

Comment on bg-2022-51

Anonymous Referee #1

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-RC1>, 2022

The manuscript by Ward et al. reports on the benthic–pelagic coupling in the Barents Sea with special emphasis on the Si cycle. The authors identified and described the biogeochemical reactions using silicic acid concentrations and Si isotopes and evaluated the reactions by reaction–transport modelling. The authors identified the dissolution of biogenic and lithogenic silica and silicon attached to dissolving iron phases as the major Si sources and authigenic clay precipitation as major sink. Also, the rapid dynamics and adjustment of the reactivity of the different sedimentary phases with respect to changing BSi supply and blooms are discussed and highlighted. Finally, the importance of benthic Si cycling for the Arctic Ocean Si budget is emphasized.

The manuscript is very well written and nicely discusses the main findings of this study. However, during reading the manuscript, I felt an increasing frustration with the many references to the other manuscript of Ward et al., which is currently under review in GCA. I appreciate that the authors provided a link to the preprint, but given that the method section (especially the sequential extraction and Si isotope measurements) and some parts of the interpretation and hypotheses are still under review leaves me with some concerns. In general, I am highly confident that the applied methods are correct and tested thoroughly, but I would only support a publication of this manuscript after the full review process and acceptance of the related GCA-manuscript. Apart from that, I am recommending this manuscript for publication with moderate revisions (see below).

Introduction: I would start with a general introduction of the importance of the benthic silicon cycling as you have done in lines 69–78.

I guess, a reference to Fig. 2 is missing in the introduction! It is mentioned first time in line 449.

Fig. 2: It is not clear at this point of the manuscript whether the reactions described in the red box are assumptions or data interpretation. Only later on in the text it becomes clear that these are modelling results.

Line 144: Instead of Ward et al., I would cite here the references you mention in the Table S2 (Lermann et al., 1975; Hurd, 1973).

Line 333-337: In this study, you discovered that some assumptions you made in your other study, which is also still under review, are not valid anymore. I would strongly recommend to use the possibility of changing the interpretation in your GCA manuscript, if you already know it is incorrect (concerning the AuSi precipitation in the upper 0.5cm)!

Line 183: definition missing for RMSE

Line: 250ff: for marine systems, no fractionation factor of authigenic clay formation is yet thoroughly established. The phrasing like it is sounds misleading. The studies you are referring to are either land-based, riverine or experimental. I agree that the size of the fractionation factor is likely correct, however, I would formulate this more carefully. Ehlert et al. (2016, GCA) modelled a fractionation factor of -2‰ for marine authigenic clay formation, which was also found in Geilert et al. (2020, Biogeosciences), but it can reach up to -3‰ in deep-sea settings (Geilert et al., 2020, Nat. Comm.), likely depending on pore water properties (pH, temperature, salinity, saturation states). This high fractionation factor would also agree with the repetitive number of dissolution-reprecipitation cycles discussed in Opfergelt & Delmelle (2012).

Lines 275-340: it would significantly help, if you would refer to the model lines (colour, dashed, ...) shown in Fig. 3, when discussing the data. Like this, it is really difficult to connect the text with the various model results. Please also indicate in the legend in Fig. 3, what conditions cause the 'best fit'.

Line 321: Considering the solubility of clays, can they really dissolve here? The dissolution rates of clays are much lower in seawater compared to primary minerals like feldspars or basaltic glass (see e.g. Jeandel & Oelkers, 2015). Would it be possible that during your sequential leaching procedure you dissolved some of the authigenic clays here as well, shifting the bulk LSi phase $\delta^{30}\text{Si}$ to lower values?

Lines 389-395: I wonder, if the model simulation gives a dissolving phase of -1 to -1.5‰, why not consider a higher contribution of lithogenic silica in this depth, which is much closer to the modelled value (about -0.9‰) than the FeSi phase (-about 2.9‰)? Do you really need a FeSi phase here to reproduce the pore water variability? I also wonder, if it is mass balance wise feasible? How much Si needs to be attached to this Fe-phase to create such a distinct peak in pore fluid $\delta^{30}\text{Si}$? And why is it then not seen in DSi?

Section 3.2: Also here it would be easier to follow your arguments if you would refer to the colour coding of the model results in Fig. 4.

Figure 4: Why are the different scenarios in 'bloom initiated' only modelled for the x30, 15ye-1, 1wk scenario? Why not for the different multipliers, duration? Do you assume the bloom lasted only for one week as mentioned in line 426? In this case, I would add a comment in the caption as well.

Line 417: Which 'certain conditions' do you mean here?

Line 426: This combination of parameters does not exist in the legend in Fig. 4

Line 525: The total ocean average BSi burial efficiency was revised in Tréguer et al., 2021 (Biogeosciences). The authors found a much higher burial efficiency compared to the findings of Tréguer & De La Rocha, 2013. How is that higher burial efficiency impacting your data interpretation?