**“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 $\Delta$ becomes infinite small and the IG (Eq. in line 122) formally becomes a $\delta$-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 ($\Gamma$) 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.

# Review of "Water masses in the Atlantic Ocean: water mass ages and ventilation" by Mian Liu and Toste Tanhua.

submitted by Mian Liu and Toste Tanhua to EGUsphere

RC2

**General comments:**

The study is of the GLODAP data product, in particular transient tracers and dissolved oxygen data are used to estimate water mass ages and ventilation. The transient tracers of CFC-12, SF6 and 39Ar are examined with the assumption of an Inverse Gaussian function in order to find the mean- and mode-age of different water masses. The resulting calculations of age and ventilation of different water masses is outlined in great detail. Further inferences regarding the oxygen utilization rate are outlined with implications for biogeochemistry.

I think that this study could be worthy of publication in EGU Ocean Sciences, once my below comments have been addressed. As further outlined below, the Data and methods section was confusing and needs a strong revision. The figures were not of a high production quality (often too small, low resolution) but could be improved with some effort. Finally, the authors should take more care in stating the limitations of the results and when the conclusions are more speculative. I have tried to note specific examples in the below comments.

On a positive note, I will say that much of the paper was clear and easy to follow (e.g. Introduction and Results sections). While the paper reads a little like a report at times, with results described in detail one after another, I think that this is probably unavoidable due to the nature of the study. Overall, I think it would be beneficial to the community to have the work published, as work on water mass age and ventilation is important to the community.

We sincerely thank Reviewer for your thorough and constructive review of our manuscript, as well as for their positive assessment of its potential contribution to the community. We are pleased that the reviewer found the Introduction and Results sections clear and well-structured.

We have carefully considered all the comments and have revised the manuscript accordingly. Below, we summarize the key changes made in response to the major concerns:

**Revision of the Data and Methods Section (Section 2):**

We have completely restructured and rewritten Section 2 to improve clarity and logical flow. The equations are now introduced with clearer explanations, including definitions of key terms (e.g., width as $\Delta$, units of the mixing ratio), and the rationale behind the use of mean-age and mode-age is more thoroughly explained. We have also incorporated clarifications regarding the Inverse Gaussian Transit Time Distribution (IG-TTD) and the assumptions involved, as suggested.

**Improvement of Figure Quality:**

All figures have been revised to enhance resolution, readability, and labeling. We have increased the size of panels in Figures 6, 8, 10, and 14, improved color contrast in Figure 14 for better distinction between parameters, and ensured that all captions are clear and complete. We have also verified that all symbols and contour lines are consistently explained across figures.

**Clarification of Limitations and Speculative Statements:**

We have tempered language where conclusions were overly speculative, particularly regarding mixing processes and climate change implications. The limitations of assuming a fixed mixing ratio ($\boldsymbol{\Delta/\Gamma} = 1$) and the steady-state IG-TTD are now more explicitly acknowledged in Sections 2 and 6. The concluding paragraphs have been revised to better reflect the scope of our study and avoid unsupported claims about climate change.

We believe that these revisions have significantly strengthened the manuscript, and we hope that the updated version meets the reviewer's expectations.

**Major comments:**

1. Data and methods section: I am not an expert in age tracer methods, so I found that this section was very confusing to follow. I suggest to strongly revise this section, keeping in mind to explain the overall methods and reasoning to the reader at each step. For example, the equations (Lines 116-122) were introduced abruptly with little explanation. I was left with basic questions e.g. is width defined as \Delta or \Delta^2? What are the units on the mixing ratio? What is 2-IG? These are just some examples. Some information later in the manuscript (e.g. Lines 352-366) would have been helpful earlier in Section 2.

   We thank the reviewer for the essential feedback on improving the clarity of our methodology section. We have completely restructured and rewritten **Section 2** to enhance its pedagogical flow and address all raised concerns:

   **Clearer Introduction of Concepts:** We now introduce the TTD concept and the governing convolution equation **before** presenting the IG-TTD formula, providing necessary context.

   **Explicit Parameter Definitions:** We have added clear definitions for all parameters immediately after the IG-TTD equation, explicitly stating that the "width" is denoted by Δ (with units of years), that the mixing ratio **($\boldsymbol{\Delta/\Gamma}$)** is unitless, and explaining what a "2-IG" distribution refers to.

   **Early Explanation of Age Definitions:** We created a new dedicated subsection (**2.2**) to explain the three "flavors" of water mass age (tracer-age, mean-age, mode-age), their physical interpretations, and their respective applications early in the methodology. This addresses RC2's point that this information was previously located too late and incorporates RC1's suggestion about the

mathematical relationship between tracer-age and the IG-TTD limit.

**Integrated Technical Details:** As suggested by RC1, we have integrated key technical details into Section 2.3, including:

1) How Γ is computed (via minimization between observed and predicted tracer values).

2) Tracer usage strategy (joint use of CFC-12 and SF6 where available, with a defined threshold).

3) Atmospheric data reference (Bullister, 2020).

4) Treatment of radioactive tracers (incorporation of decay term for $^{39}Ar$).

**Streamlined Mixing Ratio Justification:** We have simplified and focused the justification for using $\boldsymbol{\Delta/\Gamma = 1}$, acknowledging the limitation while citing established practice, as recommended by both reviewers.

We are confident that these revisions make the methodology far more accessible, especially for readers less familiar with age tracer methods, while maintaining scientific rigor.

2. Figure quality was low: figures were quite low resolution (at least in the version I had). They were often too small (e.g. Figure 6) and required zooming in to see the details. I also suggest that different colors could be used for the different parameters in Figure 14, in order to more easily distinguish between the parameters in the panels. Some captions could also do with more clarity, and I have tried to note these points in more detail in the comments below. The actual data on the figures was interesting, and I think it would be a relatively straightforward fix to improve the figure quality across the entire paper.

We thank the reviewer for the detailed feedback on figure quality. We have systematically revised all figures in the manuscript to address these concerns:

1. **Overall Quality Enhancement:** All figures have been regenerated as high-resolution (600 dpi) PDF/TIFF files. We have increased the size of key figures (e.g., Figures 6, 8, 10, 12, 14) and optimized font sizes for all labels, legends, and annotations to ensure clarity without requiring zoom.

2. **Improved Color Scheme in Figure 14:** As specifically suggested, we have implemented a distinct color scheme for the four parameters in Figure 14:

   **Mode-age**: Blue

   **Dissolved Oxygen (DO)**: Green

   **Apparent Oxygen Utilization (AOU)**: Orange

**Oxygen Utilization Rate (OUR)**: Red

This change significantly improves the distinguishability between parameters.

3. **Enhanced Caption Clarity:** All figure captions have been revised to provide more detailed descriptions. For example:

   Figure 3: Now explicitly states the units of age (years) and describes the green contour lines.

   Figure 14: Clearly defines the core neutral density for each water mass and the meaning of grey dots.

   Figure 15: Corrected to accurately describe the displayed panels (Mode-age, DO, AOU, OUR).

4. **Other Specific Improvements:**

   **Figures 1 & 2:** Redrawn as vector graphics with direct labels for "mean-age" and "mode-age".

   **Figures 16 & 17:** Replaced "pentagrams" with "star symbols" for better readability and ensured consistency in contour line definitions with other figures.

We believe these comprehensive revisions have substantially improved the visual quality and interpretability of all figures in the manuscript.

3. Being more careful outlining limitations of results and speculation: Some of the content in Section 2.1 and 2.3 outlined some of the main limitations of the results, such as assuming an Inverse Gaussian profile and assuming a mixing ratio of 1. I understand that these assumptions are required in order to get useful results out of the data, but I think that the results and conclusions section could help with re-iterating these limitations. There were also a few times where the wording was a bit strong on speculative statements, for example in the conclusion "The results show that with global warming, the age of the Atlantic water mass shows a certain degree of change." But my take on the paper was that it was showing the water age and ventilation, rather than changes with climate? So this seems a little speculative and I have tried to note these parts in the comments below.

We thank the reviewer for highlighting the need for greater caution in presenting limitations and avoiding undue speculation. We have made comprehensive revisions throughout the manuscript to address this concern:

1. **Explicit Limitations Section in Conclusions:** We have added a dedicated paragraph in Section 6 that explicitly discusses the main methodological limitations, including the assumptions of the IG-TTD, the fixed mixing ratio ($\Delta/\Gamma = 1$), and the steady-state

assumption. This provides readers with appropriate context for interpreting the results.

2. **Removal of Speculative Climate Change Statements:** We have completely removed the speculative paragraphs in the conclusion that discussed climate change impacts and policy implications, as rightly noted by the reviewer that our study presents a snapshot of water mass ages rather than analyzing temporal changes. These have been replaced with text that accurately frames our work as providing a baseline dataset for future studies.

3. **Tempered Language in Results Interpretation:** Throughout Section 3 (Results), we have carefully revised language that presented interpretations about mixing processes as definitive explanations. We have replaced strongly worded causal statements with more cautious language that presents these interpretations as consistent with the observed patterns, acknowledging other potential factors.

4. **Contextualization in Discussion:** We have added a clarifying statement at the beginning of Section 4 to remind readers that the derived oxygen utilization rates are subject to the same methodological assumptions outlined earlier.

We believe these changes significantly improve the scholarly rigor of the manuscript by more carefully distinguishing between well-supported results and interpretations, while providing appropriate caveats for the reader.

Specific comments

- Abstract: “The distribution of oceanic water masses and their properties, such as ventilation constitute fundamental parameters, for instance, the thermohaline circulation patterns and biogeochemical processes in the marine systems.” This sentence does not make sense and needs revision.

  We have completely rephrased this sentence to better convey the scientific importance of water mass distributions and their properties. The revised sentence now reads: "The distribution of water masses and their characteristics, including ventilation, provides fundamental insights into large-scale oceanographic processes such as thermohaline circulation and marine biogeochemical cycles." This version eliminates the grammatical issues and more clearly expresses the central role of water mass properties in understanding ocean dynamics.

- Line 19-22: Can remove the brackets around “mean” and “mode” in this sentence.

  Removed.

- Paragraph from Line 45-55: There has been more work recently using ocean models to discuss the age tracer, please consider citing some of these (e.g. Li, Q., England, M. H., Hogg, A. M., Rintoul, S. R., & Morrison, A. K. (2023). Abyssal ocean overturning slowdown and warming driven by

Antarctic meltwater. Nature, 615, (7954), 841-847).

We have incorporated the citation to Li et al. (2023) as recommended, adding a new sentence that acknowledges recent advances in ocean modeling and their contribution to understanding ventilation dynamics. This addition strengthens the introduction by demonstrating awareness of complementary modeling approaches while maintaining the focus on the observational gap that our study aims to address.

- End of Introduction: Consider adding a paragraph with a brief outline of the rest of the paper to orientate the reader.

  We have added a new paragraph at the end of the Introduction (Section 1) that provides a clear outline of the paper's structure. This addition will help to orient readers and guide them through the manuscript's organization, improving the overall readability and flow of the paper.

- Data and methods section: See Major Comment 1 (above). Unfortunately, by the end of Section 2 I still did not really understand why mean- and mode-ages are used for different applications. Please strongly revise Section 2, as it is by far the weakest section.

  We thank the reviewer for this critical feedback and for pushing us to substantially improve the clarity of our methodology. As detailed in our response to Major Comment 1, we have undertaken a major revision of **Section 2 ("Data and methods")** to address the confusion regarding the application of mean-age and mode-age.

  The core of this revision was the creation of a new, dedicated subsection (**2.2 "Definitions of Water Mass Age: Tracer-age, Mean-age, and Mode-age"**). In this subsection, we now explicitly and clearly separate the definitions and, most importantly, the *applications* of these different age concepts:

  - We state that the **mean-age (Γ)**, being the average transit time of all water parcels in a sample, provides an **integrative timescale**. We clarify that this makes it particularly useful for **biogeochemical studies** where processes depend on cumulative exposure time, such as calculating integrated oxygen consumption from AOU.
  - We explain that the **mode-age ($\tau_{mode}$)**, representing the most probable transit time of the dominant water fraction, is a better proxy for the **advective timescale** of the core of a water mass along its main pathway. It is therefore more relevant for discussing **physical transport timescales**.

  This clear distinction in purpose is now established early in the methodology and is consistently applied throughout the manuscript, especially in Section 4 where we justify the use of mode-age for calculating Oxygen Utilization Rates (OUR). We are confident that the restructured **Section 2** now

provides a much more logical and pedagogical foundation for the reader.

- When G(t) first introduced: Explain physically G(t) and the assumptions going in to G(t). My understanding is that you are assuming the shape of the profile as G(t) and then further assuming the mixing ratio, and either the width of the mixing or \Lambda? Then you have the profile such as in Figure 1b. But it is still not clear to me at this stage what measurement is required for Figure 1b. How does CFC-12=300ppt play into G(t)?

  We thank the reviewer for these insightful questions, which have helped us significantly improve the explanation of our core methodology. We have substantially revised **Section 2.1** to address these points directly.

  1. **Physical Explanation of G(t) and its Assumptions:** We now explicitly introduce $G(\tau)$ as the probability distribution of transit times $\tau$ for water parcels traveling from the surface to an interior point. We clarify that the fundamental assumption is that this complex process can be represented by a specific, parameterized function—the Inverse Gaussian (IG) distribution. The key assumptions going into this are:

     The shape of the TTD is an IG function.

     The ocean circulation is in a steady state.

     The chosen IG function (one-parameter with a fixed $\Delta/\Gamma$) adequately represents the net effect of advection and mixing.

  2. **Clarification of Figure 1b and the Role of CFC-12=300 ppt:** We have revised the text and caption to eliminate this confusion. The caption for Figure 1b now clearly states that it illustrates the *concept* of the different ages for a *given* TTD. We have added a new sentence in Section 2.2 that explains the computational process:

  "For a water sample with an observed tracer concentration (e.g., CFC-12 = 300 ppt), the mean age ($\Gamma$) is the key variable determined by finding the value for which the TTD-predicted concentration (the convolution of the TTD with the atmospheric history) matches the observation. Once $\Gamma$ is determined for a fixed $\Delta/\Gamma$, the full TTD (as in Fig. 1b) is defined, from which the mode-age can be calculated."

  This revision makes it clear that the measurement is the tracer concentration, which is used to infer the mean age, which in turn defines the specific TTD profile shown in figures like Fig. 1b.

- Line 144-147: "The mean-age is obtained by the average value of all the different aliquots in one water sample by considering the transport time of different pathways (Fig. 1, b). This concept of water mass age is often useful for biogeochemical studies. In other cases, such as when discussing the transport times of water masses from the formation area, the mode-age concept is useful." This

seems directly in contrast with Lines 471-473: “The mean-age, which shows the average value of all the different parts in one water sample, is used to show the static distribution of water masses. The mode age, which shows the age of the dominant water mass in the sample, is used to trace the biogeochemical phenomena.”

We sincerely thank the reviewer for identifying this significant inconsistency in our manuscript. The reviewer is absolutely correct that these two statements presented a direct contradiction regarding the appropriate applications of mean-age and mode-age. We have carefully reconsidered the physical interpretation of these age definitions and have implemented a consistent and physically sound framework throughout the entire manuscript. **The correction is as follows:**

We have unified the narrative to state that the **mean-age**, being an integrative timescale that accounts for all pathways, is the most appropriate metric for **biogeochemical studies** where processes like oxygen consumption are cumulative and depend on the full history of the water parcel.

Conversely, the **mode-age**, representing the most probable transit time of the dominant advective pathway, is the more relevant metric for discussing **physical transport timescales** from the formation area.

This correction has been applied in **Section 2.2** and carried through consistently to **Section 4** (on Oxygen Utilization Rates) and the **Conclusions (Section 6)**. The revised text now accurately reflects the standard interpretation of these concepts in the literature.

- Figure 1: How does the description of (a) in the caption match with the figure in panel (a)? Text on the figure is very small also.

  Thanks to the reviewer for pointing out the discrepancies between the description and the graphics in Figure 1, as well as the small size of the text. We have made comprehensive revisions to this figure:

  Redraw Figure 1: We have redrawn this figure using vector graphics software, significantly increasing the font sizes of all text labels, axis annotations, and legends to ensure clear readability at the publication size.

  Correct the consistency between the description and the content of Figure (a): The original Figure (a) aims to visually demonstrate the concept of "tracer age" - that is, the concentration of the tracer in the water sample and the equilibrium state of the atmosphere at a certain historical moment, and implicitly assumes "pure advection and no mixing". In the modified figure, we make the figure and the text description completely correspond through clear illustrations and annotations (such as "surface formation zone", "single advection path", "tracer matching point with atmospheric history"), avoiding misunderstandings.

  Optimize the expression of figure (b): We clarified the meaning of the curves in the figure, directly

marked the positions of "average age (Γ)" and "mode age ($\tau_{mode}$)", and provided additional explanations that this curve was calculated based on a specific mixing ratio ($\Delta/\Gamma = 1$) and observation value (CFC-12 = 300 ppt) to enhance the comprehensibility of the concept.

- Figure 2: Very small text and the vertical axis line was not present on panel (a).

  The vertical axis has been corrected, and the description of the picture has also been revised.

- Figure 3: Low resolution and tricky to make out the results as the panels are small. What are the green lines? Please also state the units of age either on the figures or in the caption. Also, it would help to use the same mixing increments as in Figure 2(b) i.e. 0.6, 1.0 and 1.4. This would connect Figures 2 and 3 better.

  The water mass age unit (years) has been added, along with explanations for the green contour lines. Moreover, Figure 2(b) has been revised to be in sync with Figure 3. High-resolution figures are uploaded separately as an attachment.

- Line 156: “ration” -> “ratio”

  Corrected

- End of Section 2: The justification of mixing ratio of 1 is not particularly strong, as it does seem like a value that could change a lot regionally depending on physical processes in the ocean. I agree that the authors are limited here so a mixing ratio of 1 is probably the right choice, but it would help to highlight that this is a limitation of the study.

  We thank the reviewer for this important comment and fully agree that the regional variability of the mixing ratio is a significant consideration. In our revision, we have strengthened the justification for using a value of 1 while also explicitly acknowledging this as a limitation of our methodology.

  - As now detailed in **Section 2.3**, we justify the choice of the standard mixing ratio ($\Delta/\Gamma = 1$) based on its widespread adoption in large-scale oceanographic studies, which allows for a consistent and comparable analysis across the Atlantic basin. Crucially, we have added a clear statement acknowledging the limitation:

  - "We acknowledge that regional deviations from this value exist due to variations in physical processes, but a uniform value allows for a systematic, basin-wide comparison given the constraints of the available tracer data."

  - Furthermore, as requested, we have reiterated this limitation in the **Conclusions (Section 6)**, where a new dedicated paragraph discusses the methodological assumptions. We state that our results rely on this fixed mixing ratio and that this represents a simplification of

complex oceanic mixing processes, which should be considered when interpreting the absolute age values.

We believe these revisions provide a more balanced and rigorous presentation of our methodological choices.

- Table 1 caption: Consider simplifying the text to something like: "Summary of the selected hydrographic cruises to the Atlantic Ocean chosen for the oceanographic sections in this study".

  Table title has been simplified

- Figure 4: What are the green contour lines on the figures?

  The explanation of the green contour lines has been added.

- Section 3.1: This sub-section starts with a long paragraph. Consider splitting it into two paragraphs for ease of reading.

  The original two long paragraphs have been divided into four smaller ones, making easier for readers.

- Figure 6: plots are small. Please make them bigger.

  This figure (as well as other corresponding figures in the article) has been adjusted according to the suggestions and has been uploaded as the attachments.

- Line 230: "… section through in the distribution …" Please remove "in"

  corrected

- Line 235-236: "In principle, the AAIW is supposed to get higher ages towards the north." Consider explaining why this is expected.

  A brief explanation is given, as the formation area is in the south, the water mass ages increase with the northward transport distance.

- Line 239-240: "The reason for the above result is the intervention from surrounding water masses." I find this sentence strongly worded for this more speculative result. Other reasons for the result could be that the IG function is not a good assumption, or that mixing ratio really does change across the ocean.

  We agree that the original wording regarding the cause of the age pattern was overly definitive. As suggested, we have revised this sentence to temper the language and acknowledge the speculative nature of this interpretation with relevant literatures as support. The original statement has been replaced with a more cautious a formulation: "The observed decrease in ages likely results from mixing with younger surrounding water masses, although other factors such as limitations of the

steady-state IG-TTD assumption (e.g., Waugh et al., 2004) or regional variations in the mixing ratio (e.g., Thomas et al., 2020) may also contribute."

- Line 240-241: "The maximum distance of AAIW to the north can reach 30 °N, but between 241 20 °N and 30 °N, this water mass mixes with the ENACW and upper NADW…" Consider saying "we speculate that this water mass mixes…" or "this water mass likely mixes…"

  We have revised this sentence to adopt a more observational and less assertive tone, as recommended. The original phrasing has been changed to: "the mixing is speculated between 20 °N and 30 °N, sicne the ENACW and upper NADW comes into contact with AAIW from the upper and lower (Liu and Tanhua, 2021)". This revision frames the mixing as an interpretation of the observed data (e.g., from the OMP analysis and age distributions) rather than a definitive physical assertion.

- Paragraph of Lines 253-262: Again, I find much of this discussion on mixing as speculative. Please consider mentioning this or weakening the statements a little.

  We agree that the original language could be perceived as overly assertive in attributing vertical age differences directly to mixing. We have revised the paragraph in Section 3.2 to temper the statements and more clearly frame the discussion as an interpretation of the observed age patterns.

  Specifically, we have:

  1. Replaced direct causal links (e.g., "due to mixing") with language indicating consistency (e.g., "consistent with the influence of mixing").
  2. Reframed the description to present the observed age structures first, then suggest they are *consistent with* or *interpreted as* resulting from mixing with adjacent water masses of known ages.
  3. Used terms like "composite water mass" and "composite age signals" to highlight that the derived ages represent an integrated signal.

  These changes maintain the logical connection between the observed age gradients and the likely physical process of mixing, while more appropriately acknowledging the inferential nature of this part of the discussion.

- Figures 8, 10, 14: Panels are small, please make them bigger.

  The relevant pictures have been modified.

- Figure 14: AOU top panel. What does it mean for this value to go negative? And how does this influence the OUR result?

  When AOU takes on a negative value, it indicates that the measured dissolved oxygen concentration exceeds its theoretical saturation concentration, suggesting that the water sample had undergone

effective oxygen exchange with the atmosphere shortly before sampling (for example, through intense ventilation, mixing, or bubble injection processes), or there was net photosynthetic oxygen production locally, causing the water body to temporarily be in an "over-saturated" state. In this figure, negative AOU exists in the formation area of AAIW with well-ventilated surface or near-surface areas. This is consistent with the aforementioned physical mechanism. Therefore, when AOU is negative, the OUR will also be negative. From a biogeochemical perspective, a negative OUR does not mean that "the oxygen consumption rate is negative", but indicates that within the considered modal age time scale, the net process of the water body is the increase of oxygen rather than consumption. This reflects that this water mass has recently undergone a process dominated by strong physical ventilation (possibly combined with biological oxygen production).

In this section, we focus on the positive AOU and older, widespread water masses (such as AAIW, NADW, AABW), which dominated the net oxygen consumption signal in the deep sea. The few negative AOU stations shown in Figure 14 were retained as part of the data background to fully present the data distribution. The negative stations of OUR is set to 0 because its negative value is relatively small. They do not affect our core conclusions regarding the spatial pattern of oxidation rates of the main water masses (such as the east-west difference). In the paragraphs where the spatial distribution of OUR needs to be explained (such as Section 4), we have focused on the typical areas where AOU is positive.

A brief explanation is added in the caption of Figure 14 to clarify the occurrence of negative AOU and its physical meaning. Note: Negative AOU values indicate oxygen supersaturation, which is typically observed in well-ventilated surface or near-surface waters.

- Figure 15: Caption does not appear to line up with shown panels.

  Thank the reviewer for pointing out the oversight. We have made the correction.

- Figure 16, 17: “pentagrams” symbols would more commonly be called “stars”.

  Corrected.

- Figure 16,17 caption: “The solid isolines show the 50% fractions of water masses and the dashed lines show the 20% fractions.” Is this consistent with the lines shown in Figure 15?

  Thank the reviewer for pointing out the oversight. We have made the correction.

- Line 442: Refers to Figure 18. Is this figure missing?

  Due to the adjustment of the figure numbers, the original 18 pictures have now been reduced to 17. However, the text has not been corrected in time. Thank you for the reviewers' suggestions. The issue has been corrected.

- Line 467-468: “As the continuous work…” perhaps replace with “In the present work…” ?

  Revised as per the suggestion.

- Conclusions: “Three different definitions of water mass ages are investigated. The tracer-age assumes the ocean as a totally advective situation without diffusion and underestimates the actual age in the realistic ocean.” Tracer-age is briefly discussed in the manuscript, but no figures or results are shown of it, so I would disagree that it has been investigated here. I suggest to just be careful of the wording here.

  While the concept of tracer-age is introduced in Section 2.2 to explain the theoretical spectrum of age definitions, our study does not present specific results or analysis for “tracer-age” as a standalone product. The quantitative investigation and all figures are based on the mean-age and mode-age derived from the TTD inversion.

  We have revised the relevant sentences in the Conclusions (Section 6) to correct this overstatement and clarify the scope of our work. The revised text now states that the three age concepts are discussed within the framework, but it explicitly specifies that the quantitative analysis and results are based on mean-age and mode-age.

  This wording more accurately reflects the content of our manuscript. We appreciate your careful reading and helpful suggestion to improve the precision of our language.

- Final two paragraphs of the conclusions: Talk about effects of climate change, but (as far as I can see) this is not really touched on in the actual results of this paper. So perhaps it is a bit speculative? It would also help to remind the reader of the limitations of the study in the conclusions.

  We made the following adjustments to the ending.

  Deleted speculative content: The core of our research is to present the current age of the water mass and the spatial distribution of ventilation, which is a "snapshot" and does not analyze its changes over time or directly attribute it to climate change. Therefore, the paragraphs discussing the impact of climate change and policy significance in the original conclusion are indeed beyond the scope of this study and are speculative. We have completely deleted these two paragraphs.

  Add a section on limitations: Following your suggestion, we have added a new subsection "Limitations and future perspectives" to the conclusion part. This subsection clearly states the main methodological assumptions and limitations of this study (such as steady-state IG-TTD, fixed mixing ratio, and the uncertainty of the tracer), and reminds readers to consider these factors when interpreting the results.

  Restated research focus and value: After removing speculative elements, we refocus on clarifying the direct contributions of this study: namely, the establishment of a quantitative baseline dataset for

contemporary Atlantic ventilation structures. The core value lies in providing a benchmark for validating models and a reference for detecting changes for future research.

We believe that with the above modifications, the conclusion section is now more rigorous, accurate, and entirely based on the results of this study. At the same time, it has appropriately responded to your suggestion regarding reminding readers of the limitations.