

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

September 2026

We would like to thank the reviewers for their second round of comments on our article.

Reviewer 1

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.

Thank you for bringing the recent preprint, Rapanague et al., to our attention. We have repeated our analysis using a constant density level based on the density of AMOC maxima at each latitude in the reference period and find that none of our conclusions would change. There is a reduction in weakening gradient as threshold increases that is greater in low emissions scenarios and less severe in high emissions scenarios. The reduction in weakening gradient is less using fixed density levels than using AMOC maxima. For example, ssp585 reduces from ~ 3.2 deg/yr to ~ 2.2 deg/yr and ~ 1.5 deg/yr using fixed density levels and AMOC maxima, respectively. There is also more variability in the weakening gradients when using a fixed density instead of the AMOC maxima. The fixed density obscures weakening north of 50N, due to the change in structure of the density streamfunction. Figures of the analysis using constant density level are included below.

We appreciate the motivation for fixed density levels, however, in this case, we don't believe that using fixed density level is the most appropriate method for quantifying the propagation of AMOC weakening under climate forcing. When measuring changes to the AMOC maxima, we are only considering one variable: changes in maximum AMOC strength. However, when measuring AMOC strength at a fixed density, we have to consider two variables: the changes in maximum AMOC strength and how much the AMOC maxima has shoaled away from the fixed density level, given that AMOC maxima shoals under climate forcing and as the ocean warms, its density reduces. The shoaling under climate forcing, which is less relevant in the constant 1950s forcing in the preprint, may vary between models and latitudes and we would prefer to control against using the AMOC maxima.

We added justification from this discussion to the methodology section of the paper (L150-154).

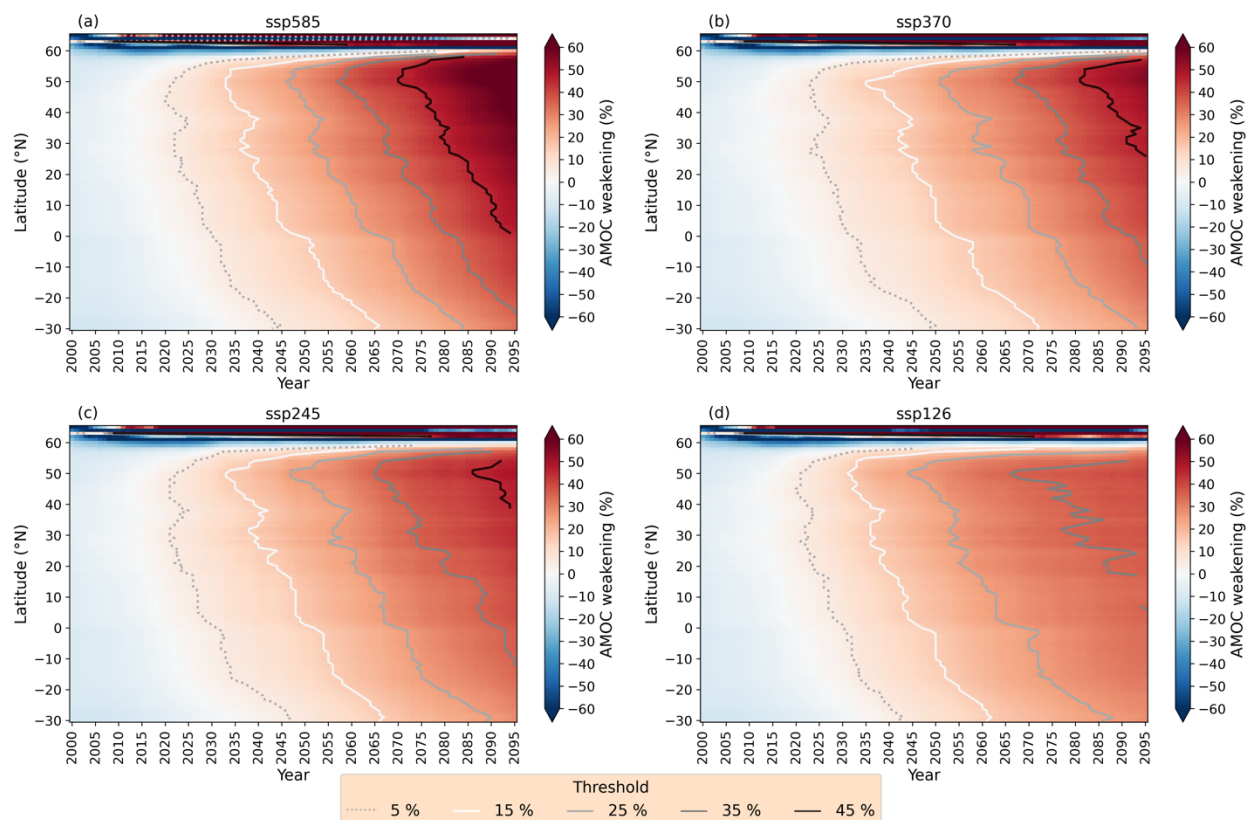


Figure A: Figure 6 equivalent.

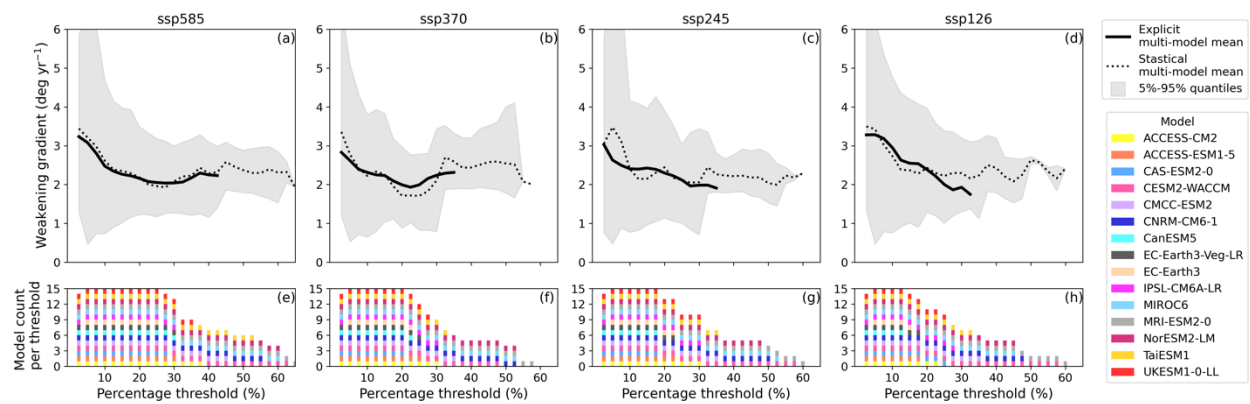


Figure B: Figure 8 equivalent.

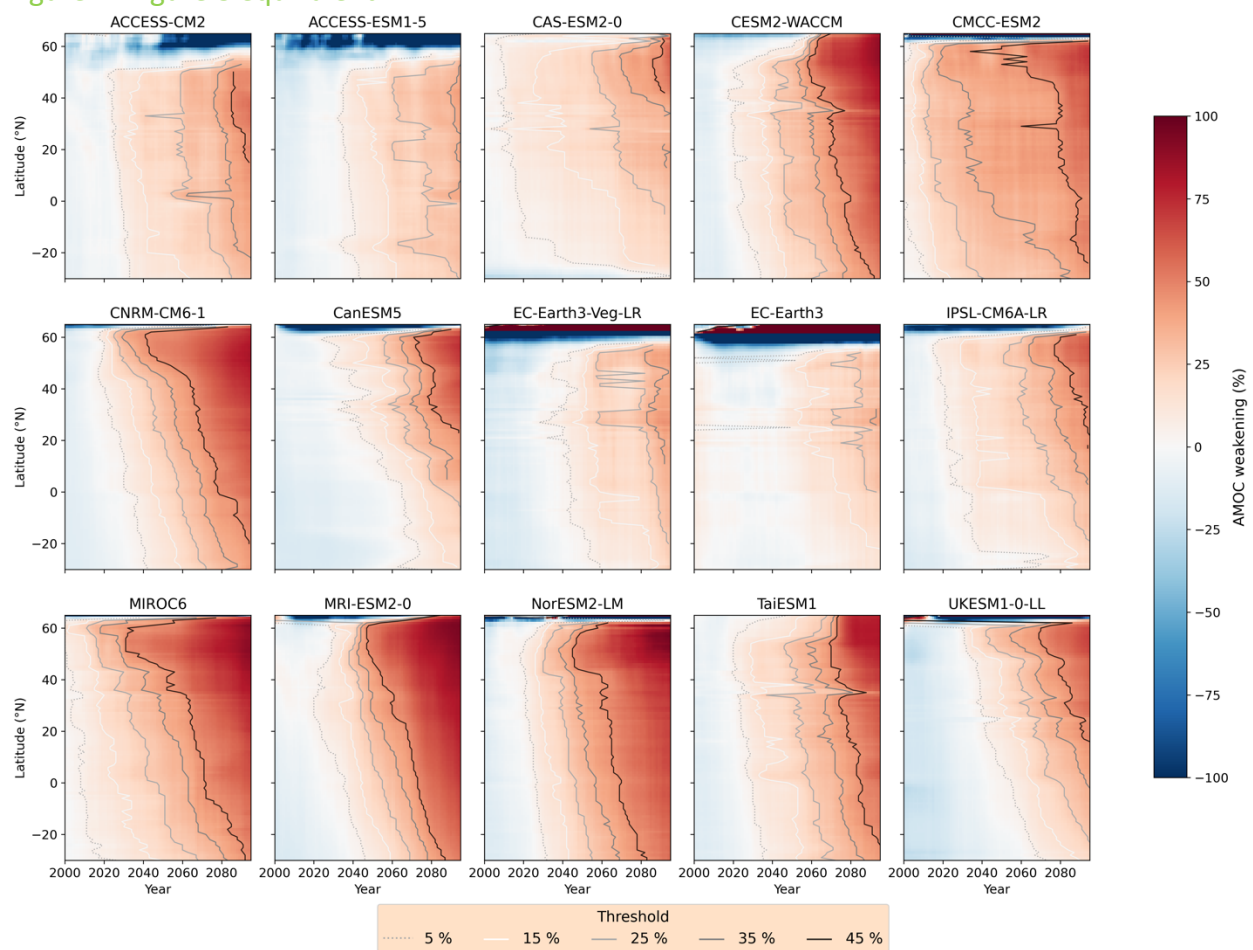


Figure C: Fixed density Hovmöller plots for individual models.

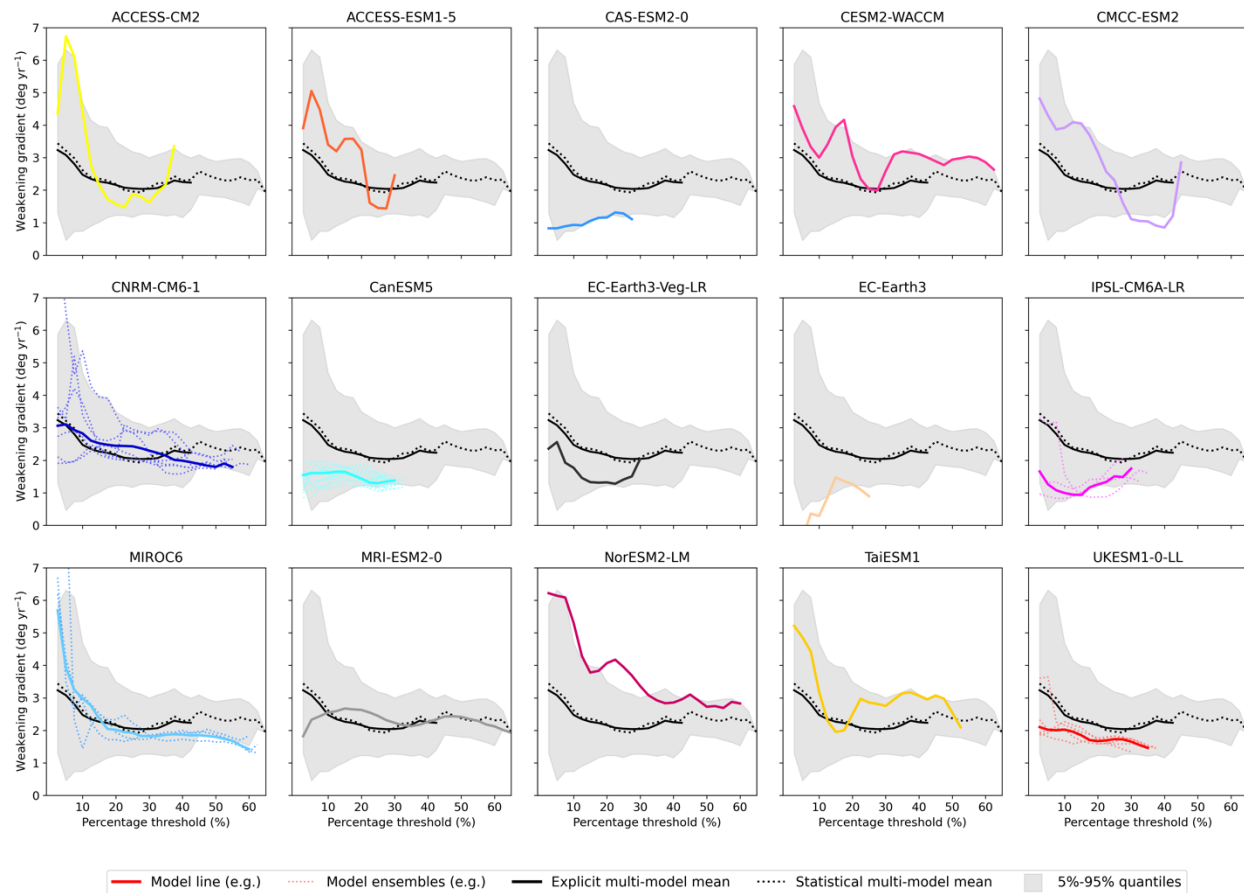


Figure D: Fixed density weakening gradients for individual models.

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.

Thank you for your comment. We have expanded our discussion of the limitations to include the lack of representation of wave-mediated processes in low resolution (L509-514). We hypothesise weakening will still be advective in higher resolution, but indeed wave mediation might mediate transport faster and experience less of a reduction that seen. A more dedicated study on the mechanism for propagating weakening gradient will be the focus of a follow up study.

Minor Comments

- line 110: These sentence is strangely constructed.

We have rephrased this sentence into two sentences to be clearer. “We also choose to use the provided density-space streamfunctions for sigma-level models. This is to avoid issues with interpolating from sigma-levels to depth-levels and back to sigma-levels.”

- line 411: theshold should be threshold.

Corrected.

- line 421: Assesssing should be corrected.

Corrected.

- line 421: show should be shows.

Corrected.

- 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.

Different colours for each model are used for consistency with Figure 8 and Figure 12.

- line 443: variablility should be corrected.

Corrected.

- line 474: subploar should be corrected.

Corrected.

- line 532: convective must be advective here?

Yes, thank you. Corrected.

- line 598: deviation should be corrected.

Corrected.

Reviewer 2 (Editor)

L40 – Remove brackets around Mecking reference.

Removed.

L125 – Appendix A is very good, but perhaps the main 2-3 sentence conclusion can be given in the text here for readers to be able to continue.

Added 3 sentences summarising Appendix A into the main text (L126-129).

Eq 3 – You are here integrating between a depth and a density. Should the lower bound not be $\rho(x,y,-H(x,y))$. Also, as given here, the units are $\text{kg} / \text{m} / \text{s}$, and not m^3/s as in Eq(1). So I’m not sure if the notation of this streamfunction equation is correct.

Thank you for this comment. We have corrected the lower bound and added $dz/d(\rho)$, the isopycnal thickness, to this equation.

In sections 2.3 and 2.4, refer to the figures that come out of this analyses.

Created a new appendix for these figures (Appendix C).

L220 – AMOC weakening begin in the north. Perhaps this informs us to help design/monitor EWS?

This is commented in the discussion. We add an additional comment at the end of this paragraph (L483-485).

Around L297: “indicates weakening of the AMOC in northern latitudes are more responsive to climate forcing than further south and that it takes longer to reach the same degree of weakening at 30°S than further it does further north.”

I had two thoughts that might be worth considering for a discussion. 1) Does this influence the idea often said that larger emissions lead to faster and more weakening? This idea now seems latitude dependent. 2) Does 30S reach the same degree of weakening as you state? I think you mean as total percentage and not in rate? Please clarify for the reader.

1) We argue that, as per section 3.2, the rate of AMOC weakening is more nuanced than larger emissions means faster weakening. For the first half of the century, ssp126 and ssp585 have the same rate of weakening. High emissions do result in more weakening by the end of the century though. The take home message is that AMOC decline originates at the high latitudes and changes further south are due to a propagation from the high latitudes and when considering AMOC weakening, we need to be aware of this propagation speed.

2) Rewritten the sentence to clarify the end of century weakening at 30S (~25%) is reached at higher latitudes sooner (2036 at 51N and 2055 at 26N).

(L305-308)

L315 – Add reference?

Signal and noise in the Atlantic Meridional Overturning Circulation at 26° N

GD McCarthy, G Hug, D Smeed, KJ Morris, B Moat

Geophysical Research Letters 52 (7), e2025GL115055

Reference added.

Section 3.3 through to the discussion is long. If some reduction in length is possible, this would be worthwhile.

We reduced the text where possible.

I think this paper could do with a two paragraph conclusion section summarising the main points (of which one is perhaps already written as the last paragraph).

We have added a short conclusion (L549-563).

Reviewer #2, major comment 1 is largely resolved. However you say that “We find that the offline calculated streamfunctions are weaker than their online counterpart and the AMOC strength is ~1-3Sv less. As our method focuses on percentage change, we find the threshold lines and onset largely agree between the online and offline versions.” Was one conclusion not that decline also depends on initial strength? Stronger AMOC declines more rapid? So then it would influence this, correct?

Thank you for your comment. You are correct. When using the offline AMOC strength instead of the online AMOC strength for the linear relationship in Figure 13, we find a greater r-value. The use of offline AMOC would more strongly support our argument. However, due to our concerns with multiple interpolations between sigma-level coordinates and z-level coordinates and the fact that none of our results meaningfully change between the use of online and offline, we elect to continue the use of online streamfunctions for these 4 models (as noted in L144-L146). At time of writing there are also data access issues with the ACCESS models which would make repeated analysis using the offline models challenging.

We again thank the reviewers for their helpful comments, which have improved our manuscript.