

Authors' response to review on "Past, Present, and Future Variability of Atlantic Meridional Overturning Circulation in CMIP6 Ensembles"

We sincerely thank the reviewers for their insightful comments and constructive suggestions, which have enabled us to clarify key aspects and enhance the overall quality of our manuscript. Below, we provide detailed responses (in blue) to each of the reviewers' comments (in black), along with a revised version of the manuscript.

Arthur Coquereau, Florian Sévellec, Thierry Huck, Joël J.-M. Hirschi, and Quentin Jamet.

Reviewer report #1 (Anonymous referee #3)

The figure enclosed in the rebuttal does not completely convince me. The area over which the radiative imbalance is calculated is placed too far south and should instead cover the deep winter mixed layers where convection takes place. A better cut-out would have been 45-70N and 60W-20E.

The authors could check whether this area shows different separation between the SSP scenarios

Second, I do not agree with their inference from this figure. There is no clear separation between red and orange lines, i.e. SSP2-4.5 and SSP5-8.5 in this figure. At least they should rephrase the last sentence of the paragraph just above the figure by "This trend could and analyses ... show hardly any separation between the SSP2-4.5 and SSP5-8.5 scenarios, but still early separation between those two and the SSP1-2.6 scenario."

We thank the reviewer for suggesting a more appropriate area for calculating the radiative forcing. We agree that the previous figure and its description could have been clearer, particularly regarding the North Atlantic average.

We have reproduced the analysis using the suggested box (45°N to 70°N, 60°W to 20°E), which better captures the deep winter mixed layers where convection takes place (see below). The new figure shows much clearer separation between scenarios in terms of local radiative forcing. While SSP2-4.5 and SSP5-8.5 were barely distinguishable in the previous figure, they now separate much more clearly and earlier in time, with a pattern more similar to the global radiative forcing. The figure has been included in the Appendix B.

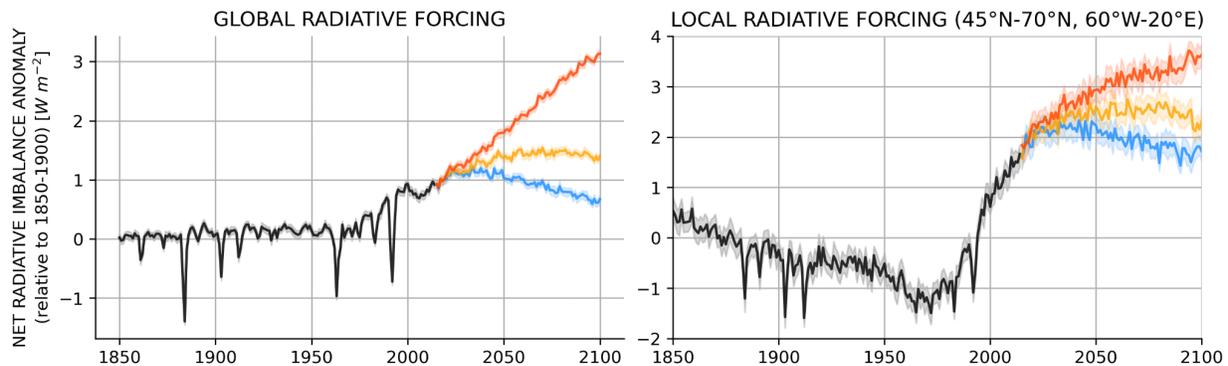


Figure B7: Evolution of the net radiative imbalance averaged across the three large-ensemble models at the global scale (left) and regionally over the North Atlantic (45°–70° N, 60° W–20° E; right). The historical period is shown in black, while future projections are shown in blue (SSP1-2.6), orange (SSP2-4.5), and red (SSP5-8.5). The multi-model mean is calculated using a bootstrapping approach, which is also used to estimate the 95% confidence interval percentiles, displayed as shaded envelopes.

The text now reads:

“Although this trend could be influenced by regional aerosol forcing (e.g., Deser et al., 2020; Menary et al., 2020; Robson et al., 2022), analyses of the local radiative imbalance over the North Atlantic (10°–60°N, 90°W–0°E) also show an early and rapid separation among scenarios (not shown).” (l. 550-552)

Reviewer report #2 (Anonymous referee #4)

Before all, I would like to apologize the fact that I did this review with much delay. Thank you to the authors for considering my comments and addressing most of them. The

manuscript has clearly improved. I still have 1 major comment, and a general comment about the level of English. Yet, given my delay, I leave to the editor the decision to let me read again or not the manuscript. Sorry again.

The line numbering refers to the version with the modifications highlighted.

- I am still not convinced by the way the authors treat intermediate period (end of historical up to 2050 roughly):

1. I am not convinced by the modifications of the text to my comment 36. The text does read as if the authors were attributing the variance peak in the 1940s to the aerosols (around line 334), while (i) I don't think aerosol forcing had really began then (I am ready to be proven wrong), and (ii) the authors acknowledge themselves later in the text (460-462) that "1900-1950 window is ... a few decades.. before the increase of the aerosol forcing. "

We thank the reviewer for highlighting this inconsistency in our discussion of aerosol forcing timing. In response to comment (i), we have revisited the literature and found that the impact of aerosol forcing on the AMOC does indeed appear quite early in the 20th century. Menary et al. (2020) show that aerosol-driven AMOC changes begin around 1930-1940, which is consistent with the variance peak we observe in our analysis.

"The first, which starts around 1940 in the larger ensembles and reaches its peak in the second half of the 20th century, is likely associated with the differences of AMOC response to aerosol forcing among models (Menary et al., 2013, 2020; Robson et al., 2022). This increase is shifted in time in small ensemble models, where it does not appear until 1970 in the smaller ones." (l. 321-324)

Regarding comment (ii), we acknowledge the confusion in our previous wording. We have revised the text to clarify that the 1900-1950 window captures the early phase of increasing aerosol forcing, preceding the period of maximum aerosol emissions in the second half of the 20th century.

"In the historical period, we select the 1900-1950 window, located a few decades after the beginning of the historical simulations (allowing for possible AMOC adjustment from the control preindustrial forcing to the historical one) and before the peak of aerosol forcing that occurred in the latter half of the 20th century." (l. 443-445)

2. Although I reckon that the manuscript is already relatively long, I think that ideally, the authors should be much more precise in their analysis of the various components of the

historical and scenario forcing, the way each forcing acts, regionally and temporally etc. It is also ok not to do that, since many points are already addressed in the manuscript, but in this case, the conclusions should clear been taken down a bit. (l. 570 of the corrected manuscript)

We thank the reviewer for raising this point regarding the balance between depth of analysis and manuscript conciseness. We acknowledge that a fully detailed decomposition of all forcing components, including their regional and temporal dynamics, would indeed provide valuable insights. However, such an in-depth analysis would significantly expand an already long manuscript and could potentially make it harder to follow for readers.

In the current version, we have sought to strike a balance by providing a general overview of the different variance components (including main effects and interactions) and their behavior, while placing particular emphasis on the three key regimes and their associated physical drivers. This approach allows us to maintain focus on the most critical aspects of our analysis without overwhelming the reader with excessive detail.

Anyway, the sentence referring to the forcing (previously line 570 in the tracked-changes version and now line 552 in the revised manuscript) has been updated regarding the improved consistency detailed in response to comment #3.

3. I remark that Reviewer#1 also had a major point of the use of the aerosol forcing to justify or not divergence before 2050. While the authors argue that “analyses of the local radiative imbalance over the North Atlantic (10°-60°N, 90°W-0°E) also show an early and rapid separation among scenarios”, I think that their figure proves that this is not true for scenarios ssp 245 and ssp585. This is an illustration of what I consider as a bit of looseness and shortcuts in the attribution of detected changes to scenarios or not. In general, these remarks relate to a lack of physical analysis to back up some of the hypothesis and conclusions. This may be ok, given the statistical advances proposed in the paper but I think it should be better acknowledged

We thank the reviewer for this important observation. As noted in our response to Reviewer #1, we agree that the description of the figure in our previous response was misleading. Following Reviewer #1's suggestion, we have recalculated the radiative forcing over a more physically appropriate domain (45°N-70°N, 60°W-20°E). The updated figure (added in Appendix B) now shows clearer and earlier separation among all three scenarios, including between SSP2-4.5 and SSP5-8.5.

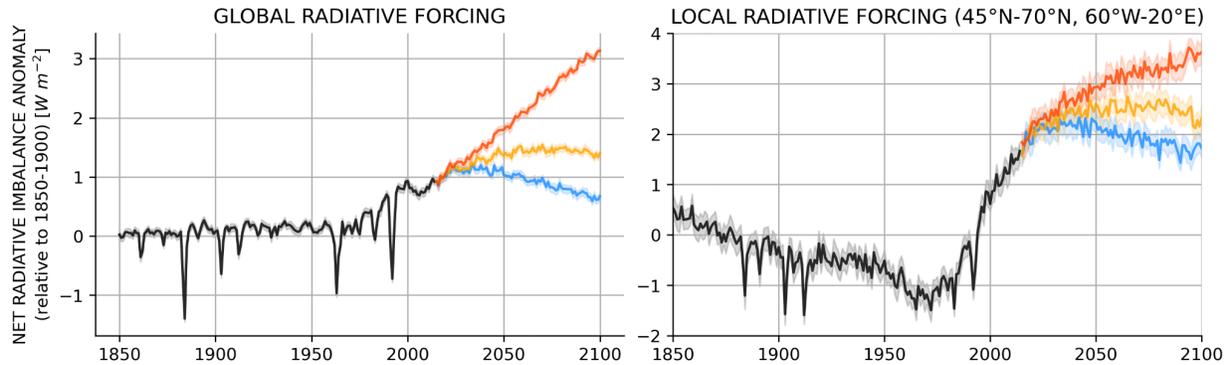


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- My second main comment still concerns the level of English language. I am not native myself, but I had the impression that in several instances, the subtlety of the reasoning and argumentation was not well reflected in the text because of too poor language level. I have suggested some changes below, the editorial team may do the rest later

We thank the reviewer for the numerous suggestions regarding the language. The suggestions below have been updated in the revised manuscript.

Specific comments

l. 7 components -> contributions ? [Updated](#)

l. 36: acknowledge the fact that nevertheless, these paleo-events are not yet fully understood? [Updated](#)

l. 44: the regional effect of aerosols should be underlined in my view [Updated](#)

I. 58: it is not an improvement of models, but rather an improvement of the protocol or of the CMIP project itself [Updated](#)

I. 78: At this stage I still do not understand which dimension you are referring to (This is defined later in section 2) [Updated](#)

I. 82: specify ensembles of what? [Updated](#)

I. 85: has changed -> changes? [Updated](#)

I. 89-90: I would rather say: In the case of the historical period, the time dimension further allows to detect.... [Updated](#)

I. 103: remove therefore [Updated](#)

I. 141: I don't think both terms "factor" and "effect" are needed here, choose one [Updated](#)

I. 165: "at the origin: do you mean "at the beginning"? [Updated](#)

I. 177: again, are both terms "effect" and "contribution" needed? [Updated](#)

I. 187: "in the paper" -> "hereafter" [Updated](#)

I. 202: "this method": please specify. Do you mean "their method"? [Updated](#)

I.208: "especially" -> "at least" [Updated](#)

I. 215 "the difficulty of capturing" -> "their difficulty to capture" [Updated](#)

I. 232: "would be accounted" -> "are accounted" [Updated](#)

I. 234: "would exceed" -> "exceeds" [Updated](#)

I. 250-251: "in the present study" goes after "analyze" [Updated](#)

I. 262 "appears" -> "can appear" [Updated](#)

I. 280: "During" -> "Over" [Updated](#)

I. 285 and following: Following one of my previous comment, the authors have changed “mixed” into “combined” in 1 instance. Yet, the verb “mix” is used in several other instances in this paragraph and I still find it misused and confusing. I would consistently change it into “combine” all along. [Updated](#)

I. 299: “While in the 21st century, the transfer occurs from...” [Updated](#)

I. 300: “I would add “that can be detected” after “variability” [Updated](#)

I. 311: the 2 references are linked (authors in common” -> I would remove “often” [Updated](#)

I. 321: “used” -> “use” [Updated](#)

I. 333 “reached” -> reaches” [Updated](#)

Around line 334: As said above, there is no reason to attribute changes to aerosols around 1940, and the signature in small ensembles appears earlier than in large ensembles. Same remark I. [This comment has been discussed above in the response to comment #1.](#)

404: why aerosols? [Updated](#)

I. 335: “due” -> “linked”? [Updated](#)

I. 337: “observed” is misleading, as nothing is observed here. “detected”? [Updated](#)

I. 349: I don’t understand why specifically the smaller ensemble and the link with the previous sentence [Updated](#)

I. 353-355: this sentence is too long, cut it in 2 [Updated](#)

I. 369 This is for example a place where I believe that specific differences among the scenarios should be discussed [Updated](#)

I. 374: “variability” -> “variance”? [Updated](#)

I. 375 “In that subsection” until the end of parag: I would remove [Updated](#)

Section 3.2.1 and elsewhere in the text: I find the term “ensemble “ a bit misleading to characterize the spread associated to R. Why not “realization”, more intuitive and consistent with R?

We thank the reviewer for this comment. In some instances, we replaced "ensemble" with "realization", for example, when discussing the R main effect, as it better captured the intended meaning. However, since "ensemble" remains the more widely recognized and established term, we retained it in broader interpretations of the results. Specifically, we use "ensemble variability", a standard expression, rather than the less conventional "realization variability" for clarity.

I. 408: add “the” before “number” [Updated](#)

I. 411: into “the details” [Updated](#)

I. 442-443: “Indeed...”: this point has already been made several times [Updated](#)

I. 444: I don’t think the “However” is justified” [Updated](#)

I. 445: What are these small perturbations, how small are they exactly, due to which forcing etc. Here, we have a very wavy argument [Updated](#)

I. 446: “amplification by the chaotic nature of the system”: similarly, this is not shown and not quantified. It is very hypothetical and approximative in my view [Updated](#)

Also I am wondering here whether the authors have taken into account the fact that the size of the ensembles differ from historical to scenario periods. Sorry if I missed that.

The ensemble size itself remains unchanged. However, the scenarios behave as if the ensemble were larger because their divergence is driven not by the external forcing, but by internal variability (see lines 422 to 432)

I. 460-462: This is inconsistent with the argument of aerosol influence on the small ensembles variance in 1940 that was given further up

[We have addressed the reviewer’s point in our response to Comment #1.](#)

I. 468: remove “therefore” [Updated](#)

I. 483: remove “therefore” [Updated](#)

I. 484 “In small ensembles” should come at the beginning of the sentence [Updated](#)

I. 493: in my view, the first “variability” should be “variance” and the second one “spread” [Updated](#)

I. 495-496: do you mean here that the historical forcings were not anthropogenic? But what about your argument on the aerosols then? Please clarify [Updated](#)

I. 502: “same” as what”? The 2-step increase is not clear to me , please clarify [Updated](#)

I. 503-505: please add temporal ranges to (i) and (ii) [Updated](#)

I. 505: already said several times, consider remove to condense a bit the text [Updated](#)

I. 512: any physical clues on the origin of the different AMOC sensitivities? [Updated](#)

I. 535 “terms” [Updated](#)

I. 538 “aims at representing” -> “represents” [Updated](#)

I. 550: isn't it worth insisting on the fact that physically, the T effect IS DUE TO anthropogenic forcing? [Updated](#)

I. 559-560: Similarly, this does not mean that the effect of external forcing is absent, but rather that all scenarios are equivalent. I have the impression this could be misinterpreted. I suggest to clarify. [Updated](#)

I. 570: as said above in my general comments, I am not sure this is fully true. Also, physical processes of AMOC decline and typical timescale of AMOC response should be considered

[We have addressed the reviewer's point in our response to Comment #3 and Reviewer #1's comment.](#)

I. 582: “facilitates” [Updated](#)