

Here below authors' response to the comments of referee#2

*Ref#2 Detailed main comments. Reorganizing the discussion I suggest the following sections and sub-sections for the discussion:*

*1. From 234Th activity to POC export fluxes*

*1.a neglecting the physics (see my last 'main comment' below)*

*1.b steady versus non-steady state fluxes (present section 4.1 but one third in size)*

*1.c POC/234Th ratio (present section 4.2 but again one third in size)*

*2. Surface fluxes (ca. present section 4.3)*

*3. Mesopelagic POC remineralization (ca. present section 4.4 without the last paragraph)*

*4. Flux attenuation in the context of seasonal maturity of the system*

We reorganized the discussion according to referee's suggestions. For Section 4, we preferred to call it "Bathypelagic POC fluxes."

*Ref#2: Section 2: in addition to compare EP100 to NP, the comparison with primary production (PP) would be of high interest. As PP was not measured on board, it could be estimated from total N-uptake and C/N ratio (as authors did for estimating NP from nitrate uptake).*

We agree that comparison with PP would have been of high interest, as mentioned in the discussion this would have allowed to estimate ThE ratio (export efficiency defined by Buesseler et al., 1998); however we decided to compare our flux data (EP100 and EP600) only with NP data because total N-uptake, as emphasized by Joubert et al (2011), may have been influenced by heterotrophic assimilation. To our point of view, PP may have been overestimated using total N-uptake data.

*Ref#2: Since the phytoplankton composition was not the same along the ocean section, the C/N ratio used for this conversion may differ from the north to the south (what C/N ratio was used to estimate NP?).*

*Ref#2: NP data were published in a separate work (Joubert et al., 2011) and the C/N ratio used for conversion was 6.6. They used the same C/N ratio for all stations although there were large differences among them (range of C/N ratio was 5.5 to 10.9). Such a large variability was another argument that C uptake rates estimated using  $^{15}\text{N}$  approach should be considered with caution.*

*Ref#2: Section 4 would focus on the comparison of PP, EP100 and EP600 at the different stations (or preferentially in latitudinal regions of similar characteristics). Martin's curve may be fitted to the data and its constant 'b' discussed in the context of the biogeochemical and biological functioning of the studied areas (see for instance Buesseler et al., 2007, Science). Seasonal maturity may be of importance in this discussion.*

We agreed with this comment that the “b” exponent would have been important to determine. However, to be meaningful, the fitting of the data required precise values for deep export fluxes (EP600), and this was not the case for 234Th-based deep fluxes. Most of our EP600 data are close to zero or even negative and did not allow a “good” fitting using martin’s curve equation.

#### *Shortening the manuscript*

*Section 2.2: the method is deeply described in Pike et al (2005). Thus, there is no need to detail it has much as it is. I suggest reducing the second paragraph (page 7, lines 6-18) to few lines (but keeping the third paragraph as it is since you modified this part of the method).*

We reduced the length of the paragraph dedicated to sample preparation procedure

*Sections 2.3 and 2.6: these sections can also be shortened since the calculations have been detailed in many previous articles.*

We removed equations (2) and (3) in section 2.3 but we decided to keep the equations related to remineralised C flux using the Ba approach since it has not been as extensively used as the  $^{234}\text{Th}$  proxy.

*Results: this section is very very long. Results are almost exhaustively described and cited. This is not needed. In many occurrence part of the text can be shortened into one or very few sentences. Some examples: page 16, lines 6-22; page 17, lines 6-13 and lines 14-29; page 18, lines 10-17 and lines 20-24; page 19, lines 9-17. Also page 18, lines 29-31, and page 19, lines 1-4: delete these sentences from section Results since it is discussion.*

We shortened the text dedicated to results’ description.

*Section 3.3: particulate 234Th and POC data are only slightly used in the discussion. To me, this section and Fig 5 are not needed. I suggest removing them from the ms. Particulate data may be shown as a table in appendix.*

Figure 5 deleted and particulate 234Th and POC data for the two size classes of particle moved to Appendix 2.

*Section 4.1: first paragraph: do not repeat the values. Page 21, lines 27-31: delete the text and refer to Fig. 7 and/or Table 1. Values were deleted in the manuscript and we referred to Table 1 and Fig. 7*

*Table 2: This table is not needed. Remove it from the ms or place it as an appendix.*

We did not agree with this comment, Table 2 resumed statistics of power law fits, and we added the POC/Th ratio obtained from averaging approach for comparison as recommended by referee#1.

#### *Uncertainty and bias associated with assumptions*

*234Th models: advective and diffusive fluxes are neglected and it is assumed (NSS model) that the two visits sampled a single water mass. This should be at least partly discussed. I'm not really sure that BGH and ANTXXIV really match (please add the longitudes of the stations in Table 1). For instance you can check the salinity of the pairs of visits. I think the attempt of calculating NSS fluxes by using the results from both cruises is a great idea but the potential bias linked to the assumption have to be discuss.*

We added longitude of stations used in the NSS approach (Table 1). As mentioned in the manuscript, BGH and ANTXXIV cruise tracks were parallel and stations locations differed only in latitude positions. Salinity and surface T were in good agreement suggesting that water masses with similar characteristics were sampled.

*Also you can use usual vertical diffusion coefficients (for the Southern Ocean) to calculate potential 234Th vertical diffusion and check if the 234Th export fluxes you have estimated would significantly change (or not).*

Potential bias linked to physical processes in the BGH area included in the discussion (section 4.1.a). Vertical advection and diffusion associated to the Antarctic divergence were discussed in previous studies and references to these earlier works were included.

*Uncertainties associated to the NSS fluxes look quite low (table 1). Calculations should be checked.*

We revised our uncertainties associated to NSS fluxes, errors were reevaluated using the equation published by Savoye et al. (2006). In agreement with ref#2, revised errors were substantially higher and taken into account in the revised version.

#### *Other comments*

##### *Introduction*

*Clearly state the aims or objectives of the manuscript. Objectives of the study modified*

##### *Section 4.3*

*Page 23, lines 31-33 and page 23, lines 1-4: it does not mirror plankton abundance but particle abundance. This paragraph does not stand since there is always a strong correlation between POC and particulate 234Th because 234Th adsorb on particles. Delete or deeply reword this paragraph.*

We did not fully agree with this comment. We observed a strong correlation between surface POC with particulate Th, and considering that surface POC mirrored the algal biomass in surface waters, we thought that this relationship could be extended to Th partitioning. It was worth mentioning that this point was one of the conclusions of Rutgers van der Loeff et al. (2011) study (section 4.1 of the latter work).

About the statement that “*there is always a strong correlation between POC and particulate 234Th*”, we considered that although a straightforward relationship could be expected due the

strong affinity of Th for particle surfaces, adsorption of Th may have varied according to several factors such as for instance particle shape and size, colloidal phase, etc. All these factors could have altered the correlation between POC and particulate Th. For example, Rutgers van der Loeff et al. (2011) explored the correlation between beam attenuation and particulate Th and found outliers to the regression line. To our point of view, we found useful to confirm such relationship in our study.

*Page 24, lines 19-22: is this negative relationship significant? Cite the p-value.* Linear regression of EP100 and urea uptake rates was revised. We still obtained a negative relationship (slope : -0.17) with a poor correlation ( $R^2:0.22$ ,  $p = 0.143$ ,  $n = 11$ ). This was taken into account in the revised version.

*Section 4.4 Page 26, first paragraph: another difference is the time integration. Time scale of  $^{234}\text{Th}$  proxy is ca. one month (for SS fluxes) whereas time scale of Baxs proxy may extend to few months. This should be taken into account in the discussion.*

We agreed with the comment. There was a difference in the time scale integrated by the two proxies. As mentioned by Ref#2, Baxs might have integrated a longer time scale corresponding to the growth season. However, as shown during a 37-d survey of a Southern Ocean bloom after Fe fertilisation (Jacquet et al., 2008, gbc), Baxs signal was built over few weeks (2-4) after a bloom starts and so at time scale comparable to  $^{234}\text{Th}$  proxy. This was included in the discussion.

#### *Technical corrections and other details*

*Page 4, line 14: replace “under sampled” with “undersampled”.* Text corrected.

*Page 5, line 12: replace “meso pelagic” with “mesopelagic”.* Text corrected.

*Page 5, line 26: also cite Waples et al 2006.* Reference added.

*Page 7, line 8: replace “which” with “that”.* Sentence removed to shorten the text.

*Page 7, line 23: replace “ro” with “to”.* Text corrected.

*Page 8, line 15 “n=14”: looks contradictory with page 7, lines 1-5; please be consistent or more clear.* We updated the text to describe all samples considered for calibration (total of 14 samples) as mentioned Page 8.

*Page 10, line 1: what filters (QMA filters?)?* We referred both to QMA and Ag filters, text updated.

*Page 13, line 27: I guess you refer to Table 1.* Yes, we referred to Table 1, text modified

*Page 15, line 21: “annuls” may be better than “cancels”?* Text modified.

*Page 18, line 28: >70 $\mu\text{m}$  or >50 $\mu\text{m}$  as indicated on Fig 8.* we compared only with >50 $\mu\text{m}$  size fraction. Text modified.

*Page 18, lines 28-29, sentence “Th matching [...] is less clear”: you may insert “even if the latitudinal trend is similar” at the end of the sentence.* We modified the sentence.

*Page 20, line 29: replace “3” with “4”. Header numbering modified.*

*Page 25, line 21: replace “height” with “depth”. Text modified.*

*Page 26, line 2: also refer to the papers of Cardinal et al. (and others from the same teams)*

*Tables: indicate in the captions what “STZ”, “PFZ”, etc. stands for.* Caption of tables modified.

*Table 3, column “NSS model”, first line: add a digit to the numbers.* Since the revised error were higher (0.9 mmol m<sup>-2</sup> d<sup>-1</sup>), we didn’t modify the number of significant digits

*Figure 1: locate the ANTXXIV stations on this fig. Caption: this is not a cruise track.* Caption already corrected.

*Figure 2: Please use the same scales for all panels; this will help the reader to compare stations. Add a vertical line to locate the MLD. Indicate what hatched and dotted areas stand for. I suggest the hatched area to be extended also for 234Th deficit.* The principal aim of this figure was to compare Th and Baxs vertical distributions, the hatched area highlighted only the zone of excess Th and Baxs accumulation. For clarity, we kept the figure as it was. For 234Th deficit, it was best seen in Figure 3

*Figure 3: indicate in the caption what vertical lines stand for (I guess fronts). Please locate the MLD as a line. Panel a: why this section does not extent down to 1000m? It should.* We modified the panel a to extend to 1000 m depth.

*Figure 5: not really needed. I suggest replacing it by a table in appendix.* As already mentioned, we deleted this figure and data were moved to Appendix 2

*Figure 7: no needed since the data are reported in Table 3. There is no need to show the data from other cruises since they are cited in the text (and the fig. is not exhaustive).*

We didn’t agree with this comment. Our goal for this figure was not to be exhaustive but to compare C/Th ratios measured during transects carried out across the ACC. We decided to keep the figure as it was.

*Figure 9: This fig. is not illustrative: only the numbers are informative. A figure like Fig.8 should be preferred.*

We didn’t agree with this comment. To our point of view, the figure gave a clear view of the geographical variability of export efficiency as gauged against NP for SS and NSS EP100. We didn’t think that a table was needed and we kept the figure as it was.

*Fig. 12: again, I find this figure not very informative. I suggest a figure of panels (one panel per station or zone). Each panel reports fluxes versus depth. Fluxes are NP (or primary production; see above), EP100 and EP600.*

We didn't agree with this comment. This kind of figure has been used in other studies (see for instance Jacquet et al, 2011) and it offered a synthetic view of the transfer efficiency through the first 1000 m. We kept the figure as it was.