Anonymous Referee 1

Received and published: 15 June 2018

General comments: 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.

We would like to thank the referee for these very positive comments. All suggestions have been taken into account and are detailed below.

- *Lines 46-49: There are two sentences that are repeated.*

The introduction section has changed quite a lot, and the repeated sentence has been deleted.

- *Method: 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.*

As suggested, section 2.1 has been deleted and has been integrated to the Introduction section where we describe now the different biogeochemical basins. Many details of the section 2.1 have been deleted and for that reason, we decided not to describe the methods to acquire the nutrient concentrations or the pigment data. We are citing publications describing the methods though (nutrient and pigment analyses according to Aminot and Kerouel (2007) and Ras et al. (2008), respectively).

Lines 50-70: The low nutrient availabilities (surface nitrate and silicate concentrations < 1 µmol L^{-1}; nutrient analyses according to Aminot and Kérouel, 2007) in the Iberian basin limits the biomass development giving the opportunity to pico-phytoplankton, such as cyanobacteria, to grow (~ 35% of the total Chl-*a* at Station 13; Tonnard et al., in prep.; pigment analyses according to Ras et al., 2008), a situation which is typical for the North Atlantic subtropical gyre (Moore et al., 2008; Zehr and Ward, 2002). The Iberian basin can also be influenced by a local upwelling, close to the Iberian margin (Costa Goela et al., 2016; Zúñiga et al., 2016; http://marine.copernicus.eu/) and potentially fueling the area with nutrient-rich, but upwelling was not active during GEOVIDE (Shelley et al., 2016).
In the subpolar region, in the Irminger and Labrador basins, phytoplankton growth is strongly light-limited seasonally (Riley, 1957) and the key parameter for alleviating these limitations is the progressive shoaling of the mixed layer. There, micro-phytoplankton, such as diatoms, dominate the phytoplankton bloom (≥ 50% of the total Chl-*a*; Tonnard et al., in prep.). Both basins were influenced by strong hydrodynamic features, such as the Irminger gyre, the Eastern Greenland Current (EGC), the Western Greenland Current (WGC), the Labrador Current (LC; Zunino et al., 2017) and the subduction

of the Labrador Seawater (LSW) which was particularly intense (1700 m-deep convection) during the winter 2013-2014 (Kieke and Yashayaev, 2015).
Between the subtropical and subpolar regions, the west European and Icelandic basins represent a transition zone where nutrients and/or light can limit primary production (Henson et al., 2009). During GEOVIDE the silicic acid stock was low (≤ 1 µmol L^{-1}) leading to the growth of nano-phytoplankton, such as haptophytes including coccolithophorids (between 45 and 80% of the total Chl-*a*; Tonnard et al., in prep.). This region is influenced by the Eastern Reykjanes Ridge Current (ERRC) and by the North Atlantic Current (NAC) with the southernmost sub-branch evolving in a cyclonic eddy and the sub-arctic front (SAF). SAF separates cold and fresh waters from the subpolar region and the warm and salty waters from the subtropical region (Zunino et al., 2017).

- *Line 87-88: 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%?*

The general phytoplankton communities observed along the transect are presented in the Introduction Section. The percentage of the phytoplankton communities relative to the total chlorophyll-a concentration are now stated in the Introduction section. By "dominant community", we mean the most represented community (characterized by the highest percentage relative to the total Chl-a).

Lines 50-54: The low nutrient availabilities (surface nitrate and silicate concentrations < 1 µmol L^{-1}; nutrient analyses according to Aminot and Kérouel, 2007) in the Iberian basin limits the biomass development giving the opportunity to pico-phytoplankton, such as cyanobacteria, to grow (~ 35% of the total Chl-*a* at Station 13; Tonnard et al., in prep.; pigment analyses according to Ras et al., 2008), a situation which is typical for the North Atlantic subtropical gyre (Moore et al., 2008; Zehr and Ward, 2002).

Lines 59-60: There, micro-phytoplankton, such as diatoms, dominate the phytoplankton bloom (≥ 50% of the total Chl-*a*; Tonnard et al., in prep.).

Lines 65-67: During GEOVIDE the silicic acid stock was low (≤ 1 µmol L^{-1}) leading to the growth of nano-phytoplankton, such as haptophytes including coccolithophorids (between 45 and 80% of the total Chl-*a*; Tonnard et al., in prep.).

The new section 4.3 makes the connection between POC export fluxes and phytoplankton size and community structure, and presents thus more details on the phytoplankton communities observed at each station. For instance, the differences observed within the Iberian basin, between Stations 1 and 13, are clearly stated.

Lines 509-510: Within the Iberian basin, the highest abundance of pico-phytoplankton was observed at Station 13 (Tonnard et al., in prep.). These conditions are typical of the subtropical and oligotrophic waters (Dortch and Packard, 1989).

Lines 514-517: However, Station 1 was characterized by a greater POC export that could be related to the mixed proportion of micro-, nano- and pico-phytoplankton and thus to the greater proportion of larger cells such as diatoms or haptophytes, increasing the particle sinking velocity.

- *Lines 94-95: "Moderate NO_3^-" and then writing ≥1 µM, which does not have an upper limit, might not be appropriate.*

Most of the details on nutrient concentrations have been deleted; but when specified in the Introduction section, the upper limits are stated.

Lines 50-51: The low nutrient availabilities (surface nitrate and silicate concentrations < 1 µmol L^{-1}; nutrient analyses according to Aminot and Kérouel, 2007) in the Iberian basin…

Lines 65-66: During GEOVIDE the silicic acid stock was low (≤ 1 µmol L^{-1}) leading to the growth of nano-phytoplankton …

- *Lines 128: How good was the agreement between the deep ^{234}Th samples and the ^{238}U concentrations derived from salinity at those depths?*

The $^{234}Th/^{238}U$ ratio averaged 1.00 in deep samples, and on 15 deep samples, the standard deviation was about 0.02, highlighting the perfect equilibrium between both radionuclides. Details have been added in Section 2.1.

Lines 106-109: Deep samples (between 1000 and 3500 m) were taken for the calibration of the low level beta counting (Rutgers van der Loeff et al., 2006) based on the knowledge that ^{234}Th and ^{238}U are generally in secular equilibrium at such depths (in this study, the deep ocean average $^{234}Th/^{238}U$ ratio = 1.00 ± 0.02; n=15).

- *Lines 131 (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.*

This is very true. As suggested by reviewer 2, many details, such as the volumes, have been deleted in order to get this manuscript less tedious to read.

- *Line 190: "only 10% of the surface value", should be "10% of its maximum value".*

OK. This has been modified.

Lines 171-172: … as well as for z representing the base of the primary production zone (PPZ), i.e. the depth where in-situ fluorescence was only 10% of its maximum value (Owens et al., 2014).

- *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.

Right, thank you for notifying this. We included the missing information and hopefully the section is clearer now.
To get the dissolved fraction, we subtracted the particulate from the total fraction. The particulate fraction was the sum of the small and large size particulate fractions (SSF+LSF).
The total and particulate fractions were not obtained by the same sampling method. It would be indeed ideal to have both fractions from the same device. However, particulate ^{234}Th cannot be obtained accurately on low volume samples (i.e., 4L) because of the adsorption of dissolved ^{234}Th on filter for example (Buesseler et al., 2006).

Lines 195-199: To estimate the rate of removal of ^{234}Th from the dissolved to the particulate form, i.e., the scavenging flux of ^{234}Th (Coale and Bruland, 1985), we deduced the dissolved ^{234}Th activities by subtracting the particulate (SSF+LSF) from the total ^{234}Th activities, keeping in mind, though, that the sampling method for the total and particulate phases differed. Because the sampling resolution was different, total ^{234}Th data were averaged at the sampling depth of particulate ^{234}Th.

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.

The four equations, presented in the previous manuscript were describing the dissolved and particulate activities with a 2-box model. To calculate the scavenging fluxes, we actually use only 2 equations (Eq. 6 and 7, below). In order to get this section clearer and again in order to get a manuscript less tedious to read, we kept the essential equations.

Lines 200-209: The mass balance equation for dissolved ^{234}Th can be written as follows:

$$\frac{dA_{Thd}}{dt} = \lambda A_U - \lambda A_{Thd} - J + V \qquad (6)$$

where A_{Thd} is the activity of dissolved ^{234}Th in dpm L^{-1}; A_U and λ are defined in Eq. 2; J is the net removal flux from the dissolved to the particulate form (scavenging flux) in dpm L^{-1} d^{-1}; and V is the sum of the advective and diffusive fluxes in dpm L^{-1} d^{-1}.
Using again the steady state assumption (dissolved ^{234}Th activities remain constant over time) and ignoring the physical terms (V), Eq. 6 becomes:

$$J = \lambda \int_0^z (A_U - A_{Thd}) dz \qquad (7)$$

where J in dpm m^{-2} d^{-1} is the net flux of scavenging integrated to the depth z. In our case, the calculation was performed at the Eq depth for comparison with the ^{234}Th export flux (P in Eq. 3).

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)

As both particulate and dissolved fractions are now described in two different sections (2.3 and 2.4), the subscripts have been deleted.

- *Line 214: "Equation 5 becomes" it should be "Equation 6 becomes"*

OK. This has been modified.

- *Line 225-226: 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?*

OK. This has been added in section 2.6.

Lines 237-239: Daily PP was then estimated by integrating the uptake rates from the surface down to 0.2% of surface PAR, which was located between 48 and 116 m depending on the station. The 0.2% of surface PAR depth was roughly corresponding to the Eq depth although, at few stations, a 42 m difference was observed.

- *Line 245: Could you provide more information regarding "the whole productive period"? How was it defined?*

OK. This has been added in section 2.7.

Lines 252-253: The whole productive period is the period between the bloom start (defined by a PP increase of 30% above the winter value) and the sampling date (Fig. 5).

- *Line 263: 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.*

OK. The caption of Figure 2 has been modified.

Figure 2: Profiles of the total ^{234}Th (closed circles), total ^{238}U (black dotted vertical line) and particulate ^{234}Th activities for the small size fraction (SSF; 1-53 µm; open diamonds) and for the large size fraction (LSF; >53 µm; closed triangles). All activities are expressed in dpm L^{-1}. The horizontal black line is the Eq depth (depth where ^{234}Th returns to equilibrium with ^{238}U), and the horizontal green line is the depth of the PPZ (primary production zone). Error bars are plotted but may be smaller than the size of the symbols. Note that the Eq depth at Station 26 is fixed at 100 m because of the lower sampling vertical resolution.

- *Line 264: 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.

Right, the citation has been changed. Moreover, the definition of the PPZ depth is now given only in section 2.3.

Lines 169-176: Eq. 3 has been solved for z taken as the depth (Eq) at the base of the ^{234}Th deficit zone (Eq = depth where ^{234}Th activity is back to secular equilibrium with ^{238}U) as well as for z representing the base of the primary production zone (PPZ), i.e. the depth where in-situ fluorescence was only 10% of its maximum value (Owens et al., 2014). The Eq depth matched relatively well with the PPZ depth, and on average, difference between both was only 16 m, with the largest difference (~ 60 m) at Stations 1, 32 and 51 (Fig. 2). Considering that there can be export (or remineralisation) below or above the PPZ depth, only the export fluxes at the Eq depth will be discussed as they represent the fully-integrated depletion of ^{234}Th in the upper waters and thus the maximal export.

- *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.*

Following your suggestions, we modified the figure and its caption. Indeed, all the errors are indicated but may be hidden by the symbols.

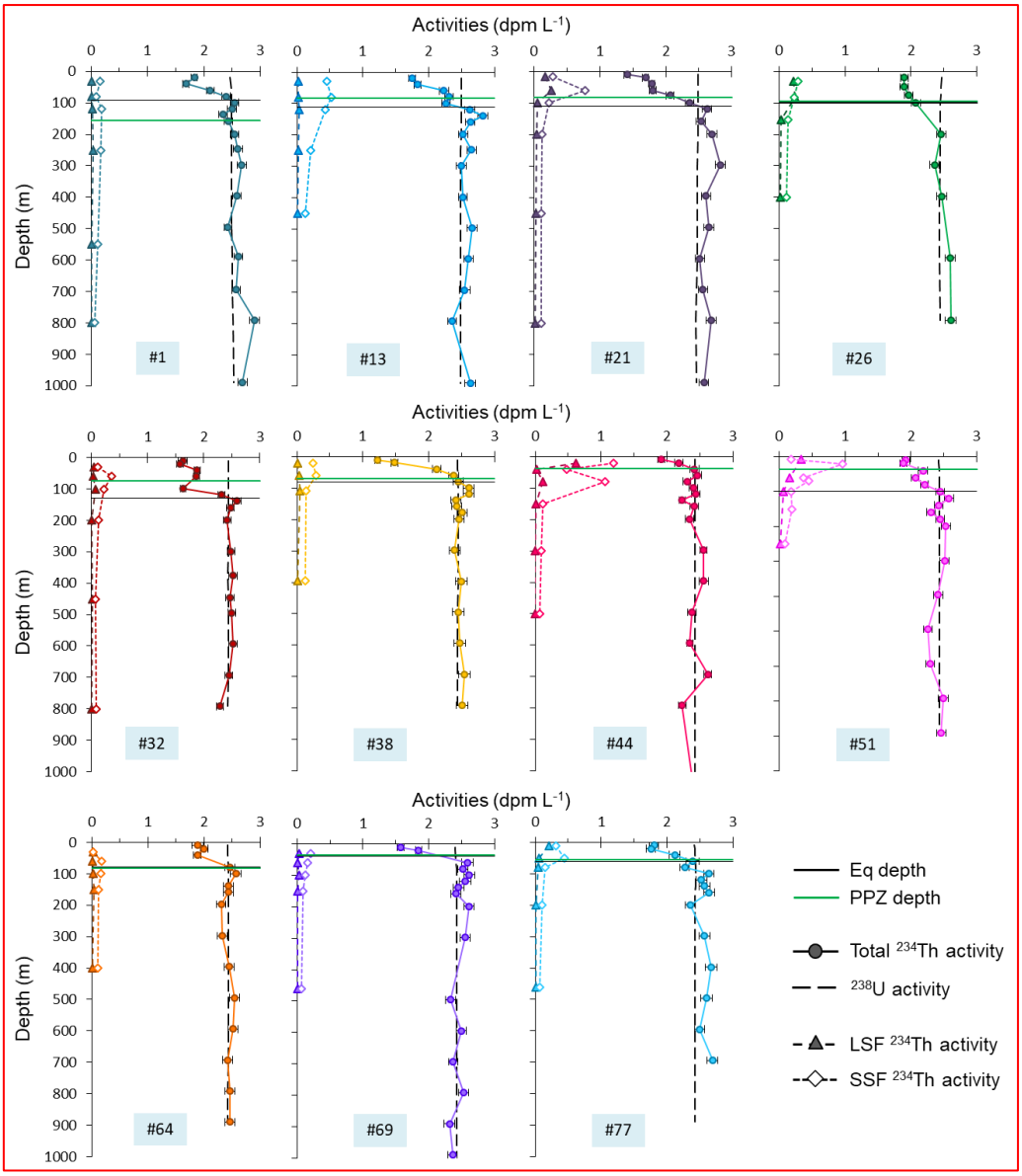

Figure 1: Profiles of the total ^{234}Th (closed circles), total ^{238}U (black dotted vertical line) and particulate ^{234}Th activities for the small size fraction (SSF; 1-53 µm; open diamonds) and for the large size fraction (LSF; >53 µm; closed triangles). All activities are expressed in dpm L^{-1}. The horizontal black line is the Eq depth (depth where ^{234}Th returns to equilibrium with ^{238}U), and the horizontal green line is the depth of the PPZ (primary production zone). Error bars are plotted but may be smaller than the size of the symbols. Note that the Eq depth at Station 26 is fixed at 100 m because of the lower sampling vertical resolution.

- *Line 265: 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.*

This sentence has been removed from the Result section.

- *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?*

The figure and its caption have been modified. Errors of POC to ^{234}Th ratios are shown and even if the size of the symbols has been reduced, they may not be visible. In the caption, we now give the median percentage of these errors relative to the value of the ratio.

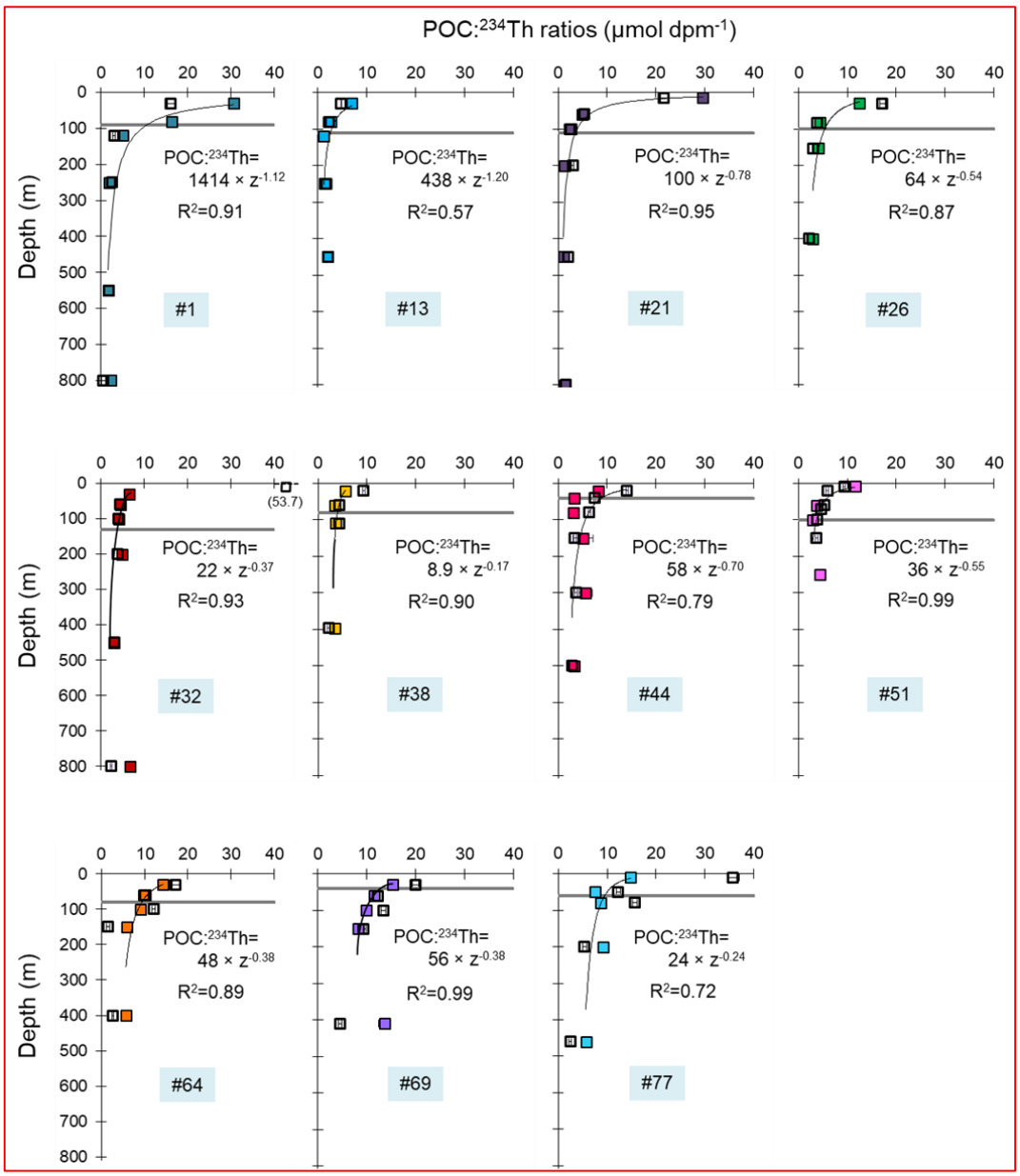

Figure 2: Profiles of the POC:^{234}Th ratios (µmol dpm^{-1}) in the SSF (open symbols) and LSF (closed symbols). The Eq depth, where ^{234}Th is back to equilibrium with ^{238}U, is indicated with the grey horizontal line. The thin black line represents the power law fit (POC:^{234}Th=a×Z^{-b}) of the LSF. The median percentage errors on POC:^{234}Th ratios are respectively representing 5 and 6% of the value for the SSF and the LSF. Error bars are plotted but may be smaller than the size of the symbols.

The uncertainty of the extrapolated ratio at the Eq depth is deduced from the fit and not from the analytical uncertainties of both POC concentrations and particulate ^{234}Th activities. The error deduced from the fit is much larger than the one from the analytical error. This has been specified within the manuscript, section 2.5.

Lines 216-221: We estimated POC export fluxes by multiplying the ^{234}Th export flux with the POC:^{234}Th ratio, both determined at the Eq depth. A power law fit was used to determine the POC:^{234}Th ratios at Eq (Fig. 3). Errors of the POC:^{234}Th ratios extrapolated at the Eq depth are deduced from the power law fit, using a root sum of square method. This error is much larger than analytical errors of both POC concentrations and particulate ^{234}Th activities. POC fluxes were determined by using the POC:^{234}Th ratios of the LSF (> 53 µm) as well as the SSF (1-53 µm) samples, and both estimations were compared (Table 2).

- *Line 349: 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.*

Right, this has been modified.

Lines 225-228: As large and rapidly sinking particles usually drive most of the export (Lampitt et al., 2001; Villa-Alfageme et al., 2016), most of the studies dedicated to POC export fluxes in the North Atlantic used the POC:^{234}Th ratios from the LSF (see Le Moigne et al., 2013b; Puigcorbé et al., 2017).

- *Line 358: 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).*

OK.

- *Line 360: Delete "the" (…PP varied by a factor of…).*

OK.

- *Line 360-369: In some cases PP are presented with uncertainties and sometimes without).*

OK, we removed the uncertainties in the text. They are written in Table 3.

- *Line 380-381: Briefly define "productive period" (Is it starting with the PP increase of 30% above winter value mentioned in L449?).*

OK. This sentence has been removed from the Discussion section and the productive period is defined in section 2.7.

Lines 252-253: The whole productive period is the period between the bloom start (defined by a PP increase of 30% above the winter value) and the sampling date (Fig. 5).

- *Line 405-411: 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).

This is right, thank you for your comment. The patchiness of the bloom within the Irminger basin is now mentioned when talking about impact of the physical processes.

Lines 415-419: The Irminger basin in spring is a really patchy and dynamic area (Ceballos-romero et al., 2016; Le Moigne et al., 2012; Puigcorbé et al., 2017) but the relatively high variability of the ^{234}Th fluxes found at these two stations (321 and 922 dpm $m^{-}2$ d^{-1}, respectively) may also indicate a potential influence of lateral advection. The higher export flux at Station 51 could reflect an input of ^{234}Th depleted waters originating from the Arctic and/or the Greenland shelf.

- *Line 417: 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.*

Right, the sentence has been deleted.

- *Line 487: Maybe reduce the number of references.*

Right, this sentence is now in the Introduction section.

Lines 92-94: In the subsurface waters any excess of ^{234}Th relative to ^{238}U, is taken to reflect particle break-up and remineralisation by heterotrophic bacteria and/or zooplankton (Buesseler et al., 2008; Maiti et al., 2010; Savoye et al., 2004).

- *Line 495: 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.*

Right, thank you. Given the fact that both studied areas (the North Atlantic and the oxygen minimum zone of the tropical Pacific ocean) are very different, we preferred to remove this comparison. However, as Black et al. (2017) were presenting R100 values for the first time (to my knowledge), we kept citing their work when presenting the calculation of the R100 values.

Lines 188-193: To estimate the intensity of shallow remineralization, export flux was also calculated for the Eq+100 m depth horizon. In case of any ^{234}Th excess below Eq due to remineralisation, export fluxes integrated until Eq+100 m will be less than when integrated until Eq. Following Black et al. (2017) the reduction of the ^{234}Th flux, R100, is expressed as:

$$R100 = P_{Eq} - P_{Eq+100} \quad (5)$$

where R100 is the flux reduction in dpm m^{-2} d^{-1} and P is the ^{234}Th export flux estimated at Eq or Eq+100.

- *Line 509: Stipulate in the following "section".*

OK. This has been checked and modified in the entire manuscript.

- *Line 583: Maybe specify that the extrapolation curves from Fig 3 were used to obtain the deep POC to ^{234}Th ratios.*

OK.

Lines 534-537: Note that the POC export flux at Eq+100 (Table 3) was calculated by multiplying the ^{234}Th flux at Eq+100 by the POC to ^{234}Th ratio of large particles for the same depth. The POC:^{234}Th ratio at Eq+100 was deduced from a power law fit (Fig. 3).

- *Lines 642-644 (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.*

Right, the potential cause of the enhanced remineralization in the Labrador Sea proposed in Lemaitre et al. (2018) has been added in Section 4.4.

Lines 585-590: This is also in agreement with the highest R100 and carbon remineralisation flux determined with the Ba_{xs} proxy (Lemaitre et al., 2018). The central Labrador basin, in proximity of Station 69, was characterized by strong subduction of the LSW during the winter preceding the GEOVIDE cruise. This downwelling could have promoted an important organic matter export leading to important prokaryotic heterotrophic activity in mesopelagic waters. This enhanced remineralisation was still observed during GEOVIDE as traced by a large mesopelagic Ba_{xs} content (Lemaitre et al., 2018).

- *Line 651: 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?).*

This is a very good point as the intensity of the bloom can be defined in different ways: at sampling time (illustrated by the *in-situ* PP), as the maximal PP intensity along the season or as an average PP intensity along the season.
In Section 4.2, we propose explanations for the magnitude of the POC exports according to these different definitions. The low seasonal PP average at Station 13 is suggested to explain the low POC export there.

Lines 474-476: One of the lowest POC export flux was determined at Station 13 in the Iberian basin, where the intensity of the bloom remained rather low along the season (seasonal VGPM-PP=81 mmol m^{-2} d^{-1}, Fig. 5) due to oligotrophic conditions (depleted nutrients; Fonseca-Batista et al., 2018).

The maximal PP intensity (highest PP peak along the season) observed just before the sampling in the west European basin might explain the high POC export there.

Lines 483-484: PP appeared maximal just before the sampling in the west European basin (Fig. 4 and 5) and could have promoted these high POC export.

The high *in-situ* PP intensity in the Irminger basin indicates that the bloom is reaching its maximum and that export did not yet start at sampling time.

Lines 490-493: Indeed, this area had the highest *in-situ* PP, a high proportion of particulate ^{234}Th in surface waters (reaching 94% of the total ^{234}Th activity at Station 44) and a very low P/J ratio, indicating that ^{234}Th was retained in the upper waters rather than being exported (Fig. 6; Table 1).

> *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.*

Thank you very much for this great suggestion. In Figure 8, we attempt to illustrate the impact of the intensity of the bloom at sampling time (using the *in-situ* PP values), the stage of the bloom (using the percentage of the *in-situ* PP relative to the maximal VGPM-PP along the season) and the different phytoplankton communities on the POC export fluxes. A significant negative correlation is found between the stage of the bloom and the POC export when not considering the stations sampled between two PP peaks (or before the PP peak: Stations 26, 32, 38). In this figure, we can also see that the stations sampled close to the bloom maximum (%max seasonal PP > 80% and with a high *in-situ* PP intensity) are characterized by low POC exports.

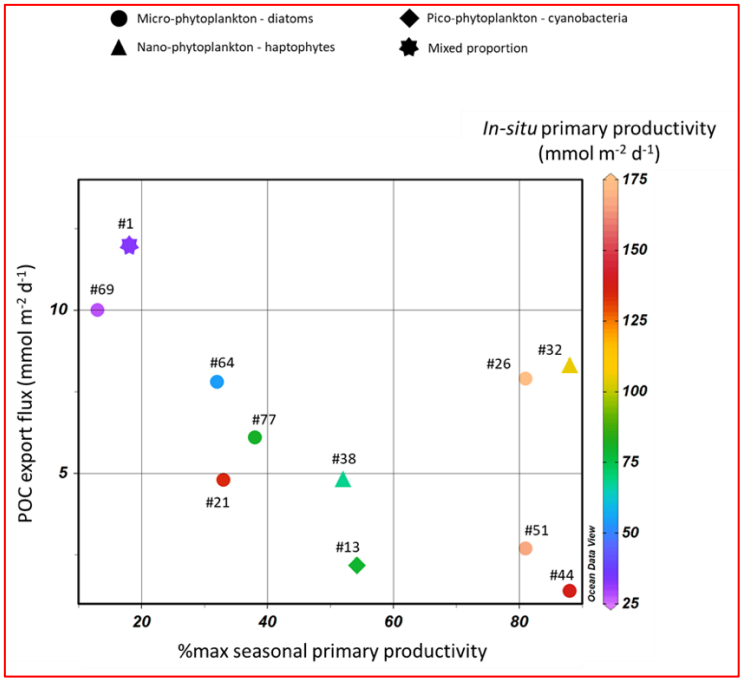

Figure 8: Percentage of the *in-situ* primary productivity (PP) relative to the maximal VGPM-PP along the season (%max seasonal primary productivity) in function of the POC export fluxes

determined at the Eq depth. The %max seasonal primary productivity illustrates the stage of the bloom (i.e., a %max seasonal primary productivity equalling 100% corresponds to a sampling time at the bloom peak). This relationship is significant when not taking into account the stations sampled between two PP peaks (Stations 26, 32 and 38, see Fig. 4): R^2=0.77 and p-value<0.01. The *in-situ* PP measured at sampling time is indicated with the colours in order to indicate the bloom intensity. The dominating phytoplankton community is also indicated, with circles indicating micro-phytoplankton dominance (with a majority of diatoms), triangles nano-phytoplankton dominance (with a majority of haptophytes) and diamonds pico-phytoplankton dominance (with a majority of cyanobacteria). Note that Station 1 is represented by a star because of the mixed proportion of micro-, nano- and pico-phytoplankton.

- *Line 660: 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.*

Right, details on this future paper has been removed, especially the potential impact of the lithogenic particles. However, the potential impact of the particle density related to the presence of diatoms or coccolithophorids on export is still reported.

Lines 598-603: The magnitude of the fluxes seems also to be related to the phytoplankton size and community structure. One of the lowest POC export fluxes was found at the stations where pico-phytoplankton dominated the community. In contrast, the areas composed by micro- and nano-phytoplankton were characterized by high POC export fluxes. These areas were dominated by diatoms or coccolithophorids, known to strongly ballast the POC export fluxes. This suggests that the size as well as the composition and density of the particles likely play an important role on the particulate sinking velocities and thus on the magnitude of the POC export fluxes.