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Discussions

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Interactive comment on "Exploring interacting influences on the silicon isotopic composition of the surface ocean: a case study from the Kerguelen Plateau" *by* N. Coffineau et al.

Anonymous Referee #2

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General Comments

The authors present new water column dissolved silicon isotope data from the Southern Ocean with emphasize on the Kerguelen Plateau. They find, in agreement with previous studies, seasonally diminished mixed layer dissolved $\delta30Si$ at low Si concentrations. Furthermore, the authors use a variable depth box model to investigate the possible reasons for these low values: water mass mixing, diatom opal dissolution and/or basalt weathering from the surrounding plateau rocks. This is a very interesting approach, which highlights the strong interannual variability that we have to consider when working with the silicon cycle.

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In general, the paper is definitely suitable for publication in Biogeosciences. However, I think the authors should comment on some points that I have listed below.

Specific Comments

A very general thing first: - Unfortunately, the authors do not provide the actual measured data from the CTD stations in a table. This is a general problem in many publications. However, especially when using these data for modeling these data should be presented to allow the reader to try reproduce the results.

Water column $\delta30Si$ (data): - It is nice to see 6 new water column profiles with $\delta30Si$ measurements from the Southern Ocean. However, the main contribution of this study is the model, which focuses on processes in the upper water column around the Kerguelen Plateau. So, it is not clear to me whether it is really necessary to present water column data from >1000 or even >2000m water depth? If yes, please specify why. - The water column $\delta30Si$ from the CTD stations were pooled into distinct water layers (p.11419, L.19-21). Does that mean that really all six CTD stations can be considered to somehow reflect similar effects? I'm not sure if that assumption can be made. With the exception of CTD station 8 all others are from very distant locations somewhere in the Southern Ocean. - Did you use all these data as input into the model? If yes, please explain how representative they are for the Kerguelen plateau. I mean if you use data from station 1 close to Antarctica how representative can that be for basalt weathering on the Kerguelen plateau? - Fig. 3 indicates a range between 0-60μM for surface waters and a $\delta30Si$ range from 1-2.4‰ especially for CTD station 8, which is closest to the Kerguelen Plateau. The authors should specify how exactly they defined their 3 categories (p.11419, L.19) from that large range. - To me it seems that the authors are looking for a single influencing factor on the samples with dSi concentrations <20μM. I'm not sure how reasonable it is to consider basalt weathering for stations/regions far away from the LIP of the Kerguelen Plateau. Is there any reason to assume that other regions in the Southern Ocean are influenced by such a process as well? - I'm a bit confused about when exactly the samples for this study, especially the ones close to

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the Kerguelen Plateau, were taken (which month)? Is it possible to plot all available data from the Kerguelen region (from this study, from De La Rocha et al., 2011; from Fripiat et al., 2011;…) on a seasonal scale in order to show the evolution over time? This could be done in comparison to the model output in Fig. 7, where the authors only plot so far the data from Fripiat et al. (2011).

Diatom production and dissolution: - One major point to be considered: The authors use a constant fractionation factor of -1.1‰ between diatoms and seawater during frustule formation. However, the recent study by Sutton et al. (2013) has shown that this fractionation factor can indeed vary considerably for different (Southern Ocean) species between around -0.5‰ and -2.1‰Å fractionation factor of -1.1‰ might represent a good average, e.g. when working with paleo records. However, when considering seasonal changes over the course of the year, as the authors do in their model, changes in diatom assemblage might cause significant changes in the water column δ30Si in the surface water during utilization but also during dissolution within the water column (with or without fractionation during dissolution). - As there are no information provided on any possible change in diatom assemblage it is hard to estimate whether that might influence the result. It would be good if the authors could provide some estimates on the maximum influence of such changes with their model.

References: Sutton, J. N., Varela, D. E., Brzezinski, M. A., & Beucher, C. P. (2013). Species-dependent silicon isotope fractionation by marine diatoms. Geochimica et Cosmochimica Acta, 104, 300–309. doi:10.1016/j.gca.2012.10.057

Basalt Weathering: - Basalt weathering is introduced here by the authors as one potential mechanism to explain the seasonal variability of δ30Si (decrease during early autumn) with rather constant low dSi concentrations. Certainly, sub-marine basalt weathering should lower the δ30Si, but this is not a seasonal process but should influence the dissolved δ30Si throughout the whole year? I think this is what the authors try to describe on p.11426, L.17-21? - Therefore, basalt weathering can be used to explain average lower than expected ML δ30Si values, but not seasonally deviating values.

Rather, as the authors state in the conclusions, bSi production and dissolution are the driving factors here.

Model inputs and results: - Fig. 5 indicates, to my opinion, a relatively strong mismatch between observed and calculated dSi and especially bSi concentrations. The model indicates much higher bSi concentrations with two "peaks" of bSi production in late spring and early autumn, whereas the measured values are lower with only one slight increase in late spring. What is the reason for this? Is it a sampling artefact? - Fig. 7: The match between data from Fripiat et al. and the model results is, with exception of St. B5, not very good. On the other hand, St. A3 should represent the plateau region very well (p.11428, L.4) but does not fit to the model output at all. Any reason/explanation for this? - I'm not sure if it plays a role, but how much of the bSi produced was allowed to dissolve within each layer of the model? This might play a role at least in the simulation where diatom dissolution is associated with isotope fractionation. - The authors state that fractionation during dissolution of bSi had a visible effect in the model "lowering the δ30Si of the dSi in the ML by 0.2‰…" which should be taken into account for paleoceanographic reconstructions because of the risk of underestimating the extent of full bloom growth (p.11429, L.1-12). I would like to state here that the presented ~0.2‰ (diminishing of the bSi signal, which represents ~13% reduction in the amplitude of the seasonal δ30Si signal) hardly exceeds the uncertainties of the δ30Si measurements. The authors state (p.11411, L.5-9) that their long-term precision on the measurements is $\pm$0.07‰ (1σ sd), which is good and comparable to other laboratories, but not good enough to state that variations of 0.2‰ are significant.

Technical Corrections - Overall the text is very well written, but there are some very long and complicated sentences from time to time. - P.11421, L.17: what exactly do the authors want to say with "an assumption that is unlikely to be true"? Please specify if necessary. - P.11423, L.20: Fig. 8b instead of 7b? - Fig. 1: Please show the flow path of major currents (ACC) around the islands and/or the location of the Antarctic Polar Front. - Figure caption to Fig. 2: what is DSibst? - Fig, 4, 7, 8, 9: It is correct

to write only "30Si (‰" on the y-scale, however, the convention is rather to use "δ30Si" instead.

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