

Supplement. Answer to reviewer #1

R1-Cx : Referee comment, **R1-Rx**: authors response.

***R1-C1*: General comments**

Rigual-Hernandez and co-workers studied the seasonal and interannual variability of particle and diatoms fluxes trapped at two locations in front areas between Tasmania and Antarctica. Station 47_S was deployed in the central SAZ (two-year record), while Station 54_S (six-year record) was located in the PFZ. They compare and discuss their data with results previously gained at both stations and at 51_S and 61_S. Fluxes are highly seasonal, with two maxima registered during the austral summer and minima during winter. Biogenic silica (opal) is the dominant component at 54_S, less important at 47_S. The strong correlation between opal and organic carbon suggests that a significant fraction of the organic matter exported to the deep sea is delivered by diatom productivity events. Seasonal diatom fluxes appear driven principally by changes in the flux of *Fragilariopsis kerguelensis*, the main contributor to the diatom/opal flux during the bloom season. In addition, a diverse diatom assemblage delivers information on species dynamics, which can be useful for understanding the signal preserved in late Quaternary sediments from the Southern Ocean.

The manuscript is well written and well organized and can be published in Biogeosciences after some revision. The Introduction and the Discussion would greatly benefit from some shortening. Below I list a few remarks and make some comments, which I hope the authors find helpful.

***R1-R1*: General comments**

We thank Dr. Oscar Romero (reviewer #1) for the careful reading of our manuscript and the constructive comments that helped improving the manuscript. Overall, the introduction and the discussion have been shortened significantly following referee #1's recommendations. The description of the PCA analysis in the results section has been rephrased and shortened. Moreover, a new graph showing the temporal variability and the interannual average of the molar ratio BSi:PIC has been included in the new version of the manuscript (supplementary figure 1). Additionally, the issues raised by reviewer #1 regarding the formation of aggregates have been clarified in the manuscript by re-phrasing and shortening the discussion and rephrasing the conclusions. Finally, all the above mentioned changes, together with a throughout revision of the manuscript have reduced significantly the number of references presented in the reference list.

***R1-C2*: Specific comments**

The Introduction addresses several subjects. Although these subjects are discussed afterward, I suggest some shortening and/or a re-arrangements of issues addressed. The first paragraphs mainly deals with the significance of the Southern Ocean in the marine realm. Part of the issues addressed here are also mentioned in "2. Oceanographic and biological setting". Paragraphs 3 (Diatoms in water and sediments) and 4 (Temporal variability of diatom productivity) can be shortened and combine into one paragraph. The research using sediment traps has been running for some decades in the meanwhile. I don't think it is necessary to extend much on this issue in the Intro. Issues

related with diatoms and/or biological pump can be combined into Paragraphs 2 and 3-4. Objectives 1. and 2. are very similar. Rephrase (or combine?).

R1-R2: Corrected according to Reviewer #1's suggestion. The introduction has been re-structured and re-written in the following manner:

- The text has been shortened to ~two-thirds of its original length (from 1415 to 1032 words).
- Paragraphs 3 (Diatoms in the Southern Ocean waters and sediments) and 4 (gaps in the knowledge) have been shortened significantly following reviewer #1's advice. However, since each paragraph deals with a different topic, they remain separated into two different paragraphs in the new version of the manuscript.
- Paragraph 5, which was dedicated to review the application of sediment traps, has been reduced to approximately half of its original size. Now the literature review skips the general applications of sediment traps and focuses on the major findings on diatom ecology in the Southern Ocean using sediment traps.
- The formation of intermediate and mode waters in the SAZ and PFZ plays a key role in the global distribution of nutrients and CO₂ and therefore this is a critical point that should be stressed in our study. Although the formation of these water masses is mentioned in both the introduction and section "2. Oceanographic and biological setting", this topic is addressed from a different perspective in each section. In the introduction the role of the Southern Ocean in the global conveyor belt is presented while in Section 2 the oceanographic circulation is addressed from a regional perspective (i.e. Australian Sector) providing the names of the intermediate and mode waters formed. Taking into account that the text dedicated to this explanation only occupied 6 lines (line 26, page 8621 and 1 to 5, page 8622; original version of the manuscript) in section "2. Oceanographic and biological setting" we believe that this part of the manuscript shouldn't be modified in the new version of the manuscript.
- The changes applied to the introduction have also reduced significantly the number of references of this section (see R1-C26).
- Finally Objectives 1 and 2 have been combined together following reviewer one's recommendations.

R1-C3: p. 8621, l. 17-18: provide a recent reference for SST. Rintoul and Trull, 2001, hardly cover the studied period presented in the manuscript.

R1-R3: Corrected according to reviewer's suggestion. The summer range of SST provided by Rintoul and Trull (2001) has been replaced by a new range obtained from the SST data presented in Figure 2. Please note that the SST data in figure 2 was obtained from the IGOSS NMC database (the Integrated Global Ocean Services System Products Bulletin, National Meteorological Center;

Reynolds et al., 2002). The source of these data is provided in section “3.7 Environmental variables”.

R1-C4: p. 8621, l. 22-24: sort of similar to what is said above in l. 16-18. Revise.

R1-R4: Corrected according to reviewer’s suggestion. The average summer SSTs for the study period has been estimated from the SST data presented in figure 3. The new SST range has been included and Figure 3 cited.

R1-C5: p. 8622, l. 6: which zones? Clarify.

R1-R5: Corrected according to reviewer’s suggestion. The words “both zones” have been replaced by “the PFZ and SAZ” (198, manuscript with tracked changes).

R1-C6: p. 8622, l. 8: chemical elements sometimes fully written, sometimes abbreviated. Please revise and unify.

R1-R6: Corrected according to reviewer’s suggestion. The whole manuscript has been revised and the chemical elements are now fully written in the manuscript. The only exception is Biogenic silica which has been replaced by BSi in order to reduce the number of words in the manuscript.

R1-C7: P. 8623, l. 14: it is not quite right to state that results are presented for a six-year record between 1997 and 2007. Please detail which years have been sampled. The info about the trapping intervals should be in the Abstract and in the Intro as well.

R1-R7: Corrected according to reviewer’s suggestion. The years that have been sampled are now detailed in the abstract, section “3.1 Field experiment” (line 237, manuscript with tracked changes), section “4. Results” (lines 388-390, manuscript with tracked changes) and listed in Table 2. However, the dates have not been included in the Introduction to avoid excessive repetition.

R1-C8: P.8624, l. 12: any suggestion of the “degree of undertrapping” being seasonally dependent? Or remained the same all-year round?

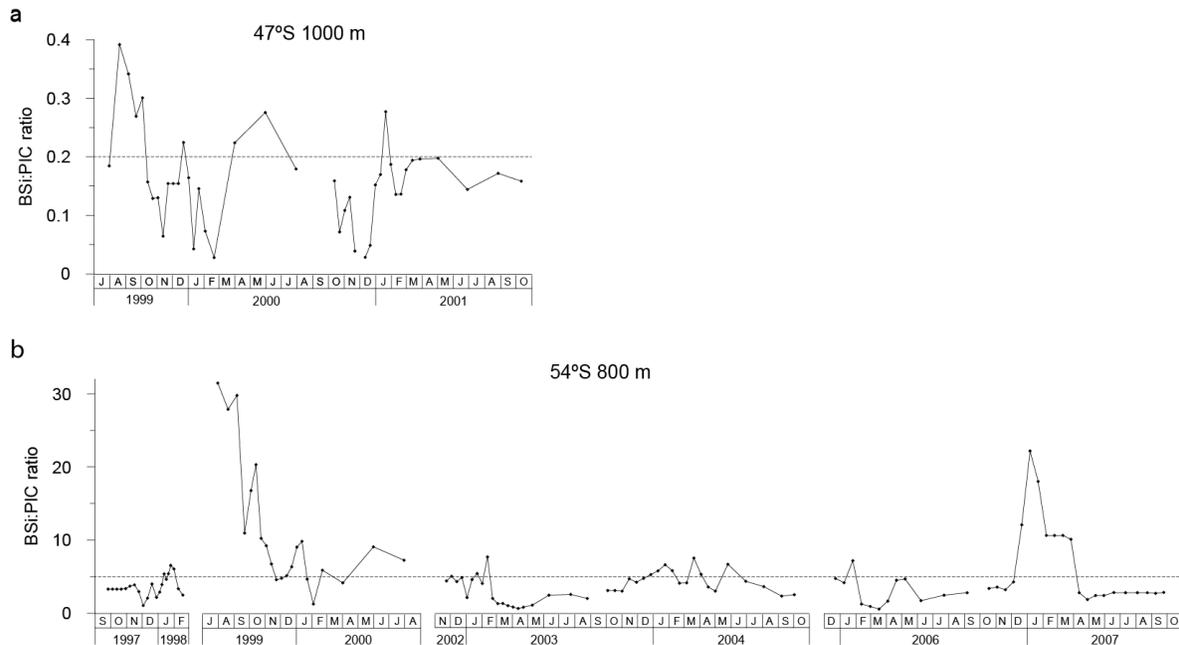
R1-R8: Current meter data at trap depths showed little seasonal variability, and therefore the seasonality of collection is likely to be small. The first half of section “3.2 Quality check of downward particles” has been re-written in order to clarify this point (lines 217-224, manuscript with tracked changes).

R1-C9: P. 8628, l. 17-19: start describing new results, move this sentence to the end of the first Results paragraph.

R1-R9: Corrected according to reviewer’s suggestion. In the new version of the manuscript we first introduce the new data that is presented in our study and then we mentioned the study by Trull et al. (2001) that covered the first year deployment.

R1-C10: P. 8629, l. 17-19: plot the temporal pattern of BSi:PIC mole.

R1-R10: The temporal patterns of BSi:PIC mole of both the 47°S and 54°S sites together with its inter-annual average have been plotted in Supplementary figure 1 (pasted below). This figure is cited in the new version of the manuscript (lines lines 417-418, manuscript with tracked changes).



Supplementary figure 1: Temporal pattern of BSi:PIC ratio at the 47°S (a) and 54°S (b) mooring sites. Dashed lines represent the average inter-annual BSi:PIC ratio.

R1-C11: P. 8631, l. 16-17: are these diversity index values annual averages? Or averages for the entire trap experiments?

R1-R11: The H' diversity indexes for the 61°S, 54°S and 47°S sites were estimated from the relative contribution of the diatom species for the entire sampling period at each site. This point has been clarified in the new version of the manuscript following reviewer #1's suggestion (line 470, manuscript with tracked changes).

R1-C12: Same question for the statement in l. 19: 43% for *F kerguelensis* refers to which interval?

R1-R12: Corrected according reviewer one's suggestion. In the new version of the manuscript it is clarified that the relative contribution of *F. kerguelensis* was obtained from the integrated diatom assemblage for the entire sampling period (lines 473-474, manuscript with tracked changes).

R1-C12: The same for the listed spp. in the following lines.

R1-R13: This point has already been clarified for *F. kerguelensis* in the previous sentence and therefore the reader can infer that the subordinate contributions of the rest of the species were estimated in the same manner as for *F. kerguelensis*. We believe that explaining again how these values were obtained would be somewhat redundant.

R1-C14: P. 8632: “4.3. Principal component analysis” OF WHAT?

R1-R14: The title of this section has been clarified following reviewer one’ request. The new title is: “4.3 Principal component analysis of diatom assemblages”

R1-C15: *In general, the description of the PCA is wordy and tedious to read. Rephrase and shorten!*

R1-R15: The description of the PCA has been rephrased and shortened following reviewer #1’s recommendation. The references to *Nitzschia bicapitata* and *Thalassiosira* sp. 1 in the 47°S PCA and *Thalassiosira lentiginosa* in the 54°S PCA have been deleted in order to shorten the description of the results of the analysis.

R1-C16: P. 8632, l. 6-7: the use of only two axes for site 47_S explain less than 40% of the variance. Whether this information is ecologically significant is a quite different issue.

R1-R16: We acknowledge the point our reviewer is trying to make- that the information provided by the two first PCA (40% of the variance) is probably not enough to explain the variability of the diatom assemblages at site 47°S. The first four axes (64% of the variance) were initially considered when analysing the PCA of the 47°S site (Table 5a). However, as mentioned in the first version of the manuscript the last two components are monospecific and had no clear ecological significance, that’s why they are not discussed in the text.

The main purpose of the PCA performed at the 47°S is to serve as a comparison with the results of station 54°S with the objective to show the contrasting role that the diatom species play in the controls of particle export in the SAZ and PFZ. Whilst at the 54°S site the diatom species are clearly grouped into clusters that exhibit a strong relationship with the major components of the flux, the distribution of diatom species in the 47°S PCA is scattered with none of the factors showing a clear relationship with the components of the flux. Finally, it is worth noting that although the groups suggested by the 47°S PCA are mentioned in section 5.3 (lines 704-708, manuscript with tracked changes) we are brief and cautious with our interpretations and their seasonal variability is not interpreted as a clear response to external forcing or environmental variability.

R1-C17: P. 8636, “5.2. Latitudinal diatom species distribution” and “5.3. Seasonal variability of diatom assemblages”: this two Discussion sections can be shortened and combined. The seasonal variability in the composition of the diatom community follows the same pattern of discussion: first the community at 47_S, then at 54_S, then the comparison with previous near-by studies. I suggest also constraining the discussion to the ecologically most significant species (based on PCA?). Section 5.3. is extremely detailed and wordy. For the audience less familiar with diatoms, it is

difficult to follow and try to pick up the main message/s. Some space can be saved by abbreviating the genera name (no need to fully write them once they have been mentioned).

R1-R17: Both sections have been shortened and re-arranged following reviewer one's suggestion:

- Section “5.2. Latitudinal diatom species distribution” has been reduced ~30% of its original length (from 954 to 692 words) and the discussion has been constrained to the ecologically most significant species as suggested by reviewer #1. The main two changes made to this section are:
 1. Elimination of the paragraph dedicated to the *Chaetoceros* resting spores (RS) and their possible advection from the Tasman coast (lines 604-617, manuscript with tracked changes). The presence of *Chaetoceros* RS was interpreted as an indicator of coastal waters advected from the Tasman coast towards the 47°S site. However, according to reviewer #2's (comment **R2-C8**), the production of *Chaetoceros* RS could have also occurred *in situ*. Therefore, in order to avoid speculations and shorten the length of section 5.2 this paragraph has been left out in the new version of the manuscript.
 2. Elimination of the references to sea-ice affiliated species at the end of the section (lines 24-28 P. 8637). The fluxes of sea-ice affiliated species were low at site 61°S (Rigual-Hernández et al. 2015) and negligible at the 54°S and therefore this information can be cut out in the manuscript.

- Section “5.3. Seasonal variability of diatom assemblages” has also been shortened to about two thirds of its original length (from 1455 to 1043 words) and re-arranged in the following manner:
 1. The first part of this section (lines 661 to 668, manuscript with tracked changes) has been deleted. The direct relationship between primary production and export fluxes is a common phenomenon in open ocean environments and thus it doesn't have to be necessary mentioned in the manuscript.
 2. The last speculations about the possible explanations for the bimodal distribution of the diatom bloom, i.e. other ecological factors and statistical funnel issues (lines 724-730, manuscript with tracked changes) have been deleted.
 3. Moreover, this section has been re-arranged following reviewer #1's recommendation and now the seasonal succession at the 47°S site is presented before the one of the 54°S site.
 4. The paragraph dedicated to *Pseudo-nitzschia heimii*, *Thalassiothrix antarctica* and *Proboscia* (lines 766-774, manuscript with tracked changes) has been deleted in order to shorten this section. However, the relationship between the sedimentation of these species with the POC fluxes is now addressed in a new paragraph included at the end of section 5.4 (lines 854-867, manuscript with tracked changes) following reviewer #2's recommendations (see Answer to referee #2 comments).
 5. In the first version of the manuscript diatom names were written in full the first time they appeared in each section. In the new version of the manuscript diatom species names are now only written in full the first time they are mentioned in the manuscript or at the beginning of a sentence. However, some exceptions were made, in particular

with *Thalassiothrix antarctica*, the abbreviated name of this species can be confused with *Thalassiosira antarctica* and therefore, we have decided to leave the name in full in the text to avoid confusion.

Since both sections deal with quite different topics, i.e. geographical vs. temporal species distribution, the new version of the manuscript still maintain the two sections separated. Moreover, the last paragraph of section “5.4 Ecological flux vectors in the PFZ” (lines 867-877, manuscript with tracked changes) dedicated to the particulate export during the autumn and winter months has been deleted in order to reduce the length of the discussion.

R1-C18: P. 8637, l.17: abundance of *F kerguelensis*: relative or absolute?

R1-R18: Relative abundance. This point has been clarified in the text following reviewer #1’s suggestion (line 464, manuscript with tracked changes).

R1-C19: P. 8638, l. 7-17: this has been already discussed above.

R1-R19: Despite the fact that the composition of the particle fluxes at the 54°S and 47°S sites has already been discussed in section 5.1, this is the first and only time in the manuscript where the correlation coefficients between mass and diatom valve fluxes are discussed. These correlation coefficients are a key argument to point out the different role of diatoms in the control or particle export in the SAZ and in the PFZ (as highlighted in the last sentence of the paragraph), and therefore we believe that this piece of text should remain in the final version of the manuscript.

R1-C20: P. 8643: as long as the authors do not provide own observations on the occurrence of aggregates in their trap samples, this part of the Discussion remains speculative.

R1-R20: Due to the postcollection changes of the particles stored in the sediment trap cups, the analysis of the size spectrum of the particles that originally settled into the traps was discarded for our study. However, there are three solid arguments that strongly support the idea that the formation of aggregates recurrently occurs during the productive season (summer) in our 54°S study site. Firstly, the study by Ebersbach et al. (2011) conducted at the 54°S site in summer 2007 with cylindrical traps filled with polyacrylamide gels showed that more than 90% of the particle flux occurred in the form of aggregates. We referred to this study in the original version of the manuscript (lines 5-6 page 8643 and from line 26 page 8645 to line 4 page 8646; original version of the manuscript). Moreover, a previous sediment trap study along the AESOPS transect (170°W) in the Pacific sector reached a similar conclusion (Grigorov et al. 2014) (line 6 page 8643, original version of the manuscript). Finally, based on the time lag between samples with similar silicon isotopic signature in the shallow and deeper traps in our mooring line at 54°S site, Closset et al. (2015) (this paper has just been accepted in *Global Biogeochemical Cycles* and the citation updated in the new version of the manuscript; lines 805-810, manuscript with tracked changes) estimated the sinking rates of the settling particles. These sinking rates were at least 35 m d⁻¹, a value that falls within the range of marine snow and faecal pellets (Turner et al. 2002), indicating that that most of the particles at this site sink in the form of aggregates during summer. These latter studies together

with the fact that all the components of the flux were highly correlated in our samples strongly suggest that the different particles co-sedimented in the form of aggregates (as suggested at the end of section 5.4).

However, in order to be more cautious with our interpretations some changes have been included in section 5.4. The factors affecting/defining the formation of aggregates (lines 814-819, manuscript with tracked changes) have been deleted. Also the sentence in line 819 (manuscript with tracked changes) “We suggest that the massive development of “High-export group” diatoms during the growth season facilitates the formation of aggregates in the upper water column” has been replaced by “We speculate that...”. Moreover, in line 816 (manuscript with tracked changes), the second sentence has been rephrased, now it reads: “Although speculative, it is possible that that the formation of aggregates during the diatom bloom also facilitated the scavenging...”.

RI-C21: In l. 10-27 factors affecting/defining the formation of aggregates is discussed: is this necessary?

R1-R21: As mentioned in the answer to the previous comment, the introduction to the factors affecting/defining the formation of aggregates has been deleted in the new version of the manuscript.

RI-C22: In P. 8644, l. 8-14, the possible effect of aggregate formation is again raised.

R1-R22: This point has already been clarified in **R1-R20**.

RI-C23: The “aggregate” issue appears also in “6. Conclusions” (P. 8647, 14-16).

R1-R23: The text referred by reviewer one has been rephrased in order to be more cautious with our statements regarding the formation of aggregates. In the new version of the manuscript it reads: “Our results suggest that the development of a group of bloom-forming diatom species during the growth season **probably led** to the formation of algal and/or faecal aggregates.” (line 951, manuscript with tracked changes).

RI-C24: P. 8644, l. 2.5: this is somehow confusing. Please revise.

R1-R24: The sentence referred by reviewer #1 has been rephrased in order to make it easier to understand. Now it reads: “In contrast, the thick-shelled *F. kerguelensis* is a more compelling candidate to be responsible for the bulk of the BSi export, because despite the fact that its relative abundance exhibited the lowest values of the record during summer, its valve fluxes always were highest during this season.” (lines 836-839).

RI-C25: Figures:

Fig C3174 2: what do the authors mean with “Others”? Lithogenics? (see also Fig 3)

R1-R25: Figure captions 2 and 3 have been rephrased and clarified (lines 996-1020). Moreover, the term “Others” has been replaced in Figure 2 and 3 by “Other”.

R1-C26: References:

I leave this up to the Editor: for a research article, the Reference list is quite long.

R1-R26: Corrected according to reviewer two's suggestion. Due to the shortening of the text and the removal of unnecessary references along the text, the reference list of the new version of the manuscript is 50 references shorter.

R1-C27: Technical corrections

P. 8639, l. 23: delete the dot before Kopczynska.

R1-R27: Corrected according to reviewer 1's suggestion.

R1-C28: P. 8644, l. 2: delete w after to.

R1-R28: Corrected according to reviewer 1's suggestion.

Additional technical corrections

Line 18 P. 8633. The value 60% corresponded to the relative abundance of the High export group, not to the relative abundance of *Pseudo-nitzschia lineola* cf. *lineola* and small *Fragilariopsis* together. In the new version of the manuscript this value has been replaced by the correct one (50%).

As mentioned in **R1-R25** the term "Others" has been replaced by "Other" in Figures 2b and 3b. Moreover, the axes of the 3b were displaced in the first version of the manuscript. This error has been fixed in the new version of the manuscript. Figures will be provided to the editorial board in the next stage of the publication process.

Table 3c. There was an error in the BSi:PIC flux of year 2002-03 (the value 5.9 should be 3.4 instead). The corrected value has been included and the annual average and SD corrected (i.e. the value 5.4 ± 2.1 has been replaced by 5.0 ± 2.2). This value has also been updated in the text (line 417, manuscript with tracked changes).