“Water masses in the Atlantic Ocean: water mass ages and ventilation”

submitted by Mian Liu and Toste Tanhua to EGUsphere

RC1:

Overview:

Mian Liu and Toste Tanhua submitted a manuscript about Atlantic water mass distribution and their (different types of) water mass ages, using transient tracers (the older anthropogenic CFC-12, the younger anthropogenic SF6, and the radioactive 39Ar) and the well-established TTD-method.

They introduce the paper with a short review of Atlantic water masses and their relevance in ocean circulation and for biogeochemical processes, followed by introducing the transient tracers and relevant methods. Moreover, they introduce the oxygen utilization and oxygen utilization rate and their importance in biogeochemical cycles.

Next, they introduce in more detail the application of the TTD method to derive water mass ages, they differentiate different types or definitions of ages and other technical aspects to compute them from the transient tracer data, which they obtained from the GLODAP data repository.

In the large results-section they show and discuss their computed water mass ages along two prominent WOCE/GO-SHIP sections and on maps in various density layers incorporating a large amount of GLODAP tracer data. They do that for several relevant Atlantic water masses, starting from the near surface layer to the deepest Atlantic water layers.

Additionally, they use their previously discussed water mass ages to derive the oxygen utilization rates for the most prominent Atlantic water masses. Finally, they compare the CFC and SF_6 based water mass ages with ages derived from the radio nuclide ^{39}Ar and discuss the deviations between the two.

Lastly, they summarize and discuss their findings and interpretation in a conclusion-section.

Dear Reviewer,

Thank you very much for your positive assessment and valuable comments on our manuscript, “Water masses in the Atlantic Ocean: water mass ages and ventilation”. We greatly appreciate your kind remarks that the manuscript is “very well written, easy to read and good to understand,” and we are particularly encouraged by your recognition of the study's strengths—namely, the “systematic application of the transient tracer-based TTD method on an Atlantic-wide large data set” and the “comprehensive discussion of the most relevant Atlantic water masses from surface to bottom,” as well as the potential utility of our findings as a “good data base for further investigations of biogeochemical cycles in the Atlantic Ocean.”

We have carefully considered all the specific comments and suggestions you provided and have revised the manuscript accordingly. Your feedback has been instrumental in improving the clarity and rigor of our work.

Once again, we sincerely thank you for your time and constructive input.

Sincerely,

Mian Liu & Toste Tanhua

General comment:

The manuscript is very well written, easy to read and good to understand. The strength of the manuscript is the systematic application of the transient tracer-based TTD method on an Atlantic-wide large data set and the comprehensive discussion of the most relevant Atlantic water masses from surface to bottom. Moreover, the authors apply their findings by calculating and discussing oxygen utilization rates Atlantic-wide. Both might be a good data base for further investigations of biogeochemical cycles in the Atlantic Ocean.

I'd like to recommend publication after minor revision.

For minor revision I have listed a few specific comments (or better suggestions):

We sincerely thank again the reviewer for their positive overall assessment of our manuscript. We are greatly encouraged by the comments that the manuscript is "very well written, easy to read and good to understand," and we appreciate the recognition of the systematic application of the TTD method and the comprehensive discussion of Atlantic water masses. We are also pleased that the reviewer finds the calculated oxygen utilization rates to be a potentially useful data product for future biogeochemical studies. We have carefully addressed all the specific comments provided by the reviewer in the revised manuscript.

General comments:

Since I am a non-native speaker; I do not comment on wording, grammar, spelling, etc. I'd like to leave these issues to the editorial board.

In my pdf all figures are of minor, very blurred quality. I guess, that happened during crating the pdf for the reviewers from the submitted manuscript and submitted figures. So, this comment goes more to the editorial board than to the authors.

Thanks again to the reviewer for this comment. Regarding the figure quality, we have ensured that all figures in our final submission are of high resolution and clarity. We will work with the editorial office to verify that the production process does not compromise their quality. Concerning language, the manuscript has been carefully reviewed for grammar, spelling, and wording, and we will rely on

the editorial board for any final polishing if necessary.

Specific comments:

Line 22-23: “eastern basin exhibiting younger ages compared to…” If this is meant in general, it should be rather opposite. Or is this specific for NEABW? Please, clarify.

Thanks to the author for pointing out the mistake. Indeed, in general, the age in the West is younger. Corrected.

Line 110-112: “The solubility (F)…” Find a better place in the methods’ section.

Thanks for suggestion regarding the placement of the solubility (F) description. We have moved the sentence about the solubility of transient tracers (originally at lines 110–112) to a more appropriate location within Section 2.2, where now naturally fits into the explanation of how partial pressures are calculated from tracer concentrations using potential temperature and practical salinity. This adjustment improves the logical flow of the methods section.

Line 113ff: You use the term “diffusive”, as widely common in this context. But wouldn’t it be more logic to use “disperse”. Many people automatically think on “molecular diffusion”, but here something different is meant, which might be better named “dispersion”.

Thank you for the suggestions made by the author. However, diffusion is a well-established term in oceanography, we should stick to this. But we could add turbulent eddy mixing (diffusivity) as a term.

Line 129-130: “Based on the TTD determination…” This sentence stays a little bit alone at this place. Skip or find better location.

We thank the reviewer for this suggestion. We agree that this sentence was isolated in its original location. We have moved it to the beginning of Section 4 ("The application of water mass ages..."), where it now effectively introduces the application of the TTD method for estimating oxygen utilization rates (OUR) and provides a better context for our subsequent analysis.

Line 140: “the TTD is only a spike.” Moreover, in such a case the Δ becomes infinite small and the IG (Eq. in line 122) formally becomes a δ-function and can be computed the same way, so it is just a special case of the same formalism and not something completely different. Eventually worth to note here.

We thank the reviewer for this insightful comment. We fully agree that the purely advective case (tracer-age) is a specific limit of the general IG-TTD formalism. Following the reviewer's suggestion, we have added a sentence in Section 2.2 to explicitly state this mathematical relationship, noting that the tracer-age concept corresponds to the limiting case of the IG-TTD when the mixing ratio

(Δ/Γ) approaches zero, and the distribution becomes a delta function. This clarification strengthens the theoretical consistency of our methodology section.

Line 141: "…underestimated by the tracer-age." Eventually worth to refer to Sonnerup, 2001, who diels with different "tracer-ages" and the systematic deviation between tracer-ages from old and young or low and high concentrations.

We thank the reviewer for pointing out this highly relevant reference. We have now cited Sonnerup et al. (2001) at the end of the sentence discussing the underestimation of ages by the tracer-age concept. This citation appropriately supports our statement by referencing the seminal work that systematically documented this systematic deviation.

Line 151-152: "The atmospheric concentration…" The sentence is important, bit at this place it stands a bit out of context. Please, find better location in section 3.

We agree with the reviewer that this sentence about the atmospheric history of the tracers is crucial but was misplaced. We have moved it to the beginning of Section 3 (Results), where it now effectively sets the stage for the age calculations and the interpretation of the results that follow.

Line 152-175: The entire Δ/Γ-section could be shortened or focused. 1. The trouble is not enough tracers to compute Δ and Γ and there is almost no way out. Many authors previously discussed this issue and using Δ/Γ=1 is widely common and accepted. 2. The entire discussion here, if Δ/Γ= 0.6, 0.8, 1.0, 1.2 etc. does not lead to a final conclusion. Moreover, why not 0.1, 0.5, 1, 2, 5? 3. In principle and in theory the authors HAVE TWO tracers (or even three with the 39Ar) and COULD compute both Δ and Γ. But I guess, that might not help, since at least CFC-12 and SF6 do not provide enough independent information. So, keep it short. All tracer people know, why, and all others don't care.

We thank the reviewer for this critical suggestion to improve the focus and conciseness of our methodology section. We have thoroughly revised and shortened Section 2.3 accordingly. The revised section now:

1. Explicitly acknowledges the common challenge of insufficient independent tracer information to uniquely determine both Γ and Δ.
2. Removes the extended discussion on specific mixing ratio values and instead directly justifies the adoption of the standard value Γ/Δ = 1, citing established literature.
3. Briefly explains that while multiple tracers are available, the similar atmospheric histories of CFC-12 and SF_6 often preclude a robust independent fit for the mixing ratio across the basin, leading to the choice of a fixed standard value.

Furthermore, as suggested by the reviewer in a subsequent comment, we have also incorporated

details on *how* the mean age (Γ) is computed from the tracers and specified the source of the atmospheric histories into this streamlined section.

After line 175:

One would like to know, HOW the Γ is computed from the tracers. I guess a least square fit or something similar. But here is my most urgent question, if the authors do that by using CFC-12 and SF6 individually or at once. It is stated somewhere further below in the manuscript, but it should already be mentioned in the methods.

Which atmospheric data do the authors use? Pleas add a reference.

We thank the reviewer for emphasizing the need to clarify these important technical details early in the manuscript. As suggested, we have now integrated the following information into the revised Section 2.3:

1. **How Γ is computed:** We state that the mean age (Γ) is determined by finding the value that minimizes the difference between the observed and TTD-predicted tracer values.
2. **Tracer usage strategy:** We explicitly state that CFC-12 and SF_6 are used **jointly** in the inversion where data are available, and clarify the primary reliance on SF_6 or CFC-12 in different concentration regimes.
3. **Atmospheric data reference:** We have added the specific source (Bullister, 2020) for the atmospheric histories of CFC-12 and SF_6.

These additions ensure the methodology is fully transparent and reproducible.

Line 177-178: In this study … vertical layers…" Could find a better place or, since independent from the following, mage an own paragraph of this sentence.

We thank the reviewer for this suggestion to improve the manuscript's structure. We have followed the advice and have made the sentence in question a separate, distinct paragraph at the beginning of Section 3. This change enhances the logical flow by clearly separating the general introduction of the water mass framework from the subsequent description of the specific sections used in this study.

Line 188ff: As mentioned above, move this to methods-section?

We agree with the reviewer that the description of the tracer selection criterion based on CFC-12 partial pressure is a methodological detail. Following this suggestion, we have removed that sentence from Section 3 (Results) and integrated its content into the revised Section 2.3 ("... In layers where the CFC-12 partial pressure was above 450 ppt, the calculation relied more heavily on SF~6~, whereas CFC-12 was the primary tracer used at lower partial pressures, following the recommendation of Tanhua et al. (2008)."). This improves the structural logic of the manuscript by

keeping the methodology in one place.

Line 197-200: Move this paragraph to a better location, e.g. together with the sentence in lines 177-178.

We thank the reviewer for this excellent suggestion to improve the logical flow. We have moved the paragraph introducing the three selected hydrographic sections to follow immediately after the sentence that states our adherence to the water mass framework of Liu and Tanhua (2021). This restructuring creates a coherent introductory paragraph in Section 3 that first establishes the conceptual foundation (the water mass framework) and then presents the observational basis (the selected sections) for the results that follow.

Figure 6: Could you add “mean age” and “mode age” into the figure? (Holds for all such figures.)

Added

Figure 7: Limit from 0 to 1000 dbar?

Changed

Figure 9: Limits from 0 to 3000 dbar?

Changed

Line 290: “over THE ridge”?

We thank the reviewer for catching this oversight. The sentence has been corrected to "over **the** ridge" to properly refer to the specific topographic feature (the Mid-Atlantic-Ridge).

Line 359-366: The discussion why to favor the mode age instead the mean age does not convince me. In my understanding, the mode age and the mean age are almost one order of magnitude apart, hence, the OUR would also be almost 1 order of magnitude apart. Thus, an argument is needed, why the one and not the other. As I understand the authors, the mean age is subject to mixing with adjacent water masses. True. But the mode-age is, too. So, this argument does not count. I have also no clue, what is the right argument for favoring the mode-age. I guess it is related to either the “right” order of magnitude OR it has something to do with the integrative effect of the mean-age (the mean age sums all the damn history) and the more advective character of the mode-age.

We thank the reviewer for this critical and insightful comment, which rightly challenges the logic of our initial argument. The OUR is not constant over the life-time of a water mass, but is strongly depth and temperature dependent. Neither using the mode-age or the mean-age will be a reflection of the local OUR. The mean-age (Γ) represents the average time elapsed since all water parcels in the sample were last in contact with the atmosphere, integrating over all pathways including long tails of older water, that most often has seen very low OUR in the interior ocean. In contrast, the

mode-age, representing the most probable transit time, better captures the advective timescale of the dominant water mass along its dominant pathway from its formation region. The OUR calculated from mode-ages thus more closely approximates the time available for respiration to occur along the core of the water mass's trajectory. Using the mean-age, which includes much older water fractions will yield unrealistically low rates. We choose to use mode-age since it provides values in approximately the right magnitude, noting again that this value is not an absolute measure of local OUR.

Line 428-429: If the authors decide to put the technical aspect of how to compute Γ into the method-section, this could also go there. As I understand right, it is just the equation from line 122 with an additional decay factor e-λ/t? Might be worth to note clearly.

We thank the reviewer for this precise technical suggestion. We have now integrated a clear explanation of the computational method into the revised Section 2.3. As correctly pointed out by the reviewer, the key difference lies in the treatment of radioactive decay. We have explicitly stated that for stable tracers (CFC-12, SF6), the convolution is performed with the atmospheric history and the TTD alone, whereas for the radioactive 39Ar, an exponential decay factor e(-λt) is incorporated into the convolution integral. This clarification ensures the methodology is complete and transparent.

Line 442: "(Fig. 18)" does not exist.

Thanks to the reviewer for pointing out the mistake. We were sorry that we failed to correct the picture numbers in the text in time after integrating the pictures before

Line 455-465: What I would really enjoy would be a Figure 18 as a property-property plot as CFC/SF6 mean-age versus 39Ar mean-age and CFC/SF6 mode-age versus 39Ar mode-age and a short related discussion of such a figure. I'm sure one could learn something from it.

Thanks for the valuable suggestion. Due to the scarcity of ^{39}Ar data, it was impossible to form a complete section. Therefore, we added a table to compare the ages based on CFC-12 and ^{39}Ar at the source of the water mass, during its transmission process, and at the end point. In the article, we also elaborated on the applicable ranges of these different tracers.

Line 467: I would put this sentence to the end of this paragraph.

Corrected

Line 468: Add 39Ar to the bracket.

^{39}Ar is added

Line 470: Replace "investigated" by "discussed"?

Agree and changed

Line 472: Replace “different parts” by “different fractions” or “different pathways”?

Agree, different pathways are now used

Line 498-505: Here I was puzzled. I can’t put this into the context of the manuscript. I can’t see climate change in the manuscript, since no repeat sections or stations are compared. Numerical simulation methods were not applied here. Please, either skip or clarify in the manuscript’s context.

We sincerely thank the reviewer for this critical correction. The reviewer is absolutely right that our manuscript does not investigate temporal changes related to climate change nor does it employ numerical simulations. The paragraph in question was a poorly formulated and inaccurate attempt at a broader outlook, which was not grounded in the actual findings of our study. We have therefore deleted the entire paragraph (Lines 498-505). In its place, we have added a concluding statement that accurately reflects the contribution of our work, emphasizing that it provides a crucial baseline dataset for future studies of ocean change and for model validation, which is a supported and appropriate claim.