Review of the manuscript "High variability of export fluxes along the North Atlantic GEOTRACES section GA01: Particulate organic carbon export deduced from the ^{234}Th method" by Lemaitre et al., submitted to Biogeosciences.

Summary

In this work, Lemaitre et al. present ^{234}Th-derived POC export fluxes from a Geotraces section in the North Atlantic. They step the reader through the process of obtaining the ^{234}Th export fluxes that will be converted to POC export fluxes using particulate C/^{234}Th ratios. The authors also provide export efficiencies using in situ and satellite-based primary production, and they also examine the transfer efficiencies along the section. The results indicate regional differences based on the dominant phytoplankton groups and the sampling time referred to the peak of the bloom. Despite not having the possibility of resampling the same location, the authors assess the potential differences between steady and non-steady state models and they also make an effort to provide an estimate of the impact that physical fluxes (advection and diffusion) would have had on the ^{234}Th export fluxes (and by extension, to the derived POC fluxes). I have some concerns with the scavenging flux calculations but they just need to clarify some aspects and discuss potential limitations (see extended comment below). Apart from that, the manuscript is nicely written and it contains a valuable dataset that will contribute to the body of literature using ^{234}Th to derive POC export fluxes to help characterize the strength and efficiency of the biological carbon pump, particularly in the North Atlantic. With some minor revisions and a bit more of discussion in certain aspects, this manuscript will be a good fit for publication in Biogeosciences.

Minor comments:

L46-49: There are two sentences that are repeated

Methods: I understand that Chl-a, phytoplankton community and nutrients (macro and micro) data are obtained from other studies, properly cited within the manuscript. However, I would have liked to see a small paragraph summarizing the methods used to obtain those datasets, particularly considering that there is a full section (2.1) (which is not really methods but more of a description of the study area), where all these nutrient, phytoplankton and chlorophyll-a data is used. Adding a few lines would make the reader's life easier by not needing to look for those papers. Also, a large part of the information included in 2.1 is also mentioned in the discussion, so the authors might want to consider deleting that section, then no needing to include the methods for those analyses.

L87-88: This statement is a bit vague, hard to quantify. Nanophytoplankton species seem to dominate but in the next sentence the emphasis is on picophytoplankton. Also, what do the authors consider when they say "dominate"? How much higher is the percentage of nanophytoplankton to consider that they are dominant? Above 50%?

L94-95: "Moderate NO_3^-" and then writing $\geq 1\ \mu M$, which does not have an upper limit, might not be appropriate.

L128: How good was the agreement between the deep ^{234}Th samples and the ^{238}U concentrations derived from salinity at those depths?

L131 (and elsewhere in this section): I appreciate the detail in providing the volumes of the spikes and carriers added, however, without the concentrations of those solutions, the information about the volumes added is not really necessary.

L190: "only 10% of the surface value", should be "10% of its maximum value"

2.5 Scavenging fluxes of ^{234}Th:

I am a bit concerned about the assumptions taken for the scavenging fluxes. In this section, the authors present the equations that have been used to obtain those scavenging fluxes but I think there is information lacking. It is not explained how the dissolved and particulate fractions are obtained: How did the authors obtained the dissolved fraction? Did they subtract the particulate fraction from the total to get the dissolved fraction? Which particulate fraction did they use, the sum of the small and the large particles from the in situ pumps? All this information should be included. Section 4.3 discusses export and scavenging fluxes but my doubts still persist.

I am concerned about the potential limitations because, unless I missed something, the total ^{234}Th was collected from the CTD rosette, and the particulate ^{234}Th fraction came from in situ pumps. These are two different sampling methods that could lead to differences when looking at the particulate fraction.

Did the authors calculated the scavenging fluxes using both equations, 8 and 9? In L281 looks like they did but for equation 9 the authors can use the particulate ^{234}Th, obtained directly from the in situ pumps, but for equation 8, again unless I am missing something, they should subtract that particulate fraction from the total ^{234}Th to obtain the dissolved fraction of ^{234}Th.

In summary, I think this section should provide more information to fully understand the calculations done and assess their robustness.

Some small details also from this section:

Eq. 6: The term *V* has been explained in eq. 2, and even though is quite obvious, maybe point out the fact that the subscript *d* refers to dissolved (same for the subscript *p* referring to particulate)

L214: "Equation 5 becomes" it should be "Equation 6 becomes"

L225-226: Could the authors provide the depths for the 0.2% of surface PAR to get an idea about down to what depth is the PP being estimated? Is it more or less close to the depth where the ^{234}Th fluxes are being calculated?

L245: Could you provide more information regarding "the whole productive period"? How was it defined?

L263: At St 26 the Eq depth in Fig 2 is placed at 100 m but it looks like the deficits goes further down and it reaches equilibrium at about 200 m, but there is lower vertical resolution. Table 1 caption mentions Station 26 has a fixed depth, maybe do the same for the caption of Figure 2.

L264: In this line, the definition of PPZ is correct, mentioning its maximum and not the surface value, as done in line 190, however citing Owens et al., 2015 would probably be more appropriate since the work by Marra et al., 2014 does not use the term Primary Production Zone, as used in this manuscript, although they show that in fact, 1% light level (common definition of the euphotic zone depth) might not be deep enough to reach the compensation depth.

Figure 2: Check Eq depth for St 26 or add explanation in the caption (see comment L263). Both, ^{238}U and ^{234}Th symbols (or line, for U) are quite thick and it is hard to see the uncertainties. I am assuming that they are there, just within the width of the symbol, right? Linked to that aspect, ^{238}U activities range from 2.19 to 2.53 dpm L^{-1}, but it is really hard to tell from Figure 2. Minor thing, the ^{238}U line for St 77 seems to be clearer than the rest. It could be useful to color code the labels of the stations to match the colors in Fig 1, or to group them by basins, or indicate to which basin they belong to.

L265: Maybe add "e.g." when citing those two studies where they integrate the Th deficits to the PPZ since there are a few more published studies that have used that same approach.

Figure 3: The uncertainties of the POC to ^{234}Th ratios are not shown on the graph but there are uncertainties reported for POC and ^{234}Th separately in Table S2. It looks like the uncertainties have not been considered in the fitting curve. What would the uncertainties of the ratios at Eq. depth be if those uncertainties on the POC and ^{234}Th content were taken into account when doing the fitting?

L349: The compilations by Le Moigne et al 2013 (global) or Puigcorbe et al 2017 (North Atlantic) include most of the papers cited and will make the citation shorter.

L358: Maybe delete "and argued". Argued is used when one wants to make a point but my guessing is that the authors mean that there is another paper that provides more information.

Also in this line, “details” should be singular (same in L571).

L360: Delete “the” (…PP varied by a factor of…)

L360-369: In some cases PP are presented with uncertainties and sometimes without.

L380-381: Briefly define “productive period” (Is it starting with the PP increase of 30% above winter value mentioned in L449?)

L405-411: The Irminger Basin in spring is a really patchy and dynamic area, as shown by Le Moigne et al (2012) and Puigcorbe et al (2017). The exercise of trying to quantify the impact of physical processes is interesting, however it is a bit of a stretch with just two stations that are also relatively distant. The reference to the Artic and Greenland shelf waters helps to support the author’s argument but I think the patchiness (bloom patchiness) during the productive season should also be mentioned (somehow done later on when discussing the bloom stage during the sampling period).

L417: I do not understand the need of the sentence “The vertical advection can also impact the distribution of ^{234}Th" when previously (L414) there is a sentence that reads as: “the vertical transport of ^{234}Th associated with small-scale structures could represent up to 20%”, it seems redundant.

L487: Maybe reduce the number of references

L495: Similar remineralization although one study was conducted in the tropical Pacific and the other in the North Atlantic Ocean. If the authors want to provide that comparison it might be interesting to discuss a bit the similarities and differences between the studies that lead to comparable values (although some higher values were reported in the tropical Pacific) since one could expect different planktonic communities in both regions, leading to different remineralization intensities.

L509: Stipulate in the following “section”

L583: Maybe specify that the extrapolation curves from Fig 3 were used to obtain the deep POC to ^{234}Th ratios.

L642-644 (and previously mentioned too): Could the authors provide a potential cause of that enhanced remineralization in the cold waters of the Labrador Sea, especially since the biogenic Ba_{xs} also shows signs of remineralization. Is it also due to bacterial activity? For how it is written it looks like the authors believe is not due to bacterial activity.

L651: This statement is not strictly quantitatively proven and although the authors provide the date of the peak of the bloom and PP values, they do not refer to the intensity of the bloom (intensity meaning magnitude of PP? Duration of the bloom? Duration of the bloom with sustained high PP values?). The authors discuss the temporality of the bloom with respect to the sampling time, which has been done in previous studies, but it could be interesting to produce a figure or correlation between the stage of the bloom (and/or intensity of the bloom, if defined) and the magnitude of the POC to support this statement in a more quantitative manner to be able to say that they are, in fact, directly related.

L660: I would delete the first sentence of the point iii) of the conclusions because that is not something that has been studied in this manuscript, it is probably going to be done in the coming Lemaitre et al. in prep. manuscript.