General comments

In “Seasonal cycles of biogeochemical fluxes in the Scotia Sea, Southern Ocean: A stable isotope approach”, Belcher et al. present a study investigating the seasonal variations of organic matter (POC and PON) and biogenic silica fluxes from two sediment traps located north-West of South Georgia in the Scotia Sea (Southern Ocean). Using stable isotope approaches the authors examine the origin and some of the processes controlling the fluxes they have observed in the traps.

They investigated the differences between two productive events (in February 2018 – summer season – and December 2018 – spring season) and the coupling of C, N and Si fluxes during these events. Their main results are: Particulate fluxes and isotopic compositions were similar in the deep and shallow trap suggesting that most of the remineralization occurred in the upper layer of the water column. Despite a very noisy d15N signal, the synchronicity if the d30Si and d13C signals highlight the coupling between these two elements and the significant role of diatoms in the export of C (and BSi) in the area. Based on the estimation of isotopic baselines associated with the two productive events, they also suggested a change in the source region of the material coming into the sediment traps.

Generally speaking, the results of this study are interesting. However, I found the manuscript rather hard to read, often unclear or confusing. I think that the manuscript would profit from an effort to make the structure of the discussion more easily understandable for the reader. More work can also be done regarding the description of the analytical and sample processing methods as well as data quality. Some important information is missing and/or unclear. But most importantly, I think the authors should re-think figure 3, which is one of the most (if not the most) important figure of the manuscript. Indeed, the way the figure is built does not support or illustrate the statements or hypothesis authors are attempting to demonstrate. Additionally, there are also some inconsistencies in the wording, and I would strongly encourage the authors to carefully read their manuscript again and have it read by an English-speaking person.

I detail these points below, together with minor points that the authors should also consider while carefully revising this manuscript. I recommend publication of this paper in “Biogeosciences” after major revisions.

Major concerns

Currently, the manuscript requires very careful reading (and re-reading) in order to understand the authors’ argumentation and get a sense of the various settings. A few suggestions:

* Re-organizing the discussion based on the three main periods that are discussed in the manuscript. For example, having three subsections in chronological order (i) Early spring event (P2), (ii) Late summer event (P1), (iii) Winter hiatus; or to fit with the main figure of the manuscript (fig 3) (i) First export event (P1), (ii) Winter hiatus, (iii) Second export event (P2).

* As it is, the manuscript needs desperately figures that will support the authors' hypothesis and statements for two main reasons: (i) Some important figures are missing. For example, figures illustrating the relationship between POC and BSi (mentioned for example L415) or d13C and d30Si (mentioned L422) with associated R2 and statistics. Right now, there is no figures to illustrate or support the relationship authors are discussing in the manuscript. (ii) Figure 3, the key figure of the manuscript, is currently very poorly designed. The choice of shading to represent fluxes in mg m-2 d-1 does not actually reflect the full magnitude of those fluxes. The most obvious example is POC flux in the shallow trap in May 2018. It seems to "peak" for a short period of time to values around 8 mg m-2 d-1 while it was sustaining this rate for a long period of time (31 days). Fluxes will appear less biased by using barplot representing the mean flux and error bar for the standard deviation. The variations of the isotopic composition of particles (d13C, d15N and D30Si) are also poorly illustrated by the choice of representing only the min and max on the figure. A mean value with error bar will be more representative of the seasonal evolution of the signal, as well as of the heterogeneity of the material (when error bars are more widely spread). It will also help with the scattering of the d15N and validate (or invalidate) the trends suggested by the authors.

* Something that need to be highlighted in the method section and briefly discussed later on is that the total collection period is 341 days. The first cup opened on Jan 25th (2018) and the last one closed on Jan 1st (2019). I would be worth it mentioning that the sediment trap series misses 3 weeks in the beginning of January where the flux is expected to be significant. Authors have not made any annual/seasonal integration of their fluxes, but they should still discuss the risk of missing a significant part of the seasonal flux early in the season. It might be of importance for the discussion regarding the isotope baseline for the first export event.

* It is not clear in the method section if authors have used the different splits of samples as replicates or if they have combined splits to do their different measurements. Figure 3 gives two values for each measurement (a min and a max) so one can guess that authors have measured duplicates out of those splits. Going through the discussion section, authors start to mention these mean values that do not correspond to anything presented in figure 3. I do not see the point of plotting only the min and max on the figure while using a mean value in the text. This is confusing, make things unclear and prevent the figure to illustrate and support correctly the text (e.g. it is hard to see some of the trends that are discussed in the text). I am suggesting using the mean values in the figure with the corresponding error bar and add those error (as standard deviation) within the text.

* Please define what "isotopic baselines" is. It is not defined anywhere in the manuscript, neither it is explained how authors have estimated the different baseline they are referring to. If it is just the lightest isotopic signature recorded just before a productive (and export) event, I am suspicious with the isotopic baseline identified for the first event since a significant part of the flux might have been missed early January 2018. Moreover, the isotopic baselines are not identified in figure 3 neither in Table 1. In general, this last part of the discussion (related to the comparison of the different isotopic baselines) is quite confusing and unclear and need some serious rephrasing.

Minor concerns

Introduction

* L43 Use biological pump of carbon instead of BCP.
* Sediment traps have bias too. A short summary (one-two sentences + ref) of them would be useful here. Especially since authors discuss some of them later in the manuscript.
* L73 Use “challenges” instead of “complications”
*Does sea-ice affect the region where the traps are located? It has been shown that the occurrence of sea ice can significantly affect stable isotopes composition (at least for d13C - e.g. Kennedy et al. 2002 - and d30Si - e.g. Fripiat et al. 2007). This could be an important aspect to consider in your discussion.
*The processes controlling the stable isotopic composition of C and N are quite well introduced but authors have been very quick concerning d30Si. More information about fractionation and the processes controlling it need to be introduced here (e.g. difference in fractionation between polar diatom species - Sutton et al. 2013 – fractionation (or not) associated with biogenic silica dissolution - Demarest et al. 2009, Wetzel et al. 2014).
*L115-116 what is the difference between annually and seasonally? Are they calculated over a different period?
* Do the stable isotopes really give information about the actual composition of organic matter?
* Perhaps what is missing in the introduction is a paragraph about why is the composition of particles important for the biological pump of carbon? For example, it will affect the sinking speed of particles (and authors discuss this later), their recycling in the water column (and later within the sediment) etc.

Material and Methods

* L152-153 it would be interesting to quantify this effect in percent of the signal
* L168 splitted
* It is not clear in the different paragraphs of the method section if these slips were combined or analyzed separately as replicates. If it is the second option (which is my guess) these replicates can be used to calculate the error or std on the samples as the potential heterogeneity of the sample will be reflected by a large error bar on the sample.
* L183 “per mil” instead of “per mille”
* Please define the meaning of “PACS international standard”
* L191 several statistical errors have been described here, although it is mentioned later in the manuscript, authors should indicate which one they have associated to their measurement).
* It would be interesting to quantify those splits in percent of the total sample
* What about lithogenic silica? Alkaline extraction method using NaOH will dissolve some lithogenic material along with BSi (Ragueneau et al. 2005). Because LSi has a light d30Si (down to -2.3 pmil, Opfergelt and Delmelle 2012), it has the potential to bias BSi d30Si measurements even with low LSi contribution (or contamination) to the alkaline digestion. For example, and in the worst-case scenario of LSi d30Si of -2.3 pmil, a contribution of 3% during period 2 and 4% during period 1 would significantly bias the result (by 0.1pmil). This is a rough calculation, but this need to be discussed as sediment traps can collect a significant amount of lithogenic

material. Nota that methods have been proposed to “correct” BSi d30Si from LSi contamination (see Closset et al 2015).
* L206 HCL or Milli-Q water as eluent?
* L222 This sentence is unclear. what are those pseudo replicates? two samples per pseudo replicates so four isotopic measurement per bottle? Isn’t the case for all isotopic measurements (and for POC and PON fluxes too)?
* L224 reference materials instead of reference standard
* L230-233 please refer to the figures
* L235 which periods? please clarify

Results
* Figure 2: It would be valuable to start at least one month before the starting of the sampling period. Because there is a lag between the timing of particles produced in the ML and when they reach the sediment trap, we are missing the peak of Chl a that corresponds to the material collected in the first cup. Moreover, it will be useful to have the timing of the peaks illustrated (arrows for example) on the figure as well. Please add a legend too.
* Figure 3: Please see my previous comments in the “Major concerns” section.
* Authors mentioned that there is a time lag in the flux of particles between the two traps but not in the stable isotopic composition (e.g. d13C). It will be interesting to discuss the reason of this difference.
* Table 1: It seems that replicate have been made so sd can be calculated and error can be propagated to better represent the error associated with the value presented in this table (for methods to propagate error see for example the Eurachem publication “Quantifying Uncertainty in Analytical Measurement”)
*L352 It would be great to mention which cup have been chosen for the microscopic analysis in the method section: Moreover, the deep and shallow cups seem to be from a different period in March 2018. Please explain why and/or any bias associated with this choice or correct the misalignment in the figure.

Discussion
* L385 Perhaps specify that the timing fits but not the magnitude of the peaks.
* L388 Please use “additional” instead of “third” since this peak is between the two main peaks
* L390 “PN fluxes followed the same seasonal trend as POC” please develop a little bit more, is it expected? why?
* L409 and after: higher sinking rates could also explain the observations and are consistent with no time lag in 2nd event compared to 1st event
* L415 A figure with simple linear regression would have illustrated this statement.
* L421 “variations” instead of “shifts”
* L422-423 Here too it needs a figure to illustrate this linear regression
* L427 If I’m correct, all diatoms belong to Bacillariophyceae not just Fragilariopsis spp
* L428-430 The low BSi d30Si at the beginning of the bloom is likely explained by a Si source that is already light, rather than more fractionation from diatom such as suggested. Using simple (not perfect) conceptual models such as Rayleigh or Steady-state, one can estimate the isotopic value of this source of Si. Diatoms at the end of summer also fractionate Si isotopes but

they use a Si source that is enriched in 30Si (higher d30Si value). Light d30Si can also come from bias due to the presence of LSi that would be more important early spring (perhaps brought from ice drafts?).
* L473 “significant” instead of “sharp”. Using conceptual models and an estimation of the isotopic signature of the Si from deeper water (same as the early spring Si source for example?), one can estimate the amount of Si supplied to the ML (see Fripiat et al 2011 for the methods using seawater samples and Closset et al 2015 for the methods applied to sediment trap samples). Although, there is no chl a in August in figure S2, so the uptake in the surface layer is probably not significant during this period. Additionally, what about LSi contribution to those samples?
* L498-500 In the deepest trap, the error associated to mean value in July is probably too high to conclude anything about a trend in d15N. It could be increasing only from June to August, just as in the shallowest trap. Although I am not against the hypothesis of material coming from different sources and at different stage of degradation, which is very likely during this time of the year.
* L504-505 Unclear
* L527 Please define “isotopic baselines” as it is not explained anywhere in the manuscript. Moreover, there is no baseline shown in figure 3 neither in Table 1. How is this isotopic baseline estimated?
* L539 Another linear regression that needs to be illustrated by a figure (at least in supplementary materials)
* L568 and after: What about the case of a continuous, or semi-continuous supply of DSi to the ML? or the influence of open water vs. sea ice diatoms? Also, sane comment as in L473, the magnitude of Si supplied to the ML can be estimated using simple conceptual models. Also, a BSi d30Si of 0.48 pmil will correspond to a Si source with a d30Si of ~1.68 pmil, which is not too light compared to the d30Si value of Southern Ocean deep water that can be considered as the Si source (e.g. in a quite similar configuration, WW Si source above the Kerguelen plateau has a d30Si of 1.71 pmil, Closset et al 2016)
* L578 Closset et al. 2022 and Cassarino et al. 2020 as reference for pore water d30Si and diffusive Si flux from sediments in the Southern Ocean
* L606 If faecal pellets or moults have been counted it has to be presented in a figure (at least in the supplementary material).

Conclusion

The conclusion (and not summary) is generally confusing and need some work to make it clearer. Although I tend to agree with the statements authors are providing, the data presented in this manuscript unfortunately do not support the conclusion (they probably have the potential to do it if better presented and described in the figures)

References

Please correct errors in the references (e.g. L791, L797)