

In their manuscript, Dr. Closset and colleagues present an impressively large dataset of silicon stable isotope data ($\delta^{30}\text{Si}$) of both dissolved and particulate silica from the region of natural iron fertilization around the Southern Ocean island of Kerguelen. Analytical and sample processing methods are very well documented and data quality is discussed in detail, providing confidence that the presented data are of high quality.

Using their data, the authors examine the isotope dynamics of Si production and export in this oceanographically complex and biogeochemically interesting region. They show systematic biogeochemical and isotopic evolution in the iron-fertilised region examined and improve previous $\delta^{30}\text{Si}$ -based estimates of Si supply to and export from the surface mixed layer. Comparison of collocated dissolved and biogenic silica allows the authors to study the Si isotope dynamics of the region in some detail, and they discuss the complex behaviour they observe. Finally, analysis of samples collected by a year-long deployment of a sediment trap provides insights into the seasonal cycle of export in this region and how this is reflected in the isotopic composition of sinking opal.

All in all, I find this holistic isotopic view of Si cycling around Kerguelen interesting and certainly worthy of publication in *Biogeosciences*. The study is well conceived, and its aims are clearly laid out and tackled. However, I found the Discussion section rather hard to read, and I think that the manuscript would profit from an effort to make the profusion of stations and data more easily understandable for the reader; without this, it is hard to understand the manuscript's main points.

Scientifically, I have three main issues with the manuscript: (a) there are some conceptual inconsistencies in the interpretation of mixed-layer isotope systematics (Section 3.3.3) that must be corrected, (b) there are errors in plotting depth profiles of $\delta^{30}\text{Si}_{\text{BSi}}$ (Fig. 3h) that may have affected the authors' interpretation of the data, and (c) the equations used to calculate quantitative results in Section 3.3.1 and 3.3.4 are not given. I detail these points below, together with more minor points that the authors should also consider while carefully revising this manuscript.

General comment: Manuscript organization

Currently, the manuscript requires very careful reading (and re-reading) in order to understand the authors' argumentation and get a sense of the various settings. I think the authors would do well to try to make the data more accessible to readers unfamiliar with the details of KEOPS-2. A few suggestions:

- I would spend some time at the beginning of the Discussion (rather than in the Methods section) to introduce the groups of stations and their oceanographic settings, and make sure to refer to these settings consistently (currently, the text sometimes refers just to station numbers, sometimes to stations "north of the PF" even though this has not been referred to before, etc.). Such clear and consistent nomenclature is vital for the reader to be able to follow the authors' reasoning easily.
- The groupings used in the text could be shown schematically on Fig. 1, i.e. show the "HNLC" region, the "Plateau" region and the "Meander" region in different shadings. Also, Fig. 1 would benefit from a more schematic representation of the flow, especially together with frontal positions.
- The timing of sampling is key to the authors' seasonal interpretation of the data but is not referred to very clearly: e.g. given their similarities in WW Si properties and use as the seasonal Si source, it is very important to know that TNS06 and A3-1 were sampled just 2 days apart. But this information is only visible in the supplementary information.

Major comment 1: Interpretation of isotope systematics

In Section 3.3.3, the authors discuss the mixed layer isotope systematics of the plateau and meander regions on the basis of the Rayleigh and steady-state models. Clearly, the isotopic behaviour in this region is complex, and the authors' discussion does justice to this, taking into consideration the various processes that may be affecting the isotope dynamics. However, I have a fundamental problem with the interpretation that BSi follows Rayleigh systematics whilst DSi does not – this simply cannot work. The evolution of the Rayleigh product (instantaneous or accumulated) is intimately tied to the evolution of

the DSi pool – i.e. the product cannot follow the Rayleigh curve unless the DSi pool *also* experiences the strong fractionation associated with closed-system systematics. Thus whilst there is clearly complexity in the isotopic dynamics here, the authors interpretation of “decoupled” systematics (Rayleigh for BSi, steady-state for DSi) cannot be correct. Could the results instead be due to a difference in the timescales over which the two sampled pools are integrating? Regardless of the specific reason, the author’s analysis shows that whilst the model framework is useful for interpreting isotopic systematics, it also has limitations when applied to the real ocean, and I would recommend a more careful interpretation that is less strictly dependent on conformance to idealised model curves.

Also, there is an inconsistency between the text and the caption of Fig. 4 regarding the Si source considered for the analysis. The text (L28 on p12) mentions “averaged Plateau-WW”, whilst the figure caption mentions “TNS6-WW”.

Major comment 2: Error in plotting $\delta^{30}\text{Si}_{\text{BSi}}$ data

Based on the data presented in the supplementary material, the depth axis of Fig. 3h appears to be wrong. This does not allow a direct comparison of BSi concentration and isotope composition for the meanders stations and makes it hard to assess the author’s interpretation of these data in Section 3.2.3, since it is not clear whether their interpretation is based on the faulty figure or not. This must be corrected.

Regardless of this error, the authors’ argument for accumulation in the deeper ML or just below does not seem to be borne out by the data, given that BSi concentrations decrease strongly below 100m. Given the many references to WML and ML in the discussion here and elsewhere, I think it would be useful to show physical parameters (or at least MLD) for the stations. Barring that, it is difficult for the reader to follow the authors’ reasoning.

Major comment 3: Calculation of quantitative results

In Sections 3.3.1 and 3.3.4 respectively, the authors calculate seasonal net BSi production and estimate surface DSi concentrations from isotope data, without giving details of the equations used (Section 3.3.1) or the assumptions made (Section 3.3.4) in order to achieve the results. Whilst I appreciate that the authors have done this before in other publications, it would be much better if their methods used for this study were documented here as well.

Minor comments

- *Meander WW evolution*: In Section 3.3.2, the authors discuss the evolution of WW $\delta^{30}\text{Si}$ in the meander region. Whilst it is possible that diapycnal interaction is responsible for this evolution as the authors argue, clearly the possibility of lateral interactions across the PF must be considered as well. After all, the intense mesoscale activity at the front acts strongly to stir/exchange tracers *laterally/isopycnally* as well as mixing them diapycnally (e.g. Dufour et al. 2015, *J. Phys. Oc.*, 10.1175/JPO-D-14-0240.1) . As the authors argue for the ML in Section 3.2.3, lateral mixing of Si-poor PFZ subsurface waters with WW could thus well produce the evolution observed. In Fig. 5, it would be interesting to compare the evolution expected from such lateral subsurface mixing with that expected from diapycnal mixing.
- *Strange HNLC station*: In my opinion, not enough reference is made to the fact that the HNLC station has a unique isotopic behaviour – its $\delta^{30}\text{Si}_{\text{DSi}}$ evolves to very heavy values whilst $\delta^{30}\text{Si}_{\text{BSi}}$ is very light, suggesting a very different expression of fractionation here. The authors explanation of the $\delta^{30}\text{Si}_{\text{BSi}}$ data from this station (L25-21, p9) is unclear and thus not convincing.
- *Silicate pump*: In Section 4 (L7-10, p19), I do not understand how the silicate pump could lead to low BSi production despite high primary productivity. Should it not be the other way around if silica is more efficiently exported than organic matter? As a mechanism of decoupling Si, N and C, I am surprised that no mention is made of the Fe-related plasticity in Si:N ratios (as shown by the canonical 1998 papers by Sunda and Huntsman or Takeda), which I think would explain direction of change better.

Various minor comments:

- *UCDW/LCDW identification* (L3-5, p5): It is UCDW that is associated with the O₂ minimum, not LCDW (e.g. Talley 2013, *Oceanography* 26, 80-97)
- *Sections 2.2 and 2.3* both have the same heading. These should be “Sample collection” and “Sample preparation” respectively. The sub-sections of Section 2.3 are numbered incorrectly. On L6, p6, could there be an error in the units given for NaOH concentration?
- *Section 2.5*: it would be better to refer to “full external” rather than “global” reproducibility.
- *Section 3.1*: Cao et al. 2012 is not a Southern Ocean study.
- *Section 3.3 and Fig. 3*: The comparison with late summer conditions during KEOPS-1 is useful, but Fig. 3 is confusing since it combines the seasonal signal (i.e. KEOPS-1 vs. KEOPS-2) with a regional/biogeochemical signal (i.e. HNLC versus Plateau). The figure should separate these regions into separate panels and also show concentration profiles to provide context for the isotope data.
- *Section 3.3.4*: It would be good to mention once again that the sediment trap is located at Station A3. On L33, p16, I don’t understand why the ML concentration estimated from sediment trap data is compared with mid-November surface concentrations at station E5, rather than with mid-November concentrations at the station where the sediment trap is located (A3-2). Concentrations here are about 2x higher than estimated from the sediment trap $\delta^{30}\text{Si}_{\text{BSi}}$ data. Also, on L29, p17, it should be mentioned that the high values seen in late summer cannot be explained by the steady-state model.
- Finally, although in general the manuscript’s English is good, it would profit from being proof-read by a native English speaker in order to iron out small grammatical errors.