

Interactive comment on “Reviews and syntheses: The biogeochemical cycle of silicon in the modern ocean” by Paul J. Tréguer et al.

Anonymous Referee #2

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Review comment on “Reviews and syntheses: The biogeochemical cycle of silicon in the modern ocean”

The manuscript by Treguer et al., revisiting the global biogeochemical cycle of silicon since the last review in 2013 is a good contribution to the literature. The paper presents new estimation of the fluxes which will have a major impact on global model and also presents the limitations in the field to have a better understanding of the silica cycle. However, as a review paper I feel that the manuscript is missing the very recent papers on benthic fluxes and that the section on the subject should be revisited. Otherwise, the manuscript seems to cover most of recent work.

Specific comments:

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L74 – 86: Here you present the different aspect/section/points of the paper, why not underline the benthic fluxes as they are emerging studies since the last review Treguer & De La Rocha 2013?

L91: One standard deviation or two standard deviation? Could you precise to avoid any mistake when people use the numbers in the future.

L108-129: This paragraph is a bit confusing. Fw is meant to represent benthic fluxes but a numerous of studies have not been mentioned here. The paragraph seems to focus on a flux representing the dissolution of lithogenic material deposited via river inputs. This input is of importance but do not represent all benthic Si fluxes. This section can be extended and also subdivided with the different type of benthic fluxes like mineral dissolution, benthic fluxes due to early diagenesis of biogenic opal (e.g. Ehlert et al., 2016 Stable silicon isotope signatures of marine pore waters – Biogenic opal dissolution versus authigenic clay mineral formation; Marz et al., 2015 Silica diagenesis and benthic fluxes in the Arctic Ocean, Ng et al., 2019 Sediment efflux of silicon on the Greenland margin and implication for the marine silicon cycle.

Furthermore, it might be of importance to mention the difference in flux magnitudes between abyssal plain and continental shelves for example.

L163 – 179: Recent study could be added to this section such as:

Hirst C, Opfergelt S, Gaspard F, Hendry KR, Hatton JE, Welch S, McKnight DM and Berry Lyons W (2020) Silicon Isotopes Reveal a Non-glacial Source of Silicon to Crescent Stream, McMurdo Dry Valleys, Antarctica. *Front. Earth Sci.* 8:229. doi: 10.3389/feart.2020.00229

Hatton JE, Hendry KR, Hirst C, Opfergelt S, Henkel S, Silva-Busso A, Welch SA, Wadham JL, Lyons WB, Bagshaw E, Staubwasser M and McKnight DM (2020) Silicon Isotopic Composition of Dry and Wet-Based Glaciers in Antarctica. *Front. Earth Sci.* 8:286. doi: 10.3389/feart.2020.00286

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L177: Are the benthic fluxes from Ng et al., 2020 included in the (sub)polar glaciers estimation? If that's the case I think they should be removed and added to the benthic fluxes in the previous section. The citation reference of Hendry et al., 2019 is missing in the References section

L186: what do you mean by focused?

L197: do you have a reference list for these 100 discrete vent fluid data? If yes could you added in the text.

L234: "Si anomaly of 0.07 ... North Pond (S18)" could you add a reference here?

L290-293: I agree that if a sponge lived without disturbance the bSi accumulation through biosilicification is a long process and it is likely that it is long compared to the deposition to the sediment but do we really know how long it take for the spicules to be deposited and then buried within the sediment and being considered as bSi accumulated? The supplementary material does not add more details neither reference for the rapid process lasting days to months.

L321: you could add Pickering et al., 2020, Geilert et al., 2020 to the reference list. Pickering, R. A., Cassarino, L., Hendry, K. R., Wang, X. L., Maiti, K., & Krause, J. W. (2020). Using stable isotopes to disentangle marine sedimentary signals in reactive silicon pools. *Geophysical Research Letters*, 47, e2020GL087877. <https://doi.org/10.1029/2020GL087877>

Geilert et al., 2020: *Biogeosciences*, 17, 1745–1763, 2020 <https://doi.org/10.5194/bg-17-1745-2020>

L345-358: This section is sightly confusing if I understand well the ocean basins are composed of a number of provinces and the domain is the provinces subdivided as coast, SO and open domain. Could you rephrase saying something like "for the domain estimate, each province was categorised either as a coast or an open domain. Only the SO is defined as a whole domain."

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Table 2 and section 4.1: To compare with the pelagic production, did you use the same parameter values (within 300km of shore) to estimate the coastal domain? It is actually very interesting to see that the production at the coast is almost half of the open ocean for the model estimation, obviously the surface area is much bigger for the open ocean. Do you think that the chlorophyll level here can be biased by other organisms for the open ocean area or maybe the lower resolution in the coastal area as many models do not cover the coast as well as the open ocean?

There is some disagreement between the main text and SI: L408 in main text: “these models are likely to overestimate the role that diatoms play, especially in the SO” L146 in SI: “potential biases in SO chlorophyll concentration (and consequently, NPP), which may be underestimated in the Southern Ocean”. As said in the SI the data disparity is a major problem in global estimates, it might be worth putting it in the main text at the end of section 4.1.3.

L451-457: why not replicate the same method used for the pelagic production, i.e. separating the coastal area and the abyssal plain. Abyssal plains are more likely going to represent the long-term equilibrium state than the coastal areas where sponges are sensitive to currents, particle/sediment accumulation, animals, seabed destruction etc.

L482: Could you define the number, something like $\text{FB}/\text{Gross bSi pelagic prod} = (7.0/255) = 2.8\%$

L496-L501: This sentence is very long, could you break it?

L511-513: What about changes in phytoplankton population due to climate changes. For example, along the West Antarctic Peninsula, phytoplankton communities are quite sensitive to the ice coverage, with the haptophyte and cryptophyte communities increasing while the diatom decreases (Henley et al., 2019. Variability and change in the west Antarctic Peninsula marine system: Research priorities and opportunities. Progress in Oceanography). It is something that needs to be more studied but worth considering as these changes will impact the pelagic production such as the burial flux.

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Section 5.4 the impacts of global change on the Si cycle: see the comment for L511-513.

L524: you could add Pickering et al., 2020.

L575: the reference Johnson et al., 2006 is incorrect in the Bibliography list.

L628-629: Coastal and continental zone in the Southern Ocean are not as limited as we think by iron for example see Annett et al., 2015 Comparative roles of upwelling and glacial iron sources in Ryder Bay, coastal western Antarctic Peninsula, Sherrell et al., 2018, A 'shallow bathtub ring' of local sedimentary iron input maintains the Palmer Deep biological hotspot on the West Antarctic Peninsula shelf.

L641-644: In coastal area the reduction of sea-ice has been shown to reduce the primary productivity not to increase it. I will suggest that the author give more details about which part of the ocean is considered, coastal or open ocean.

L694-697: As mentioned before, some very recent papers (i.e. Ng et al., 2020, Pickering et al., 2020, Geilert et al., 2020) have not been included in this review.

Technical corrections:

L63 "Silicifiers use . . . structure". I will suggest moving this sentence later. Something like Silicifiers are among the most important aquatic organisms, including micro-organisms () and macro-organisms (). They all depend on dSi that they precipitate and form biogenic silica to build their internal () and/or external () structures.

L71: independently?

L89: Silicic acid is already defined in the introduction, keep only dSi

L93: 60% not 60 %

L106: FA is only defined in the title, might need to define it also in the text.

L108-109: it seems that a verb is missing in the sentence. If not could you rephrase?

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L122: 1-3% in LaTeX cod 1–3\%

L152: “the potential flux of dissolution of quartz from sandy beaches” to avoid a series of “of”

L194: “is required” for what? This sentence feels to not be finished

L206: Either “. . . (Mottle 1983; Von Damm et al, 1991).” or change to “. . .(Mottle 1983; Von Damm et al, 1991), and it is possible . . .”

L223: a word is missing in the sentence or the sentence needs to be rephrased.

L246: “the error propagation from Bevington and Robinson, 2003.

L267: no space for the ratio, Si:C, same for L271

L275: replace Aller et al., 1996 with the more recent Aller et al 2014

L279: “³²Si activities . . . delivery to the sediments” this sentence is missing a word or needs to be rephrased.

L284: Did you mean “Fb of 7.0”?

L285: 11%

L293: Did you mean Supplement, section 3? L306: change to “considering Maldonado et al., 2019, the new best estimate for FSP is . . .”

L451: “If the production bSi that . . .” did you mean if the bSi production that . . .?

L535: remove one of the brackets, (94.3 Tmol-Si yr⁻¹)

Interactive comment on Biogeosciences Discuss., <https://doi.org/10.5194/bg-2020-274>, 2020.

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