In the revised manuscript, the authors have improved the validation and clarification of their methodology and sharpened the focus on water age and AOU in the models using a consistent methodological framework across all models. These revisions enhance both the clarity of the manuscript and the robustness of the findings. I only have a few minor comments and suggestions before the manuscript can be considered for publication.

General comments:

1. Across the manuscript and Discussion, I suggest reconsidering the use of the term "circulation" and, where appropriate, replacing it with "ventilation." Ventilation refers to the exchange of surface water properties with the ocean interior and is therefore directly linked to the cycling of oxygen and carbon. In contrast, circulation more generally describes the advective transport of water masses. Since water age (including ideal age and tracer-based age estimates such as TTD age) is fundamentally a measure of ventilation rather than circulation, using "ventilation" would be more precise and conceptually consistent.
2. Section 3.2: Based on the title and the emphasis in the abstract, I expected a more detailed inter-model comparison of $S_{\Delta age}^{\Delta AOU}$. While the section presents relevant results, the discussion of inter-model differences and their implications appears somewhat limited.

Specific comments:

Line 43: Suggest to change as "In contrast, greater accumulation of remineralised carbon in the ocean interior…." to avoid the potential confusion of "higher remineralisation rate".

Line 57: The authors may wish to consider including additional relevant references, such as Sonnerup et al. (1999, 2013, 2015).

Line 89: Please add the definition of the ideal age that you use.

Lines 169-179: The authors may simply clarify the methodological distinction from the classical oxygen utilization rate (OUR) concept: In the OUR framework, aerobic respiration rates are estimated from the spatial gradients of AOU and water-mass age. In contrast, the approach used here appears to rely on the temporal trends of AOU and age within the same grid cell to diagnose the sensitivity of oxygen changes to changes in ventilation.

Figure 2: Please clarify why the trend in ideal age can exceed 1 year per year. This may result from the redistribution of water masses rather than literal aging at that rate, but this point is not immediately obvious and should be briefly explained for clarity.

Line 345-353: The authors may wish to consider including additional relevant references on studies that estimate temporal changes in ocean ventilation, such as Waugh et al. (2013), Gerke et al. (2024), Wefing et al., (2025), and Guo et al. (2026).

Lines 354–380: I am happy with what the manuscript is and do not recommend additional analysis. Still, it might be nice to further explain some technique details.

In the first round of revision, I raised concerns regarding the use of age trends derived from a single-tracer constrained IG-TTD method, as this type of age estimate can exhibit spurious temporal trends (e.g., Guo et al., 2025). This issue is particularly relevant here because the metric is derived from the temporal trends of age and AOU; therefore, methodological uncertainties in the age trend may directly affect the inferred sensitivity. In this context, the comparison between observational estimates (based on single-tracer IG-TTD ages) and model results (based on ideal age) may not be entirely consistent.

Dual-tracer-based age estimates provide more robust and reliable temporal trends (Guo et al., 2025, 2026) and would generally be preferable in this context. However, since the authors now focus primarily on the ideal age in model simulations, this specific concern is substantially reduced.

For method comparison, previous OUR-based studies (e.g., Sonnerup et al., 1999, 2013, 2015; Sulpis, 2023) relied on spatial gradients of age and AOU rather than temporal trends, which makes their estimates less sensitive to potential spurious temporal variability in age diagnostics.

Reference:

Gerke, L., Arck, Y., & Tanhua, T. (2024). Temporal variability of ventilation in the Eurasian Arctic Ocean. Journal of Geophysical Research: Oceans, 129(7), e2023JC020608. https://doi.org/10.1029/2023JC020608

Guo, H., Koeve, W., Kriest, I. et al. North Atlantic ventilation change over the past three decades is potentially driven by climate change. Nat Commun 17, 200 (2026). https://doi.org/10.1038/s41467-025-67923-x

Sonnerup, R. E., Quay, P. D., & Bullister, J. L. (1999). Thermocline ventilation and oxygen utilization rates in the subtropical north Pacific based on CFC distributions during WOCE. Deep Sea Research Part I: Oceanographic Research Papers, 46(5), 777–805. https://doi.org/10.1016/s0967-0637(98)00092-2

Sonnerup, R. E., Mecking, S., & Bullister, J. L. (2013). Transit time distributions and oxygen utilization rates in the northeast Pacific Ocean from chlorofluorocarbons and sulfur hexafluoride. Deep Sea Research Part I: Oceanographic Research Papers, 72, 61–71. https://doi.org/10.1016/j.dsr.2012.10.013

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Waugh, D. W., Primeau, F., DeVries, T., & Holzer, M. (2013). Recent changes in the ventilation of the southern oceans. science, 339(6119), 568-570. https://www.science.org/doi/10.1126/science.1225411

Wefing, A. M., Payne, A., Scheiwiller, M., Vockenhuber, C., Christl, M., Tanhua, T., & Casacuberta, N. (2025). Changes in water mass composition and circulation in the central Arctic Ocean between 2011 and 2021 inferred from tracer observations. *Ocean Science*, *21*(6), 3311-3340. https://doi.org/10.5194/os-21-3311-2025