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Climate of the Past Discussions

CPD

Interactive comment

Interactive comment on "Nutrient utilization and diatom productivity changes in the low-latitude SE Atlantic over the past 70 kyr: Response to Southern Ocean leakage" *by* Katharine Hendry et al.

Anonymous Referee #2

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The authors present new Si isotope data from core GeoB3606-1 from the Benguela Upwelling System covering the past ~70 kyr, since MIS 3-4, which is influenced by variations in water supply/leakage of nutrient-rich waters from the Southern Ocean via Antarctic Intermediate Water. This is generally a very interesting paper. As nicely outlined in the manuscript, leakage of nutrient-rich waters from the Southern Ocean has been studied and discussed widely in the past in order to explain variations in CO2-drawdown during cold climate periods. However global records between ocean basins and regions vary widely, some indicating addition of nutrients and stimulation of primary

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production in the lower latitudes, whereas others do not. Eastern boundary upwelling systems, and here especially the Benguela upwelling system, are ideal regions to study leakage theories, because it receives direct water supply from the Southern Ocean via shoaling/upwelling of nutrient-rich Antarctic water. This nutrient rich water is source for intense primary production. And diatoms in upwelling regions have been shown before to react very sensitive to changes in nutrient supply, whereby stable Si isotopes can be used to reconstruct relative utilization of the available nutrient pool. Therefore, the study presented here is of very wide interest and can potentially provide highly useful information for paleoceanographers and paleoclimate studies providing important information on the response of the Benguela upwelling region to nutrient leakage from the Southern Ocean and its effect on the global climate system. Specifically, the paper presents Si isotope data from two large diatom species, which were hand-picked from the sediment samples in order to prevent influence of changing diatom assemblages due to variable environmental conditions and potential species-specific fractionation effects. Furthermore, the Si isotope data are interpreted in context of existing records for bSi content/accumulation, sea surface temperatures (upwelling intensity) etc. Although I generally appreciate the study and the applied methods very much, I have two major concerns about details of the method and interpretation of the Si isotope data that need to be addressed before publication in Climate of the Past.

Major Comments: The authors present a “species-specific” diatom Si isotope record, consisting of two diatom species (A. curvatulus and C. radiatus), which, as far as I understand, grow under and therefore represent different environmental conditions concerning nutrient status, temperature preference, etc. The Si isotope data are, however, presented as a single isotope record. The diatoms were hand-picked from the record. I generally think is a very good approach for the area and the research question, but it is completely unclear to me how the authors “mixed” the specimens in each sample, i.e. did they made sure to always have the same exact number of specimens from each of the two species in each sample? Could there have been an imbalance according to variations in the relative number of specimens or variations in their size/silicification

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in each sample over time? The authors acknowledge themselves that we have indications for species-specific fractionation effects for Si isotope in diatoms. Also, the fractionation behaviour for diatoms might change significantly under variable environmental conditions (high/low nutrient concentrations in the surrounding seawater with effects on the influx:efflux ratio of dissolved Si for single diatom cells, potentially affecting the preserved Si isotope ratios).

The total variation in the d30Si record is 3 per mil, ranging from -1.5 to +1.5. This range is huge compared to any other diatom record published so far. Admittedly, single-species record can have larger variations compared to mixed-species, where variations are rather flattened out. However, to me the explanation/interpretation of this range, and especially the negative values, is quite superficial. The authors assume a constant diatom Si isotope fractionation of -1.1 per mil, and also acknowledge larger possible species-specific fractionation. That's okay. However, assuming the -1.1 per mil fractionation, the most negative values in the record would imply surface water d30Si values of -0.4 per mil. This is completely unrealistic. Even in combination with a larger isotope fractionation between diatoms and seawater and other environmental effects in the upwelling region (only mentioned very broadly), I don't see how this is possible. Please explain.

Additional minor comments: L. 116: delete "(multi-collector-inductively coupled plasma mass spectrometer)", abbreviation has been introduced before L. 198: "Quantifying changes to the . . ." I'm not sure that I see the "quantification" in this section. L. 200: is river input a significant source for dSi in the BUS? Also, what about dust from the arid hinterland? Dust storms towards the upwelling region/open ocean are a regular seasonal feature in the region at least nowadays. Any proposed changes over time there? Could there be variations in Si utilization due to changes in Fe fertilization from dust directly in the region, not just leakage?

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