**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.

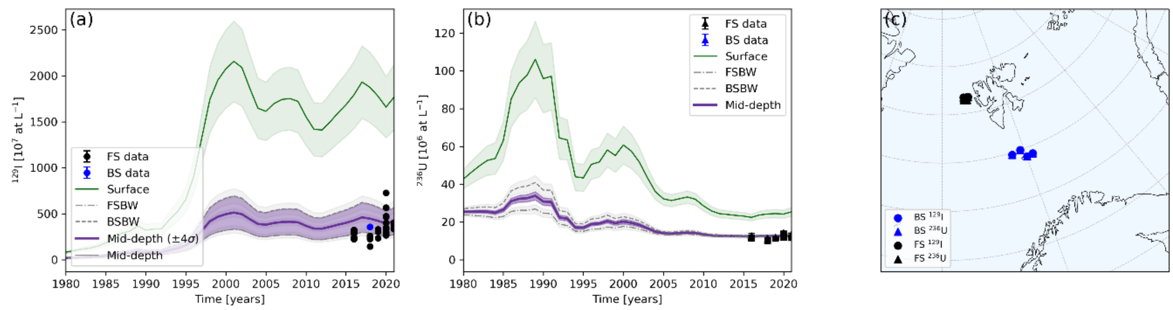
After consulting these earlier papers, I was surprised that the 129I and 236U 2018 and 2019 Fram Strait data from Wefing et al (2022) are not really mentioned / included here, at least in the discussion. Is it because there were not enough stations in 2018-2019 (mostly EGC and not very far on the shelf? (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