

Anonymous Referee#1

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 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".

[Thank you for your suggestion, we have corrected the title accordingly.](#)

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

[Corrected.](#)

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

[Indeed, we agree with both previous comments, and see how this perspective is lacking.](#)

We have rewritten that part of the introduction section as follows :

“Diatoms are known to contribute more importantly to primary production in meso- to eutrophic systems, yet several studies have emphasized that even if they are not dominant in oligotrophic regions, they may still contribute up to 10-20 % of C primary production in the Equatorial Pacific (Blain et al., 1997). In the oligotrophic Sargasso Sea (BATS station), their contribution was estimated to be as high as 26-48 % of new annual primary production (Brzezinski and Nelson, 1995) and to represent up to 30 % of Particulate Organic Carbon (POC) export, leading to an upward revision of the contribution of oligotrophic gyres to global Si budgets (Nelson and Brzezinski, 1997). Similar studies carried out in the Northern Pacific (HOT station) led to new estimates, as diatoms were found to be less important contributors to primary production. A combination of both Atlantic and North Pacific oligotrophic gyres budgets led to a revised contribution of 13 Tmol Si y⁻¹, a 51 % diminution of the previous estimate (Brzezinski et al., 2011).”

And in the conclusion:

“The mid-ocean gyres contribution to Si production was recently revised down to 5-7% of the total by Brzezinski et al. (2011) building on estimates from the North Subtropical Pacific Gyre. The present study points to even lower values for the South Pacific Gyre, confirming its ultra-oligotrophic nature, and should further decrease this estimate. These findings underscore the differences in functioning of different subtropical oligotrophic gyres between the North Atlantic, North Pacific and South Pacific and clearly warrant for improved coverage of these areas and for more complete elemental studies (from Si production to export).

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

Corrected.

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

Corrected.

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.’

Corrected.

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

The following line was added : “During BIOSOPE, nitrate (NO₃⁻) detection limit was 0.05 μM (accuracy of ± 0.05 μM), phosphate (PO₄³⁻) detection limit was 0.02 μM (accuracy of ± 0.05 μM), orthosilicic acid (Si(OH)₄) detection limit was 0.05 μM (accuracy of ± 0.05 μM). During OUTPACE the quantification limit was 0.05 μM for all nutrients.”

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

Corrected.

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

HF is diluted in filtered boric acid in our protocol. Added.

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

We meant that kinetic assays of the first NaOH extraction were carried out to determine on a few samples the optimal extraction time when all BSi is digested and prior to the linear increase of DSi showing the subsequent leaching of LSi. We have modified the sentence as follows :

"This is particularly the case when high LSi concentrations are present. Kinetic assays of orthosilicic acid were conducted in some samples from the Marquesas, Gyre, East-Gyre and near Upwelling stations during BIOSOPE to determine the optimal extraction time for BSi digestion, and results revealed negligible LSi interferences after an extraction time of 60 min."

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.

This section has been detailed as follows : "Biogenic silica export fluxes were determined from drifting sediment traps deployed for 4 consecutive days at three depths (153, 328, 519 m) at the three long duration stations of the OUTPACE cruise. For each trap samples, 160 mL were filtered onto 0.6 μ m polycarbonate membranes and the filters were treated following a two-step digestion as described above. In addition to the BSi measurements, the dissolved Si measured directly in the supernatant of each trap at the time of subsampling minus the initial dissolved Si content in the seawater used to fill the trap was added to the final BSi concentrations, to account for BSi dissolution in the trap samples during storage. This step proved necessary, as BSi dissolution ranged between 16 and 90 % depending on the samples."

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'

Corrected.

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 ^{32}Si addition.

We had mentioned that samples for kinetic curves were treated as described for in situ

samples (i.e. received a spike of 632 KBq and were filtered onto stacked 0.2, 2 and 10 μm filters). However due to experimental problems during filtration for the kinetic experiments, we have decided to remove the kinetic section altogether (see below for further details).

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.

This difference is mostly the case during BIOSOPE, and could be the result of the strong Si-pump operating in the coastal upwelling, leading to advection of low-Si waters westwards towards the gyre (Dugdale and Wilkerson, 1998). However, the discussion part of this paper is dedicated to budgets, diatom community and evidence for Si uptake in the picosize fraction, hence bottom up control factors, linked to hydrology are not a key point of this paper. We feel it would be out of place to start this 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. . .'.
Corrected.

Line 211: 'existed' rather than 'subsisted'
Corrected.

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

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

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.
The BSi/LSi figures have been changed to nmol L⁻¹ to be consistent with text and the color bar increased in non linearity to better show low concentrations.

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

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.

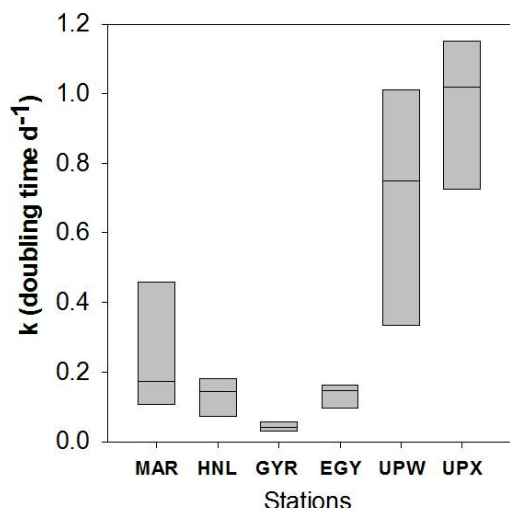
According to your comments and some other reviewer's comment on too high VSi values,

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 ^{32}Si (either due to clogging or uncaredful 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 these data entirely.

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.”
Corrected.

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.

Figure 8 (former kinetic figure) was replaced with the following showing K (doubling time) for each station.



Line 295: It is unclear what size fraction is shown in the Fig 8. Fix legend. Also where is the data for the other fraction. Please clarify.

It was the 0,2-2 μm size-fraction, but this figure has now been removed as explained above.

Line316: Maybe “the lowest” rather than “record low”.
Corrected.

Line 359-360: Maybe “We obtained size-fractionated biomass and . . . OUTPACEprogram and size fractionated production. . .during the BIOSCOPEprogram.”
Corrected.

Line362: This is a long paragraph. Maybe break here.
Corrected.

Line377: “documented” instead of “evidenced”

Corrected.

Line387: 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.

We have added this reference in the following sentence : “ This budget has been recently revised down to 13 Tmol Si y⁻¹ when considering budgets from the North Pacific (Nelson and Brzezinski, 1997) reducing the contribution of subtropical gyres to 5-7% of global marine silica production (Brzezinski et al., 2011; Tréguer and de La Rocha, 2013).”

Line390: The limited number of measurements is disappointing, but treated objectively.

We agree. Unfortunately the extent of funding at the time of the cruise and available quantity of ³²Si did not allow for more sampling, nor replicate measurements.

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.

This has been corrected as described above (answer to comment on lines 138-139).

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.”

Corrected.

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.

As we give mean and SD values for each zone, we feel that it is sufficient to characterize each region (that are defined hydrologically), as we are not trying to show any statistical differences between regions.

Line490: This is a very long paragraph. Maybe subdivide.

Corrected.

Line 542: Somewhere in this section the differences between the shape of the kinetic curves obtained herefor 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 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.

These kinetics have been removed and can therefore not be mentioned in the discussion.

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.”

Corrected as follows: “The significant contribution of the pico-size fraction to the BSi stocks during both cruises could be explained by the presence of detrital components, however its contribution to Si(OH)₄ uptake during BIOSOPE was really surprising but can be explained in the light of new findings”

Line 552 “by” rather than ‘with”

Corrected.

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.

Corrected as follows: “If the former hypothesis was true, we would expect approximately the same amounts of broken fragments on all filters (i.e. for each increasing Si concentrations) and the shape of the curve would not be hyperbolic.”