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Interactive comment on "The pH dependency of the boron isotopic composition of diatom opal (*Thalassiosira weissflogii*)" *by* Hannah K. Donald et al.

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We thank the reviewer for his comments and in particular for his recognition that, while this manuscript is a start of the exploitation of boron isotopes in diatoms, it is not the final word on the matter. We respond to each of his comments in turn below.

RC1: "I had the opportunity to analyze cultured T. weissflogii samples for their boron isotopic composition using LA-MC-ICPMS about 7 years ago. On average those analyses resulted in d11B of 14.0 +- 1.1 (1sd). Unfortunately, the results appeared too imprecise to be useful and never got published... Nevertheless, the significant difference of both, own data and those reported in this manuscript, is quite striking to me."

This is indeed quite a difference, but without more detail it is hard for us to critically evaluate this observation. However, we would like to point out that: (i) we carried out an extensive cleaning protocol to remove residual organic material; (ii) we carried out an extensive investigation of our protocol including ensuring little to no boron was lost during the purification process; and (iii) our standard addition tests support the conclusion that our d11B analytical method is accurate. It is also perhaps worth noting that all published d11B measurements to date are also isotopically light, like our results – though we acknowledge that this is a rather limited dataset to make such comparisons. In the future we would welcome engaging with the community to further explore the analytical accuracy of d11B in opal-matrices by various analytical techniques.

RC1:"I would like to see in an additional figure a direct comparison of the [B] vs. pH systematic reported by Meija et al. (2013) and this study."

This was included in the original manuscript as a supplementary figure. Given this comment (and a similar one by reviewer RC2) we will bring this comparison into the main text.

RC1: "The authors suggest the differences may be due to the use of LA and conventional ICPMS. I do not think, the LA results published by Meija et al. (2013) are inaccurate."

This is not actually what we say in the manuscript, we were careful not to apportion cause and instead we said the following: "In detail, however, our concentrations are around 2-3 times lower than Meija et al. (2013), perhaps due to the different analytical methods used (laser ablation ICP-MS vs. solution here. . .".

RC1: "So, this would indicate three possibilties: a) samples for the older LA study had not been cleaned sufficiently (which I doubt strongly) b) the sample preparation used in this study resulted in a loss of boron or c) some details in the culturing setups resulted in this observable differences."

Since we have not done a direct comparison of methods for determining B/Si it is hard to determine the specific cause. However, given this comment, we will briefly discuss these possibilities in the manuscript, expanding on the observed discrepancy (in absolute B/Si but not in the relationship between B/Si and pH).

RC1:"I would also be interested to see a figure displaying d11B vs. [B]. From figure 5 it appears there may be a stronger correlation of those two parameters than the ones of each of both vs. pH."

We will include this in the revised manuscript.

RC1: "The model proposed to explain the data (including the -10permill offset during incorporation into opal) is not really satisfying. . . This would need a better, more detailled description,maybe including a schematic figure for a better conceptual understanding."

The incorporation of a schematic figure may work well in aiding the understanding of the model we propose. We are happy to include one in the revised manuscript.

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