2019) where, in simple terms, the upstream, downstream, and fjord profiles of macronutrients (NO_3/PO_4/Si) are considered in order to deduce the enrichment arising from glacial meltwater. Fitting to observed export out of the fjord is consistent with a Si freshwater endmember of 4–10 µM. Because the fit is forced with all macronutrients, the Si endmember cannot be much higher than this, as the fit solution must account for both the observed Si, and NO_3 and PO_4 enrichments which arise from marine endmembers and don't include significant particle-dissolution on the same timescale as Si. The Meire et al. (2016) approach is a hybrid use of methods 2/3, considering the local perturbations to Si with some knowledge of freshwater Si endmembers producing an estuarine endmember of 28.7 µM. Method 3 can also be applied to extensive data from the Geotraces process study at the 79° North glacier, which is now available online[1] (we don't discuss this data because, unlike other catchments herein, hasn't been extensively discussed in the literature- but we note the concentrations of Si close to the glacier terminus are similar to, or lower than, for Sermilik and no discernible Si enrichment is evident in glacially modified water at the shelf-break).

Work on Si for Kangerlussuaq (Hawkings et al., 2017) uses method (1), with a dissolution factor for labile particulate Si of 100%. The freshwater endmember then becomes 395 µM. Applying the other methods to this fjord region is challenging given the limited data available, but if we plot the few estuarine datapoints available (Hawkings et al., 2017; Lund-Hansen et al., 2018) they are not dissimilar from other glacier fjords (Figure 3) — although (Hatton et al., 2019) implies they are. The saline endmember selected by (Hawkings et al., 2017) is odd as it refers to offshore surface seawater, but should refer to inflowing sub-surface seawater (summertime fjord circulation involves inflow at depth and net outflow at the surface). In any case, plotting either a 2 µM saline endmember as used (Hawkings et al., 2017), or a more realistic estimate of 10 µM based on other Greenland fjord systems, the maximum possible intercept at zero salinity (using method 2) is 25—30

[1] https://doi.pangaea.de/10.1594/PANGAEA.884128 and https://doi.pangaea.de/10.1594/PANGAEA.879197

µM. This would suggest a dissolution factor of 6—7% for the purpose of applying method 1. As noted by the reviewer, some incubation results are used to support the claim of 100% dissolution in the manuscript, but we would ask how relevant the timescale of the incubation (and incubation conditions with particles kept in suspension during this time) are to a fjord environment in which glacier particles are subject to rapid sedimentation? We suggest there is much work to be done to reconcile this high estimate of Si export with other work around the Arctic (Table 3). The simplest explanations that could be proposed for why this value is so high are that either it wasn't intended to construct an annual flux (i.e. represents Si dissolving on slower timescales), or that it is meant to be inclusive of all subsequent dissolution that could possibly occur from glacier-particles (i.e. direct dissolution whilst in suspension plus any subsequent benthic re-working). But it is apparent from the presentation of data in (Hawkings et al., 2017) and subsequent work by the same group, that neither of these explanations is the case. The flux is presented as an annual flux, discussed in terms of short-term PP, and is added to (no included within) additional benthic fluxes (Hendry et al., 2019).

Catchment	Method	Enrichment of dissolved Si due to dissolution	Estuarine endmember for flux calculations
Kongsfjorden	2	+13%	6.4 µM
Godthabsfjord	2		25 µM
Gulf of Alaska	2		29 µM
Bowdoin	2		8.5 µM
Sermilik	2		7.5 µM
Sermilik	3		4–10 µM
Godthabsfjord	2/3		28.7 µM
Kangerlussuaq	1	>150%	395 µM
Kangerlussuaq	2* (with large error due to limited data)		25–30 µM

Table 3 To summarize, the estuarine endmember that we would multiply by a Greenland freshwater discharge of x km^3 $year^{-1}$, inclusive of dissolved Si and particulate Si that dissolves in estuarine waters. Excluding the 395 µM value, all other work suggests an endmember of 6–30 µM for flux calculations.

We cannot find much discussion that quantitatively explains why this apparently large flux (Table 3) is more appropriate than values that can derived from other studies, which are universally much lower. As noted, much more extensive Si coverage is available for some other fjords (Kongsfjorden/Bowdoin/Godthabsfjord/Young Sound/79°NG) (Cape et al., 2019; Kanna et al., 2018; Meire et al., 2016). It is unclear why oceanographic data from these catchments, which is far more extensive, hasn't been used to test the hypothesis that global Si cycling is 'missing' a substantial glacial component given that now extensive literature has been written about this hypothesis (Hatton et al., 2019; Hawkings et al., 2017; Hendry et al., 2019; Wadham et al., 2019). We therefore maintain that our discussion of these cycles is balanced until a clearer reconciliation between 'high' estimates of fluxes can be made with large datasets.

Detailed comments on review

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 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.

The reviewer is thanked for detailed comments on the text. A point-by-point response to issues raised follows. We are critical of most recent work in the literature attempting to link particulate nutrient fluxes into the Arctic to marine primary production as a link between the two is not clear to us, primary production (PP) data does not support the hypothesis that glacially derived particles are a positive influence on marine primary production (see new section), it supports the opposite conclusion.

Benthic processes/fluxes was something raised during the initial potential list of topics alongside sea-ice, icebergs, glacial dust and others. As can be seen for the length of the review it is simply not possible to cover all of these 'glacier-related' topics in one review. We selected topics covered (and the title) to be those which were specific to the direct effect of freshwater discharge in the ocean, and those which have broad-scale effects. We had noted in the original draft that in some cases (e.g. the case of Kongsfjorden), there is direct coupling between benthic nutrient cycling and pelagic primary production - although we neglected to explicitly label this as a 'benthic' topic. We can rephrase to highlight this issue in general terms and to add some general comments on the interactions between glaciers and benthic recycling in the summary section where we link to work in related areas (dust/icebergs etc). A new sub-section ('Benthic pelagic-coupling enhanced by subglacial discharge'**) is added following and expanding on the NH_4 work of** (Halbach et al., 2019)**.**

We have of course updated the text with 2019 references in the revised text which were published between the original and revised submission dates. The (Seifert et al., 2019) **reference is particularly interesting, as the reviewer notes, a lithogenic 'ballasting' effect of particles is widely hypothesized in other contexts, but there wasn't much material in the literature to discuss this specifically with respect to glacier-derived particles at the time we wrote the review (we found only a few references, which lacked much direct evidence and weren't specific to meltwater).**

Replies

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

L 65 rephrased 'inorganic components'

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).

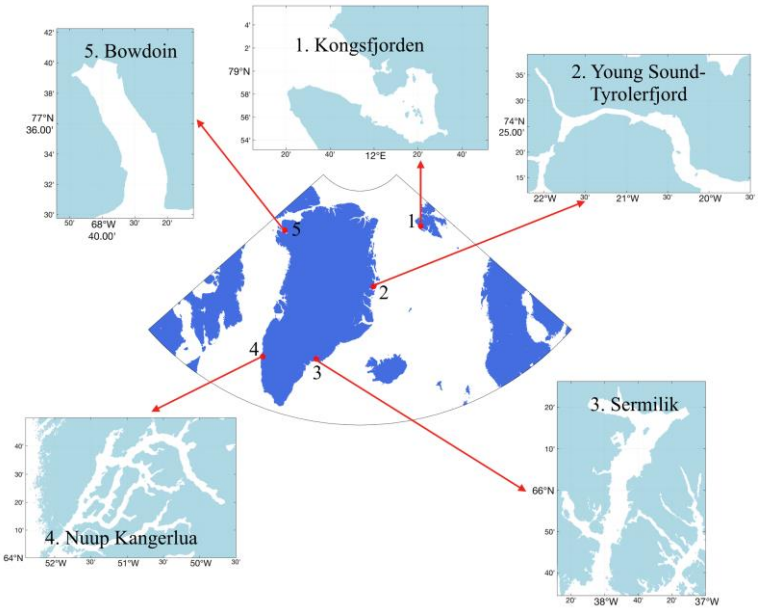
L64-68 (See general comment) Whilst these 'reactive' fluxes may be of intense interest to glaciologists the main effect of particle plumes on marine primary production (the subject of this review) is light limitation and thus we question how important the lability of these particles is. It would be an odd addition to the paper to discuss in detail the geochemical composition of particles, only to summarize that it doesn't matter in terms of its immediate effect on primary production because of light suppression. Many of these references refer to processes occurring on geological, not inter-annual timescales and are common to all ocean shelves, not specific to glacial-meltwater affected areas, and therefore a little beyond the scope of the current title.

Some comments concerning a geological timeframe could be added, but it would be very confusing to mix discussion of processes that affect PP on these two scales, recent literature concerning glacially-derived particles has focused almost entirely on inter-annual timescales which is what we discuss herein. There are fundamental differences in the relative importance of processes operating on these two different timescales which are not (particularly with respect to geological timescales) well agreed upon in the literature (e.g. see Tyrrell, 1999)**, and as noted, on these timescales the glacier-fjord systems we discuss herein cease to exist as ocean-glacier interfaces. We do not think it would be useful to discuss them together.**

It is also misleading to refer to labile lithogenic particulates as 'nutrients', as this implies they are actively taken up into biological systems, which is not generally the case either in glacial freshwater or marine systems. 'Labile particulate nutrients' in a marine context refers to organic P/N etc and not lithogenic elemental particles.

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.

A revised figure 1 is added, (this was previously a last minute add-on at the request of the editor).



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 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).

Section 3. The reviewer is referring to the very extensive freshwater nutrient work by Bristol University. We remind the reviewer that this is a marine review and thus some work which may be of major importance to freshwater environments is of much less importance in the marine environment. We started the manuscript with every piece of literature we could find concerning the impact of meltwater specifically in the marine environment. Supporting literature concerning ancillary fields of course comes later in the text. If it were the case that freshwater derived nutrient fluxes were particularly large, or a particularly dominant feature of meltwater of course this would come earlier, but we note that the direct input of freshwater constitutes <1% of nitrate inputs into these meltwater affected regions and therefore isn't particularly important for marine PP, and that the dominant effect of freshwater in the marine environment on nutrient availability is stratification (see general comment). It would be very odd to start a review with a detailed discussion of a budget component that makes a negligible effect to Arctic marine PP.

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.

There are longer datasets (multi-year) available in the marine environment for the case studies investigated (e.g. GEM portal). It is not an argument we agree with that time series of freshwater data (without corresponding marine data) should be used to make key conclusions about the marine environment when freshwater fluxes make a very small (for N/P negligible) component of nutrient input and when estuarine transformations and stratification (see general comment) fundamentally change the effect this material has in the marine environment. The term 'particulate nutrient' used by the reviewer, referring to labile particulate phases, is not used in a marine context because these phases are not actively up-taken by cells in the environments discussed, questioning the extent to which they can be described as 'nutrients'. They are more commonly described as a source of nutrients (Si etc).

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.

L 163 'Labile' elements (rather than organic C/N/P particulates) in particles are not generally considered as nutrients because they are not widely uptaken by cellular processes. With respect to PO4, NO3 and Si, measurements of these compounds are usually conducted unfiltered in the marine environment so it is more common to refer to 'macronutrients' than 'dissolved macronutrients'. Labile particulates cannot be referred to as 'nutrients' unless it can be demonstrated that they actively are taken up into biological systems.

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

L164 In the context of the ocean, all freshwater (with very few exceptions) is PO4-deficient (and often thought to be PO4-limited) so we are not sure what point is being made here. Meltwater is PO4-deplete as demonstrated by both concentrations and cellular C:P and P:N stoichiometry (Ren et al., 2019)**. Riverwater contains generally higher concentrations of organic P and thus meltwater is at the low end of freshwater 'bioavailable' PO_4 concentrations.**

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

We can add some general background here. Focusing specifically on Arctic catchments, river estuaries tend to show much higher Si, higher NO3 and similar PO4 concentrations (Cauwet and Sidorov, 1996; Emmerton et al., 2008; Tank et al., 2012). We have replotted the original Figure to include some example Arctic river catchments. It is difficult to select a 'typical' Arctic river, so we selected 3 catchments: the Lena, Mackenzie and Yenisey.

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.

L178 Re-phrased (the references were given as examples, there are a very large number of references giving freshwater nutrient concentrations for Kongsfjorden). Yes the LOD of PO4 is often problematic in these studies and we suspect if field blanks were properly/consistently reported through the literature the calculated PO4 concentrations in glacial freshwater would change. Whilst we do not particularly want to dig into methodological reviews herein, for Fe, PO4 and Si there are potential well-known problems to raise and so a brief comment on filtration/method artefacts for those compounds/elements where this may be an issue for data quality (Fe/PO4/Si) is now added alongside the data compilation (Tables 2/3 in the text).

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).

There is, we thought obviously, a strong bias throughout the text to studies conducted in the marine environment at the key fieldsites mentioned. This is explicit because we want to discuss the effect of meltwater in the marine environment, and it is very difficult to contextualise studied that don't have extensive (or any) marine data. It is also very difficult to contextualise studies that don't have accompanying data concerning salinity and other key parameters available (especially for nutrients like Fe and Si that experience significant modification within estuarine zones). The Hawkings and Bhatia works are freshwater based. The Schroth work is more useful in this context and is extensively discussed extensively concerning estuarine mixing (although the accompanying data is not available online or from the author so we cannot comment in as much depth).

To quantify why it is better to use marine/estuarine studies to study Fe/Si in the ocean, consider the following. Estuarine removal flocculates between 60 and 99% of dissolved Fe, which is highly variable between (and even within) different estuarine gradients (Schroth et al., 2014; Sholkovitz et al., 1978; Zhang et al., 2015)**. Thus the same dissolved Fe concentration measured at zero salinity could plausibly produce values varying by a factor of 40 in saline waters, which is generally much less than seasonal changes in Fe concentrations of any fraction** (Hawkings et al., 2014; Statham et al., 2008)**. Similarly, total Fe shows no straightforward relationship to salinity or to dissolved Fe. Hence it is very difficult to make conclusions about the fate of Fe from freshwater data alone, and improving accuracy in the freshwater endmember doesn't really improve this much, whereas marine studies unambiguously show the actual enrichment irrespective of what the freshwater endmember was.**

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

Costal Fe values are always high relative to offshore waters, this is not unique to near-glacier systems, and whilst these values are 100 km from the nearest glacier, they are only 1 km from the coastline and much less than this (50-200 m) from the sea-floor making it an interesting assumption that they definitely have a direct meltwater origin. Fe concentrations across a salinity gradient should always be discussed with salinity in mind. Normalised to salinity, dissolved Fe concentrations at these locations are not particularly high. Considering that Arctic concentrations (offshore) peak within the transpolar drift at 4-5 nM dFe (in saline waters) (Rijkenberg et al., 2018; Slagter et al., 2017) , glacier estuary concentrations of 1-3 nM dFe are-perhaps surprisingly- low. Total Fe concentrations are higher, but are more challenging to interpret given that they don't behave conservatively and are less relevant to determining Fe availability to primary producers. A full discussion of the Fe-cycle is beyond the scope of this text given the limited relevance to Arctic primary production, it is of course much more relevant in the context of Fe-limitation immediately adjacent to glaciers in the Southern Ocean, but we have deliberately kept an Arctic focus to avoid getting side-tracked.

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

Yes the study is relevant, although we have tried to limit general points to 3 references (we are already well over the suggested limit) and in this context the Crusius data cited covers the same region more extensively.

L211-212: What about biological uptake?

This of course results in some drawdown in most environments, hence why we started the sentence 'In the absence of biological processes'

L222-230: The first assertation 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-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.

L222- added. We also note the salinity range and spatial scale. Si dissolution is normally evident only at low salinities across rapidly changing salinity gradients, which are often missed or poorly sampled in estuarine studies, e.g. (Brown et al., 2010) shows conservative behaviour of Si in a glacier catchment, but this concerns salinities of >25. (Windom et al., 1991) is quite nice in actually capturing the low-S changes in multiple rivers, few of the other suggest references do this. We have deliberately avoided estuaries with strong anthropogenic influences as this is beyond the scope of the text, but can add a few more sentences discussing observed Si trends in other estuaries. There are adequate Arctic studies to show varying degrees of (non-)conservative behaviour (Cauwet and Sidorov, 1996; Emmerton et al., 2008). A paragraph here is added to discuss differences as, as alluded to, there are differences between different rivers and thus showing a single 'typical' river scenario is not possible. As requested earlier, we have also added 3 different Arctic rivers into the salinity vs nutrients plot to show the range of concentrations/estuarine behaviour across multiple systems.

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.

L230- We are not disputing that some particulate Si is released from particles, this is beyond doubt and evident from the shape of the Si/salinity curves. (See general points). The incubation experiments in question are extrapolated over several times the time period over which these particles would remain in suspension in a glacier fjord. It is very difficult to reconcile with large datasets elsewhere.

As the Hawkings et al flux is inclusive of 100% dissolution of the annual labile particulate Si discharge we would think this already includes any directly related benthic flux, but this is not what the later work from this group suggests (Hendry et al., 2019) **and thus we find these studies hard to understand and to reconcile with large Si datasets around Greenland.**

A sub-section has been added to expand on the direct benthic-pelagic linkages highlighted by (Halbach et al., 2019) **where we now also mention briefly more general benthic processes affected by glaciers (i.e. high sedimentation), yet benthic cycling is not unique to environments affected by meltwater so it would be beyond the scope of the review to extensively cover benthic pelagic processes (as per dust, icebergs and sea-ice), especially looking at shelf environments over long timescales (geological rather than seasonal/interannual). We explicitly titled and focused the review on 'meltwater' to keep a tight focus.**

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.

We do not think that use of new modelling approaches is appropriate for a review article on an ancillary topic, and as noted there is very little data for this fjord to force such a model, but we agree it would be a more useful exercise to do for catchments with more extensive data (any of the case studies herein). Extrapolating to zero is a standard oceanographic method for flux calculations. Yes we agree the (Hawkings et al., 2017) data are similar to other fjords (although (Hatton et al., 2019) suggests they aren't). Hence the problem, the high fluxes in the (Hawkings et al., 2017) paper arise from how they are modelled and the assumptions made in this calculation, not because the mid-salinity datapoints are particularly high compared to other datasets (see general point). The plot is replotted, no regression is shown for the 2016 data as it is limited in scope, surface data is re-shaped to distinguish surface from sub-surface data. Some higher salinity Si data for the Kangerlussuaq site is added (Lund-Hansen et al., 2018)**.**

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?

This was already touched on briefly in the original text, but we can expand the sentence slightly (original line 604-606). We had much discussion with respect to how to define meltwater as terms used between the oceanographic and glacial communities differ widely. We use 'supraglacial' when specifically referring to samples which are supraglacial, in a marine context we refer to 'surface' and 'subsurface' to define where freshwater enters the water column. These terms are more vague in a glaciological context, but reflect the reality that not much can be determined about the origin of this water from marine profiles alone.

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.*

L261 'no estimate' can also be interpreted to mean that data was available (e.g. for earlier Si data (Brown et al., 2010; Azetsu-Scott and Syvitski, 1999) and the GEM portal), but it was apparent that it was not an important term and this assumed negligible for the purposes of processes occurring at the model scale (in-fjord processes are sub-grid, and (Hendry et al., 2019) notes the lack of pronounced Si export out of the fjord in question meaning that these processes are definitively sub-grid for global biogeochemical models). When stratification is considered, it is widely recognised that freshwater fluxes are not significant positive influences on macronutrients (McClelland et al., 2011b) especially for N and P. Hence a reason why extensive fluxes cannot be found in the oceanographic literature is because the [freshwater volume × freshwater concentration approach] referred to herein lacks relevance in a broader marine context.

The comment concerning models is not strictly correct. Ocean models are forced with observed macronutrient distributions which are available around most of Greenland (excluding the North coastline) and have informed global biogeochemical models for decades, thus any distant effect of meltwater derived material (i.e. beyond sub-model-grid resolution in fjords around the coast) is inherently included in model descriptions of Atlantic macronutrients. It's just not parametrized explicitly, but this is different from being considered zero, a similar comment could be made about many processes that influence nutrient distributions.

Table 2: I was not aware that Lawson et al. (2014) measured dissolved organic nitrogen (DON).

Lawson reference corrected (wrong reference order)

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.

Added, with respect to NH4 we add a comment earlier in the text to clarify NH4 is usually below detection in the marine environment, hence why the case of Kongsfjorden with respect to benthic NH4 release being detectable at the surface is particularly interesting (Halbach et al., 2019)**. For this reason NH4 fluxes aren't included here (now expanded in the new 'benthic-pelagic coupling' sub-section).**

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.

Filtration issues are raised earlier as suggested, as this is a very specific issue it is raised alongside brief comments on other methodological issues (e.g. low PO4 detection limits, NH4 contamination, Si freezing problems) in the data compilation (Table 3 in the text). The Hopwood 2016 text shows full surface transects of a fjord in addition to a few freshwater samples. The large uncertainty in estuarine removal factors for glacial dFe (which range 60-99%) adds up to a 40-fold uncertainty on to how large Fe export is when determined from freshwater concentrations. As noted, there is no significant differences in the fluxes calculated, and if the freshwater endmember for this fjord were back-calculated (again, with inevitable large uncertainty), these concentrations would be within the estimated endmember range. As summarised, for Fe/Si if substantial non-conservative behaviour is occurring at low salinities, high accuracy in the freshwater endmember is of limited use because it doesn't provide much insight into the net addition to these elements occurring over the estuarine salinity gradient.

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

We have swapped the order of this section to make it clear that there is no meaningful difference between these fluxes.

The flux that matters to primary producers in the marine environment is the flux after any removal processes that occur on short timescales (minutes-days) after/during mixing in the ocean. The Hawkings 2014 paper indeed mentions a 90% removal flux as noted in the table, but goes on to state that 'and lastly, a number of Greenlandic glaciers discharge directly into the ocean, avoiding estuarine processing' which is an odd comment as estuarine mixing refers to the process of mixing fresh and saline waters and does not require the physical presence of an estuary (it can, for example, be mimicked in a laboratory mixing fresh and saline waters). Removal factors are not arbitrary as they dictate what Fe is available for marine primary production.

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.

L288 This is an interesting way of reading this paragraph and not the meaning that was meant. We note throughout need for discharge estimates (meaning physical data) alongside datasets in the marine environment. Given the limited concentrations of macronutrients in freshwater and the strong non-conservative behaviour of those nutrients which are present at high concentrations (Fe/Si), more freshwater data at high resolution doesn't discernibly reduce the uncertainty concerning meltwater effects on primary production. The key point was meant to be that freshwater discharge data is only useful when coupled to marine data for the same region/timescale.

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.

L298 The problem with this 'gate' is that it is not appropriate to use it to speculate about PP in the marine environment, especially for N and P, which is widely recognised when dealing with fluxes from freshwater into the marine environment (McClelland et al., 2011b), hence why such fluxes aren't routinely found in marine literature.

It's not clear what the reviewer means by 'clearly isn't observed in surface samples and on the timescales' [we discuss]. We present full depth profiles for all the case studies close to the peak of the meltwater season in studies that were specifically designed to capture the water masses moving in and out of the glacier fjords, therefore any 'flux' that is occurring into the water column on seasonal/annual timescales should be strongly evident.

It is a shame the fate of these particulates are not discussed in a balanced manner, and only somewhat negatively, if at all.

The effect of particles on scales of 1-100 km from glaciers in the Arctic is overwhelmingly negative on PP (see general comment). It is very hard to reconcile this with the hypothesis the reviewer is referring to [that these particulates have a fertilizing effect]. Considering that in an above comment the reviewer acknowledges that these -potentially fertilizing particulate- fluxes are not strongly evident in any of our catchments at the peak of the meltwater season, it is difficult to find evidence that we are missing something.

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

Changes as noted above.

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.

In the specific sentences here, this (light limitation) is not particularly the case. Whether mid-summer productivity is controlled by only light-limitation or macronutrient-limitation can be assessed by looking at nutrient distribution in near-surface waters. Recent work in this fjord (Holding et al., 2019) suggests primary producers are well adapted to the light conditions in summer and that light-limitation was only a significant proximal-control on primary production at the inner-most fjord station. The Seifert work is now discussed.

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).

As noted, an extensive discussion of benthic processing is beyond the title of the current manuscript (as per other related themes). We chose our title to be as tightly defined as possible, an extensive review branching out to benthic processes, supra-glacial processes, sea-ice processes, icebergs and dust- would be more comprehensive, but far beyond what we can achieve in a single text. Comments can be easily added however to develop the benthic NH4 story alluded to (Halbach et al., 2019) **but not flagged as a 'benthic process' in Kongsfjorden and to emphasize the overlapping nature of benthic inputs with meltwater inputs to the ocean (as we already allude to with Fe).**

449 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).

We are aware of this, a line is added (re-)emphasizing that these are very broad generalisations. It is not our intention to provide a 'standardised' conceptual model as we note throughout that glacier-fjords across the Arctic are all practically unique and plumes can vary from not being evident at all in surface waters, to surface plumes extending 10s of kilometres along fjords (but are typically more restricted).

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

We have tried to keep the text as focused as possible on the Arctic and an in depth review of differences in Fe sources between the Arctic and Antarctic is detail we do not wish to go into. To a

first approximation, the critical difference is the vast difference in remoteness, the increased shelf exposure of the Arctic covers the associated shelf/river/atmosphere influences (we have added a sentence to explain this).

471-471 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.

We disagree with this comment, even very close to some glaciers there is evidence of residual nitrate and the potential for dFe limitation. If Fe from glaciers is to have an immediate positive effect on marine PP, it has to mix into surface high macronutrient, low dFe waters. Under these circumstances, the dFe supplied will then rapidly be drawn down to low levels (unless macronutrients become depleted). In any case, low dFe concentrations cannot be used to infer a low total Fe supply. It is not misleading to state that dFe-limitation occurs close to Antarctic glaciers, on the contrary, if it didn't then there wouldn't be such a strong biological response to new dFe input in summer.

Figure 5 does not 'miss' anything essential for the interpretation with respect to Fe limitation or primary production. There are obviously only so many parameters we can show and these 3 are sufficient to see the general contrast between the two cases. Fe limitation in the ocean can be (and is) assessed in marine waters by looking at the ratio in availability of dissolved Fe to NO3 (Moore et al., 2013), this approach quantitatively assess the extent of Fe stress in cells even working across very broad Fe gradients (e.g. (Browning et al., 2017)) where particulate Fe concentrations vary from high to low alongside DFe gradients. This means either than the direct influence of particulate Fe is via the dissolved phase (i.e. the influence of particulates on Fe bioavailability is accounted for in dissolved Fe measurements) or that, if directly available to some organisms, direct particulate Fe uptake is very minor compared to that of dissolved Fe. Neither of these suppositions is surprising considering that most Fe-cellular uptake pathways are specific to organically complexed Fe or free Fe rendering particulate Fe far less accessible to most species (Shaked and Lis, 2012).

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).

We referred to these waters as 'high nitrate, low dFe' which is correct. We did not refer to the levels of productivity. It is not clear what the reviewer means here. There is almost invariably a proximal limiting nutrient (except perhaps in extremely productive eastern-boundary upwelling systems) even in very productive waters. Highly productive regions of the Southern Ocean can still be (and often are) Fe-(co)-limited as Fe is still the proximal limiting nutrient and NO3 is not fully depleted during the growth season.

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 Marsayetal. (2017), so it would be useful to provide a sentence to elaborate*

We can explain this better, the estimate of dFe released beneath an ice shelf is for freshwater ice melt, not for subglacial discharge– as this doesn't exist close to the edge of most ice shelves in the same way as it does for a marine-terminating glacier where subglacial discharge plumes are pronounced at the glacier terminus. It is therefore not derived from Marsay et al., it comes from the freshwater studies cited. We can acknowledge uncertainty in this value (there are no direct

measurements to quantify it), but also again note that the vast majority of uncertainty in this calculation comes from the estuarine removal of Fe species during mixing between saline and fresh waters. This dwarfs the uncertainty from any other source.

L584 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 concentrations (Schroth et al., 2014) and is almost as poorly understood as the ligand pool? Some balanced discussion of this is important.

Direct accessibility of particulate Fe to pelagic phytoplankton is a bit of a misnomer, there are specific examples of mechanisms individual organisms have developed to capture Fe from particulate sources (Rubin et al., 2011)**, but cellular uptake processes are overwhelmingly dependent upon dissolved Fe availability. This can be demonstrated in the ocean at large (including high particulate Fe coastal regions) by looking at the extent to which Fe stress corresponds to dFe concentrations; dFe availability explains almost perfectly the extent of Fe-limitation across regimes transiting from high to low particulate Fe, meaning that dFe is to a first approximation the principle factor in determining Fe-limitation** (Browning et al., 2017)**. The role of particulates is generally understood to be as a buffer of the dissolved pool. Further, as noted, we have specifically focused the text on the Arctic where Fe is not an extensive limiting factor for primary production and thus its biogeochemistry is of much less interest than were we reviewing a similar topic in the Southern Ocean.**

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.

More specific comments can be added about the bedrock associated with relatively high inorganic (Alkalinity/silicate) concentrations.

L620-643 p1: Linked again to my point about lack of discussion on the importance of benthiccycling,Ithinkthereshouldbesomediscussionofthepotentialroleofalkalinity production in sedimentary environments (e.g. via denitrification and sulfate reduction).

R: This is not directly connected to meltwater and is a generic shelf process which we think is well beyond the scope of the title.

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.

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 correspondinglowpCO2(Tranteretal. 1993,

SharpandTranter, 2017)? Forexample, 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).

620 Correct, it is a generally correct statement to state that freshwater generally amplifies ocean acidification, but as noted in the text glacial meltwater has a particularly low TA which means that meltwater is a much more potent acidifier than riverwater (by volume). We have added some sentences here to provide more basic detail on the carbonate cycle as it is easy to get confused. The freshwater pH doesn't really matter in terms of to what extent freshwater drives ocean acidification, i.e. it's not possible for freshwater with low TA and high pH to act as a counter-balance to ocean acidification, freshwater with low TA will always acidify because what matters is the buffering capacity. We would rather not raise confusion here by discussing freshwater pH which varies so much in glaciated catchments precisely because of the low TA. In meltwater-affected saline waters, several non-conservative effects come into play, and the carbonate system is further affected by the extent of primary production which lowers pCO2 and thereby pH, and the general under-saturation of pCO2 in meltwater (which increases pCO2 drawdown). The saturation state of meltwater and estuarine waters with respect to pCO2 is also now discussed briefly as a related issue.

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

DOM refers to dissolved organic material, DOC refers explicitly to dissolved organic C, although these two are often used inter-changeably in the literature given that the majority of DOM is DOC. We will make sure to define these at first use.

L689 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.

Given the title of the text we are primary concerned herein with the effects of meltwater in the marine environment and are therefore much more interested in the broad-scale response of biogeochemistry across saline areas than in freshwater plumes. It seems obvious that in a freshwater system, all of the DOC will be freshwater-associated, the question we are interested in here is whether any freshwater signals can be detected offshore.

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.

The study explicitly demonstrates that glaciers '*are not a major contributor of carbon or of FDOM in the system***' that '***the significant amounts of BDOC in glacial runoff reported byHood et al. (2009), Fellman et al. (2010), and Lawsonet al. (2014), may, in fact, be negligible compared to the degradation potential of the various autochthonous carbon sources that are already present in the fjord***'. This supports the sentence as cited; we can add an extra few sentences to explain this more.**

Section 8: This whole section feels extremely speculative to me and is not actually correlatedtorealworldobservations,norwithanyobservationsfromtheArctic. Mostof theliteraturecitedprovidestenuouslinkswiththeonlyevidencethatIcanseebasedon 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.

We do already describe this as a 'hypothesis' and only lines 754-755 speculates, the rest of the section is a description of reasonably un-controversial literature. The link between meltwater and stratification is well established, and the link between HABs and stratification is well established. We can of course flag that connecting these two observations with a hypothesis (that changes in glacier-discharge may affect HABs) is not well established. We can also expand the rationale behind this potentially being of relevance to the Arctic as there are HAB-forming species present in stratified areas around west-Greenland. The main study site used in *Leon-Munoz et al.,* **is not an area where meltwater is the major source of freshwater, but the regional discussion covers areas which do have a majority of local freshwater inputs from glaciers and where changes in glacially derived freshwater inputs are affecting stratification and seasonal patterns of primary production- hence the link to long term changes in glacier fjords. We clarify this more in the revised text.**

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.

Yes the wording here is incorrectly matched to the reference, we can split this sentence and separate the observations of glacier retreat and reduced runoff, and introduce the concept of peak discharge from future scenarios in model studies.

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

We were referring to the studies already cited in the same sentences, but can repeat them for clarity.

Figure 9: Nice looking figure, but I'd really like to see more balance in the interpretation oftheliteraturerepresentedinit. Onemajoromission(againI'mgoingbacktoit)isany 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).

Figure 9. **Yes this can be changed.**

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

Yes these can be updated.

References referred to:

Brown, M. T., Lippiatt, S. M. and Bruland, K. W.: Dissolved aluminum, particulate aluminum, and silicic acid in northern Gulf of Alaska coastal waters: Glacial/riverine inputs and extreme reactivity, Mar. Chem., 122(1–4), 160–175, doi:10.1016/j.marchem.2010.04.002, 2010.

Browning, T. J., Achterberg, E. P., Rapp, I., Engel, A., Bertrand, E. M., Tagliabue, A. and Moore, C. M.: Nutrient co-limitation at the boundary of an oceanic gyre, Nature, doi:10.1038/nature24063, 2017.

Cape, M. R., Straneo, F., Beaird, N., Bundy, R. M. and Charette, M. A.: Nutrient release to oceans from buoyancy-driven upwelling at Greenland tidewater glaciers, Nat. Geosci., 12, 34–39, doi:10.1038/s41561-018-0268-4, 2019.

Cauwet, G. and Sidorov, I.: The biogeochemistry of Lena River: Organic carbon and nutrients distribution, in Marine Chemistry, vol. 53, pp. 211–227., 1996.

Emmerton, C. A., Lesack, L. F. W. and Vincent, W. F.: Nutrient and organic matter patterns across the Mackenzie River, estuary and shelf during the seasonal recession of sea-ice, J. Mar. Syst., doi:10.1016/j.jmarsys.2007.10.001, 2008.

Halbach, L., Vihtakari, M., Duarte, P., Everett, A., Granskog, M. A., Hop, H., Kauko, H. M., Kristiansen, S., Myhre, P. I., Pavlov, A. K., Pramanik, A., Tatarek, A., Torsvik, T., Wiktor, J. M., Wold, A., Wulff, A., Steen, H. and Assmy, P.: Tidewater Glaciers and Bedrock Characteristics Control the Phytoplankton Growth Environment in a Fjord in the Arctic, Front. Mar. Sci., 6, 254, doi:10.3389/fmars.2019.00254, 2019.

Harrison, W. G., Platt, T. and Irwin, B.: Primary Production and Nutrient Assimilation by Natural Phytoplankton Populations of the Eastern Canadian Arctic, Can. J. Fish. Aquat. Sci., 39(2), 335–345, doi:10.1139/f82-046, 1982.

Hatton, J. E., Hendry, K. R., Hawkings, J. R., Wadham, J. L., Opfergelt, S., Kohler, T. J., Yde, J. C., Stibal, M. and Žárský, J. D.: Silicon isotopes in Arctic and sub-Arctic glacial meltwaters: the role of subglacial weathering in the silicon cycle, Proc. R. Soc. A Math. Phys. Eng. Sci., 475(2228), 20190098, doi:10.1098/rspa.2019.0098, 2019.

Hawkings, J., Wadham, J., Tranter, M., Telling, J., Bagshaw, E., Beaton, A., Simmons, S.-L., Chandler, D., Tedstone, A. and Nienow, P.: The Greenland Ice Sheet as a hot spot of phosphorus weathering and export in the Arctic, Global Biogeochem. Cycles, 30(2), 191–210, doi:10.1002/2015GB005237, 2016.

Hawkings, J. R., Wadham, J. L., Tranter, M., Raiswell, R., Benning, L. G., Statham, P. J., Tedstone, A., Nienow, P., Lee, K. and Telling, J.: Ice sheets as a significant source of highly reactive nanoparticulate iron to the oceans, Nat. Commun., 5(3929), doi:10.1038/ncomms4929, 2014.

Hawkings, J. R., Wadham, J. L., Tranter, M., Lawson, E., Sole, A., Cowton, T., Tedstone, A. J., Bartholomew, I., Nienow, P., Chandler, D. and Telling, J.: The effect of warming climate on nutrient and solute export from the Greenland Ice Sheet, Geochemical Perspect. Lett., 1(0), 94–104, doi:http://dx.doi.org/10.7185/geochemlet.1510, 2015.

Hawkings, J. R., Wadham, J. L., Benning, L. G., Hendry, K. R., Tranter, M., Tedstone, A., Nienow, P. and Raiswell, R.: Ice sheets as a missing source of silica to the polar oceans, Nat. Commun., 8, 14198, doi:10.1038/ncomms14198, 2017.

Hendry, K. R., Huvenne, V. A. I., Robinson, L. F., Annett, A., Badger, M., Jacobel, A. W., Ng, H. C., Opher, J., Pickering, R. A., Taylor, M. L., Bates, S. L., Cooper, A., Cushman, G. G., Goodwin, C., Hoy, S., Rowland, G., Samperiz, A., Williams, J. A., Achterberg, E. P., Arrowsmith, C., Alexander Brearley, J., Henley, S. F., Krause, J. W., Leng, M. J., Li, T., McManus, J. F., Meredith, M. P., Perkins, R. and Woodward, E. M. S.: The biogeochemical impact of glacial meltwater from Southwest Greenland, Prog. Oceanogr., 176, 102126, doi:https://doi.org/10.1016/j.pocean.2019.102126, 2019.

Hodal, H., Falk-Petersen, S., Hop, H., Kristiansen, S. and Reigstad, M.: Spring bloom dynamics in Kongsfjorden, Svalbard: nutrients, phytoplankton, protozoans and primary production, Polar Biol., 35(2), 191–203, doi:10.1007/s00300-011-1053-7, 2012.

Holding, J. M., Markager, S., Juul-Pedersen, T., Paulsen, M. L., Møller, E. F., Meire, L. and Sejr, M. K.: Seasonal and spatial patterns of primary production in a high-latitude fjord affected by Greenland Ice Sheet run-off, Biogeosciences Discuss., 2019, 1–28, doi:10.5194/bg-2019-203, 2019.

Hop, H., Pearson, T., Hegseth, E. N., Kovacs, K. M., Wiencke, C., Kwasniewski, S., Eiane, K., Mehlum, F., Gulliksen, B., Wlodarska-Kowalczuk, M., Lydersen, C., Weslawski, J. M., Cochrane, S., Gabrielsen, G. W., Leakey, R. J. G., Lønne, O. J., Zajaczkowski, M., Falk-Petersen, S., Kendall, M., Wängberg, S.-Å., Bischof, K., Voronkov, A. Y., Kovaltchouk, N. A., Wiktor, J., Poltermann, M., Prisco, G., Papucci, C. and Gerland, S.: The marine ecosystem of Kongsfjorden, Svalbard, Polar Res., 21(1),

167–208, doi:10.1111/j.1751-8369.2002.tb00073.x, 2002.

Azetsu-Scott, K. and Syvitski, J. P. M.: Influence of melting icebergs on distribution, characteristics and transport of marine particles in an East Greenland fjord, J. Geophys. Res., 104(C3), 5321, doi:10.1029/1998JC900083, 1999.

Iversen, K. R. and Seuthe, L.: Seasonal microbial processes in a high-latitude fjord (Kongsfjorden, Svalbard): I. Heterotrophic bacteria, picoplankton and nanoflagellates, Polar Biol., 34(5), 731–749, doi:10.1007/s00300-010-0929-2, 2011.

Jensen, H. M., Pedersen, L., Burmeister, A. and Winding Hansen, B.: Pelagic primary production during summer along 65 to 72°N off West Greenland, Polar Biol., 21(5), 269–278, doi:10.1007/s003000050362, 1999.

Juul-Pedersen, T., Arendt, K. E., Mortensen, J., Blicher, M. E., Søgaard, D. and Rysgaard, S.: Seasonal and interannual phytoplankton production in a sub-Arctic tidewater outlet glacier fjord, SW Greenland, Mar. Ecol. Prog. Ser., 524, 27–38, doi:10.3354/meps11174, 2015.

Kanna, N., Sugiyama, S., Ohashi, Y., Sakakibara, D., Fukamachi, Y. and Nomura, D.: Upwelling of macronutrients and dissolved inorganic carbon by a subglacial freshwater driven plume in Bowdoin Fjord, northwestern Greenland, J. Geophys. Res. Biogeosciences, 123, doi:10.1029/2017JG004248, 2018.

Levinsen, H. and Nielsen, T. G.: The trophic role of marine pelagic ciliates and heterotrophic dinoflagellates in arctic and temperate coastal ecosystems: A cross-latitude comparison, Limnol. Oceanogr., 47(2), 427–439, doi:10.4319/lo.2002.47.2.0427, 2002.

Lund-Hansen, L. C., Hawes, I., Holtegaard Nielsen, M., Dahllöf, I. and Sorrell, B. K.: Summer meltwater and spring sea ice primary production, light climate and nutrients in an Arctic estuary, Kangerlussuaq, west Greenland, Arctic, Antarct. Alp. Res., 50(1), S100025, doi:10.1080/15230430.2017.1414468, 2018.

Lydersen, C., Assmy, P., Falk-Petersen, S., Kohler, J., Kovacs, K. M., Reigstad, M., Steen, H., Strøm, H., Sundfjord, A., Varpe, Ø., Walczowski, W., Weslawski, J. M. and Zajaczkowski, M.: The importance of tidewater glaciers for marine mammals and seabirds in Svalbard, Norway, J. Mar. Syst., 129, 452–471, doi:10.1016/j.jmarsys.2013.09.006, 2014.

McClelland, J. W., Holmes, R. M., Dunton, K. H. and Macdonald, R. W.: The Arctic Ocean Estuary, Estuaries and Coasts, 35(2), 353–368, doi:10.1007/s12237-010-9357-3, 2011a.

McClelland, J. W., Holmes, R. M., Dunton, K. H. and Macdonald, R. W.: The Arctic Ocean Estuary, Estuaries and Coasts, 35(2), 353–368, doi:10.1007/s12237-010-9357-3, 2011b.

Meire, L., Meire, P., Struyf, E., Krawczyk, D. W., Arendt, K. E., Yde, J. C., Juul Pedersen, T., Hopwood, M. J., Rysgaard, S. and Meysman, F. J. R.: High export of dissolved silica from the Greenland Ice Sheet, Geophys. Res. Lett., 43(17), 9173–9182, doi:10.1002/2016GL070191, 2016.

Meire, L., Mortensen, J., Meire, P., Juul-Pedersen, T., Sejr, M. K., Rysgaard, S., Nygaard, R., Huybrechts, P. and Meysman, F. J. R.: Marine-terminating glaciers sustain high productivity in Greenland fjords, Glob. Chang. Biol., 23(12), 5344–5357, doi:10.1111/gcb.13801, 2017.

Moore, C. M., Mills, M. M., Arrigo, K. R., Berman-Frank, I., Bopp, L., Boyd, P. W., Galbraith, E. D., Geider, R. J., Guieu, C., Jaccard, S. L., Jickells, T. D., La Roche, J., Lenton, T. M., Mahowald, N. M., Maranon, E., Marinov, I., Moore, J. K., Nakatsuka, T., Oschlies, A., Saito, M. A., Thingstad, T. F., Tsuda, A. and Ulloa, O.: Processes and patterns of oceanic nutrient limitation, Nat. Geosci, 6(9), 701–710, doi:10.1038/ngeo1765, 2013.

Murray, C., Markager, S., Stedmon, C. A., Juul-Pedersen, T., Sejr, M. K. and Bruhn, A.: The influence of glacial melt water on bio-optical properties in two contrasting Greenlandic fjords, Estuar. Coast. Shelf Sci., 163(PB), 72–83, doi:10.1016/j.ecss.2015.05.041, 2015.

Nielsen, T. G.: Plankton community structure and carbon cycling on the western coast of Greenland during the stratified summer situation. I. Hydrography, phytoplankton and bacterioplankton , Aquat. Microb. Ecol., 16(3), 205–216 [online] Available from: https://www.int-res.com/abstracts/ame/v16/n3/p205-216/, 1999.

Pabi, S., van Dijken, G. L. and Arrigo, K. R.: Primary production in the Arctic Ocean, 1998–2006, J. Geophys. Res. Ocean., 113(C8), doi:10.1029/2007JC004578, 2008.

van de Poll, W. H., Kulk, G., Rozema, P. D., Brussaard, C. P. D., Visser, R. J. W. and Buma, A. G. J.: Contrasting glacial meltwater effects on post-bloom phytoplankton on temporal and spatial scales in Kongsfjorden, Spitsbergen, Elem Sci Anth, 6(1), 2018.

Ren, Z., Martyniuk, N., Oleksy, I. A., Swain, A. and Hotaling, S.: Ecological Stoichiometry of the Mountain Cryosphere , Front. Ecol. Evol. , 7, 360 [online] Available from: https://www.frontiersin.org/article/10.3389/fevo.2019.00360, 2019.

Rijkenberg, M. J. A., Slagter, H. A., Rutgers van der Loeff, M., van Ooijen, J. and Gerringa, L. J. A.: Dissolved Fe in the Deep and Upper Arctic Ocean With a Focus on Fe Limitation in the Nansen Basin, Front. Mar. Sci., 5, 88, doi:10.3389/fmars.2018.00088, 2018.

Rubin, M., Berman-Frank, I. and Shaked, Y.: Dust- and mineral-iron utilization by the marine dinitrogen-fixer Trichodesmium, Nat. Geosci., 4(8), 529–534, doi:10.1038/ngeo1181, 2011.

Rysgaard, S., Nielsen, T. and Hansen, B.: Seasonal variation in nutrients, pelagic primary production and grazing in a high-Arctic coastal marine ecosystem, Young Sound, Northeast Greenland, Mar. Ecol. Prog. Ser., 179, 13–25, doi:10.3354/meps179013, 1999.

Schroth, A. W., Crusius, J., Campbell, R. W. and Hoyer, I.: Estuarine removal of glacial iron and implications for iron fluxes to the ocean, Geophys. Res. Lett., 41(11), 3951–3958, doi:10.1002/2014GL060199, 2014.

Seifert, M., Hoppema, M., Burau, C., Elmer, C., Friedrichs, A., Geuer, J. K., John, U., Kanzow, T., Koch, B. P., Konrad, C., van der Jagt, H., Zielinski, O. and Iversen, M. H.: Influence of Glacial Meltwater on Summer Biogeochemical Cycles in Scoresby Sund, East Greenland , Front. Mar. Sci. , 6, 412 [online] Available from: https://www.frontiersin.org/article/10.3389/fmars.2019.00412, 2019.

Shaked, Y. and Lis, H.: Disassembling iron availability to phytoplankton, Front. Microbiol., 3, 123, doi:10.3389/fmicb.2012.00123, 2012.

Sholkovitz, E. R., Boyle, E. A. and Price, N. B.: The removal of dissolved humic acids and iron during estuarine mixing, Earth Planet. Sci. Lett., 40, 130–136, doi:10.1016/0012-821X(78)90082-1, 1978.

Slagter, H. A., Reader, H. E., Rijkenberg, M. J. A., Rutgers van der Loeff, M., de Baar, H. J. W. and Gerringa, L. J. A.: Organic Fe speciation in the Eurasian Basins of the Arctic Ocean and its relation to terrestrial DOM, Mar. Chem., 197, 11–25, doi:https://doi.org/10.1016/j.marchem.2017.10.005, 2017.

Smoła, Z. T., Tatarek, A., Wiktor, J. M., Wiktor, J. M. W., Kubiszyn, A. and Węsławski, J. M.: Primary producers and production in Hornsund and Kongsfjorden – comparison of two fjord systems, Polish Polar Res., 38, 351–373, doi:10.1515/popore-2017-0013, 2017.

Statham, P. J., Skidmore, M. and Tranter, M.: Inputs of glacially derived dissolved and colloidal iron to the coastal ocean and implications for primary productivity, Global Biogeochem. Cycles, 22(3), doi:Gb301310.1029/2007gb003106, 2008.

Tank, S. E., Raymond, P. A., Striegl, R. G., McClelland, J. W., Holmes, R. M., Fiske, G. J. and Peterson, B. J.: A land-to-ocean perspective on the magnitude, source and implication of DIC flux from major Arctic rivers to the Arctic Ocean, Global Biogeochem. Cycles, 26(4), n/a-n/a, doi:10.1029/2011GB004192, 2012.

TG, N. and Hansen, B.: Plankton community structure and carbon cycling on the western coast of Greenland during and after the sedimentation of a diatom bloom , Mar. Ecol. Prog. Ser., 125, 239–257 [online] Available from: https://www.int-res.com/abstracts/meps/v125/p239-257/, 1995.

Tyrrell, T.: The relative influences of nitrogen and phosphorus on oceanic primary production, Nature, doi:10.1038/22941, 1999.

Wadham, J. L., Hawkings, J., Telling, J., Chandler, D., Alcock, J., O'Donnell, E., Kaur, P., Bagshaw, E., Tranter, M., Tedstone, A. and Nienow, P.: Sources, cycling and export of nitrogen on the Greenland Ice Sheet, Biogeosciences, 13(22), 6339–6352, doi:10.5194/bg-13-6339-2016, 2016.

Wadham, J. L., Hawkings, J. R., Tarasov, L., Gregoire, L. J., Spencer, R. G. M., Gutjahr, M., Ridgwell, A. and Kohfeld, K. E.: Ice sheets matter for the global carbon cycle, Nat. Commun., 10(1), 3567, doi:10.1038/s41467-019-11394-4, 2019.

Windom, H., Byrd, J., Smith, R., Hungspreugs, M., Dharmvanij, S., Thumtrakul, W. and Yeats, P.: Trace metal-nutrient relationships in estuaries, Mar. Chem., 32(2), 177–194, doi:10.1016/0304-4203(91)90037-W, 1991.

Zhang, R., John, S. G., Zhang, J., Ren, J., Wu, Y., Zhu, Z., Liu, S., Zhu, X., Marsay, C. M. and Wenger, F.: Transport and reaction of iron and iron stable isotopes in glacial meltwaters on Svalbard near Kongsfjorden: From rivers to estuary to ocean, Earth Planet. Sci. Lett., 424, 201–211, doi:10.1016/j.epsl.2015.05.031, 2015.