Review of manuscript BG-2015-225 *Gregory de Souza*

Dr. Panizzo and co-authors present silicon stable isotope data from diatoms (both water-column and sedimentary) as well as dissolved Si from Lake Baikal, in order to constrain key parameters relevant to the use of diatom sediment δ^{30}Si as a proxy for past lacustrine or environmental change. The data they present appear sound and the method is well documented, although the manuscript could have been more carefully prepared. The authors demonstrate that the δ^{30}Si values of sinking diatom material collected by sediment traps over the course of a year vary minimally with depth, and are very similar to the δ^{30}Si value recorded by diatoms in the surface core-top sediment. They use these observations, together with the observation that the dominant diatom species is fragile and easily-dissolved, to argue that isotopic fractionation of Si during diatom dissolution, should it be present, has no resolvable effect on the δ^{30}Si signal recorded by sedimentary diatoms. Analogous results have been shown for marine diatoms by Fripiat et al. (2012) and Egan et al. (2012), but the authors are, to my knowledge, the first to show this for lacustrine diatoms. This result is worthy of publication in BG.

I do take issue with the authors' other main result, however, i.e. the estimation of the fractionation factor associated with diatom Si uptake. Firstly, the authors argue that their March surface-water δ^{30}Si data reflect pre-bloom conditions before the start of the growing season, i.e. $\delta^{30}Si_{initial}$. They then *directly* compare this value to the annually-averaged $\delta^{30}Si_{diatom}$ value that they obtain from the open sediment traps and derive a fractionation factor from the difference between these two values. I do not see how this difference can be taken to reflect a fractionation factor, in the face of seasonal diatom drawdown and evolution of the $\delta^{30}Si_{DSi}$ (and $\delta^{30}Si_{diatom}$) value – there is no isotope-geochemical basis for this. The authors mention the fact that they do not have very much data to constrain the fractionation factor; I would agree. In this case, it is better not to attempt such a quantification at all rather than make unjustified oversimplifications, especially since the authors' seasonally-resolved data from the sequential sediment traps would seem to be at odds with the estimated fractionation factor. Given the large sediment flux (which we might be able to assume is mainly biogenic?) associated with the sample from May, this sample seems to represent export of the spring bloom, i.e. the event for which the pre-bloom $\delta^{30}Si_{DSi}$ is most relevant, and for which the authors' difference method is most applicable. However, this sample has a very low δ^{30}Si value of +0.67‰, suggesting that the fractionation factor is significantly larger (i.e. -1.6 ‰) than the authors' estimate. But they choose to interpret this seasonally and isotopically important sample as an outlier, for which I see no basis. This issue must be fixed before the manuscript is acceptable for publication.

I also have a few minor comments, listed below.

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.

L74: The study of Wetzel et al. (2014) was conducted in the laboratory, not in the natural environment.

L117-8: "residence time".

L127-9: This sentence is very unclear (and I think "proceed" is used incorrectly).

L175: units missing.

L190: what are Z and A traps?

L208: I am surprised by these concentrations! What concentrations are the samples measured at?

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).

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?