

## Response to RC2 (comment posted 21 May 2026)

*Note: Reviewer comments in black (lines refer to the original manuscript), response in green (lines refer to changed manuscript).*

Dear RC2,

We thank you for the helpful comments and appreciate the constructive feedback. This response letter includes the responses to your comments and corresponding changes made to the manuscript.

On behalf of all co-authors,

Marcel Scheiwiller

**Review of the manuscript EGU sphere-2026-2020, 'Enhanced water mass mixing in Fram Strait in 2020 and elevated circulation timescales of Atlantic-derived waters in 2021 based on transient tracers I-129 and U-236', by Scheiwiller et al.**

### *General comments*

The study evaluates mixing and circulation in the Fram Strait utilising the anthropogenic radionuclides I-129 and U-236 applied on binary mixing and transit time distribution approaches. These radionuclides are ideal as transient tracers when applied on studies of especially horizontal spreading of Atlantic-derived waters, but the study also includes an evaluation of Polar waters. The work is very interesting and overall clear, and I recommend publication in Ocean Science after minor revision.

My main concerns are connected to the uncertainties, and how some of the result sections are presented. Detailed comments are given below.

### *Specific comments*

L62: Here you should clarify that this depth range is in the Fram Strait, if this is what you refer to. Or do you mean the Arctic Ocean?

We revised the wording in lines 64-65 to clarify the intended meaning. We made the same adjustment in line 41 for consistency.

L66: In addition to the density range for AAW you refer to, the temperature maximum is the strongest identifier of this water mass, naturally.

We added the max. temperature as an identifier in line 69.

L77: I think you should remove “Polar Water and Arctic Atlantic Water” here, which you just described, leaving the sentence as: Apart from the described outflowing layers, the Fram Strait ....

We removed “Polar Water and Arctic Atlantic Water”.

L156-157: It struck me that if all U-236 data used in this study are from Lin et al. (2023) one would expect Lin to be a co-author. Perhaps this was agreed on but wanted to make a note on this.

We thank the reviewer for making such observation. The U-236 data used in this study was already published in Lin et al. (2023) and this author was not involved in the discussion of the present study. However, we have included the U-236 results in agreement with Prof. Dr. Jixin Qiao (PhD supervisor of Lin and corresponding author of Lin et al. 2023) in a published table which is uploaded to Zenodo (<https://doi.org/10.5281/ZENODO.19387002>)

L177: The 4% uncertainty mentioned, is that the total measurement uncertainty you assess? This is not clear to me.

We concur with the reviewer that this was not well explained in the submitted version of our manuscript. This uncertainty encompasses uncertainties from the chemical separation and purification as well as from the measurement itself. We have clarified this in the text, which now states: “Additionally, an internal standard was measured repeatedly throughout the year to account for the combined uncertainty of the entire procedure, from chemical separation to final measurement. The standard deviation of these replicate measurements yielded an uncertainty of 4 %” (lines 184 to 187). We have also added more details on the uncertainties derived from the measurement and the chemical processing in the third paragraph of subsection 2.1.

Figure 2: The uncertainty shown here (as shaded areas), how large is this, and what is this based on? This is seemingly clearly more than the 4% measurement uncertainty presented, but unless I missed it this is not clearly stated.

We agree with the reviewer that this was not mentioned in the text and might create confusion. The uncertainties of the input function (Fig. 2) are not the same as the 4% uncertainties related to the internal standard. The uncertainties in Fig. 2 were propagated from uncertainties in reprocessing plant uranium releases (Christl et al., 2015) and fractions of La Hague, Sellafield, and global fallout (Casacuberta et al.,

2018) as stated in Wefing et al. (2021). We added this information to the figure caption. Following a comment from reviewer 1, we furthermore updated the uncertainty of the mid-depth input function based on recently obtained data and data from this study.

L261-263: It would be much helpful if you show numbers instead of just stating how you calculated it, and again I am not sure if your stated 4% is the total measurement uncertainty. Please clarify.

Again, we thank the reviewer for pointing again to certain sources of confusion regarding uncertainties. The uncertainty ranges of the tracer ages were obtained by propagating the total tracer uncertainty, which is now reported for both tracers in section 2.1, through the age calculation. We have added the resulting age-uncertainty ranges to the text: "The uncertainty of tracer ages was assessed by calculating the minimum and maximum age estimates based on total uncertainties for both I-129 and U-236 and resulted in relative uncertainties of 4–21 %, 4–85 %, and 4–15 % for 2016, 2020, and 2021, respectively." (lines 280-282)

L265-268: This limitation indicate that the total uncertainty may be rather significant. This may be difficult to quantify but should at least be acknowledged.

Yes, we agree that the assumption of purely advective transport leads to a significant tracer age uncertainty (for the binary mixing model only). In section 4.1, we show that the binary mixing (tracer-age) model is, from a methodological standpoint, unable to account for mixing. Consequently, the effect of mixing is not reflected in the uncertainty we calculate. We have added this statement to the manuscript to clarify what the uncertainty includes (lines 289-292).

Figure 3: The inset figures are hard to read due to too small fonts, on all axes and legends.

We concur with the reviewer that this figure was hard to read. We have revised figure 3 to improve readability.

L277-278: I believe Tanhua et al. (2009) was one of the first studies adopting the TTD approach in the Arctic Ocean. You may consider adding this.

We thank the reviewer for pointing to this reference. It is now added in the text (line 302).

L330-331: How do the results of Dodd et al. compare with earlier studies of Jones et al.? I am not sure if Jones et al. have presented results for the Fram Strait but know they have for the East Greenland Current. If they did it could be interesting to mention how they agree.

We agree with the reviewer that the fractions of Pacific Waters obtained by Dodd et al. (2012) and Jones et al. (2008) should be better indicated. We have therefore added more detail (lines 354-357) highlighting the strong variability in the reported Pacific Water fractions. Jones et al. (2008) and Dodd et al. (2012) found very low values (<10%) in some years (besides the reported 40% to 80%), whereas Laukert et al. (2017) estimated about 30%.

Section 3.1: There are several issues with this section. In general, there are lots of details that are difficult to keep track of and seems to describe a very patchy distribution; for example, L352-355. Figure 4 adds to this, with some symbols being very similar (like the stations at  $-12^{\circ}\text{E}$  and  $-8^{\circ}\text{E}$ ), and the legend displaying stations in a non-sorted (by longitude) way. In addition, the legends should be placed in the lower right corner to not covering any data points. I started to wonder if the results would not be better presented as a section, but then this is done in Section 3.2/Figure 5. Furthermore, the station at  $-17^{\circ}\text{E}$  was only sampled in 2021, but nothing is said about that (unless I missed it). You also refer to depths down to 1000 and 2500 m, but this is not shown in Figure 4. I would ask the authors to re-evaluate this section and how the results are presented. Could instead the main results be presented in a table, and avoid too patchy and confusing results? (Patchiness is of course often the case in nature, but then this should be spelled out clearly.) I would even suggest considering moving Figure 4 to the Appendix, and to merge sections 3.1 and 3.2. In a table, if this would be chosen, the results could be organised according to water masses.

We appreciate such feedback from the reviewer. In order to make Fig. 4 more clear, we exchanged the symbol for station  $-12^{\circ}\text{E}$ , sorted the legends and relocated them, as suggested. Additionally, we rearranged the panels in Fig. 4. We added a table (Table 2) that shows the Iodine concentration ranges for each region and year. We removed the description of this in the text and instead added a reference to the table, which helped reducing the text length considerably in section 3.1. We decided against showing these few datapoints at 1000 and 2500 m depth because it would decrease the visibility of the upper 400 m. Instead we mentioned that it is not shown in Figure 4. We shortened the description in the original lines (352-355) for better clarity.

Figure 5: You may want to consider another colour scale with higher contrasts; the blues and greens are rather similar. In the caption it should say "Panels b and d show cross-sections...".

We agree with the reviewer that the previous Figure 5 had not enough contrast. After trying different color scales, we decided to stick to "viridis", but we changed the range of the color scale to show the data more dynamically i.e. with better contrast. The wording was changed to "Panels b and d show cross-sections...".

L403-408: Consider rephrasing parts of the text; now it is basically repeated information: “near-vertical  $s_0$  structure” and “the front was more or less vertically aligned, with the  $27.2 \text{ kg m}^{-3} s_0$  isopycnal extending straight down ...”.

We removed the first sentence of these two and kept the second in lines 428-429.

Figure 6: It is unclear to me why there are so few data shown for Central West (panel c), since there are more data in Figure 4? You may want to consider removing this panel. You could just describe this in the text instead, for the Central West region that is. (This would be in L426.) You should also sort the order of stations in the legends; this helps the reader (now this is only done in panel b).

We agree with the reviewer that this can be a bit confusing. We have indeed removed panel c. For clarification, the estimation of tracer ages (and also of TTDs) requires having concentrations of both, I-129 and U-236, which was not the case for all samples shown in Fig. 4 (I-129 profiles). Additionally, we restricted the calculations of tracer ages to Polar Water 1 samples. That is why only a few tracer ages show up in the “Central West” part. We sorted the order of stations in the legend.

L416-419: Also in this paragraph I get lost in details. Consider if this could be presented more clearly.

We reformulated these lines and shortened the description (lines 438-443).

L443-444: It may seem counter-intuitive that the presence of younger waters would result in the width of the TTD increase to values up to 140 years. Perhaps you could clarify this.

We agree with the reviewer that this might not be straightforward to understand. A Delta (width parameter) of 140 years does not in itself indicate that the water parcel is old, but rather that it is a mixture of young and old waters. This is mostly defined by the tail of the probability density function. We added a sentence to clarify this (lines 463-464).

Section 3.4.2: Some of this section suffers the same issues as some earlier sections in that there are so many results/numbers presented next to each other that one loses track. Especially the first paragraph. For this para is also not clear to me if you refer to all regions, or specific parts? I would consider, also here, to present the results in a table instead.

We agree with the reviewer that this section had too much text and was probably not easy to follow. We have now summarized the results in a table (Table 4) as suggested. We did the same for the mid-depth layer in section 3.4.1 (Table 3).

Figure 7. In this figure, and several of the following ones you write out both “Gamma” and “G”, as well as “Delta” and “D”, which are the same things. (It’s like writing “Metre” and “m”. G is “mean age” as you write in the method section, but never use in the figures, and D is “width” of the TTD, so why don’t you use this instead, or simply only G and D? Another, but tiny remark on the figure is to move the panel letters to the lower right corner where they are not covering any data points.

We removed the written version of the name and moved the letters to the lower right corner.

L488-489/Figure 8: Possibly I am misunderstanding something here, but I can’t see that the data points shown in Fig. 8 generally aligns along the one-to-one line. Instead, the general distribution seems to be along the x axis, and thus along a rather constant tracer age at different mean ages. I agree that the agreement is better at lower mixing, but the initial statement is confusing. Please clarify.

We agree that describing the relationship as aligning along the one-to-one line was an overstatement. We have therefore removed that characterization and retained only the statement describing a "general correspondence between the tracer age and TTD parameters." (lines 504-505)

Figure 10: There is a mismatch with the order of the panels and what is stated in the caption. The panel order is now: Delta, Gamma, Delta/Gamma,  $t_{\text{mode}}$ .

We appreciate this observation. We changed the caption according to the figure order.

The dashed lines are, I suppose, added to help illustrating the changes of the average values, but this is not mentioned in the caption. Are those lines needed? The black crosses disappear somewhat on top of the dark blue markers but then you may consider another colour on the crosses?

The dashed lines indeed connect the averages for each year and we think they help to see the temporal evolution of the TTD parameters. We therefore kept them in the figure. We clarified this in the caption and we added a white outline to the cross for better visibility (Fig. 10).

L591: Where is this shown (U-236 values)? In Fig. 9? If so please refer to that figure.

We added the figure reference in line 611.

L596: It is not easy to see where those samples are taken from in the map, and also since there is no depth info in the figure. Of the three points I would think you refer

to only one is clearly elevated (with U-236 of ~40, while the other two (~20-25) are similar to one sample in 2016.

We reformulated the sentence to clarify which samples are being referred to (lines 615-617).

L601-603: Even if you conclude that conservative temperature may not be a good indicator you first compare two samples with a mean value, and one of the sample values is very close to the average (-1.51 with -1.49°C). Consider removing, or rephrasing.

We have removed this point, as it is of minor importance and may instead indeed introduce confusion.

L607-608: From Fig. 9 it is not easy to see where the samples are taken from.

We removed the reference to Fig. 9 here. The reference was unnecessary as we stated the location already in the text.

L610-611: Can you highlight this in a figure in the Appendix, or relate to the T/S figure in Fig. 5?

We highlighted these samples in Fig. 5, added a reference to the figure and mentioned the highlighted samples in the figure caption.

L617: You should refer to figure showing this. Fig. 5 perhaps?

We also highlighted these samples in Fig. 5, added a reference to the figure and mentioned the highlighted samples in the figure caption.

L619: Is the age range truly “substantial”? You may want to illustrate this by presenting a range in the text. From looking at the figures alone this is not entirely clear to me.

We considered this range substantial, given that the samples all belong to the same water mass. We changed the wording to “large” instead of “substantial”. We have also added the range to the text, as suggested (line 635).

L634: If you are referring to  $t_{\text{mode}}$  this should be Fig. 10 d,e.

Done

L636-637: Is this increase significant? Here also the sampling density plays in. For the AAW layer it seems the sampling density was lower in 2020. If this would have been higher it is not unlikely that the mean value would have been higher too, and then the “shift” would not have been clearly observed in 2021. I don’t really follow

the “extreme values” mentioned here. Do you mean in  $t_{\text{mode}}$  in 2020? Are they really “extreme”?

We rephrased the whole paragraph to tone down the observed shift. We now used the word “tendency” to describe the temporal trend which also aligns with the wording in the abstract. We included a reference to Fig. 10h and the number of samples taken in 2021 to be more concrete. We also rephrased the “extreme value” argument for better clarity (lines 653-655).

L682: Instead of just writing “very old ages” present some values/range. This would also highlight what the transit time is to the Fram Strait from the Canada Basin.

We added the three old t-mode ages (21, 24, and 28 years) to the text (line 713).

Appendix: There are several issues with the figures in the Appendix. These are mostly of technical character, but bring it up here:

A2: The colours/markers need to be defined. Add legend or/and in caption.

We added the legend.

A4: The isolines should be defined/described, similar to what was done in Fig. 3, and add values to the isolines. This should in any case also be done here to make it easier for the reader. Without these explanations it is very difficult to interpret the results.

We included the description of the isolines in the figure caption and extended the description further (similar to Fig. 3). We also added the values for the isolines as suggested.

A5: Similar comments as for A4.

We included a similar description as in A4.

A6: As pointed out earlier remove “Gamma” and “Delta” and just use the symbols.

We removed the written version “Gamma” and “Delta”, as suggested.

For most of the selected isolines the values are not seen, only partly for 27.2. You should state in the caption which ones are presented and see to that the values are visible in the figure.

The isolines in Fig. A6 were moved for better visibility and mentioned in the caption.

Nothing is shown for the deeper parts (panels g-i), and it is not clear if this is only shown for 2021? From this I would clearly suggest that you remove the lower panels.

We removed the lower row of panels.

*Technical comments*

L40: *occupying*, instead of *occupy*.

We restructured the sentence due to other comments and therefore it reads now as “occupies”.

L48: ...in the Polar Water...

Done

L67: ... entered the Arctic Ocean via ...

Done

L82: ... 78.5°N and 80°N, respectively (...)

Done

L330-331: ... Pacific Water fraction in the Fram Strait of ...

Done

L532: ...waters entering with the WSC, ...

Done

L568: CDOM should ideally be defined; Oxygen-18 is, I believe, not typically written as “delta-Oxygen-18”, but “oxygen-18” (the short version in parenthesis is correct); “Neon and Neodymium” are not written with a capital letter (i.e., should be “neon and neodymium”).

Done. CDOM was defined a bit earlier in the text.

L572 + L587: ... Arctic Ocean ...

Done

L587: Remove “again”.

Done

L597-598: The wording is strange: "influenced to old source waters." Please rephrase.

Done

L620 + 624: ... Arctic Ocean ...

Done

L669: This needs some rephrasing. Either "... mainly from the Pacific Ocean.", or, "... mainly of Pacific origin.".

Done

L731: Typo on "members".

Done

## References

Tanhua, T., Jones, E.P., Jeansson, E., Jutterström, S., Smethie, W.M., Jr., Wallace, D.W.R., Anderson, L.G., 2009, Ventilation of the Arctic Ocean: Mean ages and inventories of anthropogenic CO<sub>2</sub> and CFC-11. *Journal of Geophysical Research*, 114, C01002. <https://doi.org/10.1029/2008JC004868>