Below, we address comments from RC2. We cluster each comment and separate them as “1” Reviewer comments, “2,3” are responses and revisions. A pdf version (bg-2018-226-RC2-supplement_Krause_etal_response) of this response has been uploaded in the supplement.

Review on Biogenic silica production and diatom dynamics in the Svalbard region during spring by Krause et al.

1) Krause et al. investigated phytoplankton, especially diatoms, and nutrients at 9 stations in the Atlantic sector north of 76^{o}N. They measured silicate, nitrate plus nitrite, chlorophyll a, biogenic silica, determined di- atom assemblage, estimate productivity and export (based on sediment traps). The silicic acid concentration in the upper 50 m was always below 5 μmol L^{-1} and at most stations below the nitrate plus nitrite concentration. At several stations [$Si(OH)_4$] was below 1 or even 0.5 μmol L^{-1} in the upper 20 m. In order to investigate Si uptake limitation, the authors performed on board growth experiments over a range of [$Si(OH)_4$] at 4 stations. Michaelis-Menten functions for silicic acid uptake (Eq.1) were fit to the data yielding estimates for maximum uptake rates (V_{max}) and half- saturation constants (K_S). Let me suggest listing these estimates in a table. The estimates for K_S are much higher than some estimates (for different diatom species) reported in the literature (for example, Paasche, 1973a,b), however, lower than the higher values given by Kristiansen et al. (2000). What might explain this large range and these differences? Could it be influenced by factors (other nutrients, grazing) differing between the various investigations/experiments?
2,3) We speculate that diversity and diatom origin (e.g. more Atlantic influenced waters, perhaps residual ice diatoms) may be some of the underlying factors. However, these are (unfortunately) beyond the scope of our data. What is important, at least for a modeling perspective, is that these kinetic parameters are published and available to ground truth regional simulations.

1) The manuscript contains valuable new data, is well written, and will be of interest to many readers of Biogeosciences. I recommend publication after minor revisions.
2,3) We thank the reviewer for this assessment and reply to the revisions below.

Further remarks/suggestions:
1) L123-129 Description of trap deployment was not very detailed (’at 3-7 depths between 20 and 150-200 m, based on bathymetry’). It would be good to add a list

with depths and bottom depths (or a reference where to find this information).
2,3) This has been added to the prose. This ranged from shallow (~60 m, van Mijenfjorden) to deep (260-290m in Erik Erikssenstretet and Atlantic stations, respectively).

1) L132-134 Freezing sample for nutrient analysis: Procedures (thawing, measurements how long after thawing) and quality control (freeze certified reference material in parallel with samples) have been discussed in the literature (for example, Macdonald et al., 1986, Clementson & Wayte, 1992, Dore et al., 1996). Could you please give more details on the procedures and quality control?
2,3) Co-Author Kristiansen's laboratory has extensive experience in these analyses. Pertinent details have been added, including reference seawater from Ocean Scientific International Ltd. (UK, Line 135), and detection limits (Line 136) are available in the initial submission. Standard practices (slow thawing of silicic acid samples to allow depolymerization, three parallels measured, etc.), have been added along with prose regarding the analytical reproducibility (median coefficient of variation was 5% for NO3+NO2 and PO4, 2% for silicic acid and 9% for NO2 → higher coefficient of variation was observed when the absolute concentrations were low, e.g. <0.1 uM, hence using the median value). Because of the cruise duration and transfers, and the well-known issues of getting reliable measurements from frozen samples, no ammonium was measured.

1) L136-138 "Phosphate was analyzed, but N:P ratios for nutrients were, on average, 8 among all stations, suggesting that N was likely more important than P for primary production." N:P is below Redfield thus N might be limiting primary production before P. However, 'N was likely more important than P for primary production' sounds strange. Please rewrite.
2,3) This has been modified. "Phosphate was analyzed, but N:P ratios for nutrients were, on average, 8 among all stations; this suggests N was more likely than P to limit primary production."

1) L145,148,225 60 ^{o} C → 60^{o}C, -20 ^{o}C → -20^{o}C, -2-1 ^{o}C → -2 to +1^{o}C (no gaps; please check whole manuscript)
2,3) Gaps have been removed through the whole manuscript.

1) L175 please rewrite "dividing by the depth integration" → dividing by depth-integrated values
2,3) This has been modified as suggested.

1) L199 ml → mL
2,3) This has been modified.

1) L205-206 using C:Si (instead of Si:C) would avoid the exponent -1 in Eq.(2) and give

values more in Redfield-style, i.e. molar Si:C = 0.13 → 7.7 C:Si (only slightly higher than the Redfield C:N). What's the uncertainty of the Si:C estimate?
2,3) While C:Si and Si:C can both be used, we chose the Si:C based on convention established by other publications when making these types of estimates (e.g. Nelson et al. 1995 GBC, Nelson and Brzezinski 1997 L&O, Leynaert et al. 2001 DSR I, Brzezinski et al. 2011 DSR I, Krause et al. 2011 DSR II, Krause et al. 2015 JGR Oceans). Regarding uncertainty, please see response to reviewer 1. This uncertainty in Si:C, even if a factor of two, does not change the two main interpretations (1, diatoms "bloom and bust," 2, even when diatom biomass is relatively low their contribution to primary production is quantitatively important).

1) L295-296 "The rate of diatom biogenic silica production was reduced by ambient $[Si(OH)_4]$ in 95% of the samples examined." sounds strange. I guess you mean 'was kinetically limited by ambient $[Si(OH)_4]$' based on comparison with estimated K_S values or based on enhancement factors.
2,3) This has been modified.

1) L317,548 Spearman's Rho Test: add number of data n = ...
2,3) This has been added (n = 15).

1) L380-384 What about grazing?
2,3) Grazing would affect the standing stock of diatom biomass (and thus the absolute rate of production, Rho), but not the specific rates (e.g. V_{AVE}) which are more likely driven by growth/bottom up factors. However, in this region, grazing is likely the primary mechanism which transforms living diatom silica into detrital silica. Because the latter is a minor and speculative point given the data, we feel adding a complicated explanation about grazing here would stymie the narrative flow without adding enough clarity.

References

[1] Clementson, Lesley A. and Wayte, Sally E. The effect of frozen stor- age of open-ocean seawater samples on the concentration of dissolved phosphate and nitrate. *Water Research*, 26(9):1171–1176, 1992.

[2] Dore, J.E., T. Houlihan, D.V. Hebel, G. Tien, L. Tupas, and D.M. Karl. Freezing as a method of sample preservation for the analysis of dis- solved inorganic nutrients in seawater. *Marine Chemistry*, 53(3-4):173– 185, 1996.

[3] Macdonald, RW and McLaughlin, FA and Wong, CS. The storage of reactive silicate samples by freezing. *Limnology and Oceanography*, 31(5):1139–1142, 1986.

[4] Paasche, E. Silicon and the ecology of marine plankton diatoms. I. *Tha- lassiosira pseudonana* (*Cyclotella nana*) grown in a chemostat with silicate as limiting nutrient. *Marine biology*, 19(2):117–126, 1973a.

[5] Paasche, E. Silicon and the ecology of marine plankton diatoms. II. Silicate-uptake kinetics in five diatom species. *Marine Biology*, 19(3):262–269, 1973b.