

The manuscript of Krause *et al.* presents interesting results concerning biogenic silica production and export levels as well as estimates of kinetic constants from an opportunistic sampling near the Svalbard Archipelago in the Arctic Ocean by late spring. The data presented are the first direct (silicon-32 method) measurements of biogenic silica production in the Arctic Ocean, which in themselves deserve publication. From these data, the authors then attempt to establish the potential control of diatom production by the availability of silicic acid as well as the contribution of diatoms to total primary production. However, I think the authors are pushing their limited data set much too far and that the manuscript should be shortened by getting more concise.

- Firstly, I find it difficult to understand why the authors focus on the so-called Egge & Aksnes 2  $\mu\text{M}$   $\text{H}_4\text{SiO}_4$  threshold value, as it is clear that the data from this publication have been wrongly interpreted in several past publications (which is recognized by the authors besides). I suggest just using their kinetic values to discuss the potential limitation of diatom uptake by  $\text{H}_4\text{SiO}_4$  availability, and then shortly discuss hypotheses for growth limitation, which is another point not directly assessed in this study. ON the other hand, authors might consider that the actual limitation starts under 2 times  $K_s$ . The comparison between nitrate and silicic acid concentrations is not very clear. First, they mention a 2.5 slope (Figure 5) between nitrate and silicic acid, which means that nitrate is taken up 2.5 times faster than silicic acid, and then authors take a 1:1 ratio to discuss the potential for silicic acid limitation. I can imagine that they are trying to decipher the relative contribution of siliceous vs. non-siliceous (e.g. *Phaeocystis*) components of the phytoplankton community but this should be clearly indicated. On top of that, the use of a 1:1 Si:N ratio is questionable (large species-specific variations, see below for Si:C).
- The contribution of diatoms to primary production is also another weak point of the ms. The calculation is based on a transformation of  $\rho\text{Si}$  to  $\rho\text{C}$  by using the average Brzezinski' ratio of 0.13. This is a very simplistic way of addressing this important question as this ratio is known to be subject to large species-specific variations (e.g. Brzezinski gives a Si:C biomass range of  $0.15 \pm 0.04$  for large diatoms, which could result in a  $\sim 2$  times range for  $\rho\text{C}$  estimates). This is somehow risky business and should, at least, be acknowledged and discussed.
- Finally, the authors present data for direct diatom cell export but the underlying issue is not clearly stated: Do they want to compare direct diatom sedimentation by mass sinking to other export vectors such as repackaging? If yes this should be clearly stressed.

line 28 : "diatom cellular export" – the wording is misleading (could be export from a diatom cell). I'd rather use "export of diatom cells".

line 65 : " A more recent analysis demonstrated a decline in pre-bloom  $[\text{Si}(\text{OH})_4]$  concentrations by 1–2  $\mu\text{M}$  across the north Atlantic subpolar and polar regions over the last 25 years (Hátún *et al.*, 2017); this is consistent with the general Arctic region being a net exporter of silicic acid (Torres-Valdés *et al.*, 2013)." – I don't see the consistence between the decrease of  $\text{H}_4\text{SiO}_4$  concentrations and the net exportation of this nutrient; please rephrase.

line 68 : " This is in stark contrast to the 10–60  $\mu\text{M}$   $[\text{Si}(\text{OH})_4]$  observed in the surface waters of the Southern Ocean and the marginal ice zone around Antarctica (Nelson and Gordon, 1982; Brzezinski *et al.*, 2001), where  $[\text{Si}(\text{OH})_4]$  is unlikely to limit the rate of diatom production or biomass yield." – I disagree; There are ample references to state that actually the reverse is true, due to sometimes unusual high  $K_s$  (e.g. Nelson & Tréguer MEPS 1992, Nelson *et al.* DSR II 2001, Mosseri *et al.* DSR II 2008).

line 76 : " ... and a 2  $\mu\text{M}$  threshold  $[\text{Si}(\text{OH})_4]$  defines where diatoms are outcompeted by flagellates (Egge and Aksnes, 1992)." – I strongly disagree with that sentence. The work

of Egge and Aksnes did not evidence any real threshold (no kinetic values measured) and just merely indicated areas of realized niches for diatom vs. flagellates with regards to Si vs. P availability. Please do not cite this reference in such a way that was even not addressed by the authors of this paper. + as indicated above.

- line 137 : "... suggesting that N was likely more important than P for primary production." – As authors refer to absolute concentrations, the correct phrasing should be: "... suggesting that N was likely more important than P for potentially limiting primary production."
- line 138 : "These phosphate data are not discussed." – Even though a range would be welcome.
- line 153 : "... fixed with an aldehyde mixture of hexamethylenetetramine-buffered formaldehyde and glutaraldehyde at 0.1 and 1% final concentration, respectively, as suggested by Tsuji and Yanagita (1981) ..." – although this should be OK this is not the usual fixative for diatoms (acidic Lugol preferred), partly due to its toxicity for the microscopical examiner.
- line 164 : "... neutral density screened bags ..." – please mention the photometric levels used.
- line 207 : "Export rates were calculated using the standing stock measurements, length of deployment, and trap opening area." – Please give the model/type of sediment trap.
- line 260 : "... except for the Hinlopen ice algae, where the melt water ..." – Is that naturally-melted ice or meltwater produced by ice melting in the lab? Please clarify.
- Line 331 : "Brown *et al.*, 2003 " – Comment: For some strange reason L. Brown's incubations lasted for only 6 hours, which renders her production results questionable.
- line 339 : "... Varela *et al.* (2013) recently reported that [Si(OH)<sub>4</sub>] in surface waters (>5 μM) are unlikely to be significantly limiting to diatoms in any sector of the Bering, Chukchi or Beaufort Sea regions." – Although for Subarctic waters Brown *et al.* (2003, mentioned just above) kinetic experiments show a strong limitation (non-saturating kinetics) up to 30 μM.
- line 387 : " Suboptimal silicon availability affects the rate of diatom bSiO<sub>2</sub> production and can limit their growth. A widely cited [Si(OH)<sub>4</sub>] threshold, below which diatoms will be outcompeted by other phytoplankton, is ~2.0 μM; this metric was derived from a comparison of diatom abundance (relative to total microplankton) versus [Si(OH)<sub>4</sub>] during mesocosm experiments in a Norwegian fjord system (Egge and Aksnes, 1992)." – should be removed: No need to discuss this threshold as it is mentioned that it is strongly criticized (and see my comment above).
- line 410 : " This indeed indicates that phytoplankton can deplete nitrogen to levels below detection while they appear unable to deplete Si(OH)<sub>4</sub> pools below 0.5 μM, which would indicate 0.5 μM is the ultimate Si(OH)<sub>4</sub> concentration required to support diatom growth." – I disagree with this interpretation. The 0.5 μM Si level just reflects the residual H<sub>4</sub>SiO<sub>4</sub> stock after complete removal of nitrate.
- line 422 : "... if diatoms are limited by an absolute [Si(OH)<sub>4</sub>] (e.g. 2 μM), ..." – This is speculative: By what evidence is this proposition supported?
- line 436 : "... the relationship between V<sub>b</sub> and [Si(OH)<sub>4</sub>] also supports that Si regulates diatom productivity to some degree." – The large dispersion of data points on Figure 5 results in a very weak relationship, so that there is certainly something else explaining the low realized V<sub>b</sub> at the 4.5 μM H<sub>4</sub>SiO<sub>4</sub> level.

- line 443 : " ... Allen *et al.* (2005) observed a linear response in  $V_b$  between ambient and  $5 \mu\text{M}$   $[\text{Si}(\text{OH})_4]$ , which suggests uptake did not show any degree of saturation at this concentration." – Also in Brown *et al.* (2003); as mentioned above.
- line 517 : " At van Mijenfjorden, the rate of export in the upper 40 m represented 39% of the  $\int b\text{SiO}_2$  standing stock ( $23.3 \text{ mmol Si m}^{-2}$ ) in the same vertical layer." – I don't understand as from Table 1 it seems that the standing stock is 10.8 and the export 9.03?
- line 524 : " The rate of  $b\text{SiO}_2$  export was also at least a factor of four higher than  $\int \rho$  in the upper 20 m." – I was not able to find where did this come from.