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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 #2

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This is a good paper with timely and relevant information on a poorly studied region of the ocean. It has a both rate/biomass information along with floristic data, a combination not often available. It is unfortunate that the gyres had only a limited sampling. The data is tantalizing in what is seen, but more sampling in this area is required to confirm the extremely low rates. It is a great deal of information to present and there are some areas where either the paper structure or text is confusing. As I note below, the methods need considerable improvement. The description of replication and error bars is unacceptably vague. Claims of differences are not justified by any statistical analysis. There are very few measures of variability given and reader is left to wonder if replicates were even collected. Each measurement should have a standard

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deviation, confidence interval or some other metric of variability. The methods need to explicitly note which samples were collected in duplicate, triplicate, etc. On line 150, the uptake measurements were noted to have a precision of 10-25% for the less productive station. What is it for more productive stations? To me, the use of separate figures for hydrology, nutrients, BSi, and rates creates difficulty in interpreting the information. Multiple pages of figures are needed to understand one cruise. It would be much more clear if all the data were in a single (or perhaps 2 adjacent) multi-panel figures. However, it requires a rewrite of the manuscript to discuss each cruise in parallel rather than dealing with hydrology, nutrients, etc from the two cruises together. Since the cruises are very separate in time and space, there is no reason to treat one data type at a time. Cell counts are very time consuming and tedious. Thus, it is always disappointing when the information is lumped into a single pool of diatoms in Figures. From the methods, it is quite impossible to determine if diatom counts were from the same depths as the BSi or a subset. Please clarify this. If the data density is there, please add this to the figures as a contour plot. The data availability statement is not present nor is there an explanation of why it is not present. This is not acceptable and I cannot recommend publication until this condition is met (as noted in the Instructions to Authors for the journal). The figures lack panel labels except for Fig. 7. This needs to be corrected for publication. Paragraph breaks need to be used for clarity, be they line spacing or indentations.

Line: comment 19: Chlorophyll does not need to be capitalized. 33-34: I am not sure what “silica production. . .comparable to . . .all areas of diatomaceous sediment” means. One is a rate per volume per time, the other is mass per volume sediment. Please clarify. 39: need to define chl a abbreviation first. 50-56: While these cited authors suggested these mechanisms may be leading to diatoms blooms, they have little direct experimental or observational data to this point. Wilson (2011) was later modified when a stratification value was discovered to be to high (see later work by Wilson et al. 2013) and Calil et al and Guidi et al. have done much more direct work on the role of mesoscale features than Krause et al. (2009, 2010). These are all key points to make,

but please cite the correct papers. 104: please provide temperature and length of pre-combustion 116: cascading is probably not the best word choice. Sequential or stacked is more accurate. 123: digestion, not attack 122-134. I am curious how standards were treated to have the same pH value as the samples. Si is a pH sensitive assay, so this merits some consideration. 143: please specify how the light measurement was made and then applied to generate the incubation depth. 151: Si uptake from the chlorophyll maximum. This description needs clarification. Was uptake measured as per section 3.7 or were changes in BSi measured as per section 3.6? The kinetic curve incubation lasted 8 hours, the in-situ incubation lasted dawn to dusk. Are there potential artifacts associated with the timing of division cycles? Later in the paper, it appears isotope uptake experiments were conducted, but the reader should not have to wait until then to know this. Finally, how relevant is this measurement to the waters above the DCM? 162: please list the net specifics: mouth opening and mesh size 216: This sentence is not clear. Please rephrase. It is apparently a comparison joined by the word than. I'm not sure what you are trying to say. 266: attributed, not assimilated. 268: What is this unit of variability? Standard deviations? confidence interval? If you wish to say they are different, please refer to a statistical test showing this. The $\pm$ ranges overlap considerably. I am not convinced. 269+: The same comment applies here. Are the duplicates? Triplicates? Error bars? Statistics? The rates have up to 25% precision errors, so this is important. 312+: contribution to biomass implies some conversion to a common currency (carbon, chl). How did you do this? 322: richness based on quantitative counts or the net tows? In either case, the authors need to specifiy the total number of cells examined. If it is 50 cells in one case and 500 in another, that will clearly influence the community richness observed. 326: Dominance within the diatom community needs to be specified as based on abundance or size/surface area. One large Coscinodiscus or Rhizosolenia will equal many small bicapitate Nitzschia. The Table 1 citation seems out of place. I think you mean Table 2. 489: The authors may wish to consider the work of Shipe et al. (1999) where they noted large rare diatoms contributed up to 26% of the Si uptake in the north Pacific. There is no information

on these giant diatoms, either solitary or aggregated, from the south Pacific. Any observations they have on this would be very relevant. 593: This study is not a time series as per HOT and BATS, so the topic sentence implication that this work adds to time-series work in the south Pacific is not correct.

Figures Fig. 4: The change in color scale is a bit confusing since the tendency to compare the two transects. If Fig. 4 Outpace were the same color scale as the Bioscope figure, then all the detail of the DCM would disappear. Likewise, the use of the Outpace color scale for the Bioscope would create detail.

Fig. 9: there are typos in the 2nd panel figure axis.

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