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 #3
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Overall this is a solid study which presents a wealth of data from a vast and undersampled region. While not groundbreaking, it could be impactful if it spurs more study of Si cycling in this region. Generally, I agree with most of the study (the authors have done a commendable job with the cell count and taxonomy components) but have a few main comments:

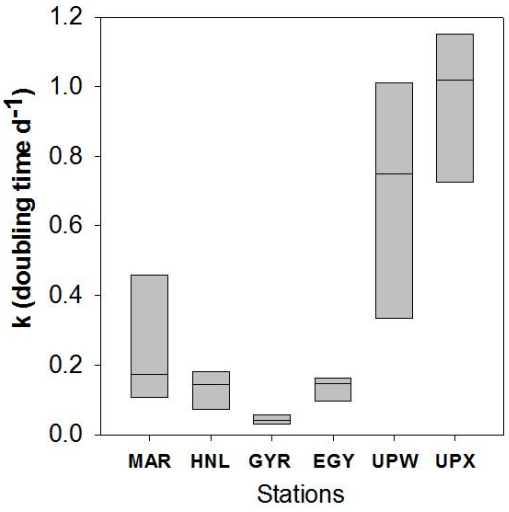
- The contribution of Synechococcus: the authors have compelling data which is consistent with recent studies but this facet is under developed. Given the Silicon per cell for Synechococcus in the two publications these authors cite (Baines et al. 2012, Ohnemus et al. 2016), could they do a similar budget of Synechoccocus silica here? Given the size of this project, surely there must be some flow cytometry data.

Ohnemus 2016 : “However, Si contents were highly variable and generally uncorrelated with measured environmental variables, suggesting that less direct effects such as community structure may drive Si accumulation in these ecosystems.” From the Table 2 in this paper, the authors estimate a contribution of Syn to the small fraction of BSi to range from 0.3 to 34 % though these contributions were always < 4% of total bSi, but they had Si per cell estimates from SXRF for each sample, and this ranged from 14 to 64 amol cell-1. We did calculate those estimates while writing this apper, but we felt that due to the strong variability and the absence of direct estimates for Si cellular quotas in Syn, this was not worth adding.
We however have these estimates and thus propose to add this sentence to the discussion as follows :
“ Using the range of measured Si cellular content per *Synechococcus* cells given in Ohnemus et al. (2016) of 14 to 64 amol Si cell^{-1} and *Synechococococcus* abundance data from the same casts obtained in flow cytometry (data courtesy of S. Duhamel, Lamont Doherty, NY), this yields a potential contribution of 3 to 14 % of *Synechococcus* to the small BSi fraction, which is close to the previous estimates.”

- Additionally, the isotope data is excellent to have but the rates for the kinetic data are unrealistic and not adequately discussed. For instance, 3.0 d-1 implies 4.3 doublings per day, 4.0 d-1 implies 5.8 doublings per day. Among all the experiments shown in Fig 8, all rates are exceptionally high as to be not believable. I think the authors need to better justify whether these data are useful and, if not, then perhaps consider eliminating.

According to your comments on too high VSi values, we have gone back to our raw data and found some inconsistencies in size-fractionated filtration between rSi and BSi. Some filters for rSi retained too much 32Si (either due to clogging or uncareful rinsing of

samples), yielding too much rSi over BSi explaining the high VSi values. If the shape of the kinetic uptake is globally fine, we acknowledge this problem, but unfortunately see no way of correcting the data adequately. We have thus chosen to remove this data entirely. We have left the vertical profiles for rSi data and replaced figure 8 (kinetics) by a figure of the mean k (doubling time) for each station.



Beyond these issues, I have numerous minor comments:

Line 64-66: Baines's estimates were indirect and extrapolated significantly, and were based on bSi associated with living cells (instead of total bSi).

Noted, we have removed this citation in this sentence.

Line 118: why the difference in filter sizes? Does this affect your results and interpretations?

During the BIOSOPE cruise, filter size (0.2 and 2µm) were chosen to reflect the standard operational size-classes of pico- and nanophytoplankton. The published work in between those two cruises dealing with the presence of Si in the picosize fraction was published using filter sizes of 0.4 and 3 µm (Baines et al., 2012), and since we wanted to confirm or not these first results, we decided to use the same filter sizes for comparison.

Line 128: given such low bSi measured, it seems like this precision is quite high (i.e. high noise to signal ratio). May the authors please explain why they would not consider this an issue?
Going back to our data, we have corrected this statement as follows :"The detection limit was 1 nmol L^{-1} for both BSi and LSi and quantification limits were 5 and 6 nmol L^{-1} for BSi and LSi respectively." We are indeed very close to detection limits at some depths during both cruises, but we did not have replicates to estimate accuracy. We do feel that the accuracy estimates, that has been estimated to 4 and 6 nmol L^{-1} respectively but for other cruises, if applied here would not significantly change any of the calculated budgets or

interpretation data, which would remain some of the lowest ever measured. We do observe that our measured in situ BSi concentrations were lowest than in most other papers, but we propose to add the following statement in the budget section, in order to underline the potential uncertainty on these baseline values. "

"For oceanic HNLC areas, values obtained (0.8 to 5.6 mmol Si m^{-2} d^{-1}) cover the range of rates measured in HNLC to mesotrophic systems of the North Atlantic, Central Equatorial Pacific and Mediterranean Sea. However, integrated rates obtained for the oligotrophic area of the South Eastern Pacific Gyre are to our knowledge among the lowest ever measured, *even taking into account the error associated to budget estimates this close to analytical detection limits."*

Line 149: Cerenkov counting is much less efficient than standard liquid scintillation methods correct? Given the low biomass (and thus low sample signal), did the Cerenkov background counts allow adequate resolution of analytically significant signals?

Cerenkov counting efficiency was estimated to be 42 % for this cruise, it is usually considered close to that value (~50%). Going over to liquid scintillation may have increased all cpm counts, but then also those of blanks, thus not improving the precision of the method.

Line 154: why go up to 36 uM? Are there prior studies which have gone this high? Recent work (Shrestha & Hildebrand 2015) show that above 25 uM diatoms start turning off silicon transporters.

Indeed the highest chosen concentration was probably too high, but it sometimes allow to show for a more linear response of Si uptake. In any case, the BIOSOPE cruise on which kientic experiments were made was conducted in 2005 so quite some time before the study you mention.

Line 229: given the high values, would the median (instead of average) be better here?

The median is now indicated at the end of the sentence, but is not notably different (13 instead of 17 nmol L^{-1}).

Line 275: 15 nmol/L/d given such low bSi means these cells are pretty active (e.g. 1 doubling per day)

Line 281, 298-299: Vmax is so high, it seems to be an error (see general comment).

See response above concerning VSi estimates

Line 296: it doesn't say in the figure caption that these are just for pico sizes, please Clarify.

This has been added to the Figure legend.

Line 353: what is the percent dissolution among these samples, could those be used to infer dissolution rates in the water column and compare to biomass-specific rates?

The % dissolution is indicated in the method section (line X) and was comprised between 16 and 90%. However it does not reflect in situ dissolution rates, but dissolution in trap samples kept at 4°C between sampling and analysis, hence not comparable to in situ rates.

Line 564: may you cite evidence for siliceous parmales in this region, aren't these only routinely observed in the subarctic North Pacific.
No they are not, there is evidence for large scale distribution, notably also for the Southern Ocean, but also large abundances have been oserved in the South Eastern Pacific (see Fig. 4 in Ichinomiya et al, 2016, ISME journal), where they can represent more than 1 % of total photosynthetic reads at both the surface and DCM depths.

Line 582: how so? There are two problems: the quotas published by Ohnemus et al. 2016 are low and the standing stock of picoplankton isn't high enough to consistently drawdown Si. Second, if these standing stocks did get high enough, then to remove Si, this material would need to be exported; yet the export rates quantified in this region were the lowest observed. This feels like a disconnect.

We agree that this role is probably not major, and have thus removed the term in the sentence. However, drawdown may be high by Synchococcus while it is also likely grazed and recycled in the surface layer. High temperatures are likely to remineralize a large part of assimilated Si in the surface layer, even though a previous drawdown and export prior to our study is necessary to explain the low silicic acid concentrations observed at the surface, and is not attributable to Synechococcus activity.

Figure 2, 3: could the color scale be more logarithmic (like in Figure 4) and similar to allow easier comparison?

We prefered giving the maximum details with color range for each graph, but have homogenized since all co-authors requested this.

Figure 7: perhaps a log scale to see the low values easier?
We modified the graph accordingly (see below)

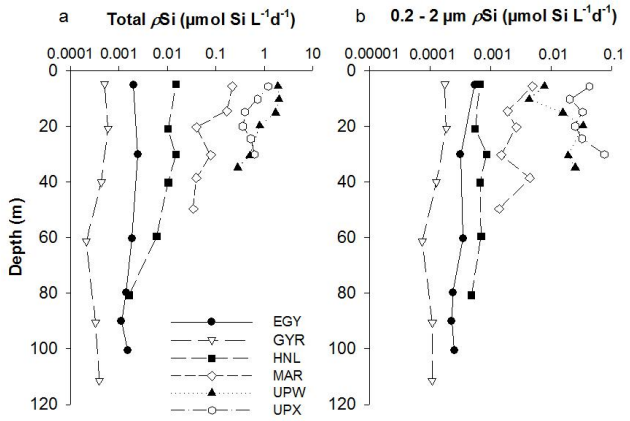


Figure 9: please detail how the lower panel values were calculated

This is now described in the method section as follows :

“Diatom sizes were measured for each species for an average number of 20 cells when possible, and converted to biovolume and C biomass following the method described in Leblanc et al. (2012). C biomass per species were then compared to chemically determined POC concentrations to yield a percent contribution to C biomass.”