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Title: Changing European Hydroclimate under a Collapsed AMOC in the Community Earth System Model

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Point-by-point reply to reviewer

July 28, 2025

We thank the reviewer for their careful reading and for the useful comments on the manuscript.

In the manuscript Changing European Hydroclimate under a Collapsed AMOC in the Community Earth System Model, eight model simulations are used to assess the impact of the AMOC strength and two different Representative Concentration Pathways (RCP4.5 and RCP8.5) on the European hydroclimate. The authors reconstruct daily potential evapotranspiration rates (PET) from CESM variables stored at monthly frequency. This allows them to diagnose the differences in dry season length and intensity under the different scenarios, as well as determining the dominant terms in the PET equation that are most sensitive to the changing climatic conditions in the eight simulations. The authors find that compared to the pre-industrial conditions (PI), nearly all scenarios show a general reduction in the potential precipitation deficit (PPD) over Europe, resulting in generally drier climate and an increase in drought extremes.

The eight scenarios allow for a comparison between the effects of the AMOC shutdown and the anthropogenic climate change on the European hydroclimate. Under PI conditions with reduced AMOC strength, the precipitation and PET rates decrease, and the latter dominates, resulting in dry conditions. In the RCP8.5 scenarios, the precipitation is reduced as well, but the PET rates increase, and the two effects combine, resulting in more intense and longer dry periods than in the AMOC off scenarios. Lastly, the RCP4.5 simulations provide the most important comparison between the European climate states under increased radiative fluxes with and without

collapsed AMOC. These results show how AMOC collapse intensifies the dry conditions in Europe in the warming climate. The RCP4.5 scenario with collapsed AMOC shows significantly longer and more intense dry season in Southern and Central Europe compared to the AMOC on case, as well as significantly increased probability for drought.

The study presents a meaningful and important contribution to understanding the risks associated with the combined effect of climate change and AMOC tipping on the hydroclimate. As such, the article is a relevant addition to the Hydrology and Earth System Sciences (HESS) Journal, both in terms of scientific content and result significance.

General Comments

The manuscript is well organized and the analysis is conducted to a high standard. Analyzing the PPD and its decomposition into precipitation and PET rates reveals the pathways driving changes in European hydroclimate across scenarios. The changes in precipitation are linked to mean sea-level pressures, and it is shown that the dominant factor affecting the PET is temperature. It is furthermore demonstrated that reconstructing PET^{day} is important for understanding the scenario impact, as the E-P signal is weaker than P-PET, since evaporation in the model is limited by the precipitation rate.

- 1. The reconstruction of PET^{day} and PET^{month} in Appendix A is well justified and described. However, comparing the RMSE of end-year PPD computed from true and reconstructed PET would be a more meaningful measure of the biases introduced with the upscaling method. Since the CESM PPD biases are significant compared to ERA5, it would be useful to know to which extent the PET reconstruction may be contributing to these biases, if at all.*

Author's reply:

Following the reviewer's suggestion, we have determined the seasonally-integrated PET over the growing season as:

$$PET^{\text{end}} = \int_{1 \text{ Apr}}^{30 \text{ Sep}} PET(t') dt',$$

and compared this to the seasonally-integrated PET^{day} and PET^{month} . We also determined the yearly-integrated PET, but the results are most interesting over the growing season as PET rates are then the largest. The root-mean-square error in PET^{end} is less than 30 mm over most European land surfaces and the end of growing season, boiling down to an error of a few percents as the seasonally-integrated PET is typically more than 550 mm. The introduced bias is limited and demonstrates that the upscaling method is accurate for our purposes. Note that we conduct the analysis on PET and not on PPD (as suggested by the reviewer), since the daily-averaged precipitation rates are not affected by the upscaling method and PPD biases are introduced through PET^{day} or PET^{month} .

Changes in manuscript:

This additional analysis will be included and discussed in the Appendix. The seasonally-integrated PET differences will be added to Figure A1. We will also add the PPD^{day} for ERA5 in Figure 2b.

2. *The communication, especially in the results section, is lacking clarity and should be improved. First of all, the significance of the k-means analysis is unclear. The authors should state clearly the significance of this analysis and how it fits with the previous results.*

Author’s reply:

We agree with the reviewer that the text was difficult to follow. In particular, the simulation names were not intuitive and disrupted the flow of the narrative (as also noted in point #4 of the review). The k -means clustering methodology should be introduced more clearly.

Changes in manuscript:

We will revise the manuscript to improve clarity and provide a clearer introduction to the k -means clustering methodology in both the Methods and Results sections.

3. *More importantly, it takes a long time to realize that the focus for the reader should be the difference between the two RCP4.5 scenarios, where the role of the AMOC collapse in contributing to the hydroclimate changes under increased radiative forcing can be identified. This*

important result drowns in the overly detailed text of the section, and is also not clearly visually indicated on the figures. The manuscript clarity would improve if both the results and the figures were streamlined to better express the focus on the differences between the results from the two RCP4.5 scenarios.

Author’s reply:

The reviewer is correct here, the most interesting comparison is made between the two RCP4.5 scenarios. This should be more emphasised throughout the manuscript.

Changes in manuscript:

We will rewrite the main text accordingly and put more emphasis on the two RCP4.5 results. We will add a statement in the Methods (section 2.1) when the CESM simulations are introduced.

4. *The naming convention for the different scenarios greatly contributes to the lack of clarity and transparency in the text and on the figures. Instead of the cumbersome ‘AMOC on ($F_H = 0.18$)’, the authors could consider simply writing ‘PI LOW’ or ‘ON LOW’ to indicate the lower rate of freshwater hosing. It is not crucial for the reader to know the exact F_H value throughout the text. It is also not obvious how the simulations in the manuscript are branched off from the simulations with $F_H = 0.66$ Sv (ll. 65-69). This can be clarified. Additionally, the figure titles and labels are very small, and increasing the font size would improve the ease of understanding.*

Author’s reply:

The naming convention was indeed hard to follow and we agree with the reviewer that the exact F_H value is not needed for the reader. A different naming convention should streamline the main text and we propose to label the simulations as follows: e.g., $\text{PI}_{18}^{\text{on}}$. The simulation name refers to the radiative forcing conditions, where the subscript indicates the F_H strength (in units of $\times 10^{-2}$ Sv) and the superscript whether the AMOC is in its strong northward overturning state (i.e. ‘on’) or in its collapsed state (i.e., ‘off’).

Changes in manuscript:

We will incorporate the different naming convention in the revision, the relevant text and figures will be changed accordingly. We will also display the figures, figure captions and labels at a greater size where possible.

5. *Lastly, the discussion about model biases is lacking. The assumption that CESM biases with respect to ERA5 stay constant is highly unlikely to hold (ll. 111-113). It would be great if the authors could include more discussion about how the significant CESM biases may affect the results.*

Author's reply:

We agree with the reviewer that this assumption is not very likely, given that climate models are tuned with an AMOC on background state. We do expect, however, that the AMOC-induced changes are (much) larger than variations in climate model biases, but this cannot be tested.

Changes in manuscript:

In the revised discussion, we will expand the discussion on the different CESM biases.

Minor Comments

1. *It is surprising that the Netherlands is chosen as the reference location to show the local changes in PPD. The authors show that the European hydroclimate is profoundly affected by the different scenarios, but choose to visualize the location that is among one of the least impacted. Would it not make sense to show the local differences somewhere where the signal is stronger? This modeling choice should be more clearly justified.*

Author's reply:

We considered this location as it can be compared with the measurement station 'De Bilt'. This location is also of interest as it is situated

in Northwestern Europe, a region that shows relatively large temperature responses under a collapsed AMOC. Two other locations (Sweden and Spain) were presented in Figure A3.

Changes in manuscript:

We will revise this and provide a clearer justification for the location used.

2. *Equations are not correctly punctuated throughout the manuscript.*

Author's reply:

Agreed.

Changes in manuscript:

Will be corrected.

3. *l. 7: 'depends' → 'depend'*

Author's reply:

Agreed.

Changes in manuscript:

Will be corrected.

4. *Table 1: Should be $m^3 s^{-1}$ not $m s^{-1}$; 'statistical equilibrium' should be defined somewhere.*

Author's reply:

Agreed. A statistical equilibrium is characterised by a stable climate (with time-invariant statistics) and any remaining model drift is much smaller than the internal climate variability.

Changes in manuscript:

Will be corrected and we add a definition of the statistical equilibrium in the Methods.

5. l. 18: *What is meant by a 'major' climate tipping point? The Armstrong McKay paper uses the term 'global tipping point', and this is rigorously defined. Are you using a different term for a reason, and if so it should be defined.*

Author's reply:

The reviewer is correct here.

Changes in manuscript:

This sentence will be rewritten in the revision.

6. l. 65: *'indirectly' and 'directly' have opposite meanings; which is it?*

Author's reply:

The description of the different climate model simulations was confusing. We will also use a different naming convention to streamline the paper and update the Methods.

Changes in manuscript:

We will rewrite these sentences in the revision.

7. l. 88: *'logarithmic' → 'logarithmic'*

Author's reply:

Agreed.

Changes in manuscript:

Will be corrected.

8. l. 130: *'the PPD' → 'the negative PPD'*

Author's reply:

Agreed.

Changes in manuscript:

Will be incorporated as suggested.

9. *l. 144: 'eigh' → 'eight'*

Author's reply:

Agreed.

Changes in manuscript:

Will be corrected.

10. *l. 165: 'decreases which' → 'decreases, which'*

Author's reply:

Agreed.

Changes in manuscript:

Will be corrected.