

This study presents an extremely large and valuable $\delta^{30}\text{Si}$ dataset of good quality. It covers dissolved silicon and biogenic silica on complete water column profiles, but also exported biogenic silica using sediment traps. This study significantly improves our understanding of the silicon cycle in the Southern Ocean and the manuscript is well written. I recommend publication only after minor revisions.

My main concern is about the utilization of the steady state model ('Open') to describe a system where there is a seasonal nearly-complete DSi consumption (Kerguelen Plateau). This model assumes steady state conditions in the mixed layer and, therefore, no changes in concentrations with time. Authors also look at figure 4 to infer which models fit the most with the observations. I'm not sure that the level of precision is sufficient to distinguish between models. Especially if we take into account the precision on both the fractionation factor and the Si-source: on figure 4, the former is shown only for the steady state model and the latter is not taking into account.

In late summer, the authors also infer that the biogenic silica pool is a mixture between new (high- $\delta^{30}\text{Si}$) and old (low- $\delta^{30}\text{Si}$) biogenic silica (from figure 4). But late summer DSi $\delta^{30}\text{Si}$ is low (figure 4), and actually not significantly different than biogenic silica $\delta^{30}\text{Si}$ (error bars in figure 4). How is it possible to produce high- $\delta^{30}\text{Si}$ biogenic silica from a low- $\delta^{30}\text{Si}$ pool? I agree that biogenic silica $\delta^{30}\text{Si}$ could be explained by being a mixture between instantaneous and accumulated products in the Rayleigh model ('closed'). But in regard of the figure 4, assuming a constant fractionation factor, such high- $\delta^{30}\text{Si}$ biogenic silica requires a DSi pool being significantly higher than described with the steady state model at any time, or than the observations.

Minor comments:

Pages 8-9 Lines 32-1: What is real WW Si properties? As written, it is not clear.

Page 10 Lines 28-29: Not sure to understand why the location of the meander being upstream of the Kerguelen Plateau is indicative of a delay in the initiation of the bloom.

Page 11 Lines 9-11: Such accumulation is not seen on BSi concentration profiles? In addition, an accumulation should imply low biogenic silica $\delta^{30}\text{Si}$, no?

Page 11 Line 30: The authors need to define better the concept behind "new silicic acid" and its relationship with silicon isotopes. As written, it is not clear.

Page 12 Lines 1-2: the northern Kerguelen shelf...(to be added)...*or from the mixing with waters north of the polar front bearing higher DSi $\delta^{30}\text{Si}$ due to the progressive export of low BSi $\delta^{30}\text{Si}$ along the meridional overturning circulation path.* From my point of view (given the circumpolar boundary between APF and PFZ), this should be the dominant driver of such increase in subsurface DSi $\delta^{30}\text{Si}$.

Page 12 Line 18: HNLC area in KEOPS 1&2 are located in different areas, East and West of the Kerguelen Plateau. Are they really comparable? It should be discussed somewhere when you highlight the differences and make some hypotheses on the water sources. For example, HNLC WW during KEOPS 1 appear to be actually more representative (from their geographical locations) of this expected source of waters originating from the South. But I agree that measurements show the opposite, as being more

similar to the HNLC area west of the Kerguelen Plateau. So, I do not really understand how the authors (latter in this paragraph) infer a source for the Kerguelen Plateau WW coming from the South based on these observations.

Figure 3: DSi concentration between KEOPS1 & 2 would be useful here.

Page 13 Line 4: In the next paragraph, the authors indicate that Closset et al. (2014) did not report measurements in the deep silica maximum. There are other studies reporting direct measurements of high D/P in deep silica maximum of the Southern Ocean.

Page 14 Line 11: But not for biogenic silica $\delta^{30}\text{Si}$, following more closely the Rayleigh trends.

Figure 4: Why the error propagation for the Rayleigh model is not shown? If presented, I expect that most of the DSi $\delta^{30}\text{Si}$ would still fit with the Rayleigh model. Especially if you take also into account the uncertainties related to the initial Si-source.

Page 14 Lines 7-22: I like this paragraph. Maybe it would be worth to say that such process is analog of what is observed for DSi $\delta^{30}\text{Si}$ across the ACC meridional overturning circulation.

Page 14 Lines 32-34: The BSi accumulation/export ratio appears to be the main driver. This should be said more strongly.

Page 15 line 16: The authors suggest that late summer newly-formed biogenic silica presents high $\delta^{30}\text{Si}$, but the residual DSi $\delta^{30}\text{Si}$ is low (Figure 4). As being the Si-source, how can you produce high- $\delta^{30}\text{Si}$ biogenic silica from this DSi pool (assuming constant fractionation factor)?

Page 15 Lines 27-29: Why low Si-uptake/supply ratios and a combination of open and closed system (also implying variable Si-uptake/supply ratios) are different approaches? As defined, the open model assumes that Si-supply equals the sum of BSi accumulation/export and residual DSi (implying also low or close to unity Si-uptake/supply ratio, depending of the relative Si-utilization).

Page 15 Lines 29-31: Why the export of biogenic silica will decrease $\Delta^{30}\text{Si}$? It should be the opposite (as discussed previously). As written it is not clear.

Page 15: Lines 31-35: Same comment than above. The authors suggest that late summer newly-formed biogenic silica presents high $\delta^{30}\text{Si}$, but the residual DSi $\delta^{30}\text{Si}$ is low (Figure 4).

Page 16 Lines 19-20: Dissolution occurs in subsurface (Nelson et al., 2002, DSR). This process can therefore also explain the observed trends. The authors should be more caution before completely ruling out the dissolution isotope effect.

Page 18 Lines 27-28: Why here the steady state model assumes, as it should be, no variations in concentrations with time, but it has been previously used to describe a situation over the Kerguelen Plateau exhibiting large variations in DSi concentration?