Review of the paper

"Silicon stable isotope distribution traces Southern Ocean export of Si to the eastern South Pacific thermocline"

by de Souza et al. (Biogeosciences Discuss.)

This paper presents the first set of Si isotopic signatures of silicic acid over the entire water column in the South Pacific along a N-S transect (sub-equatorial up to the Southern Ocean). These new data are of particular importance to complement the current δ^{30}Si dataset. The authors discuss their results in terms of global ocean circulation and marine Si biogeochemical cycle, and focus more specifically on the transport of Si (an essential nutrient for diatoms) from high latitudes to equatorial pacific via AAIW and SAMW. The paper is stimulating, very well written and nicely illustrated. I recommend publication after minor to moderate revisions listed below in their order of appearance in the ms.

Damien Cardinal

Sampling and method

p. 6413 line 1-2. I really wonder why the authors used glass fiber filters to sample for silicon isotopes? This really goes against GEOTRACES guidelines and common sense. The data look convincing so it is unlikely there was a systematic bias, but the authors should elaborate on potential contamination, especially for those samples with low Si contents (<5 uM).

p. 6413 lines 20-25. The use of SEM for 300 analyses is appropriate to estimate uncertainty on diatomite, however for seawater samples replicated with 2-5 analyses, SEM provides an underestimation of the uncertainty. It is more appropriate to use standard deviation instead and correct tables and figures accordingly. For n>1 this yields to an average st. deviation of 0.15 pmil instead of 0.07 pmil for average SEM which is also more consistent with the external reproducibility of 0.12 pmil asserted by the authors.

p. 6413 lines 20-22. Specify whether *n* analyses represent *n* replications of whole sample processing or rather *n* analyses of the same purified sample.

p. 6413 lines 23-26 and Supplementary table 1. I don't understand the uncertainties provided for samples when n = 1. Did the authors ascribe a long-term external reproducibility of 0.12 pmil as I understood from the text? Then why are there some error values in table 1 varying from 0.12 to 0.21 pmil when n=1?

Discussion

§5.1.1 p. 6419 lines 1-5 and Fig. 6. At 58°S, there is little agreement between the mixing line drawn from end-members (15m and 428) and the intermediate values (215 and

107m). Although the authors point this out in the legend, it would need further discussion.

§5.1.1 p. 6419 lines 5-7. The 20-25 % contribution of surface water to Si in WML is a value very similar to the one already estimated by Fripiat et al. for the Southern Ocean (2011 a, b). This should be underlined.

§5.1.1 p. 6419 lines 12-15. This sentence is long and unclear: "…is exported past the depth…"? The link between high d30Si value of silicic acid in WML and low d30Si of exported opal at depth is not obvious. Please clarify.

§5.2. The authors could start to discuss here Beucher et al. (2008) results, which complement their data further north.

§5.3 p. 6424 lines 23-26. Provide the mixing parameters used by the authors to find an influence of NADW< 0.05 pmil on CDW (volume mixing fractions, silicic acid contents and Si isotopic signatures).

§5.3 p. 6425 lines 1-5. Given that average st. deviation of replicates in this study is 0.15 pmil (1 s.d., cf. my comment above), and that data from the reoccupation site in the Indo-Atlantic Basin have not been replicated, this might explain why the authors did not observe a 0.2 pmil offset along ACC. This is more distinctly seen on Fig. S7. Differences between Fripiat et al. vs. Cardinal et al. data are based on a larger number of data (ca. 80) and have been shown to be statistically different in Fripiat et al. The authors provide here an additional 12 data (with, on average, larger error bars), and they should check by simple stats whether these new data cancel out the differences highlighted in Fripiat et al. while taking into consideration their own uncertainties. Merely looking at fig. S7 is not convincing.

Figures and supplementary material

Fig. 6. Ideally use more contrasted symbols and/or colours to help better differentiate the two latitudes. On a printed colour version, I could hardly notice the difference.

Fig. 7. Plot the sampling locations on this figure.

Supplementary Material A (Monte Carlo) & B (mixing equations): these sup mat. are only a few lines each. They describe standard practices and provide key and easily understandable information. I suggest putting them in the main text.

Fig. S3. Could the authors indicate on the figure the dates corresponding to their sampling for each station in order to directly indicate the timing of the sampling in comparison with biomass?

Supplementary material Table 1.For future reference and in the light of data ancillary, it could be useful to also provide d29Si.

If ODV software has been used to produce some figures, appropriate credit should be given.