

Biogeosciences Discuss., referee comment RC2
<https://doi.org/10.5194/bg-2022-51-RC2>, 2022
© Author(s) 2022. This work is distributed under
the Creative Commons Attribution 4.0 License.

Comment on bg-2022-51

Anonymous Referee #2

Referee comment on "Benthic silicon cycling in the Arctic Barents Sea: a reaction–transport model study" by James P. J. Ward et al., Biogeosciences Discuss., <https://doi.org/10.5194/bg-2022-51-RC2>, 2022

The manuscript by Ward et al. use previously published geochemical data (dissolved Si, $\delta^{30}\text{Si}$; Ward et al. 2022) of sediment pore waters and solid phases from 3 stations in the Barents Sea in an attempt to better understand the reaction pathways controlling the cycling of Si at the seafloor, (e.g. the dissolution of LSi and bSi, coupling of Fe and Si cycles, and the potential precipitation of AuSi). They employ a reaction-transport model (steady state and transient) to try and understand the pathways controlling the biogeochemical cycling of Si. The paper is mostly very well written, with some very interesting hypotheses (e.g. the possibility of being able to detect authigenic silica as a sink of silicon). However, the logic of the paper can be sometimes be difficult to follow, from the reader's perspective. There are some sections that are a bit confusing, and I have tried to provide some constructive feedback on the order of presenting the information (see below). My review includes a mixture of minor and major comments following the order of the text and sections of the manuscript.

Line 16 to 17– What do you mean by the phrase “taken-up” in this sentence? Are you suggesting that 2.9-37% of the released DSi has a value of -2 ‰?

Lines 144– The assumed value of 50 mm for AuSi_{sol} is not from Ward et al. it comes from Lermann et al. 1975, I believe.

Figure 2. It would be useful if the authors could indicate that the steady state model simulations were from what is proposed in this paper.

Section 2.1.4 – Transient reaction-transport modelling

I had a hard time following this section. Would it be possible to include a table or figure that could help the reader to understand the values associated with the input and outputs for this part of the model?

For example, **lines 219 to 223** – present an additional, more reactive BSi phase – but no data is provided. What does this look like in numbers? Also, it is mentioned **on line 225** that the conditions are either 1 or 3 weeks but that the deposition flux was -8 to 26-fold whereas in the figure 4, which is presented in the text before figure 3, The fluxes appear to be 10, 20, 30 and 26-fold. Also, it is not clear why the figure presents 3 sub-figures (bloom, 1.5 months, 3 months) based on the detail provided in section 2.1.4. I had to read section 3.2 at the same times section 2.14. Please add more information to these sections and to the title for Figure 3.

Section 2.2

Lines 249 to 260 appear to be disconnected from this section. It should be removed and/or perhaps placed in the discussion section. Also, I wasn't aware that a fractionation factor of AuSi formation had been established nor that most people assumed that silicon isotope fractionation did not occur during dissolution. These are rather controversial points that should be presented more carefully.

▪ **Results and Discussion**

The authors provide a great deal of information in this section and it would be useful to have an introductory paragraph before section 3.1 to give a brief outline of what is to be expected as points of discussion. It is quite easy to get lost in the details provided. For example, an introductory paragraph could summarize the principle hypotheses that are to be discussed. Along these lines, I am not quite sure why the authors did not choose to present the transient dynamics by phytoplankton blooms in section 3.1. I understand that this is not at steady-state, but it might be worthwhile to mention the possibility that the system is not at steady state. For example, on **lines 334-337**, the authors talk about dissolution dynamics and the lack of BSi in the Barents Sea. They could mention, in this section, that it is possible that the reaction-transport model is limited since it only operates under steady-state conditions, and then mention that they will discuss this further in section **3.2**. Again, along these lines, it might be worthwhile to include in the title of section 3.2 that this still implies the use of the reaction-transport model...

Regarding sections 3.1 and 3.2, I remain unconvinced by some of the arguments presented by the authors that the isotopically heavier signal (from 0.5 to 2.5 cm) is solely coming from LSi. I am not saying that it is not a plausible argument, but I wonder why the authors did not propose that benthic diatom activity could also be an explanation. Benthic diatoms have been found (alive) at incredible depths in the Barents Sea (Druzhkova et al. 2018), and they may be causing this observed shift in the top few cm of the sediment. At the very least it should be mentioned why this was not considered as a possibility. For example, the authors could make an argument after conducting a mass-balance calculation to show whether it is (or is not) possible.

Ideally, it seems as though it would be helpful to conduct empirical assessments (batch or open conditions) of the dissolution of sediments to test whether the hypotheses are plausible for the dissolution of LSi, BSi, and the links between the Fe and Si cycles over time. Since very little work has been done really evaluating these aspects, it would be very useful for the authors to suggest the need for more empirical studies in order to support (or not) the model results from this work.

Figure 3. I do not see the interest in showing a model of the BSi content, in particular since it is based on only 3 data points per station.

Lines-434 to 435: why does your simulation data ($k_{\text{dissbloom}}$) have higher values than the published data, even higher than the dissolution rate constant of diatom at warm temperature (14-22 degrees)? The reactivity of fresh diatoms varies due to temperature: high reactivity of diatoms has been observed at a higher water temperature region, whereas low reactivity of diatom material was observed in cold water, and the differences can be more than 10-fold (Ragueneau et al., 2000). Therefore, the reactivity of diatom bSi in your modeling of the Arctic area (<2 degrees) might be much lower than what was used in the model.

Line-457: study carried out by Moriceau et al., 2009 reported at least 2 types of bSi, this reference may also be relevant to your study.

Line 525: please use the updated values for burial efficiency provided by Tréguer et al. (2021)

Please also note the supplement to this comment:

<https://bg.copernicus.org/preprints/bg-2022-51/bg-2022-51-RC2-supplement.pdf>