

Article: AMOC weakening across latitude and time in CMIP6 future scenarios

July 2026

I thank the authors for considering my comments made on the earlier version of the manuscript. In my view, this substantially improved the article. Nevertheless, I have a few more comments before I would consider the manuscript suitable for publication in *Ocean Science*.

Major Comments

1. A recent preprint on the "Dependence of AMOC Meridional Connectivity on Resolution, Timescale, and Definition" (found at: <https://essopenarchive.org/doi/full/10.22541/essoar.15004331/v1>), demonstrates that the meridional connectivity of AMOC weakening depends strongly on the precise definition of the AMOC used across latitude. In the present manuscript, the authors define the AMOC as the maximum overturning streamfunction at each latitude. However, the referenced study shows that this definition can suppress apparent latitudinal coherence. Therefore, the results presented here are likely influenced by the specific AMOC definition adopted. I suggest that the authors repeat the analysis using fixed buoyancy levels, which may provide a more physically appropriate framework given that buoyancy anomalies predominantly propagate along surfaces of constant density. If this alternative definition does not substantially affect the conclusions, this sensitivity should at least be discussed briefly in the revised manuscript. If, however, the results are significantly altered, the implications of this dependence should be incorporated into the interpretation of the results.
2. The authors provide a physically plausible explanation for the reduced rate of AMOC weakening as the system continues to weaken, particularly by attributing this behavior to advective processes. However, the broader debate regarding whether overturning adjustment is primarily wave- or advection-dominated remains unresolved. Studies supporting wave-mediated adjustment argue that such processes are not adequately represented in low-resolution climate models. Consequently, it is possible that the mechanisms identified here may not fully generalize to higher-resolution simulations. I therefore encourage the authors to discuss this issue more explicitly and to provide a hypothesis regarding whether the reported behavior is expected to persist at increasing resolution. While the manuscript briefly acknowledges this limitation (lines 536–542), the discussion of wave-mediated adjustment and its potential implications for the weakening rate should be expanded.

Minor Comments

- line 110: These sentence is strangely constructed.
- line 411: theshold should be threshold.
- line 421: Assesssing should be corrected.

- line 421: show should be shows.
- Figure 9-11: Why use different line colors for single model results? I suggest using one line, and simply labeling that color in the legend.
- line 443: variability should be corrected.
- line 474: subploar should be corrected.
- line 532: convective must be advective here?
- line 598: deviation should be corrected.