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

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 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.
As the focus of this paper was to established basin/regional budgets for Si, we felt that discussing each parameter together, despite the temporal gap between the two cruises was more appropriate than presenting both cruises one after another, to get a sense of regional variability and that this would feel less repetitive for the reader.

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.
We do have cell abundance per species, but first, the data set for the BIOSOPE cruise was already published once in Gomez et al . 2007 in a different form but with extensive tables of diatom taxa list (Table 2 therein). The data from OUTPACE is also available which is how we managed to estimate C biomass from each taxa and calculate relative contribution to POC in %. We felt that adding yet another graph with species relative contribution which requires a long legend and many colors to convey diatom diversity was not absolutely necessary within the scope of this paper, and that Fig.9 and Table 2 were sufficient. But since the data is available we have added a link for data access in the data availability statement.

From the methods, it is quite impossible to determine if diatom counts were from the same depths as the BSi or a subset.
Diatom cell counts were done on the same Niskins as BSi/LSi measurements. We have

added the following “Seawater samples *collected from the same CTDs and Niskins as particulate Si samples* were ...”

Please clarify this. If the data density is there, please add this to the figures as a contour plot.
Potential density isolines were added to Figures 3 and 4 as white contour overlays.

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).
This is an omission. The following statement has been added.
“Data is available upon request through both cruises databases. For the OUTPACE cruise, see http://www.seanoe.org/data/00446/55743/
For the BIOSOPE cruise, see: http://www.seanoe.org/data/00446/55722/
These links will be running very shortly and before resubmission of revised version.

The figures lack panel labels except for Fig. 7. This needs to be corrected for publication.
This was added to all Figures and Figure legend.

Paragraph breaks need to be used for clarity, be they line spacing or indentations.
This has been corrected on several occasions where paragrahs were indeed very long.

Line: comment 19: Chlorophyll does not need to be capitalized.
corrected

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.
Yes the sentence was not very clear. It is a statement from Nelson et al 95 that was not clearly rewritten, we have modified the sentence as follows :
“ their silica production would be comparable to that of areas overlying major diatomaceous sediment accumulation zones.”

39: need to define chl a abbreviation first.
Chla has now been written in full text, as definition is given later.

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.
We were actually trying to find references related to diatom blooms, even though we did not state this clearly in this sentence “Furthermore, oligotrophic regions are known to experience considerable variability in nutrient injections leading to episodical blooms depending on the

occurrence of internal waves (Wilson, 2011), meso-scale eddies (Krause et al., 2010) storms (Krause et al., 2009), or dust deposition events (Wilson, 2003)."
Looking at Guidi et al. 2012 (Does eddy-eddy interaction control surface phytoplankton distribution and carbon export in the North Pacific Subtropical Gyre?), we did not find any mention of the stimulation of a diatom but rather only mention of Trichodesmium bloom which was not the reference we were trying to highlight. We have added Calil et al 2011 in the list of references concerning local upwellings or dust deposition events.

104: please provide temperature and length of precombustion
4h at 250°C (now included)

116: cascading is probably not the best word choice. Sequential or stacked is more accurate.
"Cascading" has been replaced by the word "stacked"

123: digestion, not attack
corrected

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.
When filters are digested in NaOH, the supernatant is subsequentely neutralized with HCl so that pH is close to neutral. Tests of DSi standard curves made in the same NaOH-HCl matrix as samples wer not significantly different from standard curves done in milliQ water. However when running DSi analysis directly on seawater samples, then low silicate water is used as the standard curve matrix, as salinity does impact by ~10% the calculated DSi concentrations.

143: please specify how the light measurement was made and then applied to generate the incubation depth.
The text states : "Euphotic layer depths (Ze) were calculated as described in Raimbault et al. (2008) and Moutin et al. (2018)." We have added the following detail: "Sampling depths were adjusted to on deck incubators screen attenuation using measurements from an in situ PAR sensor (LI-COR instrument) mounted on the CTD frame."

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?
Si uptake kinetics were carried following the method described in setcion 3,6. For more clarity, several methodological additions were made in this section (32Si added, size of filters). The header for both section were clarified by mentionning that both bulk Si uptake and Si kinetic

experiments were size-fractionated. In situ incubations were combined with 14C, 13C, 15N and O2 fluxes measurements and immersed 24h, whereas kinetic experiments were carried out during 8 h in paralell in on deck incubators fitted with neutral nickel screens. Given the limited amount of available 32Si and the extremely low biomass observed during biosope except at the DCM, a choice was made to only perform size-fractionated kinetic experiments at the DCM level, or at both DCM and surface levels if ever some biomass was observed at the surface. About the incubation duration, we make the hypothesis that populations are not synchronous and that Si uptake should be constant over 24 h. Furthermore, upon addition of cold Si(OH)4, stimulation of uptake occurs and as thumb rule we try to stay below a threshold of 10% of consumption of initial available Si, which is why the incubations were stopped after 8h. We surmise that size fractionated kinetics uptake experiments carried out outside of the DCM would have yielded no measurable counts on the scintillation counter, given the absence of Si biomass, and expect our kinetic results to represent the most active diatom community present in the euphotic layer. However, the results of kinetic experiments were removed entirely due to experimental problems on borad (likely filtration issues) which yielded incoherent results between size-fractions and unrealistic VSi values.

162: please list the net specifics: mouth opening and mesh size
The following information was added : "During the OUTPACE cruise, additional WP2 phyto-net hauls (mouth opening 0.26 m^2 ; 35 µm mesh-size) were undertaken at each site integrating the 0-150 m water column"

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.
This sentence was rephrased as follows : "The Chl*a* distribution during BIOSOPE was similar to that observed during OUPACE, with extremely low surface concentrations and a very deep Chl*a* maximum located between 180 - 200 m ranging between 0.15 and 0.18 µg L^{-1}."

266: attributed, not assimilated.
corrected

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 ± ranges overlap considerably. I am not convinced.
The unit is the standard deviation. Indeed, differences are not statistically significant, which is why we did not state "significantly" higher, but merely "higher". Yet we have corrected as follows : "The importance of the picoplanktonic Si biomass was higher in the SPG (36 ± 12 %, n=14) than over the MA (22 ± 10 %, n=5) but not statistically different ($p > 0.05$)."

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.
Unfortunately, given the limited amount of 32Si available, there were no duplicates on any of the 32Si uptake or kinetic measurements, which is why no error bar is indicated.

312+: contribution to biomass implies some conversion to a common currency (carbon, chl). How did you do this?

We have added the following explanation in section 3.9 : “Seawater samples were preserved with acidified Lugol’s solution and stored at 4ºC. For the BIOSOPE cruise, a 500 mL aliquot of the sample was concentrated by sedimentation in glass cylinders for six days. Diatoms were counted following the method described by Gomez et al. (2007). For the OUTPACE cruise, a 100 mL aliquot of the sample was concentrated in an Utermöhl sedimentation chamber for 48h. 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.”

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.

By richness we only meant number of taxa as indicated in Figure 9c, and we did not use any common diversity indexes. The number of taxa was derived from quantitative cell counts from Niskin bottles sampled at the surface and DCM at each site.The richness as mentionned above is based solely on the number of different taxa present in an entire sedimentation cuve. For the BIOSOPE cruise, the counting method was to sediment 500 ml. Hence counted cells would be half of the cellular abundances indicated for this cruise. For the OUTPACE cruise, we counted 100 mL aliquots, hence counted cells were equaled to 1/10th of the cellular concentration. Methods were different as the data was obtained from the publically available cruise database and counting not performed in our research group. The details about the volume of the aliquots counted has now been added to section 3.9.

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.

In this paragraph we indeed meant numerical abundance, which has been underscored now in the text. We did not present biomass estimates per species as the paper already presents and extensive dataset and only present global contribution to POC estimates. Indeed abundances were extremely low at some sites and subsequent conversions would be based on too few counts. It is agreed that large diatoms represent a disproportionate contribution to biomass compared to smaller species, but we did not feel that adding this level of detail in the discussion would be useful in the present paper. Furthermore this level of detail (specific contribution to biomass) is not available for the BIOSOPE cruise during which diatom sizes were not measured.

The Table 1 citation seems out of place. I think you mean Table 2.

Yes this has been corrected.

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.

Line 523 : pennates were the most numerically abundant species (but also the most actively silicifying as evidenced by PDMPO staining - data not shown) but it is true that large sized diatoms such as Pseudosolenia calcar avis contribute disproportionately to C biomass compared to small pennates. In our calculation, 23 500 cells of Pseudo-nitzschia sp. represented 78% of total abundance but only 1 % of C biomass. Contrarily, 190 cells of Pseudo-solenia calcar-avis represented 0,6 % of total abondance but 90% of calculated biomass.

The following sentence was added line 576 : “However, it should be noted that if small fast growing pennates were numerically dominant, their relative contribution to C biomass was very small compared to that of few larger centrics such as Pseudosolenia calcar-avis, which when present dominated in terms of biomass, similarly to what had already been observed in the South Pacific with large Rhizosolenia (Shipe et al., 1999).”

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.

We did not imply that our work was similar to time-series data, and stated that we provide complementary data from two cruises, our point was to say that we propose comprehensive data of stocks, fluxes, kietincs and diatom taxonomy and abundance that are not usually provided all together in biogeochemical studies. We removed the term “complementary” from the sentence in order to remove the meaning that our data are similar to time-series.

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.

Indeed but the ranges are very different, making it difficult to convey the profile details with the same colors. We have tweeked the median and non-linear settings of the color bar of the BIOSOPE cruise, so that the green color is close to 0.2 µg L^{-1} in both pannels.

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

Corrected