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# *Interactive comment on* "Silicon isotopes of deep-sea sponges: new insights into biomineralisation and skeletal structure" *by* Lucie Cassarino et al.

**Anonymous Referee #2**

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Review of manuscript bg-2018-328 submitted to Biogeosciences by Cassarino et al: Silicon isotopes of deep-sea sponges: new insights into biomineralisation and skeletal structure

Cassarino et al. present new silicon isotope compositions from sponges recovered from the equatorial Atlantic, and from the water they were growing in. From this, they can derive the silicon isotope fractionation associated with sponge spicule formation. In general, this falls within the same range as previous estimates. The general interpretation of sponge silicon isotope fractionation is that it is related to ambient dissolved silicon concentrations, and thus can be used as a proxy for silicon concentrations in



the ancient oceans, provided the silicon isotope composition of the water is known. However, many of the sponges analysed by Cassarino et al. depart from the published trend (e.g. Hendry and Robinson, 2012), which they show is related to the type of spicule the sponge produces. This adds nuance to our understanding of this developing proxy, and implies more care should be taken in its application to paleorecords. The authors present two hypotheses for why some sponge taxa differ in their silicon isotope fractionation, though cannot conclusively answer why.

In general, the paper is well written, and data seem of high quality, the figures are generally clear – though could be improved, and the references reasonably complete and relevant. Overall, I think this paper falls within the scope of Biogeosciences and is worthy of publication after minor to moderate modifications, which I describe below.

One issue that could be easily improved is the presentation of the data. Currently, it is not possible to recreate the authors analysis because the spicule fusion degree isn't given in the data tables – this could be easily remedied, and it should be also possible to code the symbols in the figures so one can see where the different fusion levels fall.

More generally, because it was not possible to plot the data myself, I became a bit confused at parts, regarding what the takeaway message should be. Some samples fall off all the versions of the fractionation-concentration regression: P5L20 makes clear that this is related to the degree of spicule fusion, which I interpreted from the introduction in general and P6L28 & P7L2 specifically to be related to the taxonomy of the samples, specifically whether they were hexactinellid or demosponges. But then P8L24 and the residual tests says this is not the case. It seems the deviation from the 'average' sponge is the most useful indicator, which is what the residual plots are showing – but in the end it's unclear whether or not this is related to fusion degree or to the taxonomy. My feeling is that if Fig. 4 was altered to show some measure of deviation/residual, rather than absolute values, and the discussion altered to reflect this, things would be clearer. Otherwise surely the default interpretation of Fig. 4 is that different taxa like to live in different parts of the ocean? Similarly, I stuggled to follow the rationale for the

discussion in section 4.3 – it seems trivial if you have 5 (?) tunable parameters, you can make a model produce any magnitude of fractionation. Or have I missed the point here? I would recommend trying to emphasise the key point.

Finally, I wonder if the authors have given any thought to whether this difference in Si-isotope fractionating behaviour between different sponge types is something that could be exploited rather than avoided in a paleoceanographic context?

Minor comments - this is a non-exhaustive list of small typographic, etc. errors and some comments/questions. P1L5: “ranges” P1L15: “fossils” P1L18: “and references therein” P1L20: “Since sponges rely” or “because sponges rely” P2L5: either “from” or “of”, not both P2L9: An approximate threshold size for distinguishing between micro- and megascleres would be useful P2L24: A reference to Jochum et al. (2017) might be appropriate here P2L29: Phrasing is unclear, P2 Eqn 1: the permil is not necessary. P3L9: Reference to Fontorbe et al. (2016) and/or Fontorbe et al. (2017) might be appropriate here P3L11 “result” P4L5: “to”, not “in” P4L7: How do you know all the lithogenic material was removed? Could it be contaminating the samples? P4L12: “prior to isotopic”, and “induced” P4L15: Has it been tested that the yield, including the washing step, is quantitative? P4: Were any seawater standards analysed? (AHOLA, from Grasse et al. (2017)) P4L21: “spectrometry” P5L20: The sentence starting “the d30Si_spicules. . .” is oddly phrases – it could just say ‘d30Si_spicules and apparent fractionation both increase. . .’ P6L5: Not sure it’s correct to say epsilon can result only from a biological model – epsilon is simply the permil expression of fractionation factor alpha – see Coplen 2011 “Guidelines and recommended terms for expression of stable- isotope-ratio and gas-ratio measurement results” P6L7: I don’t see the relevance of mentioning Rayleigh fractionation here, these are samples from all over the ocean, not a single site evolving through time. P7L1: Or, more generally, that different taxa have different epsilons, Km or Vmax – worth mentioning here? P7L10: “in different ways” P7L18: “from Hendry and Robinson” P7L33: “main” P8L4: “impact the fractionation”. Also, a bit of skepticism about the ab initio calculations might be war-



ranted. P8L17/18: Is there an indication of uncertainty on the ‘thermal analysis’ – are 10% and 15% really different? P8L30 (and elsewhere): ‘Sponge fractionation’ would more correctly read as ‘fractionation of silicon isotopes by sponges’ or something similar. P8L31: This sentence is missing a verb. P9L6: “described” P9L24: “enzymes” P9L28: “breaking of bonds”

Figures and Tables Figure 1: I guess the colours represent bathymetry – a color bar would be nice, and perhaps another panel showing a cross-section of dissolved silicon concentrations along the sampling transect.

Figure 2, 5 and 6: It would be convenient to show the different spicule types, perhaps color coded somehow, as in figure 7. I also notice that an outlier from Hendry and Robinson isn’t shown – would recommend mentioning this somewhere to be clear.

Figure 4: Why are the individual data points only shown for spicule fusion degree F5? More generally, wouldn’t it be more useful to plot the residuals as discussed in the main text? Otherwise all this figure could be telling us is that sponges that produce highly fused spicules prefer to live in the deep sea/high Si concentration waters (and Alvarez et al. (2017) recently showed that different groups do seem to have distinct depth preferences). See also comments above.

Figure 5: Would it make sense to plot the residuals for all data for each possible regression? i.e. then one could see where the new data sits with respect to the previously published data. Also, it would good to see a justification for an expression of this form being chosen rather than e.g. linear fits, power laws, etc.

Figure 7 caption: Atlantic.

Table 2: The recent data from López-Acosta et al. (2018) could be incorporated here.

Figure A1: Could the global data compilation also be presented here? In the caption, ‘ambient’, not ‘ambiant’.

Table A1: For this work to be most useful/reproducible, this table should include the

class of fusion degree each sample has been assigned to. I would have like to plot the data myself but was unable to.

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