

Interactive comment on “Circulation timescales of Atlantic Waters in the Arctic Ocean determined from anthropogenic radionuclides” by Anne-Marie Wefing et al.

Anonymous Referee #2

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Wefing et al. present a compilation of I-129 and U-236 measurements collected in the Arctic Ocean over the last 10 years and use this data to determine water circulation timescales using two different models (a binary mixing approach, and the transit time distribution model). The geographic distribution of their data is impressive and allows for a nice comparison of circulation times between the different Arctic basins. I have only minor suggestions, which aim to improve the clarity of the paper and to provide further explanation for some of the statements that have been made in the results & discussion. Comments are listed below, divided by section.

Introduction:

C1

Page 1, line 45: change “. . .key to the prediction of the nature of...” to “. . .key to predicting the nature of. . .”

Page 1, line 46: delete “signals” in the phrase “increased AW temperature signals”

Page 2, Line 28: Pacific waters are not considered part of the Polar Mixed Layer, they reside below this fresher, colder, surface layer. See for example, Figure 15 in Steele et al. 2004, JGR (doi: 10.1029/2003JC002009)

Figure 1: The circulation arrows in the Canada Basin on Figure 1 are not quite correct. The two surface circulation cells in the Canada Basin and Makarov Basin are divided by the Alpha-Mendeleev Ridge. As drawn, the arrows do not follow that boundary (the Makarov Basin arrows should be more parallel to the Amundsen & Nansen circulation arrows). (But nice clear map otherwise!) Be sure to make this fix in Figure 7 as well.

Throughout the paper there is switching between “AW” and “AWs” as notation for Atlantic Water(s). Choose one and keep it consistent to make it easier to understand the text, particularly since there are many acronyms in the paper.

Page 4, line 93: add “us” so the phrase reads “. . .concentrations of the two tracers then permits us to define. . .” (alternatively, you could say “concentrations of the two tracers then permits a definition of. . .”)

Page 5, line 108: You say here that there is only one study using the TTD model in the Arctic, but later you compare your results to Tanhua et al. (2009) and Stoeven et al. (2016). Why are those other two studies not mentioned here?

Materials & Methods:

Table 1: How did you choose these depth ranges? Why the gap between surface and Atlantic layer? The Rudels definitions based on density and temperature suggest that the two water masses would be adjacent (i.e. one ends at potential density 27.7, the other begins there).

C2

Page 7, line 155: GF is not defined. I assume it means Global Fallout? I suggest writing out the words rather than introducing another acronym.

Page 7, line 169: RP is not defined. As far as I can tell, this is the only place this acronym is used so I suggest writing out the words.

Page 7, lines 169-172: This explanation of the global fallout signal is repetitive with the previous section. Remove or combine with the previous section rather than repeating here.

Figure 2: In the caption, the phrase “counted from 2019” is confusing because it makes it seem as though 2019 is the year when the count begins rather than ends- it would be better to say “counting back from 2019” or “based on a collection year of 2019”. Same for Figure 4 caption.

Page 9, lines 221-223: The explanation of how this parameter relates to lateral mixing should be moved up by lines 200-205, where you first introduce this concept.

Results:

Page 10, line 246: This line says that NCC is carrying high I-129, but in this context I think you mean to say U-236

Figure 3: You make the point that I-129 concentrations are higher in the Atlantic layer compared to the surface in the Canada and Makarov Basins, but this is very hard to see in the figure because they look to be the same shade of blue (even though they are an order of magnitude different in concentration!). Is there a way to scale the colorbar such that this difference can be more clearly seen? Or add a deeper blue color for the very lowest concentrations? Maybe this would make it more difficult to see the changes at higher concentrations, but it is worth testing.

Page 13, line 288: Please explain further what you mean by a change in the circulation pattern (change from what pattern to what pattern?) Could this result also be due to a speeding up of the circulation rather than a shift in geographic location?

C3

Page 13, line 300: Data on the Arctic Oscillation is available- I am familiar with the NOAA data portal (<https://psl.noaa.gov/data/climateindices/>) but perhaps there are other international portals as well. Based on the observed trends in the AO index, can you confirm that the AO position could produce the observed trends in your data (i.e. was it in a different state in 2012 compared to 2016)? I'm also not sure I fully understand why a shift in the AO position would result in younger ages over the Lomonosov Ridge.

Figure 5: The difference between the dashed and solid lines is hard to discern (particularly for the yellow line). Is it possible to put more space between the dashes to make it clear which line is dashed?

Page 14, line 314: Why is the Atlantic layer considered to be between 250-500 m here compared to the 250-300 m range that is used in other figures/discussion sections? In general it is not clear why each depth range is chosen (aside from the entire Atlantic layer definition given for the 250-800 m range).

Page 15, line 318: Delete “in” so the line reads “determined for the Makarov Basin. . .”

Page 15: There is no discussion of the one yellow point near the Laptev Shelf in figure 6a. Why does this data point give such an old age?

Discussion:

Page 16, Line 375: Add a reference to Table 2 after the first comparison with the Smith 2011 study.

Page 16, line 388: Pacific water is not part of the polar mixed layer. The point of this paragraph is a good one (that we need more conservative tracers to use in Arctic surface waters), but this should be re-worded to remove the “Polar Mixed Layer” terminology in reference to the Alkire study. They consider Pacific water to be part of the upper halocline in Alkire et al. 2019.

Page 18, lines 403-410: You provide possible explanations for the high mean ages

C4

and significant mixing observed in the Nansen Basin and Fram Strait, but I am curious why this signal also appears near the Laptev Shelf. The mean age estimates for this sample are very different than other estimates in the literature. This large mean age also stands out on Figure 7. Since you do not discuss the Laptev Sea as much as the other basins, I suggest either removing the Laptev Sea box from this figure or expanding on the discussion of this region.

Page 19, line 443-444: This sentence should be re-worded to make it clear that you are still referencing the Mauldin study, for example: "The advective times reported by Mauldin et al (2010) for the BSBW pathway...". As written, it is not immediately clear which "reported advective times" you are referring to.

Page 19, line 446-447: It is stated that the difference between the mode ages in this study and the Mauldin study may be due to the positive AO phase in the 1990s. Please explain how that would affect the mode ages in the Canada Basin in particular. It is not currently clear how this explains the difference between the two estimates.

Table 3: Explain the acronyms used in the table, or write out the full words (e.g. LS, GL)

Page 21, line 478: Change "improving" to "improve"

Page 22, line 499: Insert "has" so the line reads "...The I and U tracer pair has been shown..."

It is confusing to me that the subscript "max" in t_{max} (mode age) suggests that this is the maximum age estimate, but the mode age is consistently younger than the mean age. Perhaps it is worth considering a switch to "tmode" or similar.

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