

Interactive comment on “Silicon cycle in the Tropical South Pacific: evidence for an active pico-sized siliceous plankton” by Karine Leblanc et al.

Anonymous Referee #1

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General comments:

This is a very important paper for the marine silicon biogeochemical community. The data presented on the silica cycle in the ultra oligotrophic South Pacific are the very first from this region and thus extremely valuable. While the extremely low biomass and silica production rates are not surprising it is extremely important that they be quantified. Those data aid in our understanding of the contribution of subtropical gyres to global silica standing stocks and silica production. While I am 100% in favor of seeing this data published it was disappointing that silica production rates were only measured at two truly oligotrophic stations. So while the authors use these data to place the

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observed rates in a global context the extrapolation is extreme as fully acknowledged by the authors.

The authors were able to conduct some very fine kinetic studies that show active silicic acid uptake by the $< 2 \mu\text{m}$ size fraction. Few diatoms would be expected in this size fraction pointing to uptake by non-diatoms. There is significant confusion as to the kinetic experiments in terms of size fractionation that must be clarified before publication. More on that below. The quantitation of diatom taxa and abundance is extensive and valuable.

I have no major issue with the interpretation of the data or the analyses. My suggestions for improvements are detailed below.

Specific comments: The title of the work emphasizes the finding that a significant fraction of the observed uptake was in the picoplankton size class. The paper contains so much more than this. Please consider expanding the title to something like “Silicon cycle in the Tropical South Pacific: contribution to the global Si cycle and evidence for an active pico-size siliceous plankton”.

Line 40: This paragraph is very long. Maybe break it at line 40.

Line 42: The data available from the north Pacific subtropical gyre cited later in the paper would be relevant here as well.

Line 40-56. This is a suggestion only. Our understanding of the role of subtropical gyres in the global Si cycle began in the Sargasso Sea which through extrapolation led the fairly high estimates for the contribution of these regions to global silica production. Data from the north Pacific led to a reduction in that estimate and the data presented here from the south Pacific lower it yet again. So what we are learning is that the Pacific is very different from the Atlantic and that the North and South Pacific differ from each other. This perspective is lacking in this paragraph which focuses on extrapolating silica production to carbon. It might be worthwhile to add a section that stays focused

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on silicon as later in the paper silicon budgets are presented.

Line 58: maybe 'studies provide evidence for a role...' rather than 'studies have furthermore evidenced a role'.

Line 75: Maybe '...strategies and analyses were conducted on both cruises...' rather than "...strategies and homogenous analyses were conducted ...".

Line 85, 86: Maybe "... transects that employed a common sampling strategy of short and long duration stations." Rather than "...transects with similar sampling strategy of short and long duration stations."

Line 99: Given the very low nutrient concentrations it the reader would benefit from knowing the detection limits of the specific nutrient analyses employed.

Line 119: 'quarters' instead of '4'. 'Plastic' petri dishes right?

Line 127: What method was used to remove the interference from HF in the LSi colorimetric analysis: boric acid or dilution?

Line 131: Kinetic assays? Do you mean you conducted time courses to test the efficiency of different digestion times?

Line 138-139: Please elaborate. It is unclear how the addition of Si was used to correct for dissolution in the face of the combined influence of dissolution of captured siliceous particles and the admixture of ambient water.

Line 141 Si & VSi rather than Si/VSi. Si/VSi looks like you are dividing one rate by the other. Line 150: 'averaged' instead of 'average'

Line 151. Many details are missing from this section of the methods. There is no indication of size fractions. Later in the paper it is claimed that kinetics were size fractionated like biomass, but I only see one set of kinetic curves and it is not clear what size fraction they represent (Fig 8.). Also in this section there is no mention of a 32Si addition.

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Lines 187-196. The observation that the nitracline is much deeper than the silicicline is also observed in the Sargasso but not nearly to the same extent. It might be interesting to speculate as to why these depths differ in the discussion.

Line 198: rather than 'The distribution of orthosilicic acid concentrations were less clearly contrasted, ...' maybe 'Horizontal gradients were not as strong for orthosilicic acid...'.

Line 211: 'existed' rather than 'subsisted'

Line 212: "magnitude" instead of "amplitude".

Line 216: Maybe: "The Chl a distribution during BIOSCOPE was similar to that observed during OUPACE with extremely..."

Line 238: The units in the figures for BSi and LSi are in micromoles per liter whereas in the text the concentrations are discussed as nanomoles per liter. Be consistent. I would suggest changing the figure to nanomoles per liter as it gets rid of leading zeros.

2467: Maybe "LSi concentration was highest at both ends of the transect but concentrations remained below those of BSi with average LSi values..."

Line 271: Here the reader learns that kinetic experiments were size fractionated. Move this information to the Methods. More importantly only one size fraction is shown in fig 8. Where is the data from the other fraction? Also the legend for Fig 8 should indicate the fraction shown.

Line 273; Maybe "... rank order of most productive stations follow the pattern observed for BSi with the highest values observed at UPW followed by UPX and MAR stations."

Line 281: It might be useful to readers if the specific rates are also translated into implied doubling times as this will give a sense of how fast or slow growth might be in the various areas.

Line 295: It is unclear what size fraction is shown in the Fig 8. Fix legend. Also where

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is the data for the other fraction. Please clarify.

Line 316: Maybe “the lowest” rather than “record low”.

Line 359-360: Maybe “We obtained size-fractionated biomass and . . . OUTPACE program and size fractionated production. . .during the BIOSCOPE program.

Line 362: This is a long paragraph. Maybe break here.

Line 377: “documented” instead of “evidenced”

Line 387: Here is a place where the influence of data from the Pacific on global budgets can be emphasized. The contribution fell when data from the NPSG was added and now it goes down again when the south Pacific is considered.

Line 390: The limited number of measurements is disappointing, but treated objectively.

Line 408: The flux is indeed incredibly low: wow! However, my appreciation of this is vague given that I do not understand the correction for dissolution in the traps discussed above.

Line 426: Maybe: DCM’s are common in mid-ocean gyres and are known to be often dominated by pico-sized phytoplankton (Chavez et al. 1996), Studies documenting.”

Line 448: As I read this discussion I find the text informative but I wonder if the stated trends might be reinforced through a non-dimensional scaling or other analysis that would provide an objective way to illustrate many of the inferred trends.

Line 490: This is a very long paragraph. Maybe subdivide.

Line 542: Somewhere in this section the differences between the shape of the kinetic curves obtained here for pico-size fraction and those for cultured *Synechococcus* should be discussed. In culture *Synechococcus* have linear uptake kinetics within the concentration range examined here whereas the data from the South Pacific clearly

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show a hyperbolic response. It’s difficult to know for sure, but it might be possible that the organism responsible for Si uptake in the small size fraction in the South Pacific is something other than *Synechococcus* which would be very interesting.

Line 545-546: Confusing sentence. Maybe “Significant BSi in the pico-sized fraction could be explained as an artifact from detritus or the contribution from a previously unrecognized taxa.”

Line 552 “by” rather than ‘with”

Line 555: To finish this argument the expected shape of a curve resulting purely from fragments should be given. I would think the signal would then be very noisy and inconsistent which is not observed.

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