Anonymous Referee #2 Received and published: 7 January 2021

Subglacial upwelling in winter/spring increases under-ice primary production

Summary: This paper aims to explore the role of the release of subglacial meltwater in the winter and spring on under-ice primary production. The premise of the study is that though subglacial upwelling of nutrient rich deep marine waters has been shown to be a viable mechanism for stimulating primary production in the summer, very few studies have examined this topic with regards to spring under-ice primary production. The study is an interesting, under-explored topic, which is only likely to become more important with global warming and prolonged glacial melt seasons, and thus, worthy of eventual publication in this journal.

We want to thank the reviewer sincerely for the comprehensive review that helps to improve the clarity of the manuscript considerably. All comments are clear and very useful. We addressed all comments as outlined in detail below. Suggested changes in the text of the manuscript are highlighted in green.

However, I think there are number of improvements that could be made to aid the study, which I outline below. Apart from issues with over-interpretation of the data (detailed below), the writing is often disorganized and unclear. Also, there often a lack of consideration of the on-ice processes that are occurring that could be affecting the authors interpretations – i.e. enrichment of the glacial meltwater itself that has been stored at the bed overwinter and is released in the spring. The fact that the submarine discharge in the spring is likely quite different to the dilute discharge characteristic of summer drainage is a fact that makes this difficult to compare to previous summer studies of glacial discharge into the ocean. To this end often the authors seem to have a pre-ordained conclusion – i.e. that the mechanism of nutrient addition was via upwelling of "deep" bottom waters by the submarine discharge, but this seemed at odd with the shallow depth of this discharge (20-m). Finally, there is also a lack of clarity with how some of the calculations are made – this needs to be rectified for these calculations to be understood. I would urge the authors to address these points, and indeed try to focus their story on the novel spring measurements they have, to maximize the potential readership of this interesting study.

In cases of over-interpretations, we either rephrased the interpretation more careful, often via adding details for clarification, or removed the statements (details below). We rewrote the sections that the reviewer considered disorganized and unclear (details below) with the most substantial changes regarding glacial processes and the chapter about subglacial upwelling and entrainment factors. We tried to clarify the relevance of on-ice processes by i) introducing the processes in more detail in the introduction, ii) mentioning the nutrient concentrations of the undiluted subglacial meltwater that we measured earlier in the results, and iii) giving more references to the role of nutrient enrichment under the glacier (weathering during bedrock contact, solute expulsion during freezing). However, our nutrient measurements of the undiluted meltwater still showed lower concentrations compared to the fjord bottom water. The concentrations are enriched compared to sea ice and UIW samples at NG and IE, but we consider upwelling of bottom water more important for nutrient dynamics in this area. We further clarified these findings by referring more strongly to the undiluted meltwater nutrient concentrations in the text. Please note that Svalbard studies by van der Poll (e.g. van der Poll et al., 2018) agree with our conclusion. Referee 2 suggests that shallow water depth might limit the relevance of this process. We suggest that the freshwater input occurred below the sharp halocline in 4-5m depth, explaining the nutrient differences between 15 and 1 m. Additionally this process is supported through a) the absence of any substantial external advection of inorganic nutrients (e.g through tides and wind), and b) strong salinity driven stratification preventing mixing apart from upwelling. Detailed calculations were added to the text or supplement.

Title: Given the confusion regarding subglacial upwelling (see below) – do you mean submarine discharge or upwelling of deeper marine waters? – I would suggest a title change.. Perhaps: Spring submarine discharge plumes fuel under-ice primary production ?

This has been a very good suggestion by the referee – we agree and changed the title accordingly to "Early spring submarine discharge plumes fuel under-ice primary production"

Abstract:

L25: "retreat of tidewater glaciers could lead to decreased under-ice phytoplankton primary production" when? in the spring? In winter? Or both? My comment on the line above points to a broader problem which is evident in the title.. which is that I think by the lack of specificity regarding the timing, winter or spring is determinantal to the paper. Presumably if the focus is on spring primary production then the authors are speaking about subglacial upwelling in the spring?

Based on the simple date definition spring starts at the 20th of March. However, the definition of winter and spring is more difficult in Arctic studies, as biological processes like algal blooms are not tight to the calendar but to changes in e.g. light and ice regime. Also algal growth (as indicator of spring) in the ice might occur prior to algal growth in the water column. Spring may also be defined as the onset of snowmelt and temperatures above freezing point (mostly in terrestrial ecology), or by the return of light. Since we sampled at a time of subzero temperatures and ice cover (winter), but with sufficient light for algae blooms (spring), we had used the term "winter/spring" in the submitted version. However, since light availabililty is often most important in Arctic marine systems to define the onset of spring we changed the term to "early spring" throughout the manuscript and added the information where it was lacking (including L25).

A minor point, but line numbers every line would be really very helpful

We added the line numbers as suggested.

Introduction:

L37: unclear what "it" is referring too

We replaced "it" with "the submarine discharge"

L39: "close to the glacier front".. meaning what? Suggest specifying. Also a reference would be helpful here. The ranges of increased primary production in front of tidewater glaciers is quite variable so specification would be good.

The exact distance is highly variable, depending on sediment load, glacier terminus depth, discharge volume and flux e.g.. Hence, it is not possible to provide accurate numbers. However, based on an earlier study (Halbach et al., 2019) which found a phytoplankton bloom at 0.1 -1.9km distance from the glacier, we included a distance range into the manuscript (hundreds of meters to kilometers).

L41: "at some distance" .. again suggest specifying here.

See comment above

L46: I'm not sure I would necessarily agree that the lack of studies of subglacial discharge in the winter / spring is due to the perception of a lack of freshwater outflow. I think it's well known from a glacier hydrological perspective that temperate and even polythermal ice masses likely have winter / spring discharge. More likely it's due to a lack of opportunity given the challenge of Arctic field conditions and the difficulty in locating such an outflow which would presumably be of low flux.

We agree and changed this statement in the following way: " Due to the challenges of Arctic field work in early spring and the difficulties of locating such an outflow, only few studies investigated submarine discharge during that time window. The few studies available suggest overall little discharge (e.g. Fransson et al., 2020, Schaffer et al., 2020) compared to summer values. The limited amount of data makes the generalized quantification of subglacial outflow difficult. In addition, studies focusing on the potential impacts of the early spring discharge on sea ice and pelagic primary production are lacking."

L52-53: Suggest defining what you mean by "Glacier terminus melt rates"

The term "glacier terminus melt rate" is adopted from the mentioned publications, but we added a short clarification. "Glacier terminus melt rate occurring at the glacier-marine interface".

L54: Svalbard glaciers are shallower compared to what?

They (the water depth at the glacier terminus) are shallower than Greenland glaciers. We added following clarification: "submarine glacier termina on Svalbard occur typically at shallower water depths than on Greenland …"

L55-56: Phrase "can persist throughout winter and specifically in early spring" is unclear. Are you suggesting that outflow persists through winter and into spring?

We included the suggested sentence by the referee and rephrased the sentence in the following way: "can persist through winter and into spring"

L57: add phrase " various other mechanisms such as:" between the words "through" and "constant". Also suggest making the part re: temperate parts of the glacier" a discrete sentence. Presumably, with regards to winter / spring discharge you are speaking about polythermal glaciers? I think this section in general needs more specifics regarding the types of glaciers that typically have winter/spring discharge and the typical fluxes and chemical composition of this discharge. I would think that all of these points are worth mentioning to set-up the discussion of this paper. The point regarding chemical composition in particular has been glossed over as being sourced from meltwater stored from the previous melt season but this meltwater having been stored at the bed over winter would have a significantly different chemical character than dilute snow and ice-melt passing quickly through the system at the height of summer. Also, what about the possibility of basal ice melt?

We replaced the sentence with a more comprehensive paragraph addressing the missing information and background: "Hodgkins (1997) described the release of subglacial meltwater stored from the previous summer melt season from various Svalbard glaciers, including cold-based glaciers. Winter drainage occurred mostly periodically during events of ice-dam breakage. During the storage period, the meltwater can change its chemical composition. During prolonged contact with silicon-rich bedrock, the meltwater becomes enriched in the macronutrient silicate (Hodgkins, 1997). Additionally during freezing of the meltwater, solutes are expelled leading to higher ion concentrations in the liquid fraction (Hodgkins, 1997). Under polythermal glaciers, various other mechanisms such as constant supply from groundwater, and basal ice melt via geothermal heat, pressure, or frictional dissipation can also be a continuous, but low flux meltwater source in winter (Schoof et al., 2014)."

L59-60: "Even low rates of subglacial outflows can be sufficient to supply nutrients to the surface".. why? How? Is it because they would be sufficiently deep enough in the water column? Are you speaking of supplying nutrients via upwelling or via direct addition of nutrients in the subglacial discharge itself? If only the former, how can the latter be discounted since subglacial discharge in the spring would likely be more chemically enriched from greater contact times with the glacier bed or being sourced from basal ice melt?

We suggest that low supply rates via upwelling can have a considerable impact due to the absence of other sources in a sea ice covered fjord with very weak advection (tidal currents, wind, Atlantic water) and a strongly stratified water column. Direct addition can of course also play a role. We added the following clarification: "We hypothesis that subglacial discharge can lead to significantly increased primary production, due to upwelling of nutrient rich deeper water or through its own nutrient load, especially towards the end …"

L60: Why would spring subglacial discharge contain less sediment.. b/c of the low fluxes? Suggest specifying why.

The referee is correct in his/her suggestion. The reduction is likely caused by the low fluxes and thereby reduced advective forcing. We added a reference to a study measuring the seasonal variation of sediment outputs at a Svalbard tidewater glacier as additional support as described in the response to RW1 (Moskalik et al., 2018) and added a specification of "due to lower fluxes".

L63: Suggest setting up this argument a bit more progressively. Explain first what nutrients are generally fueling the under-ice spring bloom initially, and then go into the timing of glacial discharge and how that might positively affect under-ice primary production. As of now, the timing of the discharge and the initial bloom and end of bloom period are all not clearly laid out and this is problematic (in my opinion).

We suggest following additions: "After light becomes available in spring, ice algae and phytoplankton may start forming blooms fueled by nutrients supplied via winter mixing with different onsets in different parts of the Arctic. The blooms are typically terminated by limitation by macronutrients, either nitrate or silicate (Leu et al., 2015). We suggest that in the absence of wind induced mixing due to the seasonal presence of fast ice cover in spring, submarine discharge of glacial meltwater can directly (ion enrichment over the subglacial storage period) or indirectly (upwelling) be a significant source of inorganic nutrients significantly increasing primary production after nutrients supplied via winter mixing are incoporated into algal biomass."

L67: delete "the" before "primary" and add "in front of tidewater glaciers" after the word "production"

We changed the sentence accordingly.

L70: Re-arrange /re-write sentence to: Once sufficient light penetrates the snow and ice layers, ice algae start growing within sea ice between March and April: : :. Etc"

We changed the sentence accordingly

L73: "nutrient additions from the water column" .. via what? How? Suggest specifying.

We suggest replacing "nutrient addition" with "advection of nutrient-rich seawater" for clarification

L74: "subglacial upwelling" .. does this refer to spring subglacial upwelling? Suggest specifying. Again, I find the timeline within the year confusing with regards to glacial meltwater discharge and effect on bloom dynamics. Suggest more clearly spelling all of this out above.

Yes, we refer to spring. We add the term "early spring" for clarification.

L78: "or at the ice edge related to ice edge induced upwelling" .. can you define this upwelling without using the words "ice edge"?

We suggest using the term "wind-induced Ekman upwelling" as described by Mundy et al. (2009).

L79: suggest replacing "coverage also" with "accumulates"

We replaced "coverage also" with "accumulation"

L81: suggest replacing "Once" with "After"

We change the term "Once" with "After" as suggested.

L83: suggest replacing "related" with "induced"

We change the term "related" with "induced" as suggested.

L86: suggest deleting "to" and replacing "fuel" with "fueling"

We change the formulation "to fuel" with "fueling" as suggested.

L87: the word "slow" is curious .. why is the subglacial upwelling slow? How do you know it's slow vs fast or continuous vs intermittent? Suggest deleting this word as it opens up a range of topics that haven't been discussed in enough detail above to warrant the use of this adjective here.

We suggest replacing "slow" with "of low total flux", which would include continuous and intermittent discharge.

L86- 88: This pivot in this last sentence doesn't make a lot of sense to me as it seems to not really address the points brought up by the sentences immediately preceding it: : : i.e. namely reduced algal biomass due to brackish ice conditions .. suggest rectifying this last sentence.

We agree with the referee to change this section. We suggest removing the last part of the sentence "…and cause different succession patterns for phytoplankton and sea ice algae." Since the succession patters are not clearly introduced or explained and not a major objective of the paper.

L90-91: How are the 2 freshwater inputs different? Suggest specifying versus keeping your reader in the dark here.

We suggest replacing "with different freshwater inputs" with "with only one glacier front supplying submarine freshwater discharge". We agree that the previous formulation is unclear and misleading, since we mostly argue for the absence of freshwater inputs at NG.

L92: "to investigate the effect of the glacier terminus" .. this is a big vague. Suggest specifying.

We suggest adding following details: "… to investigate the effect of the glacier terminus, and subglacial outflow related upwelling on the light and nutrient regime in the fjord and thereby on early spring primary productivity…"

L94: "nutrient rich meltwater".. I'm unclear what you are referring to here.. presumably since this phrase is followed by "bottom water to the surface" I think by nutrient-rich meltwater you are referring to the subglacial discharge being enriched itself in nutrients versus upwelling of bottom waters but this has not been addressed above (though I suggest doing so)

We refer to the meltwater coming from the glacier itself. We suggest following clarification: "nutrient rich glacial meltwater" and "upwelling of marine bottom water"

L95: suggest adding "under ice" before the words "primary production" if this is indeed what you are referring too?

We added the formulation "under ice" as suggested.

L95: "near the glacier front".. phrase is vague. Suggest specifying.

We suggest adding a distance estimate in the following way: "near (<500 m) the glacier front".

L95-96: "low permeability of sea ice" .. phrase is also vague. Suggest specifying. As noted above I think the introduction would benefit from some more specificity, especially regarding the types of glaciers where winter / spring discharge might occur, a timeline of how this discharge evolves from end of the season to the winter and spring, and how this discharge might affect spring bloom under-ice dynamics – considering both the possibility of upwelling of bottom waters and also addition of nutrients directly from the glacial meltwater itself as alluded to in the last paragraph. One thing that should also be likely addressed is that any spring discharge will presumably be of quite low flux.. given this how likely / effective will any upwelling be?

We suggest adding following specification: "as a result of low permeability sea ice limiting nutrient exchange and inhabitable space"

As mentioned above (RW comment on L57 and L63), we also added a more detailed introduction of the potential discharge of different glacier types and the chemical characteristics of fresh vs stored subglacial meltwater with a potential of direct nutrient input with the meltwater. We also added the statement of low fluxes in spring as mentioned above (RW comment on 87). We believe we explained the role of lower salinity waters for forming less permeable sea ice already in former lines 84ff. We added the following sentence to line 85: , 1999). Sea ice with reduced bulk salinity has a reduced permeability to more saline ice at identitical temperatures (Golden et al. 1998).

Reference: Golden KM, Ackley SF, Lytle VI (1998) The percolation phase transition in sea ice. Science 282:2238-2241

Methods:

L120: ".. were melted in 50% vol/vol sterile filtered seawater: : :" what was the reasoning for this?

Sea ice is commonly melted in 50% vol/vol sterile seawater in order to avoid osmotic shock. Since most sea ice organisms live in the brine channels with high salinity, but the salinity of a melted bulk ice core is very low, direct melting leads to osmolysis. We added following sentence for clarification: "…to avoid osmotic shock of cells (Garrison and Buck 1986)"

L155-157: Estimates of bottom water fractional contributions based on conservative mixing of nitrate.. can you rule out nitrate addition from the glacial meltwater itself? Other studies have found this (see, Beaton et al., 2017 in ES&T: https://pubs.acs.org/doi/abs/10.1021/acs.est.7b03121), especially in the early season meltwater from a distributed subglacial drainage system.

We realize that our formulation was not clear. We also measured NOx concentrations from the subglacial outflow itself. We found subglacial outflow water exiting the glacier and sampled it directly (Salinity 0). The nutrient values of the glacial outflow, bottom water, and surface water were used for the calculations. We added following clarification in the methods text: "assuming linear mixing of subglacial meltwater, bottom water (at station IE) and surface water concentration using the NOX concentration measured at IE (Table 1).

As mentioned by RW1 we added details and equations on how the mixing calculations were done.

In the manuscript we added the equations to the supplement, we added the error estimates in Table 1, and we added details about the different water types in the header of Table 1.

Here the response to RW1 which outlines our changes:

"We suggest adding following calculations to the supplement. The mentioned outlier values in SG in the UIW sample was not used for the mixing calculations as explained above. For the meltwater fraction at the surface the error related to the average IE salinity is less than 0.1 % (see comment above), the main variation of the % meltwater contribution in the surface layer of SG is related to the salinity at the surface of SG (Fig. R1). We added the error estimate of 0.1 % to the table. For nutrients, the error was estimated based on the variability in the concentrations measured in the triplicates. For NOx the estimated range of contribution by upwelling is thereby 57-59 % (± 1 %) bottom water, for Silicate 89-95 % (± 3 %), and for phosphate 46-49 % (± 3 %).

Equations. Mixing calculations for estimates of the fraction of meltwater (MW_{Sal}) based on salinity, and for bottom water based on nutrient concentrations (BW_{Nuts}). Sal indicates the average salinities measured at the IE (Sal_{IE}), SG at 1m depth (Sal_{SG1m}), subglacial outflow (Sal_{glac}). Nut indicates the nutrient concentrations of nitrate and nitrite (NO_X), silicate (Si), and phosphate (PO4) at 1m under the sea ice at SG (Nut_{1mSG}) and IE (Nut_{1mIE}), the bottom water of the IE (Nut_{BW}), or subglacial outflow water (Nut_{glac}).

$$MW_{Sal}[\%] = \frac{Sal_{IE} - Sal_{SG1m}}{Sal_{SG1m} - Sal_{glac} + Sal_{IE} - Sal_{SG1m}} * 100$$

$$MW_{Sal}[\%] = \frac{34.7\ PSU - 23.6\ PSU}{23.6\ PSU - 0\ PSU + 34.7\ PSU - 23.6PSU} * 100 = 32\ \%$$

$$BW_{Nut}[\%] = \frac{Nut_{1mSG} - MW_{Sal}[\%] * \ Nut_{glac} - \ Nut_{1m_{IE}} + MW_{Sal}[\%] * Nut_{1m_{IE}}}{Nut_{BW} - \ Nut_{1m_{IE}}} * 100$$

$$BW_{NOX}[\%] = \frac{6.52\mu M - 0.32 * 2.06\ \mu M - 3.27\ \mu M + 0.32 * 3.27\ \mu M}{9.57\ \mu M - 3.27\ \mu M} * 100 = 58\ \%$$

$$BW_{Si}[\%] = \frac{4.30\ \mu M - 0.32 * 1.79\ \mu M - 1.59\ \mu M + 0.32 * 1.59\ \mu M}{4.46\ \mu M - 1.59\ \mu M} * 100 = 92\ \%$$

$$BW_{PO4}[\%] = \frac{0.41\ \mu M - 0.32 * 0.09\ \mu M - 0.34\ \mu M + 0.32 * 0.34\ \mu M}{0.67\ \mu M - 0.34\ \mu M} * 100 = 46\ \%$$

“

Change in Table 1:

Table 1. Properties of 1) marine surface and 2) Marine deep water (both station IE), 3) subglacial discharge melt water and 4) station SG surface water and the relative contribution of the water types 1 to 3 to form water type 4. The calculations are given in the Supplement and are based on different salinities and nutrients in the 4 water masses.

	1 Surface water (IE 1m)		2) Bottom water (IE)		3) Subglacial discharge Meltwater		4) SG (1 m)
Salinity [PSU]	34.7		34.7		0	32 ± 0.1 %	23.6
Temperature [°C]	-1.4		-1.4		0		-0.4
Silicate [µmol L^{-1}]	1.59	0 %	4.46	> 84 %	1.79	32 %	4.30
NO_X [µmol L^{-1}]	3.27	10 ± 3 %	9.57	58 ± 1 %	2.06	32 %	6.52
Phosphate [µmol L^{-1}]	0.34	19 ± 3 %	0.67	49 ± 3 %	0.09	32 %	0.42

L215: I’m confused by the words “reciprocal transplant experiment” .. I don’t think a “transplant experiment” is described above.. just primary production incubations. I also find the description of this experiment (L215-218) unclear and thus the overall purpose of the experiments to the study also unclear. As written, I cannot assess these experiments so I’d suggest a re-write of this paragraph.

The words “reciprocal transplant experiment” is mostly used in plant ecology, when plants are planted/grown on different soil/ environments in order to see if the different soil/ environment has an effect on their fitness or growth. We did an analogue experiment in which we incubated algae communities in different water/ environments in order to test if the water chemistry has an effect on algae growth. We considered other more descriptive terms such as “water exchange experiment”, but prefer keeping the term “reciprocal transplant experiment” due to its established and wide use in ecology. We rewrote the paragraph to clarify the experimental design in the following way:

“For testing the effect of the water chemistry on phytoplankton growth, we designed a reciprocal transplant experiment where the phytoplankton communities at SG and IE (1 m and 15 m) were transplanted into the water of both SG and IE. 50 ml of the water containing the phytoplankton community were transferred into 50 ml sterile filtered (0.2 µm) seawater of SG or IE in 100 ml polyethylene bottles. The bottles were then incubated in situ at the appropriate depth and primary production measured as described above. The aim of the experiment is to test if water chemistry alone is sufficient to increase primary production, or if the different communities, light regimes, or temperatures are more important.”

L225: Unclear what map you are referring to in sentence starting with “The map..”

We refer to the map in Figure 1 and added the figure reference. (Fig. 1)

L232: I’m wondering why you chose to you swarm to cluster versus amplicon sequence variants (see Callahan et al., 2017: https://www.nature.com/articles/ismej2017119)

We are familiar with both approaches. ASVs would indeed give more details on ecotype level. However, the aim of the study was not to dive into detailed taxonomic differences and identities, but to a) identify larger groups (e.g. flagellates, diatoms) and their potential functions and ecological role in relation to the biogeochemical data and b) to show and discuss overall community differences between the samples/sites. For this purpose we believe that swarm clustering of OTUs is appropriate.

L235: Was the data trans-formed in anyway before making the dissimilarity matrix? I’m only asking because it seems doing some type of transformation (e.g. Hellinger) is increasingly common.

We did do Square root transformations and Wisconsin double standardizations and added this for clarity to the text. “… (NMDS) plots are based on Bray-Curtis dissimilarities of square root transformed and double Wisconsin standardized OTU tables…”

Results:

L243: replace “were having” with “had”

We replace “were having” with “had” as suggested.

L244: why is Fig 2 c, d referenced before Fig 2 a, b.. did I miss the reference to a, b somewhere?

Since Fig 2c,d (Salinity profiles) is mentioned before a,b (Temperature profiles) in the text we suggest changing the order of the figure panels (Salinity profiles: a,b, Temperature profiles: c,d)

L265: Are there any photos of the subglacial outflow described in L267-268? Since there is a lack of field data at this time of year I think that these would be of value.

We do have a photo that shows the sampling location of the subglacial discharge water, but the picture is not very clear since the liquid water was sampled below a layer of ice (Icing). We showed the picture in an earlier version of the manuscript, but removed it after the editor pointed out that the picture does not show clearly where we took the sample. We suggest adding the photo again in the supplement with a description where the sample was taken.

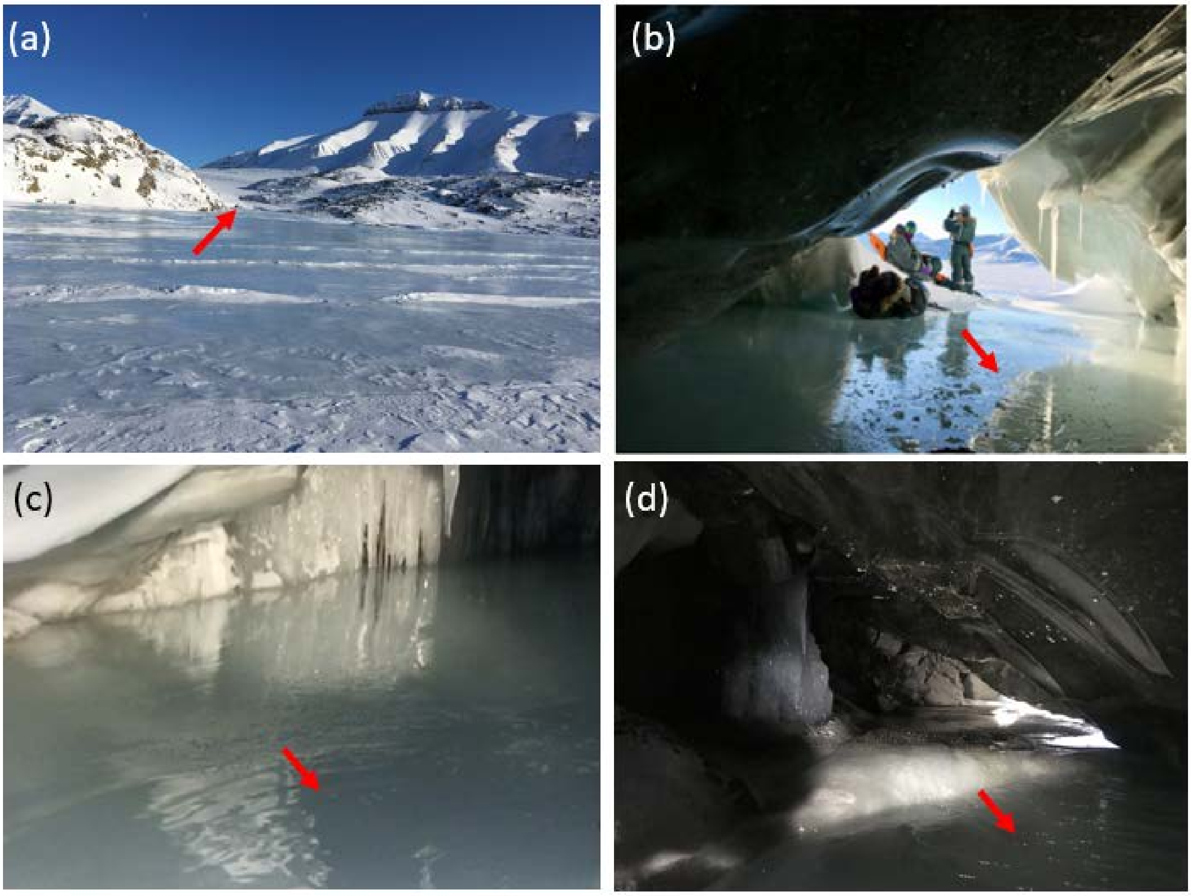

Figure S1. Sampling site for the subglacial discharge water. a) Aufeis on land in front of the southern part of the glacier and location of the ice cave shown in b-d (red arrow). b-d) Inside the ice cave with red arrow pointing to the liquid water sampled. The liquid meltwater was mostly covered by a layer of ice. Picture credits: a,c) Josef Elster, b) Marie Sabacka, d) Tobias Vonnahme.

L283: When reading about the very high nitrate+nitrite and silicate concentrations below the ice at SG I found myself really wondering if this could be coming from the subglacial meltwater itself versus upwelling of deeper marine waters. I believe you have data of the glacial meltwater itself? You mention these samples in lines 101-102 .. and I see further on that you present this data in L295. I'd suggest re-organizing so that this comes before the marine data.

We agree that the glacial meltwater data should be shown earlier to answer this question before it arises. We suggest moving the sentences about the subglacial outflow to the start of the paragraph.

L295: missing units for silicate in the outflow water

Thanks for spotting this omission. We add the units of $\mu mol\ L^{-1}$

L300: The definition of conservative mixing is not quite right. The sentences in lines 300- 302 are especially problematic. I see that the other reviewer has already adequately commented on this so I will defer to those comments. In the rest of the paragraph I would avoid the words "positive mixing patterns" and "positive relations". I also found the color scheme in Fig 5 (red and pink) challenging to interpretation.

Concerning the color scheme in Fig 5, we used the same colors as in the rest of the manuscript for consistency. However, we agree that the colors appear too similar in Fig 5 and added a black outline to the red circles which will help improve clarity while keeping it consistent.

Concerning the conservative mixing we changed the text in the following way as described in the response to RW 1:

Nutrient versus salinity relations can provide indications of the endmembers (sources) of the nutrients (Fig. 5) with a linear correlation being indicative of conservative mixing. A positive correlation indicates higher concentrations of the nutrients of the saline Atlantic water endmember, while a negative correlation points to a higher concentration in the fresh glacial meltwater endmember. Biological uptake and remineralisation could weaken or eliminate this correlation, indicating non-conservative mixing. In the water column at NG and IE silicate (R^2=0.66, p=0.008), NO_X (R^2=0.62, p=0.01) and phosphate (R^2=0.69, p=0.005) showed conservative positive mixing patterns with higher nutrient concentrations in the Atlantic water (Fig. 5a-c). SG showed a negative correlation for silicate pointing to a higher concentration in the glacial meltwater (R^2=0.86, p<0.0001). The absence of correlations for NO_X and PO_4 at station SG indicate non-conservative mixing pointing towards the relevance of biological uptake and release measurements (Fig. 5d-f).

L310: I echo the other reviewer that these calculations of nutrients supplied via upwelling vs the glacial meltwater should be shown.. how were these calculated? What is the error on these calculations? This paragraph needs more explanation for these values to be believed especially considering (as pointed out by the other reviewer) the single outlier values that are driving the gradient in SG samples. Also, at SG, it seems, at least from Fig 5 d-f, that the lower salinity water had higher silicate concentrations but these concentrations were much higher than those reported for the glacial meltwater above. What is the source of this silicate?

Concerning the source of silicate, we prefer to keep this as part of the discussion. (Se ch. 4.4.3 first paragraph). Briefly, the mixing calculations show that the high Si values can be attributed to the subglacial discharge water itself AND bottom water reaching the surface. So, the bottom water appears an important source.

Concerning the calculations and error estimates, we provided following response to RW1 (the error estimates will be added to the text and tabl1 (See above)) that explains our methodology and the inclusion of text as supplement:

We suggest adding the following calculations to the supplement. The mentioned outlier values in SG in the UIW sample were not used for the mixing calculations as explained before. For the meltwater fraction at the surface the error related to the average IE salinity is less than 0.1 % (see comment above), the main variation of the % meltwater contribution in the surface layer of SG is related to the salinity at the surface of SG (Fig. R1). We added the error estimate of 0.1 % to the table. For nutrients, the estimation error was estimated based on the variability in the concentrations measured in the triplicates from each water type. For NOx the estimated range of contribution by upwelling is thereby 57-59 % (± 1 %) bottom water, for Silicate 89-95 % (± 3 %), and for phosphate 46-49 % (± 3 %).

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$$MW_{Sal}[\%] = \frac{Sal_{IE} - Sal_{SG1m}}{Sal_{SG1m} - Sal_{glac} + Sal_{IE} - Sal_{SG1m}} * 100$$

$$MW_{Sal}[\%] = \frac{34.7\ PSU - 23.6\ PSU}{23.6\ PSU - 0\ PSU + 34.7\ PSU - 23.6PSU} * 100 = 32\ \%$$

$$BW_{Nut}[\%] = \frac{Nut_{1mSG} - MW_{Sal}[\%] * Nut_{glac} - Nut_{1m_{IE}} + MW_{Sal}[\%] * Nut_{1m_{IE}}}{Nut_{BW} - Nut_{1m_{IE}}} * 100$$

$$BW_{NOX}[\%] = \frac{6.52\mu M - 0.32 * 2.06\ \mu M - 3.27\ \mu M + 0.32 * 3.27\ \mu M}{9.57\ \mu M - 3.27\ \mu M} * 100 = 58\ \%$$

$$BW_{Si}[\%] = \frac{4.30\ \mu M - 0.32 * 1.79\ \mu M - 1.59\ \mu M + 0.32 * 1.59\ \mu M}{4.46\ \mu M - 1.59\ \mu M} * 100 = 92\ \%$$

$$BW_{PO4}[\%] = \frac{0.41\ \mu M - 0.32 * 0.09\ \mu M - 0.34\ \mu M + 0.32 * 0.34\ \mu M}{0.67\ \mu M - 0.34\ \mu M} * 100 = 46\ \%$$

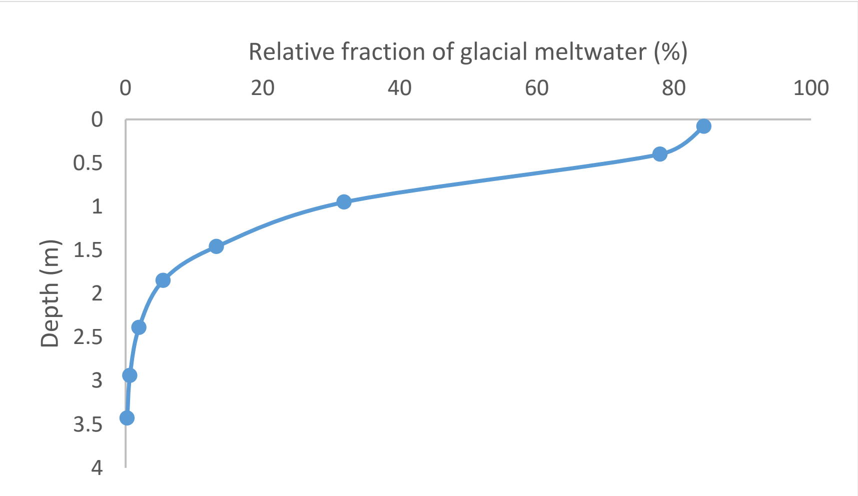

Figure R1. Estimated fractions of glacial meltwater in the surface layer of SG.

L333: Like the other reviewer I'm confused by the term "vertical export of Chl" – what it means, how it was estimated, and what the errors on this estimate are.

See response to RW1 (The error is based on Chl a tripiclates and given in Fig. 6):

The vertical export flux of Chl a is based on Chl a measurements in the sediment traps. We first convert the measured Chl concentrations (mg m^{-3}) to mass (mg) in order to calculate the flux as the mass of Chlorophyll a per unit area and time sedimenting to a certain depth.

Suggested change:

This leads to higher (14 times) vertical export flux based on the sediment trap measurements than production at IE and considerably lower (5 %) export than production at SG (Table 2).

L337: "assuming absence of grazing".. this doesn't really seem realistic?

The assumption is necessary since we did not estimate grazing rates. If grazing would be considered the loss rate would be higher. For clarity, we added following sentence.

"As grazing was not estimated in this study, the suggested loss terms of chl a based on the sediment trap data are likely underestimations."

L348: I'd suggest explaining more fully again the goal of the "reciprocal transplant experiment" before giving the results.

See changes in the methods:

"For testing the effect of the water chemistry on phytoplankton growth, we designed a reciprocal transplant experiment where the phytoplankton communities at SG and IE (1 m and 15 m) were transplanted into the water of both SG and IE. 50 ml of the water containing the phytoplankton community were transferred into 50 ml sterile filtered (0.2 µm) seawater of SG or IE in 100 ml polyethylene bottles. The bottles were then incubated in situ at the appropriate depth and primary production measured as described above. The aim of the experiment is to test if water chemistry alone is sufficient to increase primary production, or if the different communities, light regimes, or temperatures are more important."

We also suggest adding a short introduction of the experiment to the results:

"The reciprocal transplant experiment aimed to show the effect of water chemistry on primary production in the absence of effects related to different communities, temperature, or light. The results (Fig. 7) showed clearly …"

Fig 6: The quality of this figure should be improved. The numbers in the parentheses are very difficult to read.

For the final version the quality will be substantially better due to the use of vector files (pdf) instead of png (as in the current pre-print file). We will also increase the font size of the error ranges in the parentheses)

Fig 7: The x-axis with the experiment name are not clear. What does "com" stand for?

We suggest writing "community" instead of "com"

Fig 8: Define UIW in the legend as you have for the other abbreviations

We will write it out as "Under ice water" as suggested.

L355-356: "The first [NMDS1] axis separated sea ice from water communities with no overlapping samples".. this really isn't evident in Fig 8a.. sea ice is the square and what water and under ice water samples are the triangles. These regularly are in the same ellipses, unless I'm missing something? Also, is the glacier outflow sample actually a under ice water sample? What is the salinity of this sample? I guess I'm wondering if this is a true non-marine glacial outflow sample or one that could be diluted by marine water? I think this is an important point that needs to be clarified above.

We agree that the figure needs some clarifications.

1. We agree that sea ice and water samples are not directly separated by axis 1, but by axis 1 and 2 and remove the reference: "Sea ice and water communities are clearly separated with no overlapping samples."
2. The ellipses include subglacial meltwater (Salinity=0), glacier ice (Salinity=0), surface water and sea ice at SG in 2019 and 2018, and the remaining water and sea ice samples (including deeper water samples from SG). For clarity, we suggest coloring the ellipses and add a legend for the eclipse colors. In the figure caption we suggest following clarification: "… Groups highlighted in eclipses: glacier ice (top right), undiluted subglacial outflow (top left), surface samples (UIW, sea ice) at station SG 2019 (top blue), surface samples (1m water, sea ice) at

station SG 2018 (bottom blue) and others including deeper water samples at SG (bottom). The fraction of shared OTUs (in %) are shown as lines scaled to the fraction [%] of shared OTUs.

3. We also suggest using a separate symbol for glacial outflow to avoid confusion about the origin (under the sea ice, or from the subglacial outflow)
4. The aim of the eclipses is to support the discussion of OTU turnover between trhe subglacial outflow and marine samples, which we use for a rough estimate of fluxes and connectivity. Since we only do the analyses for 16S samples (due to short generation time and availability of complete glacier samples), we did not show ellises for the eukaryotic communities.

L358-360: What was the stress on this NMDS? How robust is this ordination you show? I'm always weary of interpreting the axes in this manner, i.e. axes one shows X and axes 2 shows Y .. i.e. similar to how one might view a PCA. I agree that looking at Fig 8a your communities are different but I don't think you can go as far to say that axis 1 is separating ice vs water and axis 2 is separating glacial vs marine. The ordination of this NMDS would likely change each time you ran it.. maybe something to consider?

The stress values are given on top of the NMDS plots (0.07 for 16S, 0.14 for 18S and LM). The stress values are indicative of a very good to good representation in the reduced dimensions. For clarity, we added the information also in the figure caption. We removed the description of which axis separates the community. With the R function used (metaMDS) the ordinations stay the same (The plot is reproducible with the same code).

L371: "Overall the same NMDS clustering has been found as for the 16S rRNA sequencing" .. but in the 18S plot (Fig 8b) no ellipses are drawn.. does this indicate that these group divisions were not significant? The written text doesn't seem to match the figure.

The aim of the eclipses is to support the discussion of 16S OTU turnover between the subglacial outflow and marine samples, which we use to estimate fluxes and connectivity. Since we only do this analyses for 16S samples (due to short generation time and availability of complete glacier samples), we did not show ellipses for the eukaryotic communities. However, for comparability and due to descriptions of clusters in the written text, we suggest adding the ellipses for Fig. 8b, but do not show OTU turnover since the information is not used in the manuscript. We tested for significance using ANOSIM and describe the significant ($p<0.005$) differences in the text.

Fig8c – the separation in the samples is quite striking on this NMDS. How come there are no ellipses on this plot? Were the differences shown in the NMDS not significant? Could try a perMANOVA to test the significance of differences between the groups perhaps?

For Fig 8c we prefer to not add ellipses, since the sampling design differs and the information is not used further in the manuscript. As described in the text, differences between sea water and sea ice are significant (ANOSIM, $p<0.005$), but not the differences between SG surface samples, and other stations. For Fig 8c we also suggest following changes in the text for clarity: "Furthermore sea ice species composition at SG station differed from NG and IE (Fig. 8c)."

Discussion:

L388-391: These first few lines are a great summary and really the abstract and introduction needs to be better set-up to frame these important points: (1) evidence for subglacial upwelling at a shallow tidewater glacier under sea ice and (2) that this upwelling persists in the winter / spring and supplies nutrient-rich glacial meltwater and upwelling of bottom water: : : I actually think part of the confusion is the use of the term "upwelling" to describe the release of submarine discharge into the ocean and also the upwelling of bottom water. Perhaps a change of language throughout would be helpful – i.e. saying "submarine discharge" vs "subglacial upwelling". And as per points above the case about nutrient-rich glacial meltwater needs to be set-up and made earlier as it's really a central finding.

The referee has a good point that subglacial upwelling and submarine discharge are two different processes. We suggest changing the terminology of submarine upwelling to submarine discharge where necessary (e.g: "(1) evidence for submarine discharge at a shallow tidewater glacier under sea ice and (2) that this submarine discharge persists in the winter") throughout the manuscript. As mentioned above, we also moved the results description of nutrients in subglacial meltwater to the beginning of the nutrient section and added an introduction about the effect of water storage underneath a glacier over winter on the water chemistry (silicate enrichment by prolonged contact with the bedrock -> weathering, ion concentration by solute expulsion during freezing of stored meltwater)

L406: The phrasing "which does not allow basal glacial ice to melt" is unclear. The whole sentence is too long and should be made into 2, but are the authors saying that because there is not Atlantic inflow water there can be no basal ice melt? Basal ice melt can result from geothermal heat flux, overburden ice pressure, and sliding friction. Warm ocean water is not the only mechanism. I suggest looking at a textbook (e.g. the physics of glaciers) and reviews on this topic: e.g. Hubbard and Sharp, 1989

We realize that we used the wrong terminology here. We are discussing glacier terminus (glacier-marine interface) ice melt, and not basal (glacier-bedrock interface) ice melt. We corrected the terminology throughout the discussion. We also agree that the sentence can be splitted in 2.

L407: "Subglacial meltwater itself is unlikely to lead to basal ice melting due to its low salinity". This sentence is very unclear to me. I'm not sure what this sentence is saying or trying to say.

We agree that this sentence is very unclear and suggest removing it.

L407-408: "However, basal ice melt is likely more important in systems with Atlantic water inflows: : :" as per above this seems to ignore the possibility of basal ice melt underneath temperate and polythermal glaciers. This may not be what the authors mean but as written it reads this way.

As mentioned above, we meant glacier terminus ice melt and not basal ice melt and correct the terminology.

L420: "remains from the previous melting season" is unclear. Can you specify what you mean by remains.

We refer to fresh meltwater that entered the fjord during the previous melting season (summer), remaining at the surface (due to its lower density) throughout winter due to limited mixing and advection. We suggest following change: "may be meltwater introduced during the last summer to fall melting season and remaining throughout winter."

L433: Can you specify what data you are referring to when you say "estimated bacterial growth rates". I searched for this term in the paper and did not see it previously defined. It really should be so that the basis for this calculation of doubling time is clear.

The estimated bacterial growth rate is given in table 2 as bacteria biomass production. We suggest replacing the term "growth rate" with "biomass production" for consistency and to add a reference to table 2 in the text.

L442: Why does the supply have to be "constant" ? It seems like (from the methods) that samples for community analyses were only taken once at each station? How does a single-time point sample give an indication of the timescale of submarine discharge into the fjord? This might be a bit of a reach based on the community data alone – suggest tempering this statement.

We agree that "constant" appears to be the wrong term. We suggest using the term "continuous" instead. The argument is that we assume that the Bacteria that are only present in subglacial outflow and surface SG water are inactive and not growing. Considering the doubling time of the entire bacteria community, these inactive not-growing bacteria would be replaced by active bacteria in the time frame of the doubling time. In addition to overgrowth, inactive bacteria would also be exposed to losses due to grazing, viral lysis, and sedimentation. We acknowledge that these assumptions are very simplified and suggest to also add some terms to show the uncertainty of this estimate: "Thus, we suggest that the presence of shared OTUs between SG and the glacial outflow may indicate a continuous supply of fresh inoculum to sustain these taxa."

L442-444: When you say the "southern part of the glacier" is this part on land or in the ocean? If it's on land you should specify. I also think that this assumption that this outflow is being released under the marine-terminating portion can be backed up by your marine data? This sentence seems out of place here.

Yes, we refer to the land-terminating part. We suggest adding the detail in the following way "land-terminating part south of the glacier". We also agree that we have marine data to support this hypothesis (e.g. Salinity profiles). The observed subglacial outflow on land is simply an additional piece of evidence. We suggest replacing "the clearest evidence" with "clear evidence" and adding a sentence about the marine evidence. "In addition, our marine evidence by salinity and nutrient profiles, turbidity, and communities support the occurrence of submarine discharge in early spring."

L445- to end of paragraph: This explanation of glacier hydrology really needs to come earlier. As written this whole section on the potential magnitude of upwelling is poorly organized. Suggest first setting it up by talking about processes on the ice and then what's happening in the ocean.

We addressed this comment by 1) introducing the glacier hydrology more extensively in the introduction and 2) moving the section about glacier hydrology (442-451) to the end of chapter 4.1 since it is part of the evidence for submarine discharge and not directly for the magnitude/ flux.

L456: "Our mixing calculations estimate".. where are these calculations described?

See comment above. We added the equations and calculations to the supplement.

L457: At what depth is the submarine discharge exiting the glacier? I find myself wondering at what depth these different water masses occur (can you specify this) and how deep the DLAWis being entrained from? Is it sufficiently below the nutricline to be replete in nutrients? Also the calculated entrainment factor of 1.6, how was this calculated exactly? And you state "which pulled 1.6 times more DLAW" .. more than what? This is not clear.

Considering the estimated depth at the glacier terminus of 20 m, this would be the depth of the discharge exiting the glacier. Nutrients are depleted at the surface, but not at 15m, indicating that the discharge happens below the nutricline and has therefore the potential for upwelling.

We suggest adding this information in the following way: "Nutrients were depleted in the UIW, but not at 15 m depth, showing that the nutricline had to be shallower than 15 m. Hence, submarine discharge at a glacier terminus depth of 20 m would cause upwelling ofc nutrient rich DLAW to the surface."

The entrainment factor is the proportion of contributions from DLAW to SGO at the surface (53% DLAW: 32% SGO = 1.6 DLAW:SGO at 1m depth). We suggest replacing "more" with "as much" for clarification. We also specified the calculation by replacing the "(53%)" by "(53 % DLAW : 32 % SGO = ratio of 1.6)" in the manuscript.

L458-459: "Fransson et al. (2020) found that 30-60% of glacier derived meltwater was incorporated in the bottom sea ice : : : again indicating that it is a widespread process at marine terminating glacier fronts" .. what is a widespread process? The release of submarine discharge and its incorporation into bottom sea ice OR the entrainment of different water masses (i.e. DLAW) as the plume rises (as discussed in the previous sentence). Again, this is a case in point of the organizational structure and lack of specificity of terms "submarine discharge" vs "upwelling of bottom waters" to be a source of confusion.

We suggest following clarification "… indicating that winter/ spring submarine discharge and the resulting formation of sea ice with low porosity is a widespread process…".

L461: "Compared to the massive subglacial plumes of summer systems" .. where? This should be specified .. different glaciers have widely different discharge fluxes. The citation seems to be from Greenland but these glaciers will bear little resemblance to Svalbard, perhaps citing summer discharge fluxes from Svalbard glaciers too would be useful – particularly from your study site if the intent of this sentence is to contrast with spring discharge fluxes as seems to be the case.

We agree that the structure of the entire chapter needed improvement. Thus, we rewrote the entire chapter, considering all comments. Concerning this specific comment, we specified the location and time of each tidewater glacier system compared. We start with stating the conditions in our study, continue with the most similar glacier on Svalbard, and finish with a wider picture by comparing the data to the larger and deeper Greenland glaciers.

The sentence mentioned by the RW was rewritten in the following way: "Our study suggests that subglacial upwelling in spring results in a small volume transport with only about >1.1 m^3 m^{-2} $month^{-1}$ (approx. 2 m^3 s^{-1}). This estimate is based on the flux of nutrient rich bottom water needed to maintain the measured primary production assuming steady state conditions and is therefore a rough, but conservative estimate. The most comparable estimate on the magnitude of the upwelling is available at Kronebreen for summer. This Svalbard tidewater glacier is of similar size and had one order of magnitude higher upwelling rates compared to our study (31-127 m^3 s^{-1}, Halbach et al., 2019). Due to their size, summer subglacial upwelling in Greenland is two to four times higher than at Kronebreen (250-500 m^3 s^{-1}, Carroll et al., 2016)."

L462: "subglacial upwelling in spring is a small volume transport".. where is this data from? This study? This should be explicitly stated. Suggest re-writing this entire sentence. Also, the last part of the sentence regarding upwelling needed to maintain primary production should be a new sentence as this is a different point then the discharge flux.

The data are from this study. We agree that this should be stated. We also agree that the information "needed to maintain primary production should be moved to a seperate sentence. We rewrote the entire chapter, considering all comments. As suggested by RW1 we also converted the discharge units of the three studies (Greenland, Kongsfjorden, our study) to the same units for comparability. Concerning this comment, following changes were made:

"Our study estimated only a small volume transport through subglacial upwelling in spring of only about 1.1 m^3 m^{-2} $month^{-1}$ (approx. 2 m^3 s^{-1}). This estimate is based on the flux of nutrient rich bottom water needed to maintain the measured primary production assuming steady state conditions and is therefore a rough, but conservative estimate."

L464: "This careful estimate".. I'd remove the word "careful".. the more so because the sentence before this one is unclear! Is this estimate of freshwater input for Billefjorden in the summer or spring? It's unclear. The estimate from the Halbach paper is I believe from the summer so you want to make sure you are comparing like with like.

As pointed out by RW1, "careful estimate" is a misleading formulation. We suggested to replace it with "rough, but conservative". We also realized that the reason for comparing our spring study with summer values is not clear and suggest specifying that we do not know of any other spring studies with similar estimates. The study in Kongsfjorden is the most comparable estimate to our study (glacier size, terminus depth, location). We suggest following changes: "To our knowledge, our study provides currently the only available estimate of subglacial upwelling in early spring. …. The most comparable estimate on the magnitude of the upwelling is available at Kronebreen for summer. This Svalbard tidewater glacier is of similar size and had one order of magnitude higher upwelling rates compared to our study (31-127 $m^3 s^{-1}$, Halbach et al., 2019)."

L465-466: The fact that you have less entrainment than the Hopwood study is really not surprising at all considering the depth of discharge and flux of discharge at the much deeper, larger glaciers in that study. I'm not sure what the purpose is of this statement? As written now it's failing to provide relevance to this study.

We agree that this fact is not surprising and rephrased the statement. We still argue that it is necessary to compare entrainment rates and state that the glacier terminus depth is typically the controlling factor, apparently independent of the time of the year.

"In our study about 1.6 times as much bottom water (DLAW) as subglacial outflow water (SOW) reached the surface at SG (Entrainment factor of 1.6 – see above) through the upwelling process. The entrainment factor is mostly dependent on the depth of the glacier front (Carroll et al., 2016). The glacier terminus at SG was shallower (approx. 20 m) than any other studied tidewater glacier on Svalbard (70 m depth at Kronebreen, Halbach et al., 2019) or Greenland (> 100m, Hopwood et al., 2020). Hence, the higher summer entrainment factors estimated in Kongsfjorden (3, Halbach et al., 2019) and Greenland (6 to 10, Hopwood et al., 2020) are not surprising. Overall, glacier terminus depth appears to be the main control of entrainment rates, likely independent of the time of the year. However, turbulent mixing may cause increased entrainment during times of very high subglacial discharge rates."

L466-467: "each volume of SGO water pulled about the same volume of DLAW with it to surface".. this is unclear.. do you mean each volume over a certain timeframe (a day? A week? A month?) .. what is the volume exactly? What was the volume of DLAW entrained? This should be stated if you are speaking about volumes here. And again the comparisons to the Hopwood study don't' seem relevant if you are comparing to large Greenland glaciers. You should specify where and what type of glaciers in the Hopwood review you are comparing too.

We refer to proportion of volumes (Vol DLAW : Vol SOW), which is a value comparable to chemical volume percentages (e.g. 70% Ethanol in MQ vol/vol). Thereby an exact volume is meaningless. To avoid confusion, we rephrased the sentence in the following way.

"In our study about 1.6 times as much bottom water (DLAW) as subglacial outflow water (SOW) reached the surface at SG (Entrainment factor of 1.6 – see above)"

We also specified the type (depth, size, location) and time (summer) of the compared studies as mentioned above.

To our knowledge, our study provides currently the only available estimate of subglacial upwelling in early spring. ….The entrainment factor is mostly dependent on the depth of the glacier front (Carroll et al., 2016). The glacier terminus at SG was shallower (approx. 20 m) than any other studied tidewater glacier on Svalbard (70 m depth at Kronebreen, Halbach et al., 2019) or Greenland (> 100m, Hopwood et al., 2020). Hence, the higher summer entrainment factors estimated in Kongsfjorden (3, Halbach et al., 2019) and Greenland (6 to 10, Hopwood et al., 2020) are not surprising. Glacier terminus depth appears to be the main control of entrainment rates, likely independent of the time of the year."

L470: This is the first mention of the depth of the discharge. As you say, 20-m is quite shallow. Are nutrient concentrations sufficiently high enough here to augment surface concentrations? In other words, is this depth below the nutricline.

As mentioned above, we now mention the depth earlier in the chapter. We also provide information on the depth of discharge in relation to nutricline (see comments above).

"The entrainment factor is mostly dependent on the depth of the glacier front (Carroll et al., 2016). The glacier terminus at SG was shallower (approx. 20 m) than any other studied tidewater glacier on Svalbard (70 m depth at Kronebreen, Halbach et al., 2019) or Greenland (> 100m, Hopwood et al., 2020)."

We also mentioned that the submarine discharge enters the fjord below the nutricline in the end of the chapter.

"In spite of the shallow depth, and the low discharge and entrainment rate of our study, subglacial upwelling appears to be the main mechanism to replenish bottom water with high nutrient concentrations to the surface and can substantially increase spring primary production due to; (i) submarine outflow below (approx. 20 m) the nutricline (<15 m), (ii) the absence of any other terrestrials inputs, (iii) Atlantic water blocked by a shallow sill (Skogseth et al., 2020), (iv) very weak tidal currents (Kowalik et al., 2015), and (iv) wind mixing blocked by sea ice in Billefjorden, and (v) undiluted subglacial meltwater having lower nutrient concentrations than the DLAW."

L473-to end of paragraph: This seems to directly contradict previous statements regarding the glacial meltwater discharge being enriched in nutrients (e.g. silicate?). Also many of the comparisons you are making are to summer discharge fluxes and summer entrainments.. the spring discharge will of course be lower but more chemically enriched from the glacial meltwater discharge? I think if you are going to use the summer values to compare, which you might have to do out of necessity and lack of other comparisons, you need to state so explicitly, and the limitations of such comparisons.

The glacial meltwater is enriched in silicate, considering its salinity (0) and compared to UIW and sea ice at NG and IE, but not compared to the bottom water. We tried to clarify it by following statement:

"…(v) undiluted subglacial meltwater having lower nutrient concentrations than the DLAW"

As mentioned above, we fully agree with the confusions about the comparisons. We rewrote the entire chapter in the following way:

"To our knowledge, our study provides currently the only available estimate of subglacial upwelling in early spring. Our study suggests that subglacial upwelling in spring results in a small volume transport with only about >1.1 m^3 m^{-2} $month^{-1}$ (approx. 2 m^3 s^{-1}). This estimate is based on the flux of nutrient rich bottom water needed to maintain the measured primary production assuming steady state conditions and is therefore a rough, but conservative estimate. The most comparable estimate on the magnitude of the upwelling is available at Kronebreen for summer. This Svalbard tidewater glacier is of similar size and had one order of magnitude higher upwelling rates compared to our study (31-127 m^3 s^{-1}, Halbach et al., 2019). Due to their size, summer subglacial upwelling in Greenland is two to four times higher than at Kronebreen (250-500 m^3 s^{-1}, Carroll et al., 2016). Due to their size, summer subglacial upwelling in Greenland is two to four times higher than at Kronebreen (250-500 m^3 s^{-1}, Carroll et al., 2016). In our study about 1.6 times as much bottom water (DLAW) as subglacial outflow water (SOW) reached the surface at SG (Entrainment factor of 1.6 – see above). The entrainment factor is mostly dependent on the depth of the glacier front (Carroll et al., 2016). The glacier terminus at SG was shallower (approx. 20 m) than any other studied tidewater glacier on Svalbard (70 m depth at Kronebreen, Halbach et al., 2019) or Greenland (> 100m, Hopwood et al., 2020). Hence, the higher summer entrainment factors estimated in Kongsfjorden (3, Halbach et al., 2019) and Greenland (6 to 10, Hopwood et al., 2020) are not surprising. Glacier terminus depth appears to be the main control of entrainment rates, likely independent of the time of the year. Kronebreen is the most comparable tidewater glacier in terms of glacier terminus depth and entrainment rate. Although the estimated entrainment rate was low at Kronebreen (3), it substantially increased summer primary production in Kongsfjorden (Halbach et al., 2019). In spite of the shallow depth, and the low discharge and entrainment rate of our study, subglacial upwelling appears to be the main mechanism to replenish bottom water with high nutrient concentrations to the surface and can substantially increase spring primary production due to; (i) submarine outflow below (approx. 20 m) the nutricline (<15 m), (ii) the absence of any other terrestrials inputs, (iii) Atlantic water blocked by a shallow sill (Skogseth et al., 2020), (iv) very weak tidal currents (Kowalik et al., 2015), and (iv) wind mixing blocked by sea ice in Billefjorden, (v) undiluted subglacial meltwater having lower nutrient concentrations than the DLAW."

L480: The word "Surprisingly" seems to not be the right word choice here.

We suggest removing the word "Surprisingly".

L438: "Substantial subglacial upwelling" .. I'm unclear was to what you are referring to here – is this submarine discharge of glacial meltwater or upwelling of bottom waters? In either case the word "substantial" seems ill-advised here given the preceding discussion and should be removed. Could it be that you didn't observe much light limitation because the plumes were not that "massive" (compared to summer).. i.e. you just have a much smaller discharge flux and therefore plume in the spring? This seems likely and unsurprising.

We agree that the formulation is misleading and removed it.

L485-86: Unclear what the phrase "where light is not considered limiting" is referring too.

We suggest specifying in the following way: "where light sufficient for photosynthesis".

Line 511: "rations" should be "ratios"?

We replaced the term "rations" with "ratios".

L515: Can you really call it "deep water upwelling" if the water is being entrained from only 20-m? This is problematic (at least for me) and needs to be clearly addressed I think.

We suggest replacing the term "deep water" with "bottom water".

L517-519: The discussion on iron seems unrelated and as written is unconvincing.

We consider a short discussion of iron important for a comprehensive discussion. Without the information the reader may consider iron as important micronutrient not considered and potentially important, which would weaken the robustness of the study. By acknowledging that iron may be imported in large amounts, but is not limiting in coastal Arctic systems, we clarify this potential question briefly. We suggest adding following clarification and an additional reference: "However, iron limitation is untypical in coastal Arctic systems (Krisch et al., 2020)."

L520: "nutrient concentrations may simply be higher due to the shallower depth at SG" .. why? It's unclear what you are trying to say. Suggest re-writing with more detail and explicity.

Nutrients are typically higher close to the sea floor due to benthic regeneration of organic matter in the sediments. If the surface water is only 30m over the bottom, vertical mixing via diffusion or advection needs consequently less time and/or physical forcing than at 150 m depth. We suggest following clarification: "nutrient concentrations may be higher due to less physical forcing and time needed for vertical mixing at the shallower SG compared to IE.

L529: Was the Frasson study done at this same site?

No the study was done at the neighboring fjord. We added the information in the following way: "The role of bedrock derived minerals and particles for composition of sea ice chemistry have been described in detail in the neighboring fjord (Tempelfjorden) by Fransson et al. (2020)."

L530: "The values" .. vague.. specify what kind of values you are referring to.

We replaced "The values", with "Silicate concentrations"

L535: Paragraph ending here is rambling and needs to be re-written. Suggest taking out the iron since you have no data on this to compare.

We agree and removed the last sentence about iron.

L536: "related".. what do you mean by this word? Specify.

We suggest following clarification: "…which was introduced via subglacial upwelling in Kongsfjorden…"

L538: Were are you proposing this nitrification is occurring? In the ocean or in the glacial meltwater? Could the high nitrate come from the subglacial waters itself? See papers by Beaton et al. in Greenland, Jemma Wadham, Boyd et al., 2011 (AEM) and Wynn et al., 2007 (Chemical Geology). Do you have measurements of the outflow un-diluted by seawater so you can rule this possibility out?

We propose the nitrification to happen in the UIW. We suggest adding the following information: "Ammonium regeneration and subsequent nitrification under the sea ice…". We disregard high nitrate inputs from the glacial meltwater itself since we did not measure high nitrate concentration in our samples from the outflow of undiluted meltwater (see Table 1). For clarification we suggest adding the following statement: "Nitrate can be supplied through the subglacial meltwater itself (Wynn et al., 2008), however we did not find high nitrate concentrations in the undiluted subglacial outflow water in our study."

L566: Were you able to resolve any low-light level species in your molecular community composition data to back this statement up?

In general, diatoms are know to be quite well adapted to low light levels. Diatoms were also th most common taxon of the UIW phytoplankton community (based on light micsroscopy, which is more quantitative). We suggest adding a statement of the capability of diatoms to grow under low light conditions. "In particular diatoms, the most common taxa of under ice phytoplankton blooms (von Quillfeldt, 2000, this study) are known to be well adapted to low light conditions (Furnas, 1990)."

Furnas MJ (1990) In situ growth rates of marine phytoplankton: approaches to measurement, community and species growth rate. J Plankton Res 12:1117–1151

L581: "their" .. unclear what this is referring to.

We suggest replacing "their production" with "primary production"

L646: "In winter and spring, this would result in the lack of subglacial upwelling".. but with more melt there would be longer melt seasons and presumably more submarine discharge and associated upwelling – at least in the shorter term?

We suggest adding following information: "In the shorter term, a longer melt season and presumably increased submarine discharge may lead to increased subglacial upwelling in winter and spring. However, on longer time scales , tidewater glaciers will retreat and transform towards land terminating glaciers (Błaszczyk et al., 2009), which would result in the lack of subglacial upwelling and systems more similar to the IE with less nutrients and light available for phytoplankton."