General comments:

This study by presenting a new elegant δ^{30}Si model allows a better understanding of the processes setting the δ^{30}Si in the Southern Ocean. Such model presents the advantages (i) to use a varying mixed layer depth, describing realistically the Southern Ocean dynamic, (ii) to simulate realistically a seasonal productivity (production, mortality, dissolution, and export), and (iii) to assess the complete annual variation (winter, spring, summer, autumn). However, we need to see the sensitivity to the different processes, e.g., with and without winter BSi production, in order to assess the need of such processes to describe the observations. In the current state of the manuscript, the readers have to take as granted the only presented run. Also, I'm not really convinced that the model fits enough with the data (from De La Rocha et al., 2011; Fripiat et al., 2011a) and some assumptions made are not in agreement with previous studies without those being discussed:

(i) Except for the station B5, the BSi δ^{30}Si (stations A3 & B1) are significantly higher (>0.5‰) than simulated. Measured BSi δ^{30}Si at the end of summer are actually similar to those simulated during bloom condition; this would indicate that post-bloom processes do not affect so much BSi δ^{30}Si (Fig. 7). We cannot see for these two stations a decrease in BSi δ^{30}Si due to the winter growth of BSi from a lower DSi δ^{30}Si, as proposed by the model. Need to be discussed.

(ii) Station B5 has been characterized by previous studies to be still in the blooming phase (not post-bloom as suggested in this study; e.g. e.g., Mongin et al., 2008; Mosseri et al., 2008; Trull et al., 2008). Such difference with A3 and B1 stations is shown with the DSi concentration, being > 10 and 1-2 umol l^{-1} for B5 and A3-B1, respectively. B5 data should be therefore more on the left side (x-axis) of the Fig. 7. Need to be discussed.

(iii) The DSi δ^{30}Si from De La Rocha et al. (2011, Autumn) are significantly lower (~1.8‰) than simulated (>2.5‰), and are not presented on Fig. 7.

(iv) The δ^{30}Si values for the 'above plateau' winter water (= 1.6‰) are in disagreement with the assumptions/measurements of Fripiat et al. (2011a) and de Brauwere et al. (2012). In the latter studies, the authors reported significantly different DSi δ^{30}Si for the winter water on the plateau (1.9‰) and out the plateau (1.5‰). Ctd 8 (new data in this manuscript) is more similar (1.4‰) to the 'out plateau' situation, in agreement with its location (water column depth > 2000m; South of Heard Island). The authors should take the winter water representative of the 'above plateau' situation (1.9‰), or at least discuss why they decided to do differently. In addition, Kerfix cannot really be considered as an 'on plateau' station for the model-data comparison (water column depth > 1000-1500m).

The discussion about 'what really sedimentary BSi δ^{30}Si is telling us' is really interesting. However given the implications, it needs to be much more discussed/referred. For example, it is well known that in the sediments, the diatom assemblages are not representative of the blooming diatom species (e.g., Smetacek et al., 2004; Crosta et al., 2005). The actual sedimentary δ^{30}Si is therefore not recording the main BSi export production (blooming

species which are dissolved in the water column) but more the 'background' diatom species (large species with slow growing rates but low mortality rates), constituting the bulk of the sediments. As already mentioned and without a sensitivity test to the winter BSi growth, the hypothesis given by the authors is not convincing and the rationale needs to be better developed/referred.

Specific comments:

Abstract and Conclusion: To be rewritten according to the revised text.

Page 11406, Lines 2-3: It would be useful to indicate which zone in the Southern Ocean, e.g., south of the Polar Front, Antarctic Zone, ...

Page 11408, Lines 1-8: I suggest to indicate that the much higher primary production above the Kerguelen Plateau is due to iron fertilization (e.g., Blain et al., 2007).

Page 11408, Lines 9-14: As written, the sentence indicates that the data from Fripiat et al. (2011a) are fully in agreement with the Rayleigh fractionation kinetic, assuming Si-uptake only (or with the steady state equations). This is not true: both the DSi δ^{30}Si and the Δ^{30}Si (= DSi δ^{30}Si - BSi δ^{30}Si) cannot be satisfactory described with such models. This has to be also corrected in Page 11421, lines 6-8.

Page 11410, equations 1 and 2: These equations are the same than in Cardinal et al. (2003). I suggest therefore to remove them (not really useful).

Page 11411, line 16: Somewhere you need to discuss about the fact that Kerfix (model) is not really the 'on plateau iron-fertilized' bloom (data). The water column depth is > 1500m at Kerfix (e.g. Mongin et al., 2008, with satellite chl-a images).

Page 11412, line 5: I would like to mention that it would be great to have a variation in the biological activity across the mixed layer, e.g., to try to simulate a deep BSi maximum as observed in the Kerguelen plateau area (e.g. Mosseri et al., 2008). However, the current configuration of the model is totally fine for this manuscript.

Page 11412, line 16: The winter waters above and out the plateau were significantly different in the Fripiat et al. (2011a) dataset. As 'above plateau' mixed layer data have been used for the model-data comparison, I suggest to use the 'above plateau' winter water values (1.9 ± 0.1‰). see also the comments 'general comments' and ' Page 11425, line 2'. In addition, CTD8 is not really above the plateau (water depth > 2000m). The winter water δ^{30}Si from this station are more similar to the out plateau stations in Fripiat et al. (2011a; 1.5‰), in agreement with its location.

Page 11415, line 1: Beer-Lambert instead of Beer-Lamberg.

Page 11415, line 15: Si:N ratio are variable in the Southern Ocean (e.g., Mosseri et al., 2008, reported such uptake ratios above the Kerguelen plateau (A3 and B1 stations) and there are significantly different, closer to an iron-replete situation). Si-uptake kinetic parameters are highly variable too (e.g., Nelson et al., 2001; Mosseri et al., 2008). I agree that you have to

choose some representative values to run the model but the uncertainties have to be discussed and referred.

Page 11419, Lines 10: CTD8 is characterized by a quite deep water column (>2000m). I'm not sure that we can define this station as 'above the Kerguelen Plateau'.

Page 11419, Lines 21: Some CDW, especially the Upper Circumpolar Deep Water, are found at lower concentration than 90 umol l^{-1} (e.g. Garcia et al., 2005). What about the ctd 5 and 8 in Fig. 3, below 1000m, presenting DSi concentration lower than 90 umol l^{-1}. This should be Upper Circumpolar Deep Water.

Page 11420, Line 7: Samples 58 is not really on the Kerguelen plateau. If yes, this has to be shown on Fig. 1.

Page 11420, Line 18: This is not really all Southern Ocean data, e.g., Cavagna et al. (2011), Fripiat et al. (2011b), and de Souza et al. (2012). It should be interesting to plot all Southern Ocean data because there are no reasons to discard them and they could be used to strengthen your hypothesis.

Page 11420, Line 18: Cardinal et al. (2005) dataset is from South of Tasmania, instead of New Zealand.

Page 11421, Line 18-24: Fripiat et al. (2012) already suggested that such discrepancy between the expected DSi δ^{30}Si, assuming the Si-uptake only, and the observations at low DSi concentration, was due to (i) winter water supply of low-δ^{30}Si DSi and (ii) BSi dissolution (see the abstract), both at the end of the growing season. However, this is true that the latter study has been much more focused on the Δ^{30}Si variation (= DSi δ^{30}Si - BSi δ^{30}Si; see Fig. 3 in Fripiat et al., 2012) instead of spatial DSi δ^{30}Si variation (i.e., the added-value of this study). See also the Fig. 8b in de Brauwere et al. (2012), presenting a simulation of the δ^{30}Si above the Kerguelen Plateau (same data) with a similar shape than Fig. 7 (due to the winter water supply) but with a better fit for BSi δ^{30}Si (see general comments). These previous studies should be cited and discussed here.

Page 11421, Line 22-24: I don't completely agree. If for example, there is 1 umol l^{-1} of DSi in the mixed layer with a δ^{30}Si of 2.6‰ after the bloom (as for A3 and B1 stations): If you add only 1-2 umol l^{-1} of DSi with a δ^{30}Si of 1.6‰ (winter water), the resulting mixed layer DSi δ^{30}Si would be 2.1-1.9‰. The latter being close to De La Rocha et al. (2011) value (1.8‰ with DSi concentration more than 6 umol l^{-1}) and the winter water above the plateau in Fripiat et al. (2011a; 1.9‰). So mixing alone (from 1-2 to more than 6 umol l^{-1}) can change much more efficiently the δ^{30}Si than [DSi], and can explain the difference between late summer (A3-B1) and autumn (De La Rocha et al., 2011) observations.

Page 11422; Line 3: Somewhere in this section, the sensitivity of the model to a deep BSi maximum (as massively observed in Mosseri et al., 2008, on the Kerguelen Plateau, station A3-B1) should be discussed.

Page 11422; Line 3: Somewhere in this section, the fact that Kerfix (water column depth > 1000-1500m) is not the 'iron-fertilized' bloom above the plateau, has to be discussed. Look for example the satellite chl-a images in Mongin et al. (2008), the mixed layer depth in Park et al. (2008), and the winter water properties (e.g., DSi concentration and δ^{30}Si).

Page 11422; Line 3: The sensitivity of the model to winter BSi production/export should be discussed. Such test should confirm or not the need to have a winter diatom growth to explain the δ^{30}Si observations.

Page 11422; Line 12: The BSi concentration above the plateau for the station A3 and B1, post-bloom scenario (from 5 to 20 umol l^{-1}; Mosseri et al., 2008), are larger than the maximum concentration found in the model (5 umol l^{-1}; Fig. 5). Such difference results probably from the fact that the plateau is an 'iron fertilized' area and Kerfix not. This needs to be discussed.

Page 11422; Line 15: Mosseri et al. (2008) for this station (A3, post-bloom) reported Si-uptake rates from 0.1 to 0.4 umol l^{-1} d^{-1} in summer (decreasing to 0.1 umol l^{-1} during the station occupation, January to February). BSi production rates in summer are below 0.2 umol d^{-1} l^{-1} (Ragueneau et al., 2002; Beucher et al., 2004) and should be even lower in autumn (no data). This is well below the average of 0.4 umol d^{-1} l^{-1} simulated in summer/autumn (Fig. 6).

Page 11423; lines 11-12: There are significant differences between the simulation and the observations, as mentioned in my general comments: BSi δ^{30}Si for A3 and B1 stations are significantly higher than simulated; Station B5 is still blooming, so should be more on the left side of Figure 7 (x-axis). The observations seem to suggest that BSi keep much more efficiently the residual δ^{30}Si (bloom) than DSi, as suggested by Fripiat et al. (2012, see Figure 3 and abstract). Moreover, such discrepancy between the model and the observations seems to indicate that BSi winter growth is not really necessary, since the resulting decrease of BSi δ^{30}Si (by producing new biomass with low δ^{30}Si) is not really observed at the post bloom station (A3-B1).

Page 11423, line 18 to Page 11424, line 4: I don't really agree. If you have significantly depleted the silicic acid pool (down to 1 umol l^{-1}, as observed for A3-B1 station), any small input (e.g., 1-3 umol l^{-1}) would affect much more efficiently the δ^{30}Si than the concentration. See also the previous comment 'Page 11421, Line 22-24'. In addition, the model does not reach the values of De La Rocha et al. (2011) in autumn (significantly lower than simulated). The model missed therefore something, and maybe a higher vertical supply and a lower winter BSi production should be able to bring back the values to the measured ones.

Page 11424, lines 19-25: Much higher D:P ratio are reported in summer (post-bloom) in the Southern Ocean than in the model (e.g. Brzezinski et al., 2003; Beucher et al., 2004). Such discrepancy with the observations can explain why there is no significant difference between summer/autumn BSi δ^{30}Si with and without an isotopic fractionation associated with BSi dissolution.

Page 11425, line 2: The choice of 1.6‰ is not so clear. Fripiat et al. (2011a) reported significant difference in winter water δ^{30}Si between out and in plateau stations. The value given by De La Rocha et al. (2011, 1.8‰) fits well with the winter water values given by Fripiat et al. (2011a, 1.9 ± 0.1‰) above the plateau. It seems that we reached in autumn the winter water conditions (as reported also by Altabet and Francois, 2001, in the pacific sector; winter mixed layer = winter water). De La Rocha et al. (2011) reported values for DSi concentration between 6 to 16 umol l^{-1} (well above A3-B1 stations). Following the comment 'Page 11421, Line 22-24', such increase, if associated only with winter vertical mixing, should imply mixed layer DSi δ^{30}Si close to winter water conditions, as observed. The authors should discuss more about such uncertainty.

Page 11425, line 5: If winter growth is real, please provide some references.

Page 11425, lines 6-14: The model equations should take that into account, and they are. I've checked that if one runs the equation (Mixing term = [DSi(WW)-DSi(ML)] x m; m being the mixing coefficient) with the same concentration but different δ^{30}Si in the two pools, concentrations do not change but δ^{30}Si do (they converge).

Page 11425, lines 15-21: Together with the comment above ('Page 11425, line 2'), I suggest to actually see if from (i) the WW value, (ii) the depletion level, and (iii) the fractionation factor, the exported BSi δ^{30}Si fits with the one expected from the Rayleigh Fractionation kinetic. If yes, the statement in the abstract saying that BSi δ^{30}Si does not record the level of depletion is wrong.

Page 11425, lines 22-29: I would like to see the sensitivity of the model to the residence time of biogenic silica in the mixed layer. As the authors mentioned, we have a poor constraints on such processes.

Page 11426, lines 4-10: I really like the idea behind such statement (what are really telling us the BSi δ^{30}Si in sediments). But given the implications of such statement, the discussion should be much more detailed. The main export occurs in spring (associated with the diatom bloom species) and sedimentary δ^{30}Si should be mainly influenced by this export. At the other hand, sedimentary diatom species are mainly composed by the non-blooming species, the so-called "background species" (e.g. Smetacek et al., 2004; Crosta et al., 2005). Since the bloom species are dissolved in the water column, it is unlikely that their δ^{30}Si is preserved in the sediments. I suggest to discuss more about these features, bloom species vs non-bloom species, and their occurrence in sediments.

Page 11426, lines 17-18: Please add 'in accordance with Fripiat et al. (2011a) and Jeandel et al. (2011)'. Above in the paragraph, such basalt input is for the shallower plateau (less than 250m). As written, it seems now that such input is representative of the whole plateau. Need to be discussed. In addition, Kerfix and CTD8 are not representative of the shallower plateau (water column depth more than 1000m).

Page 11426, lines 25-27: The high positive values on soil solutions are due to plant uptake and/or secondary mineral precipitation (clays). Are those processes likely to take place for the

Kerguelen basalt weathering? Regarding the discussion below, it could aslo be congruent dissolution of basalt. I don't know if there are authigenic smectite around the Kerguelen plateau as suggested in this section, but some reference should exist on this.

Page 11427, lines 21-24: Again (see comment 'Page 11423; lines 11-12') the observations do not suggest low δ^{30}Si for biogenic silica (at least in comparison to the lowering of DSi δ^{30}Si). Such observations do not seem to confirm the basal weathering hypothesis since both BSi and DSi δ^{30}Si should be lowered at the same extent by this process.

Figure 1: I suggest to add the position of the fronts. It will make the figure easier to read.

Figure 3: A zoom on the upper ocean would be helpful.

Figure 4: It is difficult to know from which dataset the data are coming from (as well as from which seasons). Please, add whenever possible also the data from Cavagna et al. (2011), Fripiat et al. (2011b), and de Souza et al. (2012).

Figure 5: DSi depletion and BSi production are larger on the plateau than Kerfix. This needs to be discussed.

Figure 7: I don't understand why the values of De La Rocha et al. (2011) of 1.8‰, in autumn, are not presented on this figure. It seems that the model cannot reach such low observed value, maybe due to the higher simulated winter BSi production than in the reality. The sensitivity to such winter production has to be done and shown.

Table 2: Where the value of the K_S is coming from? Huge variability are reported in the Southern Ocean (e.g. Nelson et al., 2001). Same comment for the mixing coefficient. From Park et al. (2008b), the vertical mixing rate is higher on the plateau (data) than outside (model). The dissolution rate given in Demarest et al. (2009) is for a temperature of 15°C. As quoted in the latter study 'Dissolution rates where highly influenced by temperature, with specific values at 3°C being only about one-fourth those at 15°C over the 0-15% dissolved interval, and one-fifth the 15°C values over the 20-35% dissolved range'. 3°C is closer to the maximum temperature at Kerfix (e.g., Pondaven et al., 1998). I couldn't find Tyrrell (1999) in the bibliography. 1.8‰ is heavy for the isotopic composition of DSi released during basalt weathering, why?

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