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Biogeosciences Discussions

# *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.

- 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.

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).

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

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?

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?

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.

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

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).

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

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?

Line 564: may you cite evidence for siliceous parmales in this region, aren't these only routinely observed in the subarctic North Pacific.

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.

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

Figure 7: perhaps a log scale to see the low values easier?

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

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