<u>Replies to reviewers comments.</u>

Two reviewers are thanked for detailed comments on the text. Please find below replies to reviewers, alongside an annotated, revised text (any new references discussed, but not referred to in the manuscript, are added at the end).

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

R: We thank the reviewer for detailed comments on the text. It was not our intention to apply similarities between Arctic and Antarctic systems, or to draw extensive comparisons between the two in general as there are obviously important biogeochemical differences in the marine context between the two. We add some brief discussion of this at appropriate points in the text. E.g. new lines 729-733.

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

R: We have added further details around this. See new sections: 127-138 and 619-629

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

R: Agreed, we have corrected this statement 'around the Arctic' (rather than 'globally') and we have noted to difficulty in distinguishing different Fe sources close to glaciers, especially in Southern Ocean where observations are sparse (new lines 641-650)

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

R: Agreed, we can add a comment to this effect in the text. (new lines 611): *'Yet determining the extent to which these events affect fjord-scale mixing, biogeochemistry and how these rates change in response to climate forcing…'*

My general feeling is that while the comparisons may be obvious, there are important functional differences between the Antarctic and Arctic. And so supporting information should be appropriate. For example L203-206 has two well-known studies of particulate iron in the Antarctic (Gerringa et al., 2012, and Annett et al., 2017). The authors may choose to mention this is an important question in general for particle-enriched iron sources. (Fitzsimmons et al. 2017)

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

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

R: We can add a few lines discussing this in addition to our already present notes to the complexity of the Fe cycle. New lines 302-306

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

R: We can add some general comments on nutrient stoichiometry here, it is perhaps a little detailed to specifically address the subtleties of Fe-diatom limitation, but in general terms the issue can be discussed as an influence on taxonomic groups (new lines 695-698)

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

R: Explicitly stated.

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

R: Yes, there is no doubt to us that the biological utilization of labile particulates is something under-studied and we can add a few sentences to raise the complexity of biological Fe uptake here in general terms. We note that budgeting exercises show that only a few percent of the 'sedimentary' Fe added downstream of such island plumes has to be solubilized to explain

observed primary production. Exactly what this fraction is not particularly clear yet. Also there are other confounding factors such as light limitation, *'However, even in these HNLC waters there are also other concurrent factors that mitigate the effect of glacially derived Fe in nearshore waters where light limitation from near-surface particle plumes may offset the positive effect of Fe-fertilization'*

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

L672: ": : :additional subsidies of labile carbon: : :"

R: Ammended.

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

R: Agreed, we have noted now the data deficiency when discussing differences between Arctic and Antarctic systems (729-723*): 'However, we note a general lack of seasonal and interannual data for Antarctic glacier fjord systems precluding a comprehensive intercomparison of these different systems.'*

L758: "of Patagonia"

R: Corrected.

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

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

L819-822: How then do we reconcile the expansion of the icesheets and the decreased availability of sediments eroded by wind?

R: With difficulty on regional scales, but on global scales this is not implausible. High latitude dust sources may be significant locally, but globally are minor compared to dust from the world's low latitude dessert regions. Changes in 'global' dust signals may therefore be highly sensitive to what happens at low latitudes and relatively insensitive to what happens around the world's IceSheets/glaciers.

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

[end of review]

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.

Sedimentary processes remain a little beyond the scope of the present title, but nevertheless are directly linked in some aspects so we introduce new short sections to better link to associated topics (5.1 and 5.2) which includes research which was not published when we submitted our original text.

We have now introduced the text with a section on primary production which explains why a link between lithogenic 'particulate nutrients' and primary producers [suggested by the reviewer] is not clear to us (new section 3.0). Multiple lines of evidence show that primary producers and primary production is negatively affected by Arctic glacier discharge on a scale comparable to that over which sedimentation occurs- with the notable exception of marine-terminating glaciers that are discussed later in the text- and thus we focus primarily on phenomena that can explain this pattern in primary production.

Lines 401-435 explain why we explicitly focus on dissolved macronutrients and why it is difficult to reconcile field observations of primary production with a few of the reviewers' comments.

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.

More recent publications which were not available when we wrote or submitted the original text are now incorporated.

The wording in section 8 (now 9) is altered to make it clear where uncertainties remain. We emphasize that a link between stratification and HAB events is well established, as is a link between changing freshwater runoff and HAB events. This is not unique to glacier systems, but is not speculative. We think it is interesting to raise this as a topic for future research as changing glacier discharge does affect many (if not, all) of the drivers responsible for HAB events. The section is clearly highlighted as a question and we now clearly flag that this subject is not well explored in the Arctic, unlike many of the other topics highlighted.

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

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).*

Lines 248-271 now more extensively discuss sediment loads in glacial discharge.

Whilst these 'reactive' fluxes may be of intense interest to glaciologists/geochemists, the key question herein is how are they linked to biology? All of our available field evidence suggests that the main link on this spatial/temporal scale in the Arctic is suppression of marine primary production (see section 3)– presumably via light limitation. Thus it is not clear to us what the relevance of lithogenic elemental fluxes is to primary producers on inter-annual timescales. We now introduce the text with this primary production data to better explain this rationale (new section 3) and in lines 402-435 try to reconcile the reviewers' arguments with the field studies highlighted herein.

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 was, we assume, referring to extensive datasets on freshwater water composition. In section 11 (see especially Figure 11) we discuss the relative importance of different processes and emphasize that freshwater composition is not a major consideration in terms of the large-scale impact of meltwater from the Arctic in the ocean.

There remains an inherent bias in the text towards studies conducted at the 5 field sites discussed in detail. These were selected at workshops based on the availability of marine data for any glaciated regions across the Arctic as this is essential to cover the manuscript topic. We don't therefore think that the literature selection is illogical. There are several study areas which could have been selected as better alternatives if the text had been focused entirely on terrestrial or freshwater systems, but we explicitly focus herein on the marine environment and thus it is difficult to extensively discuss literature from sites where there is a lack of extensive (or any) marine data. In terms of published literature, the only other potential candidates we identified for focus regions were Ryder Glacier and the '79 North' glacier which are both subject to extensive on-going research programs, but (at the time of writing) were not extensively discussed in terms of the topics covered herein, or other small catchments which are very close to Godthabsfjord or Kongsfjorden.

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.*

We add a new introductory section on primary production in Arctic glaciated regions (section 3) which better explains the rationale for the topics within the text. We think the reviewer here is referring to freshwater nutrient time series. It is not an argument we agree with that time series of freshwater data (without corresponding marine data) can be used to make conclusions about the marine environment (see Figure 10) because this is such a small budget term that even defining it with very high resolution doesn't really enhance our understanding of changes in marine primary production.

It's impossible to make even basic comments on fjord-scale processes without some 2D temperature/salinity data and thus we cannot really draw any conclusions about the marine environment from freshwater(only) timeseries-even if the resolution is particularly good.

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.

We add lines 402-435 to better explain why a clear distinction is made between dissolved macronutrient fluxes and lithogenic elemental fluxes. '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

This section is re-written to include new PO4 data from Arctic rivers for comparison (new lines 238-248). Considering the organic and inorganic P phases in rivers, PO4 in meltwater is low. In absolute terms, both meltwater and rivers contain limited quantities of PO4.

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

We have added more data to support a general overview of rivers/meltwater as sources of different macronutrients including an expanded figure 3 (originally Figure 2).

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.*

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 produce a methodological review 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 consistent 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 2-3 references (we are already 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 almost all environments, hence why we started the sentence 'In the absence of biological processes'

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

As per earlier comments on riverine PO4, we expand this section and include data from the 3 best studied Arctic river estuaries for comparison to show the range of behaviours observed in different macronutrients with a brief over-view of the underlying reasons for variation between Arctic estuaries (new lines 238-248).

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.

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. 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 (Fig. 4).

A sub-section (5.1) 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 (in writing the text, we reviewed this again, and there certainly isn't enough data even to define the fresh/saline endmembers for this fjord, so it would be meaningless to construct even a 2D model for the fjord), but we agree it would be a more useful exercise to do for catchments with more extensive data (any of the case studies herein). Figure 3 (now 4) is re-drawn as suggested. 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 high compared to other datasets.

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?
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. We have added a paragraph to explain the rationale (new lines 376-389).

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.*

(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 which is consistent

with them not being explicitly parametrized. 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 present at very low concentrations in marine waters, 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 5.1).**

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 and that apparent differences simply reflect a different flux gate. 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. 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 as we think is clearer from (new lines) 475-491. 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 in this context [marine PP] 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 changing primary production 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).

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.

In a geochemical context sediment loads (lines 248-271) and the associated lithogenic elements fluxes (see lines 400-406) in glacier catchments are high. The question addressed herein is how these loads affect biota. As the net effect on primary producers is negative or negligible (see section 3), it is challenging to consider any fertilization effect of these particles to be a significant feature that should be discussed compared to those effects which can explain the observed patterns of primary production and water column distributions.

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 apparently negative according to measurements of primary production (see new section 3). We do not think it is unbalanced to focus on effects that could explain this, rather than effects that would have the opposite effect.

It is very hard to reconcile this with the hypothesis the reviewer is referring to [that these particulates have a fertilizing effect]. We attempt to explain why these different perspectives may have arisen in new lines 400-428.

We have reviewed carefully the literature concerning glaciers and primary production in glaciated Arctic regions. We cannot find evidence that glacially derived particles are linked to high primary production in the Arctic despite extensive comments in recent glaciology papers, without supporting citations, that this is the case. The argument in (Wadham et al., 2019) for example cites work that refers to secondary production (Lydersen et al., 2014), not primary production, in Kongsfjorden and this high secondary production is thought to occur precisely because of the negative effects that particle plumes have on primary producers (new lines 508-517). The reference supports the opposite conclusion, that glacier plumes negatively affect primary production.

There is literature discussed herein to show mechanistically that meltwater/particles (on small scales the effects of the two are very hard to distinguish from field observations) in the Arctic have effects on the balance between different taxonomic groups, potentially on bloom timing, mixing (which does facilitate increased primary production) and C export; but we are not aware of any work specifically reporting positive effects of meltwater (in the absence of subglacial discharge inducing mixing) or lithogenic particles in Arctic glacier-fjords (or associated regions) on total marine primary production. There are also several cases where references (as per the Lydersen reference) are miss-cited to imply such a link, for example the Arrigo et al., 2017 manuscript has been cited on multiple occasions to link increasing freshwater with increasing productivity, but the manuscript shows no link between increasing freshwater and increasing productivity, it shows a link between freshwater arrival and bloom timing which is a different concept. Similarly Meire et al., 2017 has been cited to link productivity to meltwater, but the manuscript shows that no such general relationship exists- except for the special case of marine-terminating glaciers. The concept of 'bioavailability' is also been extensively mis-used in recent literature to refer to elemental which are at most 'bioaccessible under some circumstances' which perhaps explains why there is a general miss-perception, as suggested by the reviewer, that these particle plumes are 'highly productive'.

The comments we can find referring to a particle-fertilization effect in the Arctic are circular, they refer back to papers (largely recent Hawkings/Wadham references) which present large lithogenic fluxes and speculate that they may be driving enhanced primary production. But the link is unsubstantiated, and what data we can find for the Arctic shows a negative (not positive) association (see section 3).

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

Changed 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.

The discussion concerning particle plumes and turbidity is now expanded as per earlier suggestions, 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 with total primary production instead limited by sparse nitrogen supply. The Seifert work is now discussed in a new section (5.2).

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 are added however to develop the benthic NH4 story we already 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, new section 5.2).**

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 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 the glaciers where we can find available data 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 primary production, 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- which for NO3/PO4 doesn't occur on any large scale around Antarctica). Even in productive areas of coastal polynyas around Antarctica this is the case e.g. new lines 707-718). In any case, low dFe concentrations cannot be used to infer a low total Fe supply as low dFe is typical of post-bloom conditions, even in places not considered to be Fe-limited or HNLC (e.g. Celtic shelf, Birchill et al., 2017). 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, productivity and nutrient-limitation are different concepts. There is almost invariably a nutrient proximally limiting phytoplankton growth (except perhaps in extremely productive eastern-boundary upwelling systems). Highly productive regions of the Southern Ocean can still be (and generally are) Fe-(co)-limited and NO3 is not fully depleted during the growth season (e.g. Sedwick et al., 2011).

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*

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 (see new lines 804-815).

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.

Just because particulate concentrations of Fe are high does not mean that organisms will change their biochemistry to access this less labile Fe rather than relying on dissolved Fe. As noted in earlier comments, the major cellular Fe uptake systems all rely on dissolved organic-Fe species. We are not sure that these references support the point the reviewer is making, the Kranzler 2016 work explicitly demonstrates that transformation from the particulate to dissolved phase is required prior to uptake, which is consistent with our comments on the utilization of these particles by biota (308-315).

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.

Rephrased.

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. 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 of its buffering capacity (alkalinity), which is invariably low (a few local exceptions to this rule are speculated to occur e.g. see Benetti et al., 2019, but all of the large datasets we can find show meltwater is a low TA freshwater source).

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 and the non-linear effect of salinity on pCO2 also raised (new lines 889-895).

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 degra-dation potential of the various autochthonous carbon sources that are already present in the fjord****'.***
This supports the sentence as cited in the revised text.

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 describe this as a 'hypothesis'. Most of the section is a description of reasonably un-controversial literature concerning patterns in primary production and stratification. 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-although we have added new references from Alaska and Greenland which mechanistically explain why glacier fjords in these locations may be increasingly affected by HABs in the future.

We have also expanded 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 data presented covers areas which do have a majority of local freshwater inputs from glaciers, 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.**

With respect to studies not referring to 'glacier' or 'meltwater', this is common in oceanographic studies because freshwater sources cannot be easily distinguished at distance from source. Many of the Greenlandic studies referred to similarly refer to 'meteoric water' or 'freshwater' with the meltwater component of this freshwater component being highly variable.

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 was incorrectly matched to the reference, we have split this sentence and separated the observations of glacier retreat and reduced runoff, and introduced the concept of peak discharge from future scenarios in model studies which is what we meant to refer to.

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, for the purposes of ranking glaciers by discharge there is some small difference depending on the time period chosen.

References referred to (which are not in the main text):

Birchill, A. J., et al. (2017), Seasonal iron depletion in temperate shelf seas, Geophys. Res. Lett., 44, 8987– 8996, doi:10.1002/2017GL073881.

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