

Interactive comment on “Export fluxes in a naturally fertilized area of the Southern Ocean, the Kerguelen Plateau: ecological vectors of carbon and biogenic silica to depth (Part 2)” by M. Rembauville et al.

Anonymous Referee #1

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General comments

The MS by Rembauville and colleagues report quantitative biogeochemical and biological (diatoms and faecal pellets) data gained with a shallow sediment trap deployed during one year in a naturally fertilized area of the Southern Ocean, the Kerguelen Plateau. Comparison of sed trap data with measurements in the mixed layer suggest a strong flux attenuation between 200 and 300 m. Resting spores of *Chaetoceros* *Hyalochaete* and *Thalassiosira antarctica* appear to be responsible for about 60% of the annual POC export at 300 m. The authors suggest that diatom resting spores

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might be key vectors of POC export in many other oceanographic settings in the world oceans. The seasonal succession of diatom taxa groups seems to be linked to the stoichiometry of the registered particle fluxes. The seasonal progression of faecal pellet types suggests a transition from small copepods in spring, euphausiids and large copepods in summer and salps in autumn and winter. The manuscript by Rembauville is an important contribution to our understanding of the marine biological carbon pump and provides new insights to the understanding of present-day seasonal dynamics of diatom populations in naturally fertilized marine systems. In spite of the need for clarification, the MS deserves to be published in Biogeosciences. Below I list several suggestions, which I hope will contribute to clarify some issues and help the authors to improve their MS.

Specific comments

Along the text the authors often refer to cups instead of months, it would be more useful for the reader to know the months or seasons instead of the cup numbers.

1. Introduction

Is there a particular reason why the authors present the fluxes in mol m⁻²? Many sediment trap studies in the Southern Ocean (e.g. Honjo et al. 2000; Fischer et al., 2002; Trull et al. 2000) show the data in g or mg. I would suggest to at least include the data in these other units in Table 1 to facilitate comparisons with previous studies. “Highest diatom fluxes (> 109 cells m⁻² d⁻¹) are observed in the Seasonal Ice Zone (SIZ) near Prydz Bay and Adélie Land and are dominated by *Fragilariopsis kerguelensis* and small species of *Fragilariopsis curta* and *Fragilariopsis cylindrus* (Suzuki et al., 2001; PilskaIn et al., 2004).”

Are these values annual averages or the highest values registered by the traps? Please clarify.

Instead of “and small species of *Fragilariopsis curta* and *Fragilariopsis cylindrus*” it

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would be better to say: “and smaller *Fragilariopsis* species such as *Fragilariopsis curta* and *Fragilariopsis cylindrus*”.

“These high fluxes occur in spring and are associated with the melting of sea ice. Changes in light availability and melt water input appear to establish favorable conditions for the production and export of phytoplankton cells (Romero and Armand, 2010). In the Permanently Open Ocean Zone 5 (POOZ), diatom fluxes are two orders of magnitude lower $\sim 10^7$ cells $m^{-2} d^{-1}$ (Abelmann and Gersonde, 1991; Salter et al., 2012; Grigorov et al., 2014) and typically represented by *F. kerguelensis* and *Thalassionema nitzschioides*, except in the naturally fertilized waters downstream of the Crozet Plateau where resting spores of *Eucampia antarctica* var. *antarctica* dominate the diatom export assemblage (Salter et al., 2012).”

Same comment as above. Are the diatom valve fluxes provided here annual averages or highest values recorded? Please clarify.

“These high fluxes occur in spring and are associated with the melting of sea ice.”

These studies reported maximum fluxes during summer not spring.

2. Material and methods

The depth of the water column should be mentioned in this section. Sediment traps deployed at shallow depths are subject to several hydrodynamic biases. Despite the fact that the efficiency of this sed trap is assessed in the companion paper a general statement about the efficiency of the trap should be given here as well. The companion manuscript (Rembauville et al. 2014) has already been accepted or just submitted? It probably would be better to cite it as (Rembauville et al., this issue). Please check with the journal guidelines and correct if necessary.

2.2 Chemical measurements

“POC and PON analyses have been previously described in Rembauville et al. (2014).” POC and PON data have already been presented in the companion paper and there-

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fore shouldn't be presented as part of the results of this manuscript. The abstract should also be corrected accordingly.

2.3 Diatom identification, fluxes and biomass

“...thereby facilitating the observation of diatom frustules...”

The word “frustules” should be replaced by “valves”.

“Diatom enumeration and identification was made from one quarter to one half of the counting chamber (depending on cell abundance) following the taxonomic description in Hasle and Syvertsen (1997).”

How many diatoms per sample were counted? 300? 400? Why was this number chosen? With the magnification used in this study (especially with x200) it can be quite hard to differentiate an empty complete frustule (i.e. two valves together) from a single valve. This can be particularly difficult for some species such as those of the genus *Pseudo-nitzschia* or small *Fragilariopsis* species. Were the authors able to make such a difference? And if so, most of the empty frustules were found as separate valves or forming a complete frustule?. Please provide the raw frustule and cell counts as supplementary material. Are all the species identified in this study described in Hasle and Syvertsen (1997)? Authorships for all the species should be provided in Table 4 or in a separate appendix.

“We directly compared the micropaleontological and biological counting techniques in our sediment trap samples and noted the loss of several species (*Chaetoceros decipiens/dichaeta*, *Corethron pennatum/inermis*, *Guinardia cylindrus* and *Rhizosolenia chunii*) with the micropaleontological technique. We attribute this to the aggressive chemical oxidation techniques used to “clean” the samples which appears to selectively destroy/dissolve certain frustules. For the species that were commonly observed by both techniques, total valve flux was in good agreement (Spearman 10 rank correlation, $n = 12$, $r = 0.91$, $p < 0.001$, data not shown) although consistently lower with the

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micropaleontological technique, probably due to the loss of certain frustules described above. Full details of this method comparison are in preparation for a separate submission.”

This is an interesting observation and something worth looking further into. However the authors should be cautious when referring to the “micropaleontological technique” as different authors use different versions of this technique. For example, after the acid cleaning treatment some researchers centrifuge the samples several times to wash and buffer them to a neutral pH. In contrast, other authors wait 24 h between washings allowing the diatom to settle without the use of the centrifuge. The use of the centrifuge could also be a responsible, at least partially, for the fragmentation of weakly silicified diatoms. Something similar occurs with the acid treatment, various concentrations, temperatures, and times of treatment are used in the literature and thus results may vary between authors.

“Biomass calculations for both *Chaetoceros* and *Hayalochaete* resting spores...”

Why did the authors decide to estimate the biomass of *Chaetoceros* RS and *Thalassiosira antarctica*? At the beginning of this paragraph the authors should briefly explain why these particular species were chosen. Perhaps it would be preferable to start the paragraph with the last sentence.

2.5 Statistical analyses

“Chi2 distance is very sensitive to rare events. Consequently, only full- and empty-cells fluxes > 10% of the total mean flux of all sample cups were retained in the correspondence analysis.” Do the authors mean that only the species representing more than 10% of the annual assemblage were used in the statistical analysis? Please clarify. A list with the annual relative contribution of each species should be provided in Table 4 or in a separate figure or table. Results

3.2 Chemical composition of the settling material

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Satellite chlorophyll-a estimates belong to material and methods not to this section.

POC fluxes have already been presented in the companion paper and therefore shouldn't be presented in the results section of this paper. This should also be clarified in the abstract, as it written now it seems that these results belong to this paper.

Was the total mass flux estimated? What was the relative contribution of the biogenic silica fraction to the total mass flux? Did the authors estimate the contribution of the carbonate fraction? I would like to see the annual relative contribution of each bulk compound in Table 1 or in a new table or figure.

In order to facilitate comparisons with other sites, fluxes of all the bulk compounds should be also provided in $\text{mg m}^{-2} \text{d}^{-1}$ and annual estimates in $\text{g m}^{-2} \text{yr}^{-1}$ in Table 1.

3.2 Diatom fluxes

In order to compare the results with data from the sedimentary record, it would be useful that the authors present the annual relative contribution of each diatom species as I already suggested previously. In this section, the authors describe the changes in the relative contribution of the different diatom taxa but this information is not shown in the graphs or in the tables. This information should be included in Figure 2 or in a separate figure. The authors should explain in more detail how the annually integrated empty:full ratio presented in Figure 4 was estimated. Did the authors take into consideration the flux of each species in each cup for this calculation or was it just an average of the empty:full ratio of each species of all the cups?

Discussion

4.1 The significance of resting spores for POC flux

Please briefly resume in a sentence or two why the authors know that there was a strong attenuation of flux between the WML and 300 m. The reader should be able to understand the paper without reading the companion manuscript. Also, please provide the depth of the WML.

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“We did not observe any full cells of the vegetative stage of *Chaetoceros Hyalochaete*, a feature possibly related to its high susceptibility to grazing pressure in the mixed layer (Smetacek et al., 2004; Quéguiner, 2013; Assmy et al., 2013)”.

Silica dissolution in the upper water column should also be considered as an important factor determining the absence of full vegetative cells in the sediment trap. Weakly silicified diatoms such as *Chaetoceros* may lose one of their valves or the girdle band more easily than more heavily silicified and compact diatoms, which would facilitate the remineralization of its cellular content.

“Numerous sediment trap studies have reported a strong contribution, if not dominance, of CRS to diatom fluxes at depth in various oceanographic regions (e.g. Antarctic Peninsula (Leventer, 1991), Bransfield Strait (Abelmann and Gersonde, 1991), Gulf of California (Sancetta, 1995; Lange, 1997), Eastern Equatorial Atlantic (Treppke et al., 1996), East China Sea (Kato et al., 2003), coastal North Pacific Ocean (Chang et al., 2013) and the subarctic Atlantic (Rynearson et al., 2013)). CRS are also found to be dominant in surface sediments in the coastal northeastern Pacific (Grimm et al., 1996), the North Atlantic (Bao et al., 2000), the northeast Pacific (Lopes et al., 2006), the North Scotia Sea (Allen et al., 2005), Antarctic sea ice and coastal regions (Crosta et al., 1997; Zielinski and Gersonde, 1997; Armand et al., 2005), and east of Kerguelen Island (Armand et al., 2008b). Moreover, the annual POC export from the A3 station sediment trap at 289m (98.2 ± 4.4 mmolm⁻² yr⁻¹) falls near annual estimates from 15 deep sediment traps (> 2000 m) located in the naturally fertilized area downstream of the Crozet Islands (37–60 and 40–42 mmolm⁻² yr⁻¹, Salter et al., 2012) where fluxes were considered as mainly driven by resting spores of *Eucampia antarctica* var. *antarctica*. The frequent occurrence and widespread distribution of diatoms resting spores suggest their pivotal role in the efficient transfer of carbon to depth. Although they are 20 frequently observed in blooms heavily influenced by the proximity of the coast, large scale advection might explain that their impact on carbon export is not restricted to neritic areas.”

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Most of the sites mentioned in the list are coastal regions where *Chaetoceros* RS are quite abundant, however, in open ocean regions of the SO *Chaetoceros* RS show very low numbers or are absent (e.g. Fischer et al. 2002, Grigorov et al. 2014, Rigual-Hernández et al. 2015) and therefore they don't play an important role in the carbon transfer at these sites. Authors should be more cautious in the last two sentences of the paragraph as resting spores are not an important vector of carbon export in all marine systems.

4.2 “Contribution of faecal pellets to POC flux

“...mesozooplankton abundance at A3 in spring (Carlotti et al., 2014) and have been observed at station...”

Carlotti et al. 2014 appears in the references as in preparation. Has this paper already been published or submitted? Please check and correct if necessary.

4.3 Diatom fluxes

“This value falls between the POOZ (≈ 107 cellsm⁻² d⁻¹, Abelmann and Gersonde, 1991; Salter et al., 2012; Grigorov et al., 2014) and the SIZ (> 109 cellsm⁻² d⁻¹, Suzuki et al., 2001; PilskaIn et al., 2004).

What do these values represent? maximum fluxes or annual average values?

Most of these studies present the diatom fluxes in valves m⁻² d⁻¹ and some do not provide annual estimates. When the authors estimated the values presented in the text from these publications, did they divided the results of these authors by two (i.e. two valves = one cell)? Please check and correct if necessary.

“Although the resting spore formation strategy is typically associated with 5 neritic areas (Smetacek, 1985; Crosta et al., 1997; Salter et al., 2012), their very high export and transfer efficiency together with advection can explain their contribution to deep open ocean fluxes (e.g. Rynearson et al., 2013).”

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“can explain their contribution to deep open ocean fluxes”? This sentence is not clear, please clarify.

“Highest *Pseudo-nitzschia* spp. full cell fluxes were observed in summer, concomitantly with the second export event (cup #9). *Pseudo-nitzschia* species are rarely found 15 m in deep sediment trap studies and are absent from the sediment diatom assemblages due to their susceptibility to dissolution (Grigorov et al., 2014; Rigual-Hernandez et al., 2014). The genera have been reported to accumulate in summer in deep chlorophyll maximum, benefiting from nutrient diffusion through the pycnocline (Parslow et al., 2001). This ecological characteristic, together with the shallow sediment trap depth (289 m) may explain our observations of peaks in *Pseudo-nitzschia* spp. fluxes during summer.”

“Genera” should be replaced by “genus”.

Only some species of this genus have been found in association to a DCM, and therefore it is incorrect to state that all the species of this genus have similar ecological affinities. Parslow et al. do not report this taxon, it was Kocczynska et al. (2001) who reported the species composition of the DCM.

In regard to the last sentence, why does the shallow depth of the trap explain the enhanced *Pseudo-nitzschia* fluxes during summer? Please explain better.

“Although their fluxes were very low, species of the *Rhizosolenia* and *Proboscia* genus were mostly exported as empty cells at the end of summer and during autumn (cups #8 to #11), occurring in parallel with the full cell fluxes of the giant diatom *Thalassiothrix antarctica* (Table 4). It has been suggested that these species belong to a group of “deep shade flora” that accumulate at the subsurface chlorophyll maxima in summer with their highly silicified, large frustules protecting them from grazing pressure (Kemp and Villareal, 2013).”

Many species of the genus *Rhizosolenia* exhibit weakly silicified frustules.

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Kemp et al. (2000) DSRII, should be mentioned here.

In the first line replace genus by genera

Replace “as the fall dump suggests” by “as the fall dump hypothesis suggests”

4.4 Preferential carbon and silica sinkers

“The annual BSi :POC ratio of the exported material (1.16) is much higher than the usual ratio proposed for marine diatoms of 0.13 (Brzezinski, 1985). Moreover, the BSi :POC ratio of the exported material in spring (2.1 to 3.4, cups #1 to #3) is significantly higher than the BSi :POC ratio of 0.32 ± 0.06 in the mixed layer of the same station during spring (Lasbleiz et al., 2014). Numerous chemical, physical, biological and ecological factors can impact BSi :POC ratios of marine diatoms (e.g. Ragueneau et al., 2006). However, the ten-fold differences in BSi :POC ratios of exported particles between spring and summer is unlikely to result simply from physiological constraints set during diatoms growth (Hutchins and Bruland, 1998; Takeda, 1998).”

Authors should explain the meaning of differences between the BSi:POC ratio in the mixed layer and in the trap during spring.

“These observations are consistent with previous studies of natural (Salter et al., 2012) and artificial (Assmy et al., 2013) iron fertilization that identified *C. pennatum*, *D. antarcticus* and *F. kerguelensis* as major silica sinkers and *C. Hyalochaete* vegetative cells, CRS and *E. antarctica* var. *antarctica* resting spores as major carbon sinkers. “Notably, resting spore formation was not observed in the artificial experiment and carbon export was attributed to mass mortality and aggregation of algal cells (Assmy et al., 2013).”

Of all the species listed in this paragraph only *F. kerguelensis* is classified as a silica sinker by Assmy et al. 2013. Moreover, Assmy and colleagues do not report RS in their study (as mentioned here), so it is incorrect to use this reference to support the findings of the authors, (at least as it is written now). This statement should be rewritten

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clarifying these points.

The study European Iron Fertilization Experiment (EIFEX) was performed in an open ocean location remote from coastal influence. This probably is the main reason why they did not register RS.

4.5 Seasonal succession of ecological flux vectors over the Kerguelen Plateau

“The species succession directly observed in our sediment trap samples differs somewhat to the conceptual model of ecological succession in naturally iron fertilized areas proposed by Quéguiner (2013), although the general patterns are similar. The first diatoms exported in spring are indeed small species of *F. kerguelensis*, *T. nitzschioides* spp., and small centric species (< 20 μm).”

F. kerguelensis and *T. nitzschioides* are generally considered as relatively large diatoms. Do the authors mean that the size of the specimens of these species found in spring were smaller than the ones found later in the year?

“However we observe that these species are exported almost exclusively as empty cells.” Why however? The authors should succinctly describe conceptual scheme proposed by Quéguiner (2013) or at least provide some insights of this scheme in order to compare their results with it.

Carlotti et al. 2014 manuscript is in preparation or has been submitted? Please check correct if necessary

“The main difference in our observations and the conceptual scheme of Quéguiner (2013) is the dominance of *Chaetoceros* *Hyalochaete* resting spores to diatom export assemblages and their contribution to carbon fluxes out of the mixed layer in summer, 10 probably triggered by $\text{Si}(\text{OH})_4$ limitation. Resting spores appear to efficiently bypass the “carbon trap” represented by grazers and might also physically entrain small faecal pellets in their downward flux. “

It is generally accepted that zooplankton activity diminishes with depth toward the lower
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mesopelagic zone (~ 1.5 km). Therefore, it is possible that RS are grazed or affected by other processes after 300 m. This should be also addressed in the discussion.

Conclusions

“Despite iron availability, . . .”

The authors should provide values and references of the iron concentration in the surface layer during the experiment (if available). This information should also be mentioned in the discussion.

Figures and Tables

Table 1. A column with the number of days that each cup was open should be included. The units of the column LSi seems to be erroneous. Please revise all the units on the table and text. Table 2 , 3 and 5. The word bold shouldn't be in bold in the caption figures. Table 4. The heading of the first column “Species-taxa group/Cup number” is confusing, please correct. Figure 1. It is not clear what the two curves plotted in figure 1a represent. Please clarify in the figure caption.

Figure 7 is not mentioned in the text.

Technical corrections

In the penultimate paragraph of the discussion (line 18) change “sport” by “spore”.

In Table 1 please correct the name of these species:

Rhizosolenia antennata/*styliformis* (two "n")

Thalassiosira antarctica (two "s")

Interactive comment on Biogeosciences Discuss., 11, 17089, 2014.