Review of Balancing water column and sedimentary ^{234}Th fluxes to quantify coastal marine carbon export, by Healey et al.

This article provides quasi-seasonal coupled water column and surficial sediment sampling of Th-234 and POC to evaluate and refine the carbon budget in the Bedford Basin. I find the research necessary because, as highlighted by the authors, not many studies using the commonly applied approach of Th-234 to assess carbon export have been conducted in coastal areas. Additionally, the combination of water column and sediment measurements is not a usual approach either, yet it provides complementary data allowing to better understand the fate of the particle fluxes in coastal areas. By combining both types of sampling, the authors conclude that there is no major particle loss in the Bedford Basin and that the magnitude of the export and burial fluxes is similar to those reported in previous studies in coastal areas, which are relevant sites for organic carbon sequestration.

The paper is well presented, the text and the figures are clear and provide the necessary information to support their findings. I have specific comments regarding some aspects that need clarification or small corrections, but overall, the study is robust and contributes to the growing body of literature discussing carbon budgets in coastal environments.

We thank reviewer 1 for their thoughtful and detailed comments, which helped us to significantly improve the manuscript. We agree with all their comments and made the corresponding changes in the manuscript. Below, we answer each of their comments in detail. The reviewer's comments are in blue, our responses are in black.

- **L111**: It mentions that the water samples were collected with 10L Niskin bottles and that, after Th-230 was added, 2L were processed for total Th-234. I believe the way it is written can lead to confusion, since it might look like 10L were collected, spiked and then 2L subsampled, which I believe was not the case.

Response: In the revised manuscript, we clarify that 10 L Niskin bottles were used for sample collection, but only a 2 L subsample was spiked and processed for total ^{234}Th measurements.

- **L143**: Please specify the pore size used to obtain filtered and UV treated seawater (no need to specify in that line the pore size of QMAs since it is mentioned in L141-142)

Response: The pore size used for seawater filtering was 0.2 µm. This is now mentioned in L143 of the revised manuscript. We have also removed the repetition regarding the QMA pore size, which the reviewer correctly identified.

L152-154: "*For comparison, the POC:^{234}Th ratio on the small particle fraction was also calculated using Eq. 4*" is not correct. Please rephrase since Eq. 4 calculates POC flux, no ratios.

Response: We agree with reviewer 1 and have rephrased the relevant sentences. The POC flux can be calculated with either (a) the POC:^{234}Th ratio on small particles or (b) the POC:^{234}Th ratio on large particles using Eq. 4. We clarify in the manuscript that the fluxes reported using either a) or b) represent two alternative approaches to estimating flux, and that these should not be considered additive, but rather complimentary perspectives on export.

- **L151-154** and **Eq. 4** can be simplified by adding a subscript next to the ration in the formula so that the text just needs to say that POC fluxes are calculated using Eq. 4 using the ratios of the different particle fractions.

Response: Change made as suggested.

- **L157-165**: The first two sentences seem a bit repetitive and can be streamlined. I think the repetition of "*novel proxy and residual beta activity*" makes it look repetitive. Use the "abbreviation" R_{ap243} instead after defining it at the beginning.

Response: Change made as suggested.

- **L169**: Can you provide the uncertainty of n_{p2} and the blank? These blank counts come from the dipped filters, correct? Also in that line, notice that cpm was used before and defined later.

Response: We thank reviewer 1 for pointing out these issues. As the reviewer correctly suggests, the blank counts (n_0) were obtained from dipped filters. Dipped filters are deployed with the pump to depth, but, importantly, no seawater is pumped through them. They can be considered a seawater process blank and are often referred to as a "dipped blank", following Lam et al., 2015.

We now report the uncertainties for both n_{P2} and n_0 based on counting statistics.

The revised sentence reads:

"Where n_{P2} is the second β counting rate of particulate ^{234}Th (0.30 ± 0.07 cpm), n_0 is the blank mean obtained from dipped filters (0.24 ± 0.02 cpm), ϵ is the detector efficiency (0.3), and V is the volume of seawater pumped (L). The term "cpm" (counts per minutes) is now defined at its first occurrence.

- **Section 2.5** Excess ^{234}Th inventories requires more detail:

Response: We have now revised section 2.5 in the manuscript to include more details on the method & processing of the sediments. The details are explained below.

- How thick were the sections of the cores (0.5 cm, 1 cm) and how far down where they cut/measured (6 cm, based on Fig. 8?)

Response: Cores were generally sliced into 0.5 cm thick sections. In some upper core intervals, we took 1 cm thick sections, as the sediment was too soft to cut thinner sections. To clarify, all cores were measured for ^{234}Th down to 10 cm, some cores were only measured down to 6 cm for TOC.

Ideally, we wanted to slice cores into 0.5 cm samples for higher resolution in the upper most sediments. If this was not possible, the cores were sectioned into 1 cm samples.

- How quickly were the cores cut after collection? Were they kept in a freezer until processing?

Response: Cores were either sliced on the same day they were retrieved in the field or stored in a walk-in fridge (4°C) for 1 – 2 days (max) prior to slicing.

- How were they dried? For how long approx.? Was the weight checked multiple times to ensure that the sediment was dry?

Response: The sediments were dried at 55∘C in our laboratory oven for approximately 24 hours. After this period, they were weighed and returned to oven and reweighed at regular intervals until a constant weight was achieved.

- Was the dry weight corrected for salt content?

Response: Yes, the dry weights were corrected for salt content. Salt contribution from porewater was estimated based on salinity measurements of the overlying water and subtracted from the measured dry weight. This clarification has been added to the method section of the revised manuscript.

- How much sediment was approximately used for the measurement?

Response: Approximately 15 g of dry sediment was used for gamma counting.

- How long were the samples measured for and how long after collection (since Th- 234 has a 24.1 d half-life)?

Response: Gamma analysis was typically within less than 24 days, i.e., one half-life of sediment retrieval in the field. Samples were counted for 7 to 24 h depending on the time to achieve counting uncertainties of less than 5 % on the primary peaks.

- How was the mean supported activity obtained? Did you remeasure all the sections or just certain ones? L465 suggests only section 4.5 to 6 was used as supported. Please provide more details.

Response: We thank reviewer 1 for this comment. All cores were measured for ^{234}Th down to 10 cm. The mean supported ^{234}Th activity was determined from the deepest sediment intervals in each core where no excess ^{234}Th was detectable, i.e., below the penetration depth of excess ^{234}Th. For our cores, this was consistently at ~4.5–6 cm depth (i.e. at 4.5–6 cm, the ^{234}Th activities were consistent and showed no further decrease in ^{234}Th activity). These deepest sections were measured directly and used to define the supported activity, which was then subtracted from the total ^{234}Th activities in the overlying sediments to calculate excess ^{234}Th. We have now clarified this in the methods section.

- What type of gamma detector was used? The gamma counting was done using the petri-dish, so it was not done in a well-type gamma counter, right? Mention it.

Response: Correct. Gamma counting was conducted using Canberra Intrinsic High-Purity Germanium (HPGe) detectors in a planar configuration, not a well-type of detector. Samples were counted in low-background geometry using petri dishes, which were placed directly on the detector surface to optimize efficiency for low-energy gamma emissions (such as the 63.3 keV peak of ^{234}Th). We have added this information in the revised paper.

- **L191-192**: Please explain how the organic carbon samples were processed before measurements. Were they also fumed with HCl, as per filters?

Response: We did not fume the sediment samples with HCl prior to CN analysis, and spell this out in the revised text.

Previous work on Bedford Basin sediments has compared acid-fumigated and non-acid-fumigated sediment samples (Black et al., 2023). The average carbon content did not differ between the acid fumigated (5.5% ± 0.5%, n=29) and non-acid fumigated (5.7% ± 0.8%, n=39). This published finding is consistent with additional unpublished measurements that were made over years at Dalhousie University. Collectively, the data imply that most of the sedimentary carbon is organic in nature, which is why fumigation is not necessary. We thank the reviewer for their comment, which allowed us to clarify the text.

- **L205**: Please provide a threshold of Chl-a or some definition for the blooms in the region or a usual range of Chl-a values during blooms. The text reads that a 11.1 mg/m^3 chl-a signal is a weak bloom but would be good to have some sort of a scale to assess that.

Response: Based on observations made by the Bedford Basin Monitoring Program since 1993, "background" chlorophyll-a typically ranges from ~3 to 10 mg m^{-3} (surface, 1–10 m) in the summer. In contrast, spring blooms range from ~7 to 10 mg m^{-3} and up to ~28 mg m^{-3}. Extreme short-lived events have reached chlorophyll concentrations over 100 mg m^{-3}. Therefore, we define a "weak bloom" in Bedford Basin as chlorophyll-a concentrations just above the summer background (e.g., >10 mg m^{-3}) but below intense bloom threshold of 28 mg m^{-3}. We will include these ranges and citations to make the terminology clearer in a revised manuscript.

- **Figure 4**: "*Total particulate ^{234}Th is equal to the sum of the large and small particles*" Why is that sentence there for Th-234 and not for POC? Shouldn't the y-axes just be Particulate POC and Particulate ^{234}Th, since there are different size fractions represented in both plots?

Response: We agree that axis labels and definitions should reflect the size-fractionation. In our study, particulate material was collected sequentially on a 51 µm screen and a 1 µm filter from the same pump casts. After corrections for blanks, yield (for ^{234}Th), and volume, we define total particulate ^{234}Th (>1 µm) as the sum of ^{234}Th (1–51 µm) and ^{234}Th (>51 µm). Similarly, total particulate POC (>1 µm) is defined as the sum of POC (1–51 µm) and POC (> 51 µm). We have clarified this in the methods section of the revised manuscript and updated the figure axes to read "Particulate ^{234}Th (>1 µm)" and "Particulate POC (> 1 µm)". The legend now also correctly identifies the 1–51 µm and >51 µm fractions.

- **L431**: "*POC export fluxes (Fig. 6) were calculated following Eq. 4 and using >51um ratios (Fig. 5*a)" it's correct, but not complete, because the export fluxes shown in Fig. 6 are also calculated using the 1-51um ratios from Fig. 5b. Please refine this paragraph to streamline it. You could cite both figures and then start talking about the small fraction fluxes, were no pattern with depth was observed, and then talk about the large fraction fluxes, since they are discussed a bit more.

Response: We thank the reviewer for this suggestion. We have revised the paragraph to clarify that fluxes were calculated using both the small (1–51 µm) and large (>51 µm) POC:^{234}Th ratios (Figs. 5b and 5a, respectively). We now introduce both fractions, then describe the small fraction first, followed by the large fraction, where more variability was observed. L431 now reads "POC export fluxes (Fig. 6) were calculated following Eq. 4 using both the 1–51 µm and >51 µm POC:^{234}Th ratios (Figs. 5b and 5a, respectively). Fluxes based on the small size fraction (1–51 µm) showed no consistent pattern with depth, while fluxes from the large size fraction (>51 µm) varied more strongly. At the deepest depth sampled (60–65 m), large fraction POC fluxes had a mean (± s.d.) of 20 ± 14 mmol C m^{-2} d^{-1} (Fig. 7a) and ranged from 3.6 ± 1.1 to 44.5 ± 4.1 mmol C m^{-2} d^{-1}."

- **Figure 6**: The a) should be moved in front of "*Profiles of cumulative….*" since "*Bedford Basin 1D steady state fluxes in 2021-2024"* applies to the three panels. Make the y-axes to match between the three panels, otherwise it creates a visual distortion as if the POC ones were deeper. Why all the different symbols in the Th-234 flux? Pick one, as done for the POC fluxes based on particle size. Also, the R^2 shown in graphic a) is not explained. Maybe add the whole linear regression and cite the figure in the main text (L428).

Response: We thank the reviewer for their insightful comments and keen eye. We have since made the appropriate changes by making the y-axis consistent and choosing one symbol. The R squared value comes from the linear regression of all dates (L427). This R-squared value will be added into the text of the manuscript.

I am a bit confused regarding the information provided in Fig. 6 in the sediment part of panels b) and c) and what it is shown in Fig. 7. Isn't it the same data? Why show it twice? I would combine these figures as if Fig. 7 was a zoom-in of the sediment part of Fig. 6b and Fig. 6c. Please double check that the numbers match, I have the impression that, for example, Jan_2024 >51um POC flux shown in Fig. 6b is higher than when plotted in Fig. 7a

Response: Figure 6 shows the flux at 20 m, 40 m, 60 m, and the sediment accumulation flux.

Figure 7 shows the 60 m flux again to further emphasize (1) the quantity that is being available for deposition on the seafloor and (2) the difference between seasons.

L462-463: It is confusing to say that the EQ depth was always ~4.5cm and then write that the majority of $Th_{xs,0}$ was confined to the top ~2-4.5cm. Also, looking at Figure 8 (please cite figure in that first sentence), the largest excess is found in the upper 1.5 cm.

Response: We thank the reviewer for pointing out this confusing phrasing. We have revised the text to cite Fig. 8 and to clarify that the majority of excess ^{234}Th was concentrated in the upper ~2 cm (with the highest activities in the top 1.5 cm), and that equilibrium with supported ^{234}Th was consistently reached by ~4.5 - 6 cm. The revised version of the text now reads – "Excess ^{234}Th was concentrated in the surface sediments (upper ~2 cm; Fig. 8), with the highest activities observed in the top 1.5 cm. Excess ^{234}Th decreased rapidly with depth, and equilibrium with supported ^{234}Th was consistently reached by ~4.5 - 6 cm.

- **L463**: "*Excess ^{234}Th in the EQ was variable*". There should not be excess ^{234}Th at the equilibrium depth. Do you mean above the EQ?

Response: Yes, we meant "above" the EQ and thank the reviewer for catching the mistake. We changed the manuscript accordingly.

L467: "*were consistent with core depth between 0 – 4 cm*". Not sure what do you mean. Are you referring to the concentrations and percentages found in those depths in other similar cores? Or do you mean that concentrations did not vary in the upper 4 cm of this study's cores? What concentrations and percentages of POC did you find between 4 cm and 6 cm? Was POC only measured in the upper 4 cm? There are POC:Th ratios deeper than 4 cm, were they extrapolated?

Response: Thank you for catching this. What we mean here is that the % POC concentrations were both consistent downcore (0 – 6 cm) and between cores. The text is now revised to include this. This consistency downcore and between cores was also found in Black et al. (2023) who observed that average % POC was statistically the same for all their core retrievals in different seasons. This consistency is also evident in historical coring efforts in the Bedford Basin (Buckley et al., 1995). The POC:^{234}Th ratios presented were not extrapolated.

- **Figure 8**: Please polish the caption for panel b). The a) should be placed before "decay corrected". b) says "*POC:$^{234}Th_{xs,0}$*" but the title says "*POC:^{234}Th*". Also, no need to explain the supported in the caption, it has been explained in the main text and it is not shown in the figure.

Response: Change made as suggested.

- **L559-560**: Could you expand on Lampitt (1985) statement? I understand the previous sentence about the fact that the ratios would be higher if there was resuspension of the top sediments, but I feel like Lampitt's sentence is not properly integrated in the discussion or lacking some more information. Not only the composition of the particles in the sediment-water interface facilitates resuspension, but hydrodynamics are also crucial, meaning that, even if those particles are lithogenic and not detrital, with enough hydrodynamics they can be resuspended. Lampitt's 1985 study area is also 4000 m instead of the 70 m at Compass Station. Not sure it is the best comparison to make, unless you want to explain a bit more.

Response: We thank reviewer 1 for their insight here. We have since edit the manuscript to retain Lampitt's (1985) observation as a historical reference on particle composition facilitating resuspension but clarify that a direct comparison is not made. In section 4.2, we now describe more that our discussion of sediment resuspension is informed by both particulate β activities and local hydrodynamics. Although residual particulate β activities provides evidence of limited resuspension of the fine fraction, we now also include information about hydrodynamics & bottom currents (as you suggest). Recent physical oceanography measurements (Mali et al., 2024) report very weak near-bottom currents (typically < 1–2 cm s^{-1}) even during tidal cycles at the Compass Station. By contrast, studies in other shallow or coastal environments indicate that near-bed velocities of 20–30 cm s^{-1} are generally required to erode bottom sediments (Ziervogel et al., 2021).

- **L567-569**: Boetius et al. 2013 talks about large algal chunks found at depth fresh and seen by cameras, not sure they refer to measuring fluorescence along the water column and finding high values at depth, so I am not sure the comparison applies. In any case, I would assume the deeper fluorescence signals found in this study are due to mixing, having some surface phytoplankton reaching deeper layers. Yet, blooms take place when water column presents higher stratification. Did you do isotopic analyses on the POC samples?

Response: We thank the reviewer for their comment. Boetius et al. (2013) report observations of large, fresh algal aggregates at depth rather than fluorescence profiles, and we have clarified the text to reflect this. The deeper fluorescence signals in our study are likely due to vertical mixing of surface phytoplankton. We did not perform isotopic analyses on the POC samples, and our discussion relies on bulk POC composition and depth profiles to infer particle transport and flux dynamics.

- **L567-573**: A high particle load is mentioned. Is that based on visual assessment (the water looked turbid) or there was some kind of measurement done that would indicate that (e.g., light penetration).

Response: We observed elevated chlorophyll fluorescence in the water column together with high particulate ^{234}Th activities. To avoid ambiguity, we have since revised the text and specifically mention these measured parameters rather than using the general phrasing of "high particle load".

- **L580-582**: Bolanos et al. (2020) citation refers to importance of small phytoplankton in terms of biomass. Mention it.

Response: We have since revised text in manuscript (L580–582): "...small phytoplankton can play an important role in carbon export and biogeochemical cycling and have been shown to contribute substantially to biomass in the North Atlantic (Bolanos et al., 2020)."

- **L583-584**: Cite the reference for the *Synechococcus* statements.

Response: Change made. The citation for this statement is Bolanos et al., 2020.

- **Fig. 9**: Please clarify y-axis, I don't think I understand it and it is not described in the caption.

Response: We thank the reviewer for pointing this out. The y-axis represents normalized sample depth in the water column, calculated as the depth of the sample divided by the total water depth at the site. We use normalized depth instead of actual depth to emphasize vertical distance from the seafloor, a relevant metric in the context of sediment resuspension. For example, a sample taken at 20 m in a basin 70 m deep is plotted at 0.28 on the y-axis (i.e., 20/70). To clarify this, we have updated the y-axis label to "Normalized depth (sample depth / total water depth)" and revised the figure caption as follows:

"Profiles of RA_{P234} are plotted versus normalized water depth (i.e., sample depth divided by total water depth, whereby 0 = surface, 1 = seafloor)."

L605: Just to clarify, the 8 dots that are above the detection limit had 0 RAP234 from the large fraction, so those values are entirely from the small fraction particles or the difference from the detection limit value and the RAP234 value in those cases is coming entirely from the small particles?

Response: The reported RA_{P234} values represent the sum of both large and small particle fractions. In the eight instances where the RA_{P234} values were above the detection limit, none of the large particle RA_{P234} values were above the detection limit. Therefore, the RA_{P234} signal originates entirely from the small particle fraction.

- **Section 4.4** I appreciate the authors revisiting Black et al (2023) POC:Th data and discussing the discrepancies observed. They could have avoided it and focus only on their current dataset, but instead they highlight the substantial differences and mention the potential sampling artifacts causing the extremely elevated ratios in that previous work.

Response: We agree with the reviewer that this discrepancy is important to the field. The large particle ratio is typically thought to be representative of sinking particles and is what used to report fluxes in many studies. We think that the data presented in the present study represent the first accurate measurements of large particle POC:^{234}Th ratios in the Bedford Basin.

- **L648**: Table 3 does not show other biogeochemical measurements; it shows estimates of POC export fluxes by others using other methods.

Response: We agree. In the revised manuscript, we have updated the table title to reflect its content more accurately and added a note to clarify the origin and method of the cited values.

- **L654:** Also similar to modelled POC flux reported in Black et al. 2023, based on the data shown in Table 3.

Response: Change made.

- **L662-663:** I obtain slightly different burial rate, which leads to slightly different burial efficiency. Minimal differences, but please double check the numbers.

Response: We thank the reviewer for double checking the numbers. Our reported burial efficiency of ~33% is derived from a mean depositional flux of 20 ± 14 mmol C m^{-2} d^{-1} and a burial flux of ~6.9 mmol C m-^{2}d^{-1}, which we calculated using the measured weight % POC (5.7%), mean sedimentation rate and dry bulk density. The reviewer's slightly different value likely arises from small differences in the chosen input parameters (e.g., rounding of sedimentation rate, bulk density, or POC%), or from comparing annualized fluxes rather than daily means.

- L671: Add at the end of the sentence "i.e., export efficiency". The following sentence starts talking about export efficiency and not ThE-ratio and might seem like they are different things for those not familiar with the thorium approach.

Response: We thank the reviewer for this helpful suggestion. We agree that clarifying these definitions will improve readability. In the revised manuscript, we only refer to this concept as "export efficiency" rather than using "ThE" and export efficiency.

- **L672-673**: It is the other way around, export/production = ThE; as stated in L676: "*divide the mean POC… by the mean NPP*"

Response: Agreed. Change made in revised manuscript.

- **L677**: 29.4 mmol C m^{-2} d^{-1} / 66 mmol C m^{-2} d^{-1} = 44.5% not 49% Please double check the numbers. L679 also mentions 49% ThE ratio and in the conclusions. Also check the burial efficiency reported in the conclusions (see my comment for L662-663).

Response: We thank the reviewer for catching this calculation error. The correct ratio is 29.4 mmol C m^{-2} d^{-1} / 66 mmol C m^{-2} d^{-1} = 0.445 (44.5%). We have since corrected this value in the results (L679), discussion and conclusions. The burial efficiency has likewise been double checked and is 33% (see above).

- **Table 3**:
  - The value reported by Hargrave and Taguchi (1978) is under "*Particulate flux*" and the units are in mmol C m^{-2} d^{-1}. If it is not POC flux (otherwise I assume it would have said it) it should be total C to match the units reported. Is that the case? Please specify it in the title: "total C flux" or provide the necessary information as a table foot note.

Response: Hargrave & Taguchi (1978) report an annual mean sedimentation rate of ~ 17 mmol C m^{-2} d^{-1} which is based on total particulate carbon from sediment traps at 60 m in the Bedford Basin. In the revised manuscript, we have modified the table title to "Total Particulate Carbon Flux (mmol C m^{-2} d^{-1}) and we included a footnote for the Hargrave & Taguchi (1978) number – "values from Hargrave and Taguchi (1978) represent total particulate carbon sedimentation fluxes obtained from sediment trap measurements and are not limited to POC fluxes".

- The fluxes in the sediment reported in Black et al. 2023 are not shown in this table due to their extremely high values because of the particle sampling method used there, correct? It would be good to expand the first sentence of section 4.5 to explain the summary table 3, what was included in it and what not, and why.

Response: We thank the reviewer for this suggestion. Black et al. 2023 report sediment fluxes that averaged 2000 dpm m^{-2} and ranged from 1100 – 3600 dpm m^{-2}. Indeed, the extremely high sediment fluxes reported in Black et al. (2023) are not included in our Table 3 because they result from the specific particle sampling method used in that study, which is not directly comparable to the approaches applied here. We have revised the first sentence of Section 4.5 to clarify what is included in Table 3 and the rationale for exclusions. The revised sentence now reads:

"Table 3 summarizes POC export fluxes derived in this study alongside comparable biogeochemical measurements in the Bedford Basin. Only measurements obtained using methodologies directly comparable to ours are included; the high sediment fluxes reported in Black et al. (2023), which likely arise from their distinctly different particle sampling approach, are excluded here to enable a meaningful comparison."

Technical comments:

- L124 Eq. 1 and L126 Eq 2.: Make sure the superscripts are shown as such for ^{238}U and ^{234}Th

Response: Change made.

- **L166**: Eq. 5 uses Ra_{p243} but later on, in L603-605 RA_{P243}. Check, this term is used in other lines not cited here.

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Response: The manuscript has been changed to consistently use RA_{P234}

- **L190**: Multiplied by

Response: Change made

- **L191**: Add (OC) after "Organic carbon" since it is used later in L194, or write it entirely in L194.

Response: Change made

- **Figure 2 panel E**: The image definition of this panel is worse than the others. Also, please provide the y-axes titles in the corresponding side of the graph. The legend is split but not the actual titles. The vertical line on the right side of the panel looks odd. Can it be just the actual y-axis with its values? Also, the font size of the y-axes is much smaller than for the other panels. L281, T, S, O have not been defined before, add them in the previous line.

Response: We agree with the above comment. Panel E was made separate from A – D and merged to appear in the same format as A – D. The suggested changes are made for better clarity and conciseness to the format of A – D.

- **Figure 2**: Add depth [m] also in panels B, C and D.

Response: Change made

- **Table 1**: Nowhere in the text nor in the caption it is defined what LSF or SSF stands for.
  One can figure it out, but it would be good to add the whole wording in the caption.

Response: Edit made to now write out "large size fraction" and "small size fraction".

- **L404**: please cite the number of data point considered ($p < 0.01$, n= X)

Response: Change made.

- **Figure 5**: The legend is the same for both panels, even though the shape of the symbols is diZerent for large and small particles, correct? The same symbol could be used while highlighting the size fraction a bit more (maybe add it to the title?).

Response: Agreed. We use the same symbol in each panel and write "small size fraction" and "large size fraction" next to "POC:^{234}Th"

- **L425**: "*spring summer 2019*" missing an "and"

Response: Change made.

- **L436**: BB for Bedford Basin is only used here and in L657-658. I would just right the whole name in those lines.

Response: Change made.

- **L436**: *"...that this event brought additional…"* remove "it"

Response: Change made.

- **L474** cites only Fig.6b and 6c but it could also cite Fig. 7. Although, see my previous comment regarding the combination of these two figures in one.

Response: Change made.

- **Figure 8**. I don't mind the different symbols for the different sampling periods (although this is not done in the previous figures) but they seem to have different size? It could be an optical effect due to the different shape/color, but the dark blue triangles seem bigger than the rest.

Response: Change made. We have since corrected for this so they appear consistent in size.

- **L511**: ^{234}Th appears as 234^{Th}

Response: Change made.

- **L590**: It says "Jan 2022" but in all other instances the whole months it is written down, i.e., January. Same in L619 and L620.

Response: Edit made to fully write out "January"

- **L603-605**: Caption of Figure 9 – the residual activity here it is expressed as RAP234, different than in L166. Check the whole text and make sure to be consistent.

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Response: Thank you for catching these details. Change made.

Work cited:

Black, E. E., Algar, C. K., Armstrong, M., & Kienast, S. S. (2023). Insights into constraining coastal carbon export from radioisotopes. *Frontiers in Marine Science*, *10*, 1254316. https://doi.org/10.3389/fmars.2023.1254316

Boetius, A., Albrecht, S., Bakker, K., Bienhold, C., Felden, J., Fernández-Méndez, M., Hendricks, S., Katlein, C., Lalande, C., Krumpen, T., Nicolaus, M., Peeken, I., Rabe, B., Rogacheva, A., Rybakova, E., Somavilla, R., Wenzhöfer, F., & RV Polarstern ARK27-3-Shipboard Science Party. (2013). Export of Algal Biomass from the Melting Arctic Sea Ice. *Science*, *339*(6126), 1430–1432. https://doi.org/10.1126/science.1231346

Bolaños, L. M., Karp-Boss, L., Choi, C. J., Worden, A. Z., Graff, J. R., Haëntjens, N., Chase, A. P., Della Penna, A., Gaube, P., Morison, F., Menden-Deuer, S., Westberry, T. K., O'Malley, R. T., Boss, E., Behrenfeld, M. J., & Giovannoni, S. J. (2020). Small phytoplankton dominate western North Atlantic biomass. *The ISME Journal*, *14*(7), 1663–1674. https://doi.org/10.1038/s41396-020-0636-0

B.T. Hargrave, G.A. Phillips, & S. Taguchi. (1976). *Sedimentation measurements in Bedford Basin, 1973-74*. Fish. Mar. Ser. Res. Dev. Rep. https://doi.org/10.13140/2.1.2815.3125

Buckley, D. E., Smith, J. N., & Winters, G. V. (1995). Accumulation of contaminant metals in marine sediments of Halifax Harbour, Nova Scotia: Environmental factors and historical trends. *Applied Geochemistry*, *10*(2), 175–195. https://doi.org/10.1016/0883-2927(94)00053-9

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