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Dear Associate Editor,

The authors would like to thank the reviewer, Gregory de Souza for his comments on the entitled manuscript "*Insights into the transfer of silicon isotopes into the sediment record*". We would like to direct the Associate Editor and the reviewer to our responses below (in blue). The authors have strived to accommodate all of the comments and so we hope that they will both be satisfied with the revised manuscript. Reference to the new line numbers has been provided so as to locate the amendments to the manuscript more easily and a PDF submission also attached displaying the track changes.

The main criticism from the reviewer is the use of the mean $\delta^{30}Si_{diatom}$ open trap composition to derive estimations of ε_{uptake}. This is accepted by the authors. This was perhaps not the most representative comparison (and provided an over simplified estimation) given the hint that progressive enrichment occurs to the surface DSi pool over the course of the spring bloom (e.g. heavier July and August trap $\delta^{30}Si_{diatom}$ compositions). As such, we derive the estimation using the May $\delta^{30}Si_{diatom}$ composition and the mean March surface water DSi composition. The bulk of the corrections have been made to Section 5.1 and the necessary changes also made to the abstract and conclusion (see attachments). In response to this, Figure 4 has also been amended to remove reference to the previous estimation of ε_{uptake}.

However, we also emphasise more strongly that we use this data set more as a snapshot or as a discursive point with regards to the estimation of ε_{uptake}. In other words, due to the absence of seasonal DSi and BSi data, we cannot fully constrain calculations of ε_{uptake} based on existing data provided from Lake Baikal. However, due to the paucity of such estimations from lacustrine environments (compared to the marine environment) and given that we do present some seasonal data, we decide to include the findings. We believe that although not fully constrained it is still of value to the scientific community. If anything as it highlights the need for more in depth studies in lacustrine environments in the future that will hopefully address this issue more comprehensively.

We trust that both the reviewer and Associate Editor are satisfied with these corrections to the manuscript. We have aimed to be more transparent about the shortcomings of the data set presented (with regards to ε_{uptake}). However, given the paucity of such lacustrine Si studies (when compared to the marine environment) we hope that they will consider it of value and accept if for publication in Biogeosciences.

We would like to thank all reviewers for their constructive comments.
Many thanks,
Virginia Panizzo

Authors' corrections to minor comments:
L64-5 and L315-6: Since the authors mention the numeric values of the fractionation factor derived from early culture studies, it seems only fair to also mention the large range of values found by Sutton et al. (2013), rather than mentioning this study only as a caveat.

The authors have added a few lines of text in response to this comment. They agree that it would be a worthwhile addition to include the range in fractionation effects estimated from this study. Please refer to lines 68-70 and 321-325.

L74: The study of Wetzel et al. (2014) was conducted in the laboratory, not in the natural environment.
Thank you. This has now been corrected (line 80).

L117-8: “residence time”.
This has been amended (line 123-124).

L127-9: This sentence is very unclear (and I think “proceed” is used incorrectly).
This has been amended to “follow”. Line 133.

L175: units missing.
These have been added now. Line 181.

L190: what are Z and A traps?
These stand for the open and sequencing traps (as per Table 2) and this reference has been included in the text here. (Line 196).

L208: I am surprised by these concentrations! What concentrations are the samples measured at?
Waters were pre-concentrated in order to pass through resin. As such they have a higher concentration than so that post resin were a concentration close to 3 ppm. Trap samples were very concentrated following fusion chemistry, which derived a concentration close to 30 ppm. A concentration closer to 10 ppm was desired post resin.

L287-290: Something is wrong here. If the bloom is coincident with summer stratification, it cannot also be coincident with the periods of overturn (i.e. destratification).

Perhaps this wasn’t very clear in the text. The authors meant that blooms develop 1) after ice off and 2) after periods of ML summer stratification. The blooms are coincident with overturn. This has been rephrased slightly to hopefully make this clearer (line 294-295).

Figure 3: Given typical DSi profiles in the southern basin of Lake Baikal (e.g. Jewson et al., 2010), the very low DSi concentration near 200m in Fig. 3 is quite surprising! Can this be real?

The authors also acknowledge that this is a very low concentration, especially compared with the 1997 data of Jewson et al (c. > 1 ppm). However, these data are also supported by spectrophotometer readings, from different samples, collected on the same day and from the same depth. We are not quite sure how to resolve it but in response to the reviewer, yes, they are “real” data.