

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

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The article, "*AMOC weakening across latitude and time in CMIP6 future scenarios*" examines the evolution of AMOC weakening across latitude and time in a set of CMIP6 models under four different future emission scenarios. The authors analyze the propagation speed of the weakening in both depth and density space, concluding that the latter may provide a more suitable early warning signal. They further find that the propagation speed of a given weakening threshold decreases as the threshold increases, with this decrease being more pronounced in low-emission scenarios than in high-emission scenarios.

The study presents interesting results with clear relevance for the current AMOC observing system. However, I have major concerns regarding the interpretation of the results and their physical and statistical robustness. Only if these concerns are adequately addressed would I consider the manuscript suitable for publication in *Ocean Science*.

Major Comments

1. The result that the weakening gradient declines with increasing percentage threshold is interesting. My concerns regarding this result are twofold. First, Fig. 9 illustrates that this behavior is not a general feature across all models. As noted by the authors, this may partly be explained by natural variability. However, since this appears to be one of the main conclusions of the manuscript, it is essential to demonstrate that the result is statistically robust. I therefore suggest repeating the analysis using the ensemble mean for each model in order to reduce the influence of internal variability.

Second, if such a statistically robust relationship indeed exists, the physical mechanism underlying the decline in weakening gradient with increasing percentage threshold should be discussed. At present, the manuscript provides no plausible explanation for this behavior, nor any suggestion for how it might be investigated. While I agree that a comprehensive analysis may be beyond the scope of the present study, the authors should at least outline a possible physical interpretation and suggest directions for future analysis, supported by clear arguments.

2. While the mechanism underlying the propagation is stated to be beyond the scope of the present study, the Discussion section should at least provide a more detailed quantitative comparison between the propagation speeds diagnosed in these models and those expected from theoretically proposed mechanisms. Such a comparison would help place the results in a clearer physical context. Lines 453–460 briefly mention this point, but no quantitative interpretation is provided.
3. The first two to three paragraphs of the Discussion and Conclusion sections substantially overlap. I suggest combining these sections into a single Summary and Discussion section to avoid repetition and improve the overall structure of the manuscript.

4. The abstract presents the results in a very formal and quantitative manner. I suggest revising it to focus less on specific numerical values and instead emphasize the main qualitative findings, their interpretation, and their implications for the AMOC observing system.

Minor Comments

- Line 29: What about the uncertainty associated with AMOC tipping? The possibility of AMOC tipping introduces a substantial model spread and should be acknowledged.
- Line 36: Such a mechanism is not exclusive to climate change forcing. See, for example, *Diagnosing the causes of AMOC slowdown in a coupled model: a cautionary tale*.
- Line 69: The study by Marotzke and Klinger (2000) should be mentioned here.
- Line 106: I do not understand why the streamfunction is estimated from the mass transport. Models that do not provide ψ typically output vo . Why not calculate the volume transport directly from vo instead of using vmo , which may introduce errors in the presence of a strongly variable density field?
- Section 2.3: How does the mean weakening found in this subset of models compare to results from earlier studies using larger ensembles (e.g. Weijer et al., 2020; Fox-Kemper et al., 2021)?
- Line 177: Does the standard deviation refer to the spread within each model ensemble?
- Line 184–188: Redundant.
- Line 195: What does “slow” mean in this context? Please quantify.
- Line 207: Does the spread in years represent inter-model spread?
- Figure 4: Why are fewer models available for the weak percentage thresholds compared to, for example, the 10% threshold?
- Line 270: This may be a transient feature. At 30°S the system may still be far from adjustment.
- Line 277: Is this behaviour consistent across most models, or is it dominated by one or a few outliers?
- Figure 6: Why use a different colour scale compared to Fig. 3? Also, why is SSP585 shown twice?
- Line 333: Why not use the abbreviations EMMM and SMMM?
- Section 3.3: The section is organised somewhat confusingly. Lines 344–356 first discuss results, and only afterwards describe the models from which these results are obtained. It would be clearer to first introduce the models used, then present the figures, and finally discuss the results they imply.

References