Dear Editor Dr. García-Ibáñez,

We are pleased to resubmit our manuscript, "Enhanced Water Mass Mixing in Fram Strait in 2020 and Elevated Circulation Timescales of Atlantic-Derived Waters in 2021 Based on Transient Tracers ^{129}I and ^{236}U," for consideration for publication in *Ocean Science*. We have revised the manuscript in response to both reviewers comments. A complete list of changes and a point-by-point response is provided below.

Motivated by the reviewers comments, we additionally addressed two further points:

- We identified and corrected several additional minor inconsistencies in the reference list.
- We added a sensitivity analysis for the surface dilution correction (section 4.5), with the corresponding figure currently placed in the appendix. We would welcome your input on whether this figure might be better suited to the main text.

We are grateful for the reviewers comments, which have substantially improved the manuscript.

On behalf of my co-authors, I confirm that all authors have approved the final version of the manuscript and accept responsibility for its resubmission.

Yours sincerely,
Marcel Scheiwiller

Relevant changes:

- Updated the color scheme for the three sampled years and made minor visual refinements across all figures to improve readability.
- Expanded and clarified the description of uncertainties in the Methods section.
- Reevaluated the surface input function, the global fallout dilution, and the corresponding correction applied in the surface Transit Time Distribution model.
- Reevaluated the uncertainty in the mid-depth input function.
- Added further detail on the nutrient-based correction we tested.
- Added summary tables to the manuscript to improve the overview of the results and shortened the corresponding text accordingly.
- Added a sensitivity analysis with a corresponding figure for the surface dilution correction (section 4.5).
- Identified and corrected several minor inconsistencies in the reference list.

Response to RC1 (comment posted 14 May 2026)

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

Dear RC1,

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

This paper adds the information (from 129I and 236U) for two Fram Strait sections in 2020 and 2021 to earlier (2016) radionuclide data, in order to better understand changes in mixing and Arctic circulation time scales (for the Atlantic water component). It also compares results from binary and TDD model approaches to interpret the results (including in the surface layer). The difference found between the 2020 and 2021 results is potentially interesting, fitting with the expectation that a positive polar index would trigger more confined Beaufort circulation (this more toward cyclonic regime, and thus more transpolar drift in 2021) (and, of course, with a little lag).

There were three other papers by Wefing et al. on Fram radionuclide sections (at least the Wefing et al 2019 and 2022 are with binary model, and not the TDD one (and the 2021 paper only subsurface); So, the originality of this paper would be the use of TDD on the different sections… (but I have not read the 2025 Wefing et al paper: is it on line, as no journal mentioned; on the other hand probably not so relevant here, as dealing with central Arctic?). The other originality is the one of extending the model to the surface data, but that is based on some assumptions to take into account salinity variations and propotion of Pacific water (TTD model already used by Wefing et al (2021), but not for the surface; here there is a salinity normalization and some assumptions on the proportion of Pacific water for the fallout component in 236U).

The recent paper by Wefing et al. (2025) can be found under https://os.copernicus.org/articles/21/3311/2025/. We have updated the entry in the references, and thank the reviewer for pointing out that the journal was missing.

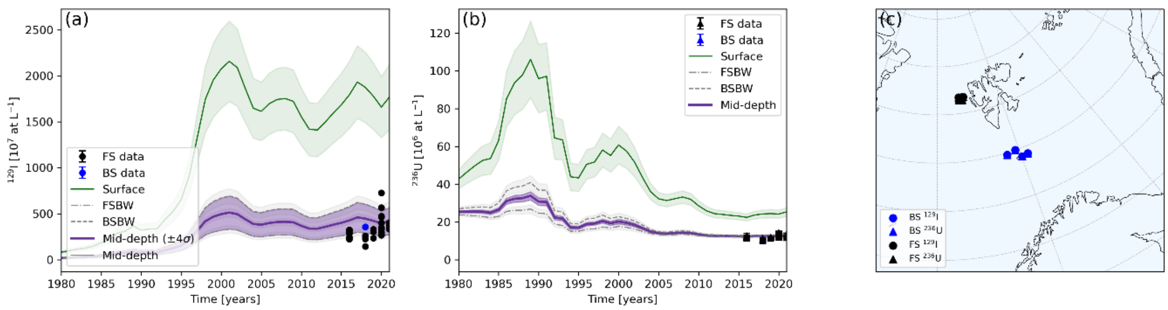
After consulting these earlier papers, I was surprised that the 129I and 236U 2018 and 2019 Fram Strait data from Wefing et al (2022) are not really mentioned / included here, at least in the discussion. Is it because there were not enough stations in 2018-2019 (mostly EGC and not very far on the shelf? (or that many stations do not have the pair of measurements) or is there is some doubts related to the 2018 and 2019 238U measurements? Altogether, why are they not included. After all: 2016, 2018, 2019, 2020 and 2021 would (apparently) make for a more consistent time series than having the gap between 2016 and 2020, and help to better figure out whether the conclusions derived by comparing 2020 and 2021, also hold for the earlier years.

We thank the reviewer for pointing this out. A time series of both radionuclides covering 2016 and 2018-2021 would be more consistent and allow for a more comprehensive analysis of temporal changes. In 2018 and 2019, U-236 has been sampled and measured in a similar spatial resolution as in 2020 and 2021, and the data is of good quality. However, only very few data points are available for I-129 in the outflowing waters for the years 2018 (n=5 in the core EGC and n=5 in Norske Trough) and 2019 (n=7 in Norske Trough). Since both the binary mixing model and TTD approach need two tracers at least, these could be only applied to one or two stations in both years and we therefore decided not to include them.

The use of a source function (NCC and other branches; Figure 1b) is key for the analysis. It is based on some reasonable assumptions as discussed in Casacuberta et al (2018) and Wefing et al (2021). However, should the error bars that are plotted here (Figure 1b) be reevaluated (maybe to phrase it differently: how much confidence does one have in these error bars?)

We thank the reviewer for pointing this out. The uncertainty of the mid-depth input function could indeed be re-evaluated based on data collected in the WSC and the Barents Sea since the publications of Casacuberta et al. (2018) and Wefing et al. (2021). Available data are shown below, along with the three input functions from Casacuberta et al. (2018), as well as the mid-depth input function used in our study from Wefing et al. (2021). For I-129, we increased the uncertainty of the mid-depth input function by a factor of four to account for the larger spread in I-129 concentrations measured in the Fram Strait inflow and the Barents Sea in recent years. This increased uncertainty is plotted in panel a. For U-236, the available samples support the mid-depth input function and its uncertainty, so we did not adjust it.

No additional data is available yet for the surface layer input function, so we kept the uncertainty range used in previous studies. Input functions are estimated from the reported releases and all previous works were based on these, producing meaningful results in all cases. While having observations would help validating them, these should be based on repeated measurement all year long to reproduce seasonality and get the annual averages. At the moment, we don’t have enough observations to validate the input function and thus we keep using the estimated one from releases (here with an increased uncertainty range for the mid-depth layer).



Furthermore, 2020 and 2021 sampling separated by almost a month seasonally (later in 2020, thus with likely more vertical mixing/vertical diffusion of the surface signal (and more melt of sea ice): could this explain the absence of the shallow subsurface peak in that year, compared to 2021 (and maybe also some of the near-surface differences between the two years).

We thank the reviewer for pointing to the seasonal differences in the sampling campaigns and their potential influence on the interpretation of observed differences in the tracer concentrations. We consider it unlikely that enhanced vertical mixing can explain the absence of the subsurface peak in 2020 that is clearly present in 2021. Karpouzoglou et al., (2022) find that the near-surface salinity difference (between 25 and 55m) in the East Greenland Current increases sharply in late September and early October, reaching its maximum stratification in October before the winter mixed layer develops. The 2020 campaign took place later in the season, so any seasonal effect would have rather rendered the 2020 water column more strongly stratified, which would tend to preserve a subsurface tracer maximum rather than erode it.

Sea-ice meltwater accumulating at the surface likewise strengthens near-surface stratification. Dilution of the tracer concentrations by meltwater is also unlikely since it would lower both the tracer concentration and the salinity, which is not observed.

It is very helpful to have these sections in two successive years. It would have been better if there had been more 2021 (and 2020) data collected in the SCC Atlantic water inflow west of Svalbard, to ascertain that there are only little differences in these water masses between the two years, as expected (otherwise, I wonder how one can fully eliminate that the difference beween the two sections do not result from data/measurement errors in one of the two sections).

We agree with the reviewer that more data from the WSC in the eastern Fram Strait would be valuable to assess the inflow of I-129 into the Arctic Ocean. However, the main focus of radionuclide sampling in 2020 and 2021 was the Arctic outflow (i.e. EGC) as the aim was to investigate water mass provenance and timescales of these waters.

We do not quite understand the reviewer's comment on data/measurement errors in one of the two sections and how this could have been ruled out by a better data coverage in the WSC. The radionuclide data presented in this study has been carefully quality-controlled and we found no irregularities in the chemical processing and measurement which would point to systematic errors. For U-236, some samples have been processed and measured by two different labs (ETH Zurich and DTU/University of Vienna) and the results showed very good agreement (see Appendix Figure A1).

Comments:

1. 154: '… and adding data…'

   Done

2. 155: one needs to add where those 236U samples were measured. According to Figure A1, I guess that it is at the Danish Technological Institute in Aarhus.

   The Technical University of Denmark published the dataset and the measurements were done at the University of Vienna. Both were added to the Fig. A1 caption .

3. 243: remove 'than' thus 'estimates of … updated from the ones used in…'

   Done

4. 332: this is not stated, but I believe that the argument for choosing the 55% for fGF is that it is the one for which most data of all years fit onto the TDD grid (and not outliers on the plots). On the other hand, why would not it vary between the three years (after all, the later comment on the circulation change between 2020 and 2021 could also have an impact on Pacific water proportion). Could you get an agreement for specific years, such as 2020, with a different fraction? To summarize: what motivates the choice to retain a common constant fraction? (I realize that the nutrient approach is not 100% proof, in particular due to the denitrification on shelves, but any idea why this seems not to work at all? I am still puzzled by the statement)

We thank the reviewer for pointing this out. Initially, we applied a fixed correction fraction across all years to ensure a consistent treatment. However, we agree that this choice contradicts our finding of stronger Amerasian Basin influence in 2020.

In our original approach, the correction combined two distinct effects diluting the surface input function into a single term: dilution by Pacific Water and dilution along the shelf region (both with waters carrying global fallout concentrations). While this simplified the method, we recognize that this approach rather leads to confusion. We have therefore revised our approach as follows: we now apply a 50:50 dilution of the surface input function with global fallout concentrations to account for the dilution along the shelf (lines 230–234), following Wefing et al. (2022) which can be found under https://doi.org/10.3389/fmars.2022.973507. We then apply a separate correction that isolates the Pacific Water contribution. The second correction is now dynamic, varying between years (lines 357–359), and we have added a corresponding sensitivity analysis (lines 659–669; Fig. A8).

Regarding the nutrient-based approach, we present the TTD grids in which samples are corrected for the Pacific fraction derived from this method in Fig. A6. This is the approach referenced in the manuscript when we state that the method did not yield conclusive results. We added an explanation for this to the methods (lines 361-367).

Section 3.4: seems that the scatter is really large in the different TDD parameters (for example in 3.4.1 Mid-depth Arctic Atlantic water). Greenland slope, indeed seems more homogeneous / consistent (and overall larger gammas…)

Indeed, the scatter of the obtained TTD parameters quite large, which we attribute to different pathways and hence circulation times of waters exiting Fram Strait. The Greenland slope region (here 4°E - 7°E) is where the core of the EGC is located (as pointed out in line 459), which could explain why the obtained circulation times are more consistent compared to other regions, where more mixing and recirculation of waters takes place.

Figure 1 is fine for focus on Fram Strait. I would still place other arrows across Canadian archipelagos for completeness.

We agree and added arrows in the Canadian archipelagos and also further downstream in the Baffin Bay and Labrador Sea in Fig. 1.

Figure 8: I find that the slope is rather different from 1 (even negative in 2020 between tracer age and gamma (but of course, as commented, these are for larger delta, and I understand the argument on the influence of delta, and the possibility of misinterpreting the binary model results). But I am still puzzled with this huge scatter in gamma (and such large gammas in this surface Polar water I). First, I found that a bit counter-intuitive (but long (and varying) storage in Beaufort gyre could explain it? Probably OK with the later discussion)

We thank the reviewer for this observation. We agree that the original description of the agreement between tracer ages and TTD parameters was not accurate and have revised it accordingly. The focus is now on the discrepancy between the models as Delta increases (lines 505–509 and lines 512–516). We also revised Fig.8, combining all years into a single panel for each relationship, resulting in two panels that show the tracer age–mode age and tracer age–Gamma relationships, respectively. This change better illustrates the Delta-dependency.

The large range in Gamma for 2020 is mainly driven by the large range in I-129 concentrations for this year, especially towards lower I-129 concentrations, which is also well visible in Fig. 9. High Gammas are in line with our interpretation that this indicates a shift towards more waters coming from the Amerasian Basin in 2020, which have been circulating in the Arctic Ocean for a longer time compared to the Eurasian Basin pathway. Storage in the Beaufort Gyre could be a possibility.

Gamma is the mean of the inverse Gaussian distribution, which is skewed towards younger ages. As pointed out in earlier studies (Smith et al., 2011, Wefing et al., 2021), the "age" of a water parcel being transported laterally through the Arctic Ocean is best described by the most probable age of the distribution (t-mode). Each water parcel is the product of multiple transit times, and a high Gamma rather indicates the presence of some waters with very high transit times, with a low probability.

Other minor comments:

1. 476: instead of 'discrepancy' use 'difference'

   Done.

2. 545: after 'largely' a word missing.

   The sentence was reformulated for better clarity.

3. 555: I am sorry that water isotopes (nutrients) are not used here as this would help see more directly (with salinity) whether there is a shift to more Amerasian contribution in 2020 (the reference suggests 2019 and not necessarily 2020?) (and water isotopes would help separate brine signals associated with ice formation and meteoric input). Actually, nutrients were used but suggested not to be conclusive. I would suggest that nutrient data be shown (in Supp mat). I am aware that this is not 100% proof (due in particular to denitrification processes along the way). Maybe those, as well as alkalinity and CDOM (among the parameters that are likely to have been collected/measured during the cruises) would help provide supportive information (I understand that neodymium or gallium might provide further evidence, but I doubt that they were measured on most sections).

We thank the reviewer for raising the point of using nutrient-derived fractions. As noted in our response above, we have included the corresponding TTD grids in Fig. A6.

The Pacific fraction derived from the nutrient method varies considerably from sample to sample. Correcting the samples based on this fraction distributes them along a line extending from the origin (zero I-129 and U-236) toward higher concentrations, in several cases displacing samples beyond the bounds of the model grid entirely. We therefore regard this method as unrealistic.

In our approach, we instead apply a fixed correction value for each year. This produces a more compact cluster of sample concentrations and, correspondingly, more coherent TTD parameters, which we consider a more realistic representation.

Figure 1: green and purple not that well differentiated on left figure. Then, there are black arrows on both panels. No mention on what this refers to (surface + subsurface, maybe). There is also a dashed arrow along Greenland (based on where this is located, has to be surface; probably the dashes refer to intermittent coastal flow)

We defined the black arrows in the Fig. 1 caption as the general surface circulation regime outside of the input function domain. We turned the dashed arrow into a normal arrow as the dashing does not add any relevant information and leads rather to confusion.

We chose green and purple consistently throughout the paper to refer to the surface and mid-depth layers, respectively. For example, we chose the same colors later in Fig. 2 for the input functions and in later figures as background color to guide the readers visually. We also thought that purple is a more intuitive representation of mid-depth waters than for example orange, green or yellow. We changed the initial dark green into a lighter green for better visibility.

Figure 2: I have not read Wefing et al (2021) where it is certainly discussed. I was wondering (personal interest; not for the paper)why in the surface layer is the peak before 1990 in 236U so sharp and prominent and preceding by a few years the 129I signal (from La Hague-Sellafield).

The temporal evolution of the input function is mainly defined by the releases from the two reprocessing plants in Sellafield and La Hague. The temporal evolution of the releases is due to various factors, such as technological evolution and changing regulations. Christl et al. (2015) explain that in the 1980s and 1990s the industry shifted toward reactors using enriched uranium which produce more U-236 and therefore the U-236 in the releases from the reprocessing of these spent fuels increased thereafter.

Figure 5: I would add 2020 between top and bottom of panels b, and 2021 between top and bottom of panel d

Done

Figure A1: I assume that the bars represent +-1 estimated rms errors.

The error bars in this figure represent 1-sigma uncertainties of the U-236 concentrations. We added this information to the Fig. A1 caption.

To elaborate further on the agreement between the two datasets and how well they agree within the stated uncertainties, we added the mean square weighted deviation (MSWD). The MSWD equals 1.13 which is a good agreement.

Figure A2: one needs to again indicate in the caption what is the symbol for each year.

Done

References, lines 924-925: reference incomplete

Done

References, lines 1010-1011: reference incomplete

Done

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 EGUsphere-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 and –8°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°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°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 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_{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_{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_{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 Neodynium" are not written with a capitol 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_2 and CFC-11. *Journal of Geophysical Research*, 114, C01002. https://doi.org/10.1029/2008JC004868