

Interactive comment on “Latitudinal variations of $\delta^{30}\text{Si}$ and $\delta^{15}\text{N}$ signatures along the Peruvian shelf: quantifying the effects of nutrient utilization versus denitrification over the past 600 years” by Kristin Doering et al.

Anonymous Referee #1

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Latitudinal variations of $\delta^{30}\text{Si}$ and $\delta^{15}\text{N}$ signatures along the Peruvian shelf: quantifying the effects of nutrients utilization versus denitrification over the past 600 years

Doering et al.

The authors present some new diatom $\delta^{30}\text{Si}$ records from the last few centuries from three cores off the Peruvian upwelling margin. These recent records are hard to come by, and the data are of good quality – also, the combination of different isotope systems to probe the decoupling of N and Si cycles is an interesting and useful approach (and

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one that I think is underused in the community). The paper is generally written well, and structured clearly. As such, I think that these records should be published, and Biogeosciences is an appropriate outlet.

However, I have a few reservations about the (potential over-) interpretation of the data in places, I would like to see some additional detail in the methods, and I have a few additional minor comments and suggestions.

1. Interpretation of data:

I really like the novel cross-plot approach used in this manuscript, comparing the relative changes between silicon and nitrogen isotopes. I find this approach more convincing than some of the descriptions of the downcore variations in each isotope system individually. Also I wonder to what extent the data available can substantiate the conclusions. I think the novel approach is worthy of publication in Biogeosciences, and this study is a good illustration of what is possible, but I also think that the data description needs to be clarified, and that there are some caveats in the discussion should be included.

In section 3.1. I'm not very convinced by the descriptions – they don't seem to match up well with the plots in figure 3 to me. For example, the on line 193 say that between 12 and 15°S the $\delta^{30}\text{Si}$ have a mean lower value during the LIA than the CWP – however, this really isn't the case for B0405-6, and isn't thoroughly convincing for the other cores either. This statement also hides variability observed within the LIA. There are other examples of this throughout the section when referring to both $\delta^{30}\text{Si}$ and $\delta^{15}\text{N}$. There are also examples of this in section 3.2.2 e.g. lines 379 onwards – at both 12 and 15°S there are $\delta^{30}\text{Si}$ values from the humid LIA that are the same as the modern values (if I've interpreted the grey horizontal bars on figure 3 correctly). Please make sure that your words fit the data.

The authors use -1.1 per mil as a fractionation factor, but there is, in fact, a large range in this fractionation factor. The authors use this value in their calculations (line 261)

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but how does the uncertainty on this value influence the findings? Perhaps the authors could think about some sensitivity studies?

2. Methods:

There is no mention in the manuscript about the uncertainties that we have about the fractionation factor of silicon isotopes during uptake by diatoms (see comment below). It is possible that the downcore variations are driven by diatom species differences (I'm not saying that they are – it's just a possibility). This possibility can be readily dismissed by including information about downcore species differences. Ideally, diatom counts would be done on the separated and cleaned material (mentioned in lines 143 onwards). However, if this isn't possible at this stage (i.e. there is not cleaned material remaining), then perhaps the authors could at least plot their downcore isotope variations relative to the diatom abundance data mentioned on line 181 (Fleury et al., 2015)? This would at least give some indication of whether or not species changes are driving the isotope variations.

Lastly, there is no real mention in the methods section about how the sampling was carried out with respect to the fine-laminations (line 132). Were the samples taken from individual laminations? Was there any possibility of signal aliasing? Given the discussion about resolution in the manuscript later on (e.g. line 209), I think it would be valuable to clarify the sampling resolution upfront in the methods section.

3. Minor comments:

The title is appropriate for the contents of the paper. The abstract is a generally good, concise summary, although the authors should make it clear in the abstract that it's only some of the $\delta^{30}\text{Si}$ data that are new to this study. I didn't glean initially that the $\delta^{15}\text{N}$ data were published, and was confused to start with as to why there was no $\delta^{15}\text{N}$ methods section!

The references are generally good. However, in the introduction, the authors should at

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least mention some of the caveats associated with diatom $\delta^{30}\text{Si}$ interpretation, namely the possibility of species specific fractionation (e.g. Sutton et al., 2013) and dissolution (Demarest et al., 2009). See comments above regarding species specific fractionation; dissolution impacts on $\delta^{30}\text{Si}$ is more challenging to investigate as there isn't agreement in the literature about how big the dissolution signal might be (Egan et al., 2012; Wetzel et al., 2014) – however, I think at least a sentence should be included to note it as a possible complicating factor.

On line 91, the authors should be more specific than “high amounts” – are you referring to high concentrations, fluxes, or both?

What do the +/- signs on lines 97 onwards represent?

One line 114, the authors could add a few words to explain why the steady-state system is appropriate here. This arises again later in the manuscript, but I think it would help to clarify the choice here as well.

On line 138, the authors should remove “in study of”.

The sentence on line 178 is not complete – please rewrite. Also the short paragraph on line 186 onwards seems a little misplaced – I'd suggest the end of that section is rephrased.

Line 290: please avoid using “a bit lower” – rephrase.

Line 352: I'm not sure what you mean by “horizontal alignment” – could you please clarify?

Line 311: Is there no means of assessing changes in downcore phytoplankton assemblages, as a comparison to the modern data from Sanchez et al? Biomarkers?

Figure caption 4: The caption points towards figures c-e, when they should be figures b-d.

Figure 6: The fonts are too small in places.

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Additional references:

Demarest, M.S., Brzezinski, M.A. and Beucher, C., 2009. Fractionation of silicon isotopes during biogenic silica dissolution. *Geochimica et Cosmochimica Acta*, 73: 5572-5583. Egan, K. et al., 2012. Diatom silicon isotopes as a proxy for silicic acid utilisation: A Southern Ocean core top calibration. *Geochimica et Cosmochimica Acta*, 96: 174-192. Sutton, J., Varela, D., Brzezinski, M.A. and Beucher, C., 2013. Species-dependent silicon isotope fractionation by marine diatoms. *Geochimica et Cosmochimica Acta*, 104: 300-309. Wetzel, F., de Souza, G. and Reynolds, B., 2014. What controls silicon isotope fractionation during dissolution of diatom opal? *Geochimica et Cosmochimica Acta*, 131: 128-137.

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