This paper provides the first silicon isotopic composition of sediment trap samples in the Lake Baikal (the largest and deepest lake in the world). The authors show that the isotopic signal does not vary with depth and is similar to core tops. They interpret it as evidence for the absence of isotopic fractionation during dissolution. They also estimate a fractionation factor of silica production by diatoms. The data are of good quality, original and deserves to be published, however the paper is often frustrating mostly because the discussion is extremely limited (<900 words and 70 lines) and the only two conclusions of the paper are not sufficiently supported by the data. Moreover some key parameters (which are likely to be available) are missing (sediment trap fluxes, mixed layer concentration from May to August...). I recommend that the authors add such data and extend their discussion to strengthen the paper. I provide below a list of minor and main concerns.

Damien Cardinal

P9372, L23. Fripiat et al. 2012 do not provide d30Si from sediment traps but from deep BSi (sampled by in situ pumps). No d30Si-BSi data is provided in Fripiat et al. 2011 and this reference should be deleted here.

P9373, L2. "We document the perfect transfer of d30SiDSi" I would replace "perfect" by "good"

P9375. L21. Even for sediment trap, you have processed at least 0.7g? This seems quite large for sediment trap samples. In addition to silica fluxes (see my comment below), indication of mass fluxes would be useful.

P9379 L25 and p9380 §5.2. The authors assume here that most of diatoms collected during one year are coming from the spring bloom. To check whether it is the case, the authors could provide silica fluxes from their monthly sequencing trap May to August and see whether spring flux dominates summer flux. It would be also interesting to use the progressive shift to heavier d30Si from +0.67 in May to +1.37 pmil in August and convert it into silicon utilisation in the mixed layer. In §5.2 the authors ignore completely such possibility and discuss the May light value as an outlier with limited justification. It would be also valuable to compare the shift of d30Si of exported silica to concentration and/or d30Si of the mixed layer dissolved silicon if available.

P9380 §5.1. On what rationale the authors calculate a mean fractionation factor by simply subtracting <u>pre-bloom</u> d30Si-DSi to the <u>annual mean</u> of d30Si-silica? From their sequential trap we see that the d30Si of exported silica is getting heavier when the season is advancing, so the d30Si of DSi is likely to also vary. According to both Rayleigh and steady state models, fractionation factor could be calculated with simple subtraction only when comparing synchronous DSi and BSi records or if relative utilisation is very low, not a snapshot of initial DSi subtracted to annual average of BSi. E.g. if we calculate epsilon by comparing pre-bloom DSi (+2.28 pmil) with spring BSi (+0.67 pmil in May trap), this gives a fractionation factor of 1.61 pmil, very different from 1.05 pmil. I'm aware that Rayleigh and steady state models are not always suitable and maybe I missed

one point in the authors' reasoning. They should provide more justification / discussion on isotopic fractionation rationale behind their approach.

P9381 line 10. I agree that the d30Si signal is nicely transferred from the upper water column to core top sediments and that this supports that no significant isotopic fractionation is occurring. However, this may be due to the absence of fractionation factor during dissolution (as the authors prefer to interpret it) and/or because of limited silica dissolution during its transfer by settling. This latter hypothesis is not discussed except when considering core top and assuming that these 2 samples have gone through a significant dissolution. The authors could simply provide silica fluxes on their open sediment traps and show how much silica has been dissolved from 50 to 1400m. If a significant part of upper silica flux has been dissolved, then, this would prove without doubt the absence of isotopic fractionation during dissolution in their samples.

Table 2.

- should be d30Si-BSi instead of DSi.
- I do not understand why only some analyses are provided along with 95% confidence level (those with 1 as superscript). What then does mean uncertainty of other data?
- The authors should provide BSi fluxes to develop the interest of the paper beyond just a comparison between surface DSi, trap BSi and core tops. This is required to see whether dissolution (with or without isotopic fractionation) takes place from 100 to 1350m but also for seasonal signal (May to August).

Figure 2. The DSi concentration profiles are very unusual: (i) there is large variability between the two samplings (strangely not reflected in d30Si) (ii) on one profile DSi content decreases with depth, (iii) on the other, the shallowest sample is particularly low. Too little discussion is given in the manuscript on such atypical profiles.