Authors' response to:

Interactive comment on "Subglacial upwelling in winter/spring increases under-ice primary production" by Tobias Reiner Vonnahme et al.
Anonymous Referee #1
Received and published: 10 November 2020

Vonnahme et al., conduct spring-time measurements in a tidewater glacier fjord and provide evidence that entrainment of subsurface-fjord waters by early season freshwater discharge is a measurable nutrient source to under-ice phytoplankton blooms which would otherwise be nutrient-limited at this time of year. The hypothesis and general idea is quite novel, supported by some in situ data as well as incubation studies, and I think it is an interesting addition to the field. I have no specific expertise in seaice or in the analysis of algae community composition- I defer to a more appropriate reviewer on these aspects. I do have quite a few comments throughout the text, but nevertheless found the discussion paper an interesting read suitable for the journal. Most of my comments are minor, or simply requests for a little more clarity.

We want to thank the reviewer sincerely for the thorough and very helpful review. We addressed all comments as described below and believe that the changes improved the manuscript considerably. We used following font colors and highlighting for the changes:

- Grey text: Reviewers comments
- Black text: Our response
- Red text: Text from the manuscript with the suggested changes
- Yellow highlights: Parts of the manuscript that we suggest to change

12 in some Arctic fjord systems, many don't have this and some of the few available case studies don't have high primary production.

We agree that the current formulation is inadequate, since not all fjords have tidewater glaciers, and the primary production in these tidewater-influenced fjord is typically high compared to other summer systems, but not high compared to spring blooms.

We suggest the following change:

Subglacial upwelling of nutrient rich bottom water is known to sustain elevated summer primary production in tidewater glacier influenced fjord systems.

25 You do briefly comment on this once, but considering the timescale required to get significant shifts in the groundling line of these glaciers, could you also think about trends in sea-ice loss as well. Could the timing of sea-ice loss from some of these fjords shift, or could sea-ice even completely disappear, before significant changes in grounding line are evident?

We agree that sea-ice loss is certainly important and should be discussed together with the glacier retreat.

We suggest following change in the abstract:

We suggest that climate change caused retreat of tidewater glaciers could lead to decreased under-ice phytoplankton primary production, while sea ice algae production and biomass may become increasingly important. However, under a scenario of climate driven earlier loss of the sea ice cover, spring phytoplankton primary production may increase.

We also added more details about sea-ice loss and changed in timing of the spring bloom due to earlier sea-ice break up, increased DOM and sediment inputs (brownification) to the discussion.

35 "During summer" Here I think the reference you mean is a similar paper by the same author that deduces the 'upwelling' effect in summer has a measurable impact on silicic acid availability1, the paper cited is from the same area but concerns the spring bloom in the same fjord (which has some sea-ice cover in spring)2. I think you could be a little more precise here.

We thank the reviewer for the comment and changed the reference accordingly.

39 Is time not also important here? If you didn't have any sediment close to the glacier front, the nature of the upwelling followed by outflow at the surface would surely lead to relatively low primary production at the glacier front anyway, because the freshly upwelled water is being laterally advected away from the front i.e. I assume you would never see the highest cell counts here even in the absence of high turbditity? Looking specifically at the system studied by Meire et al., the bloom also seems to peak a fair way downstream of the large glaciers, even though there isn't much turbidity in the inner fjord.

We agree that time and advection are also important here. While phytoplankton biomass increases towards a bloom, it is already advected away from the glacier front.

We suggest following change:

Primary production is typically low in direct proximity to the glacier front due to high sediment loads of the plumes absorbing light, but potentially also due to lateral advection and the time needed for the phytoplankton bloom formation (Meire et al., 2016a,b Halbach et al., 2019).

46 I don't think there's a perception of no freshwater release from these systems overwinter, there are several papers demonstrating this (for a good recent Arctic example3), although I agree there is a big problem with bias in the distribution of data towards the peak meltwater season, and model discharge curves do I think include a little early/late season discharge comparable to that observed here4. I think a more accurate statement would be that whilst it's known that a little discharge occurs early/late season there simply isn't much data to quantify it.

We agree that the formulation need clarification. There are indeed studies on the subglacial upwelling in winter and spring, but they are rare, compared to summer studies.

We add the suggested references and suggest following change:

Studies on

The few studies conducted during the winter/spring period indicate only a small amount of freshwater discharge (e.g. Fransson et al., 2020, Schaffer et al., 2020) compared to the peak melt season. However, the limited amount of data makes the quantification of subglacial outflow difficult. Currently, studies on the potential impacts on sea ice and pelagic primary production are lacking.

48 I think you should distinguish 3 sources here, as written icebergs and terminus melt, but also subglacial discharge at the grounding line

We agree and suggest following change:

In addition to subglacial discharge at the grounding line, tidewater glacier related upwelling mechanisms can also be caused by the melting of deep icebergs (Moon et al., 2018), or the melting of the glacier terminus in contact with warm seawater (Moon et al., 2018, Sutherland et al., 2019).

51 in summer. (Reference?)
We added the references by Moon et al. (2018) and Sutherland et al. (2014)

Moon, T., Sutherland, D. A., Carroll, D., Felikson, D., Kehrl, L., and Straneo, F.: Subsurface iceberg melt key to Greenland fjord freshwater budget, Nat Geosci, 11(1), 49-54, https://org/10.1038/s41561-017-0018-z, 2018.

Sutherland, D. A., Straneo, F., & Pickart, R. S.: Characteristics and dynamics of two major Greenland glacial fjords, Journal of Geophysical Research: Oceans, 119(6), 3767-3791, 2014.

61 "while at the same time entrapping considerably less light absorbing sediments" I'd be genuinely interested to know if you can provide data/refs to support this. I'm not sure we know what turbidity looks like in these sub-surface plumes and to what extent it represents particles from the ice melt/basal erosion, or resuspension, and how this changes through the year, although I would agree it would be expected to be low in this environment given the description of the fjord

We agree that some support in form of references and data is helpful here. We added a short reference to a study at Hansbreen, another polythermal Svalbard tidewater glacier. The study monitored SPM throughout the year and found the lowest SPM value in winter and spring at a depth of about 5-10 m, which fits to our study. The origin is resuspension, as well as subglacial discharge.

We suggest following addition to the introduction:

Sediment inputs during this time of the year are low with highest sediment concentrations deeper in the water column, indicating limited light limiting effects of surface primary production (Moskalik et al., 2018).

Moskalik, M., Ćwiąkała, J., Szczuciński, W., Dominiczak, A., Głowacki, O., Wojtysiak, K., and Zagórski, P: Spatiotemporal changes in the concentration and composition of suspended particulate matter in front of Hansbreen, a tidewater glacier in Svalbard, Oceanologia, 60(4), 446-463 2018.

63 "this spring upwelling mechanism could be the primary mechanism to significantly increase primary production" does not read well, try "could be a mechanism via which primary production is increased in tidewater fjords compared to similar fjords without these glaciers: : :" or similar.

We changed the text accordingly:

We suggest, that in the absence of wind induced mixing due to the seasonal presence of a fast ice cover, upwelling of subglacial outflows could be a mechanism increasing primary production in tidewater fjords compared to similar fjords without these glaciers, especially towards the end of the ice algal/phytoplankton spring bloom when nutrients become limiting (Leu et al. 2015).

65 I think you need a reference here. Retreat may generally co-occur with shoaling of the grounding line, but not always, and there may also be an increase in discharge which could offset shoaling to some extent as entrainment also depends on freshwater discharge volume. Also, you comment on upwelling being eliminated, but wind-driven upwelling will remain, so maybe be a bit more precise.

We clarified the statement in the following way and added a reference:

Higher glacial melt rates and earlier runoffs may initially increase tidewater glacier induced upwelling, due to increased subglacial runoff (Amundson and Carroll, 2018). However, their retreat and transformation into shallower tidewater glacier termini may lead to less pronounced upwelling, unless the shallower grounding line is compensated by the increased runoff (Amundson and Carroll, 2018). Eventually, the tidewater glaciers transform into land terminating glaciers, where wind induced mixing is still possible, but subglacial upwelling is eliminated (Amundson and Carroll, 2018) – potentially reducing primary production.

107 'shallow' Do you know the approximate depth?

We estimated the approximate depth based on bathymetric maps and added the information to the manuscript:

The fjord is separated from Isfjorden, a larger fjord connected to the West Spitsbergen current, by a shallow approximately 30 to 40 m sill (Norwegian Polar Institute, 2020),…

120 "were melted in 50 % vol/vol sterile filtered" Is there a reason for this?

Sea ice is typically melted in in sterile filtered seawater to avoid osmotic shock and lysis of organisms in the ice. Microorganisms in the ice live mostly in brine channels with high salinity, while the frozen ice around is very fresh. Melting the ice around would lead to an overall very low salinity, leading to severe stress to the high-salinity adapted organism.

We added following information:

…were melted in 50 % vol/vol sterile filtered (0.2 µm Sterivex filter, Sigma-Aldrich, St. Louis, MO, USA) seawater to avoid osmotic shock of cells (Garrison and Buck 1986), …

Garrison, D. L., and Buck, K. R.: Organism losses during ice melting: a serious bias in sea ice community studies, Polar Biol 6:237-239, 1986.

131 One metre or 1 m

Since it is the beginning if a sentence we suggest "One metre".

140 Does the exact salinity you use for your inflowing seawater have a large impact on your calculations, can you state what the value of 34.6 refers to? Also, can you clarify to what extent small changes in this would matter (you may want to calculate the saline endmember uncertainty and propagate it?

For the mixing calculations, we used initially the salinity of meltwater (0 PSU) and the bottom water at the glacier front. However, we realize that the average salinity of the well-mixed water column at the ice edge reference site with a salinity of 34.7 is better suited for the calculations. We changed the salinity and added the information where the value comes from and what the standard deviation is. Since the value of 34.7 as the bottom water endmember is very stable, variations would lead to <1% changes in the estimate of the calculations.

We suggest following changes:

The fraction of fjord water vs subglacial meltwater for the water samples was calculated assuming linear mixing of the two water sources with different salinities (glacial meltwater salinity = 0 PSU, average seawater salinity at IE=34.7 ± 0.03 standard deviation), since no other water masses in regard to temperature or salinity signature were present (Table 1). The variability of the IE sea water salinity leads to a small (0.09%) uncertainty in the estimated value of the relative contributions of sea water vs subglacial meltwater.

197 What does 'net haul' refer to? -> Mentioned one paragraph above

We gave details about the phytoplankton net hauls in line 181, but changed the term net haul to "phytoplankton net" for clarity.

Suggested change:

For qualitative counting of algal communities, the phytoplankton net and bottom sea-ice samples

285 "where NOX (10 _mol L-1) and silicate (19 _mol L-1) levels were exceptionally high (Fig. 4)." Am I right that this is basically driven by one data point? If so, how do you explain such high NOx concentrations? Is it possible that this value is an anomaly? And what are the implications of this? From your profile, and from the range of the other samples I guess that this is an outlier for both NOx and Si, with the NOx harder to explain from environmental processes. If this is an outlier, I think the calculations throughout need amending or flagging to reflect this, noting that there is a large difference depending on whether or not this data point is included.

We agree that these exceptionally high values have to be treated carefully. We took great care during the nutrient analysis itself and the calibration of the auto-analyzer, and we have no indications on instrument caused errors in our data record. Local remineralization and dissolution of algae biomass at the sea ice-water interface may provide part of the explanation, but other anomalies cannot be excluded since the values are indeed driven by only one sample with no correspondance or obvious source in the values below or above. Thus, we did not use this value for any mixing calculations or estimates, but used instead the value 1 m under the sea ice for all further calculations. The 1 m value is more consistent with the measurements in the water column below and sea ice above. Thus the exceptionally high values had been considered as outliers and not used in our estimates.

We suggest following change:

where NO_X (10 µmol L^{-1}) and silicate (19 µmol L^{-1}) levels were exceptionally high (Fig. 4). As these values are driven by a single sample, we cannot exclude anomalies to be responsible for these high values. Wetherefor used the values measured 1 m under the sea ice for further calculations in this manuscript as surface water reference.

291 "N:P ratios were generally highest: : :" Somewhere it would be interesting to comment on what drives this trend? Is it a source of N, or a sink/dilution of P? If saline water inflow dominated the N and P supply, would you expect such strong shifts? I suspect you need some sort of local process leading to a net accumulation of N or loss of P to get these ratios (you do comment on this for NH4 briefly), and whilst there are no other spring studies I can think of looking at this, I think a few papers have commented on some not particularly well explained P loss in similar environments in summer 5,6.

We added suggest to add a more thorough discussion of the N:P ratios, including the suggested reference in the following way:

Ammonium regeneration and subsequent nitrification (Christman et al., 2011) may explain the exceptionally high nitrate concentration of the UIW at SG, which can be part of the explanation for the high N:P ratios. In fact, bacterial activity was higher at SG potentially allowing higher ammonium recycling. Another explanation for the high N:P ratios and low phosphate concentrations can be related to phosphate scavenging by iron as discussed by Cantoni et al. (2020).

300 (306) "Nutrient versus salinity profiles give indications of the endmembers (sources) of the nutrients (Fig. 5). A positive correlation for example would indicate conservative mixing (assuming high salinity Atlantic water endmember had higher concentrations than melt water). Biological uptake and remineralisation as well as physical processes, such as external inputs by meltwater could inverse or eliminate the correlation."
This isn't quite right and needs a bit more clarity, you will find a lot of literature on this in marine chemistry or in a good textbook. In simple terms, a linear correlation shows conservative mixing, the absence of a non-linear correlation suggests non-conservative processes (although there are some subtleties to this, some physical factors can also lead to non-linearity). The gradient, not the strength of the correlation, indicates whether fresh, or saline, endmembers have a higher concentration, i.e. an increasing nutrient concentration with salinity (positive gradient) suggests saline inflow has higher nutrient concentrations, whereas a decrease with salinity suggests (negative gradient) a higher freshwater concentration.

We suggest correcting the paragraph in the following way:

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 indicate non-conservative mixing pointing to the relevance of biological uptake and release measurements (Fig. 5d-f).

We also suggest correcting the figure legend of Fig. 5 (See below).

310 “The contribution of nutrients by upwelling as well as freshwater inflow from glacial meltwater was estimated by linear mixing calculations”. Can you show these, maybe in the supplement, I am a little confused mainly because of the unclear description above. Similarly for the % nutrient values, please clarify how these were calculated and consider the error on them – especially if it is the case that the single SG value with very high NOx and Si is basically dominating the trend and an outlier.

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 (NOX), 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}).

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

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

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

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

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

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

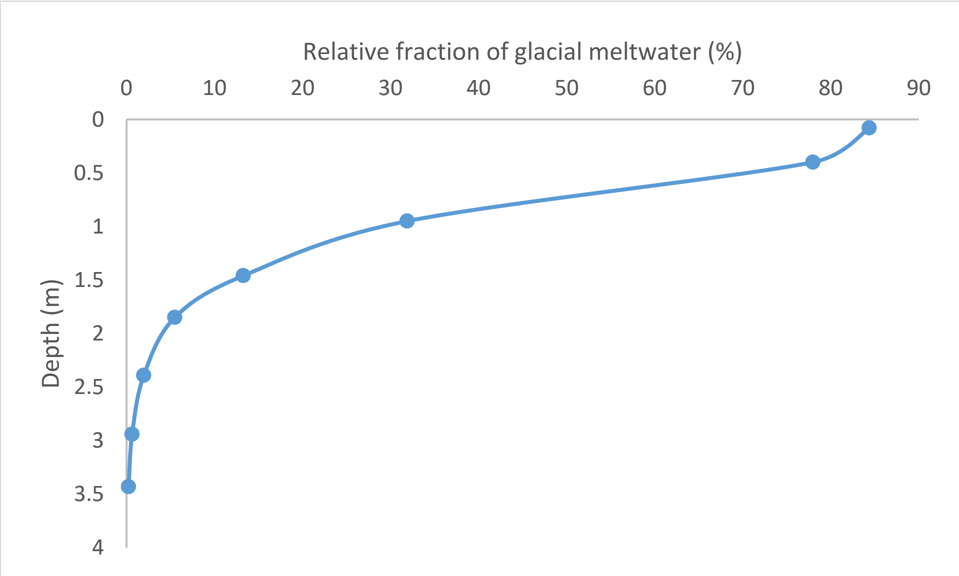

Figure R1. Estimated fractions of glacial meltwater in the surface layer of SG.

333 Can you clarify what you mean by the vertical export of Chl a and how this was calculated please

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).

The sediment traps are cylindrical bottles, filled with sterile filtered water, incubated at different depths for about 1 day. The material (e.g. Chl a) sedimenting out (vertical flux) is collected in these cylinders. Since we know the concentration of Chl a in the sediment trap (**C** in mg m^{-3}) and the volume of the cylinders (**V** in m3), we can calculate the mass of Chl a sedimenting into the trap (mg). With the knowledge of the area above the sediment trap opening (**A** = m^2) we can calculate the amount of Chl per area (mg m^{-2}). With the information of the exact incubation time (**t** in days), we can then calculate the vertical flux (mg m^{-2} d^{-1}). The calculation is described in Wiedmann et al. (2016), but we could also add the equation below to the supplement.

$$Vertical\ flux = \frac{C * V}{A * t}$$

462 These values are hard to compare as written because the first (250-500) refers to the vertical plume volume, whereas the second (1.1) refers to the volume transport over the fjord surface, with different units and spatial scales, it would be better to calculate a set of numbers with the same units for comparison.

We suggest adding our estimate in the same unit:

Compared to the massive subglacial plumes of summer systems (250-500 $m^3\ s^{-1}$, Carroll et al., 2016), subglacial upwelling in spring is a small volume transport with only about 1.1 $m^3\ m^{-2}\ month^{-1}$ (approx. 2 $m^3\ s^{-1}$) upwelling…

462 'careful' implies errors/uncertainties are quantified, at the moment I would say it was a little crude.

We agree and suggest following change:

This rough, but conservative estimate…

465-468 There are 2 sentences here basically saying the same thing

We suggest removing one of the sentences.

469 'depth of glacier front' it would be better to cite the physical studies which specifically show this rather than a review

We suggest citing Carroll et al., 2016 instead. Carroll et al. (2016) also reviews different studies, but for coming to a conclusion of the depth of the glacier front being related to the amount of upwelling, requires a review, or meta-analyses. Since we use the citation as evidence for this specific relationship, we suggest this review as most appropriate. The physical studies alone do not have sufficient data to come to this conclusion.

Carroll, D., Sutherland, D. A., Hudson, B., Moon, T., Catania, G. A., Shroyer, E. L., Nash, J. D., Bartholomaus, T. C., Felikson, D., Stearns, L. A., Noël, B. P. Y., and van den Broeke, M. R.: The impact of glacier geometry on meltwater plume structure and submarine melt in Greenland fjords, Geophys. Res. Lett., 43, 9739–9748, https://doi.org/10.1002/2016GL070170, 2016.

470 It would be useful to mention the grounding line estimate earlier in the text

We suggest adding the information already in the methods description.

486 required for photosynthesis or something more specific (primary production can occur in the dark)

We suggest adding following clarification:
…where light is not considered limiting for photosynthesis.

519 Yes I would agree, but I don't think the review cited explicitly shows this. You can however find a lot of work that suggests most of the Arctic is basically nitrate limited based on observed macronutrient distributions in summer7, and I think this has been explicitly tested closed to Svalbard showing no significant effect of Fe additions8.

We agree that the review is not the most appropriate reference and added the study by Krisch et al. (2020) instead.

520 I'm not sure what you mean by nutrient concentrations are higher at shallower depths, something due to relatively more evident benthic input as you mentioned for NH4 briefly?

We suggest that at a shallower water depth, less physical forcing not necessarily related to subglacial upwelling (e.g. tides (low in Adolfbukta), currents, or wind (unlikely under sea ice), is needed for vertical mixing leading bottom water to reach the surface.

We suggest following clarification:

Besides the subglacial upwelling, nutrient concentrations may be higher due to the shallower water depth at SG compared to IE, facilitating easier vertical mixing down to the bottom.

523 As above, I think this is not quite right. Non-conservative silicic acid behavior (but conservative N/P behavior) would suggest glacier associated input from dissolution of glacier-derived particles either directly into the water column or from sediments into the water column, although conservative silicic acid behavior is also equally often observed downstream of glaciers so this is not really a clear universal meltwater signature9. I think the only generalizations you could make would concern concentrations that melt is generally expected to be a low or not significant source of nitrate/phosphate, and a more important source of silicic acid, see Ref1 and the supplement for a summary.

We suggest following correction:

The differences in the relation of nutrient concentrations versus salinity indicate, that glacial meltwater was not a major source for N and P at SG while the different relation for Si provide evidence for supply through meltwater inflow (Hopwood et al., 2020).

530. I'm not sure these values are low compared to Greenland work if you compare to the dissolved values in freshwater. See the supplement for Ref for a summary1, I suspect if you calculate mean/median for data available for Greenland or Svalbard your values are likely not atypical (for silicic acid especially, I think the mean and range is high because 1 or 2 catchments have exceptionally high concentrations, but median concentrations are likely a few micromolar.) Note spelling Hawkings (I assume).

We are quite confident that the values are low, but would be thankful if the reviewer has any suggestions for references with lower Silicate values in glacial outflow water in Greenland. Overall the data for glacial outflow in Greenland are sparse. We do not think comparing Arctic rivers with our measurements of subglacial outflow would be useful, since additional processes, including additional weathering and uptake by freshwater diatoms would play a role. Overall, rivers have also higher Silicate values. The only samples with lower concentrations than our study are from icebergs (Meire et al., 2016a). The other values in the study by Meire et al. (2016a) are measured from glacial rivers, with the lowest value of 3.4 µmol L^{-1}, the lowest mean value of 5.5 µmol L^{-1} and typical mean values around 10 µmol L^{-1}. For clarity, we suggest adding the values of our study and the range of the values from Greenland.

The nutrient concentrations in subglacial outflow water were lower (<1.5 – 2 µmol L^{-1}) than estimates in Greenland (Hawkings et al., 2017: 0.8-41.4 average 9.6 µmol L^{-1} , Hatton et al., 2019: 9.2-56.9 average 20.8 µmol L^{-1}, Cape et al., 2019: 10 ± 8 µmol L^{-1}), indicating that direct fertilisation in spring may be even more important in other tidewater glacier influenced fjords.

533 This is curious, do you have any idea why? Based on the Ref10 cited, this source would be expected to be quite large (i.e. silicic acid entering solution from glacier mderived particles) compared to direct glacier inputs of dissolved silicic acid, but I'm not sure how much evidence there is for this, elsewhere around Svalbard I think the same summary can be made as herein that there doesn't seem to be strong evidence for a significant silicic acid source from glacier sediments6.

As indicated by rather low silicate concentrations in the subglacial outflow water, we suggest that the bedrock below Nordenskiøldbreen is overall poor in silicate, at least at the areas, where the subglacial drainage system is in contact with the bedrock. We suggest following change:

However, bottom water nutrient concentrations were similar at SG and IE, indicating a limited role of higher silicate inputs from sediment, presumably due to silicate-poor subglacial bedrock.

534 Besides: : : This is repetition, I don't think it's particularly controversial to assume NOx as the limiting nutrient in this environment11,12, I think a very brief comment about Fe would suffice, there is more than ample evidence for really high Fe inputs in and around Svalbard13,14 and low nitrate levels through most of the growing season.

We agree and suggest removing the sentence, since the information about iron is already given above.

539 Given the lack of relevance for atmospheric inputs to under-ice blooms, I don't think you need to discuss this, unless you are writing about incorporation of such nutrients into sea-ice

Since atmospheric inputs can be an important N source for sea ice algae, we suggest keeping the discussion. Especially at the SG station, we found high biomass of sea ice algae higher up in the ice, indicating that atmospheric inputs may play a role and need to be discussed.

549 I'm not sure which value in the cited review you're referring to here, it would be more useful to cite the specific studies that measured primary production (there are many studied on primary production for Svalbard including values specifically for spring which are presumably the best comparison)12,15,16.

The value is given in a table (Table 1 in Hopwood et al., 2020) and is based on many different studies (6 fjords, 33 datapoints), which makes citing one original research paper tricky. Discussing all studies separately would repeat the review effort of Hopwood et al. (2020) and go beyond the scope of our discussion. Thus, we suggest keeping the review article as main reference. We added however the range of PP in tidewater influenced fjords for clarification.

For a comparison of Kongsfjorden as a similar system on Svalbard, we also agree that adding more specific references to van de Poll et al. (2018) and Hodal et al. (2012) is helpful.

We suggest following changes:

The integrated primary production to 25 m at SG of 42.6 mg C m^{-2} d^{-1} is low compared to values from other marine terminating glacier influenced fjord systems in summer with mean integrated NPP of 480 ±403 mg C m^{-2} d^{-1} (reviewed by Hopwood et al., 2020), including studies in Kongsfjorden on Svalbard (250 -900 mg C m^{-2} d^{-1}, Van de Poll et al. 2018). Also studies in the same time (1 May) observed higher primary production rates in a marine-terminating glacier influenced fjord system, such as Kongsfjorden (1520-1850 mg C m^{-2} d^{-1}, Hodal et al., 2012).

570-575 As written this is fine, but please note I think the 'seed' hypothesis specifically referring to inner-fjord communities seeding outer-fjord/shelf areas is not particularly well supported by literature, especially since in the context of sea-ice covered fjords, I think the bloom generally occurs earlier outside the fjord than it does inside (I'm not sure if that is the case here). Elsewhere on seasonal timescales there is evidence of marine inflow changing the in-fjord bloom and not really of the opposite17,18.

Our main support is the paper by Hegseth et al. (2019), which describes microalgae derived from sediment upwelling/mixing in the fjord as crucial source of inoculum for a spring bloom. Especially in Billefjorden with little Atlantic water inflow due to the shallow sill, slow tidal currents, and a suspected net advection away from the glacier front, we expect this mechanism to also be important in Billefjorden. However, based on your literature, we realize that this hypothesis is not widely accepted and formulated the statement more carefully.

We suggest a more careful discussion in the following way:

…, may be a viable seed community for summer phytoplankton blooms, once the sea ice disappears and light levels increase (Hegseth et al., 2019).

585 These are averages you're referring to? It may be worth commenting on the variability, I expect there's a huge range when you're writing about all Arctic glacier-fjords

We agree and suggest giving the range instead of the average. We also add a reference citing the original study, instead of the review by Leu et al. (2015).

Only Greenland fjords (0.1-3.3 mg Chl m^{-2}) or pre- and post-bloom systems had comparably low biomass (Mikkelsen et al., 2008, Leu et al., 2015).

589 'limited by phosphate' do you actually show this, or do you mean than based on measured concentrations, there was a deficiency of phosphate?

We suggest changing the term "limited" to "deficient".

595 This appears very speculative, because I think you are comparing broad regional averages to a spot measurement?

We agree and realize that this discussion is not crucial for the paper and, thus, suggest removing it.

599 -8.3 above average doesn't make sense
We referred to 8.3 not -8.3 and corrected the error.

644 Here, in this section, I think you need to consider where sea-ice cover occurs and also how that and the timing of its breakup may also change in the future.
We agree that sea-ice cover and changes with climate change need to be discussed here and suggest following additions:

Another impact of climate change will be the reduction and earlier break-up of sea ice and Atlantification of fjords, leading to increased light and wind mixing. In the ice free Kongsfjorden, higher primary production rates have been measured in the same month, indicating that the lack of sea ice may lead to increased overall primary productivity at that time of the year (Iversen & Seuthe, 2010). However, Kongsfjorden is still influenced by subglacial upwelling, supplying nutrients to the bloom (Halbach et al., 2017). In systems not affected by subglacial upwelling the additional light through sea ice melt will most likely not lead to substantially higher primary production as indicated by lower measured rates in these type of fjords (Hopwood et al., 2020). Due to the shallow (20 m) grounding depth in our study site Billdefjorden the estimated fluxes and the nutrient entrainment factors are rather low. In an ice-covered fjord with limited wind and tidal mixing, we suggest subglacial upwelling as a major source for nutrients to the euphotic zone. However, if sea ice disappears, we hypothesize that wind induced mixing would be strong enough to exceed the role of subglacial upwelling. However, katabatic winds reaching the surface water could also increase subglacial upwelling as described by Halbach et al. (2017) and the overall effects are unclear. Direct silicate fertilization would likely have a limited effect in an ice-free fjord since the primary production in the fjord is nitrate and not silicate limited due to the later stage of the spring bloom (Hegseth et al., 2019). In summary, we suggest that subglacial upwelling in winter/spring is important for phytoplankton blooms, but only in a sea-ice covered fjord. The future of the winter/spring

650 "the seed material from the deeper sediments would not reach the water column, leading to a reduced and delayed phytoplankton summer bloom" Whilst I've read this hypothesis in a few places, I'm not sure there's much evidence for this, can you cite studies specifically showing this does affect the summer bloom?

Our main support is the paper by Hegseth et al. (2019), which describes microalgae derived from sediment upwelling/mixing in the fjord as crucial source of inoculum for a spring bloom. Especially in Billefjorden with little Atlantic water inflow due to the shallow sill, slow tidal currents, and a suspected net advection away from the glacier front, we expect this mechanism to also be important in Billefjorden. However, since the support lies in another study and not in our data, we suggest removing this sentence.

650-660 There are a lot of ideas in these paragraphs which are not extensively developed. I think it would be good to either develop these a bit more, or remove them. For the later comment, Holding et al., is probably the best ref I can think of – you also need to think about stratification19 if you want to write about changes in summertime, but I generally suggest you cut this given the spring focus of your manuscript. The writing concerning spring is much better developed and the comments concerning changes in summer bloom lack discussion of the many factors (changing discharge, stratification, circulation) that change seasonally and are generally beyond the scope of the manuscript.
In your comments about how significant/important this process is, maybe you could think about how it works with respect to the availability of nutrients and timing.
If I understood correctly, the entrainment occurs from only 20 m depth, so if it started slightly later in the season it would be presumably much less effective as nitrate would already have been drawdown and meltwater would just be mixing into an already nutrient deficient top 20 m layer? Presumably this means the relative timing of bloom onset, and early discharge is an important feature to think about in determining when/when this is important?
(And, also sea ice break up, the dates of which presumably are also changing?)

We suggest removing all references to summer and focus on spring changes and extend our discussion on sea-ice retreat, timing of the bloom and sea-ice retreat vs glacier retreat effects in the following way (See response to comment on line 644):

Another impact of climate change will be the reduction and earlier break-up of sea ice and Atlantification of fjords, leading to increased light and wind mixing. In the ice free Kongsfjorden, higher primary production rates have been measured in the same month, indicating that the lack of sea ice may lead to increased overall primary productivity at that time of the year (Iversen & Seuthe, 2010). However, Kongsfjorden is still influenced by subglacial upwelling, supplying nutrients to the bloom (Halbach et al., 2017). In systems not affected by subglacial upwelling the additional light through sea ice melt will most likely not lead to substantially higher primary production as indicated by lower measured rates in these type of fjords (Hopwood et al., 2020). Due to the shallow (20 m) grounding depth in our study site Billdefjorden the estimated fluxes and the nutrient entrainment factors are rather low. In an ice-covered fjord with limited wind and tidal mixing, we suggest subglacial upwelling as a major source for nutrients to the euphotic zone. However, if sea ice disappears, we hypothesize that wind induced mixing would be strong enough to exceed the role of subglacial upwelling. However, katabatic winds reaching the surface water could also increase subglacial upwelling as described by Halbach et al. (2017) and the overall effects are unclear. Direct silicate fertilization would likely have a limited effect in an ice-free fjord since the primary production in the fjord is nitrate and not silicate limited due to the later stage of the spring bloom (Hegseth et al., 2019). In summary, we suggest that subglacial upwelling in winter/spring is important for phytoplankton blooms, but only in a sea-ice covered fjord. The future of the winter/spring

Data files: These are generally well organized but I could not find the nutrient data in the file which the readme says it is in, did I miss something? -> I will double check after PhD submission(The same for finishing the ENA submission)

We added the missing data to the DATAVERSE archive.

Fig. 3 The blue line doesn't quite display properly in my version

We will upload a figure with higher quality. For the final paper, we will submit vector based PDF files for each figure.

Fig. 4 There are a couple of suspect anomalies here, along the line that represents the ice boundary there are a few nutrient concentrations that appear well above the trend for either ice or water column concentrations, are you sure these are real? -> mentioned the outliers in the results

As mentioned above, these values are based on 1 sample (UIW at SG for NOX and Silicate) and may well be outliers or anomalies based on sampling artifacts, or locally high remineralization/dissolution rates. Thus, we highlight them as outliers in the text and do not use them for the mixing calculations, or detailed discussions.

Fig. 5 As in text, the description of 'conservative mixing' isn't quite right. "Conservative mixing shows as a positive correlation, non-conservative mixing as a negative correlation". The strength of the correlation indicates roughly how conservative it is. The sign of the gradient indicates whether the concentrations are increasing or decreasing with salinity i.e. whether freshwater or saline water has the higher concentration. It would be useful to have the actual p values written somewhere.

As mentioned above, we agree and changed the text in the manuscript and figure legend accordingly.

Suggested change for figure legend:

Fig 5. Linear salinity-nutrient correlations of NG and IE water samples (a–c), NG, IE, and SG water stations (d–f) and sea ice samples of NG, IE and SG (g–i). A higher concentration in saline Atlantic water results in a positive correlation, a higher concentration in glacial meltwater in a negative correlation. Significant correlations ($p<0.05$) are asterisk marked behind the R^2 value.

Fig. 6 This took a while to read, there are a lot of abbreviations.

Due to the large amount of data in this figure, we argue that the amount of text, containing information and assumptions in the methods are necessary. We suggest writing out the abbreviations on top, to make the figure more understandable without reading the legend.

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