

Dear Editor,

Please find below the detailed response of the authors to both reviewers, and the comments from Dr. K. Hendry. The authors would like to thank the reviewers for their comments and where possible they have tried to take them on board. Where changes have been made, new line numbers and Figure numbers have been provided in the responses below. Furthermore, where the authors feel that they have not been able to take comments on board, they describe in detail their response to the respective reviewers.

Please note that there is now a new Figure 2 on the suggestion of microscopy images of diatom isotope samples. Figure 3 is now therefore re-named Figure 4 and contains new total dry mass sediment flux (also added to Table 2) data and Figure 5 is new, displaying sequencing trap total dry mass sediment fluxes.

In principle, the authors have more fully outlined the main constraints of the data set (namely the absence of $\delta^{30}\text{Si}_{\text{DSi}}$ and $\delta^{30}\text{Si}_{\text{diatom}}$ monthly data), which was in particular picked up by Reviewer 2. These should now be clearer to the reader. As such, the authors argue that the data set provides a snapshot of modern day diatom fractionation factors (in particular ϵ_{uptake}), given the constraints of the data presented. More importantly, we argue that the data highlights the potential to apply stable isotope reconstructions in Lake Baikal, due to the findings that relate to the absence of diatom dissolution derived fractionation ($\epsilon_{\text{dissolution}}$) down the water column and surface sediments.

In relation to reviewer 1, the authors have tried to address some of the more pertinent issues that they raise. However, in some instances the authors felt that some of the comments were already addressed in the existing manuscript and they did not feel further repetition would be a worthwhile addition as reference in many instances is made to existing review literature within a now well-established discipline. The authors hope that the Editor will accept this stance.

We would like to thank the reviewers for their time and considered comments and we hope that they, and the Editor, will accept our revised version of the manuscript.

Many thanks,
Virginia Panizzo and co-authors.

Author's reply to reviewers:

Reviewer 1

Comment 1, “the main objectives are not entirely clear”

Reviewer 1 raises a number of comments, which the authors feel have a common theme. Reviewer 1 suggests that the main objectives of the study are “not entirely clear”. While the authors would like to thank the reviewer for their comments, they would argue that the main aims of the paper are clear. However, we have now added a further section at the end of the Introduction (lines 91 to 111), which outlines in more detail the importance of this research (please also refer to comment 3). Furthermore, the main objectives of the manuscript are also detailed here in key bullet points, thereby addressing this comment.

Comment 2, “define the terms precisely”.

With regards to defining terms, the authors feel that these terms are already well established and known within the scientific community and reference is made to the key literature where they are fully defined. Within the manuscript the authors refer to key leading papers (e.g. for ϵ_{uptake} : De La Rocha et al., 1997; Fripiat et al., 2011; Milligan et al., 2004; Varela et al., 2004; $\epsilon_{\text{dissolution}}$; Demarest et al, 2009; Egan et al., 2012; Wetzel et al., 2014) (lines 60-68 and lines 72-81), which fully explore and define these terms.

However, some additional text has been added in the Introduction now to help clarify this further. ϵ_{uptake} has also been more fully defined in the discussion section (lines 332-364), picking up on comments by Reviewer 2 which discuss some of the limitations of the data set presented (e.g. absence of monthly DSi and BSi data). Where more full definition cannot be derived (e.g. detailed modeling via closed and open system approaches and respective equations provided) due to these limitations, this is now discussed. We hope this will also address this comment of reviewer 1.

Comment 3, “provide more background information on $\delta^{30}\text{Si}_{\text{diatom}}$ as a palaeo proxy”

While the authors would like to thank the reviewer for their comments and while we understand this suggestion we do not feel that this would be a very valuable addition to the manuscript as the main aim (lines 105-111) here is to identify contemporary fundamentals of the proxy, rather than its application per se. Reference is made in the text to review papers that have touched at length upon the application of this method as a palaeo proxy (De La Rocha, 2006; Hendry and Brzezinski; Leng et al., 2009; Tréguer and De La Rocha, 2013).

Within the introduction (and as one of the main objectives of the manuscript) we discuss the main limitations of the proxy, namely being able to constrain fractionation factors associated with biomineralisation (ϵ_{uptake}) and dissolution ($\epsilon_{\text{dissolution}}$). Of particular importance, reference has now been made to Sutton et al (2013) (lines 67-68), which highlights the importance these studies have in addressing these key limitations and addressing this comment. Some of these key limitations of the data set (and method) presented have also been added in

the abstract (lines 33-41) and conclusion to make it clearer to the reader (426-430) as well as in sections 5.1 and 5.2.

Comment 4, “be clear about their definitions”

As outlined in our response to comment 2, further text has now been added to address this, although we appreciate that full equations are not provided in the text (essentially as we argue a snapshot estimation of fractionation factors as we cannot fully constrain these processes via the closed or open system modeling e.g. Varela et al, 2004 and De La Rocha et al, 1997). For example, in Section 5.1. We would refer to Editor/reviewer to our response to comment 2 for further information.

Comment 5, “provide some context to why the data are relevant for the development of a paleoproxy”

Given the comments from reviewer 2, some additional text has been added to sections 5.1 and 5.2. In this text, we highlight more fully some of the limitations of the data set provided in enabling conclusive estimations (in situ) of ϵ_{uptake} and $\epsilon_{\text{dissolution}}$. The authors feel that this also addresses comment 5 of reviewer 1. In addition, further text was added to the end section of the Introduction (lines 91 to 111; see response to comment 1 also) which outline in more detail the importance of this research in addressing key principles in the development of $\delta^{30}\text{Si}_{\text{diatom}}$ as a palaeoproxy.

Reviewer 2 (Damien Cardinal).

P 9371, L23

This is a valid comment and the reference to Fripiat et al (2012) has now been amended, in addition to the reference of Fripiat et al (2011) being removed. Please refer to new lines 82.

P.9373, L2

The authors have taken this comment on board (line 88).

P9375, L21

The authors agree that this is not very clear. The surface sediment weight is dry weight and reference to this has now been made in the text (line 192). However, there is a similar sample weight as the traps contained a high water content. As trap samples were not dried prior to diatom isotope preparation nor weighed after opal purification, an estimation of their dry mass flux isn't possible. However, total mass dry weight fluxes are available from both the sequencing and open traps and these data have now been included in (Table 2/Figures 4 and 5, Results lines 297-302). Although the reviewer does raise an interesting point, with regards to estimations of BSi fluxes, the authors are unfortunately unable to calculate these. However, some existing literature from the years 1996 and 1997 (Ryves et al 2003), which contains estimations of this is now included in the discussion (lines 325-326).

P9379, 25 and P9380, Section 5.2.

As mentioned above, monthly and annual BSi fluxes are not available. However, data has now been included from Battarbee et al (2005) and Ryves et al (2003) (lines 325-326, 390-402), which demonstrates that *Synedra acus* var *radians* is (at least for the period 1996 and 1997) a spring/summer species (dominating phytoplankton between May to August). As such, with continued summer season diatom growth, the reviewer is correct to highlight the possibility of progressive enrichment in the surface layer DSi pool. The authors have therefore included more of a discussion on this in Section 5.2.

However, due to the absence of monthly DSi compositional data (synchronous with sequencing trap data) this cannot be fully explored. Nor can quantitative estimations be made (via closed or open system modelling) to the degree of DSi utilisation over the season (or indeed variations in ϵ_{uptake}). Therefore, while we now highlight this possibility (addressing the reviewer's comment) we feel we cannot fully constrain and quantify this due to the limitations of our data set.

P9380, Section 5.1

This is again a valid comment. The authors have added some more information to this section in order to comment on the reviewer's points and also more clearly define the terminology applied (refer to Reviewer 1's comments also). However, as mentioned in the above response, synchronous DSi and BSi signatures are not available for the surface layer over the course of the growing season. This would indeed have helped to constrain ϵ_{uptake} more comprehensively, particularly when addressing the above comment with regards to progressive enrichment of the DSi pool in the surface layer.

Instead, the authors detail the limitations of the data set (which we hope also addresses some of reviewer 1's comments). As such we propose to use the data set to estimate a mean spring/summer seasonal ϵ_{uptake} based on a snapshot $\delta^{30}\text{Si}_{\text{DSi initial}}$. While this is a constrained estimation, we feel it at least acts as the first application of the technique in Lake Baikal. Furthermore, it also highlights the importance of this estimation, as sediment archives of diatoms will themselves portray an amalgamation of diatoms that have bloomed throughout the dominant periods of the year (as with trap assemblages). This argument has also been added in the abstract (lines 33-41) and conclusion to make it clearer to the reader (426-430).

P9381, Line 10

The reviewer does raise an interesting discussion here with regards to the transfer of diatoms down the water column, into the sediment record and their preservation throughout. Unfortunately, diatom (BSi) fluxes are not available (see previous two comments) and the flux data presented here is only based on total dry mass fluxes (Table 2, Figures 4 and 5). However, diatom concentration data from the open traps are displayed (Figure 3) which does show a variation in the presence (and/or preservation) of diatoms through the water column. Of particular note are the values from the open trap at 1,350 m where concentrations sharply decline.

To further address this, the authors have included a few more lines that detail more fully the sensitivity of *Synedra acus var radians* to dissolution both during transportation through the water column and into the surface sediments of Lake Baikal (Battarbee et al, 2005; Ryves et al, 2003). This diatom is one of the more sensitive diatoms to dissolution with only 5% being incorporated into the sediment record (Ryves et al, 2003), so we feel confident that some dissolution has likely occurred. We hope that this discussion is clearer and that the conclusions are clear to this end, given the near constant composition of surface sediments and open traps (mean spring/summer compositional data).

Table 2:

1. The mistake has been corrected. Table headings now correctly refer to $\delta^{30}\text{Si}_{\text{diatom}}$, not $\delta^{30}\text{Si}_{\text{DSi}}$.
2. The 95% confidence interval is based on a weighted average of replicate samples when MBC and MEAS values were within analytical error as were multiple sample replicates. In this case, a 95% confidence of the weighted average sample value is given. This has been more fully explained (as per Table 1) in the Table 2 footer.
3. Unfortunately, BSi fluxes are not available. However, total dry mass fluxes are provided (Table 2, Figures 4 and 5; previous Figures 3 and 4). We are unable to quantify exactly the BSi flux however.

Figure 2.

Note, this is now Figure 3 after the addition of the two light microscopy images.

The authors acknowledge the comments of reviewer 2 in relation to the DSi concentration data. These are data that were collected 9 days apart from each other in March 2013. Concentrations were analysed via ICP-MS (data presented in this manuscript; Table 1) and via spectrophotometer methodology, both giving similar results (latter data not presented). However, the authors feel that they cannot fully explain the variation in the data. DSi concentration data is similar for the depths 10-50 m. Data below 50 m was not collected for BAIK13-1b and the main discrepancy appears to be in the surface sampling where concentrations are much lower for BAIK13-1b. We feel that we are unable to fully constrain why this discrepancy exists given the data we have. If anything, the authors feel that these data highlight the variability and therefore application of a surface water (1-180 m) weighted average composition for the purpose of providing a DSi initial for estimations (snapshot) of ϵ_{uptake} . As such, they unfortunately do not feel that they are able to discuss in much more detail these data in this instance.

Dr. Katherine Hendry Comments:

1. The authors also feel that reference should be made to the work by Sutton et al (2013). Particularly given its findings with regards to diatom species dependent fractionation effects. Please refer to lines 67-68.

2. Light microscopy mages have now been added (x 1000) of the clean opal samples (Figures 2a and b).
3. The error in Table 2 (heading) has now been amended.
4. A more full explanation has now been made with regards to the 2SD errors in Table 2. Please refer to the footnote.