This is my first review of the manuscript *"Global and Regional Hydroclimatic Responses to Alternative Global Reforestation Pathways"* from Mortezapour et al., which investigates the long-term hydroclimatic impacts of alternative large-scale reforestation pathways using one single Earth System Model simulations. The study addresses a timely and highly relevant topic, providing valuable insights into both local and remote climate responses to reforestation under different land-use scenarios and extending the analysis beyond the 21st century.

Overall, I find the manuscript interesting and potentially suitable for publication after the authors address the major and minor comments listed below. In particular, I believe the manuscript would benefit from (i) a more explicit assessment of uncertainty and internal variability, (ii) a clearer justification and characterization of the "reversed" reforestation scenario, and (iii) stronger quantitative support for several proposed physical mechanisms.

The comments below are intended to help strengthen the robustness, clarity, and overall impact of the study.

Introduction

Line 84: Please clarify the rationale for focusing on SSP1-2.6. If the intention was to minimize the influence of anthropogenic forcing and better isolate the effects of reforestation, this should be explicitly stated and discussed, including the implications for the generalizability of the results. The simulated response still occurs within a climate system already affected by low anthropogenic greenhouse-gas forcing. To what extent could the reported reforestation effects depend on the SSP1-2.6 background climate state? Could anthropogenic forcing amplify, attenuate, or otherwise modify the simulated reforestation response or viceversa? At least, a discussion of this potential limitation would help assess the broader applicability of the results.

Missing literature in the Introduction:

Fahrenbach, N.L.S., De Hertog, S.J., Jäger, F. *et al.* Reforestation scenarios shape global and regional temperature outcomes. *Commun Earth Environ* **7**, 204 (2026).

It worth highlighting the role of regreening and afforestation in semiarid regions, for instance this review, which also looks at Sahel

Abdel Nassirou Yahaya Seydou *et al* 2025 *Environ. Res. Lett.* **20** 073001

And ongoing reforestation initiatives:

Ingrosso R and Pausata F S R 2024 Contrasting consequences of the great green wall: easing aridity while increasing heat extremes One Earth 7 455–72

Methods:

Main comment

The construction of the "reversed" scenario requires further clarification and, in my opinion, represents a key methodological issue. The manuscript states that forest cover is restored on lands converted to cropland after 1850 based on LUH data. However, cropland expansion could not be exclusively occurred at the expense of forests, but also through other natural ecosystems. It is therefore unclear whether the "reversed" scenario truly represents a restoration of historical forest cover or whether it also includes substantial afforestation of naturally non-forested ecosystems. Since this scenario constitutes one of the central experiments of the study, the authors should better document how the original land cover was determined and discuss the implications of this assumption for the simulated climate and hydrological responses. Ideally, a quantitative breakdown of the land-cover transitions involved in the reversed scenario should be provided.

Lines 109-115: The construction of the "reversed" scenario requires further clarification. The manuscript states that forest cover is restored on all lands converted to cropland after 1850 based on LUH data. However, cropland expansion has not exclusively occurred at the expense of forests. Could the authors clarify how the pre-agricultural land-cover type was determined? Is the assumption that all cropland expansion since 1850 originated from forested land justified?

Figure 1: please, improve the readability of the legend (just put one, maybe outside the maps) and the colorbar labels (they are too small). I would write percentage change as some areas show a decrease.

Lines 132–137: More information is needed regarding the perturbations applied to generate the ensemble members. The authors should also justify why five ensemble members are deemed sufficient to isolate the forced hydroclimatic response from background internal variability.

Results:

Major points:

- Several physical interpretations are presented with a level of confidence that is not always supported by the diagnostics shown. Additional quantitative evidence is needed to substantiate the proposed mechanisms linking reforestation to temperature, soil moisture, circulation, and ITCZ changes.

- An estimate of uncertainty is needed in Figure 4,5,6. But, more in general, the use of an ensemble is motivated as a means to sample internal variability and isolate the forced response. However, the manuscript presents almost exclusively ensemble-mean results, with little information on ensemble spread or uncertainty. Given the strong internal variability affecting several hydroclimatic variables, it would be valuable to provide a more explicit assessment of ensemble robustness (e.g., ensemble spread, confidence intervals, signal-to-noise ratios, or member agreement), particularly for the regional analyses.

Line 157: The statement "widespread cooling" appears somewhat overstated. While a relatively widespread cooling signal is evident over South America, the cooling response in Central Africa and Southeast Asia appears more spatially localized and heterogeneous. Moreover, a slight warming signal is observed across parts of the Sahara/North Africa region for the sustainable scenario.

Figure 2: The use of a continuous color scale may also make it difficult to assess the spatial extent of the signal. Consider using discretized color bar intervals and/or providing additional quantitative information to better support the characterization of the cooling as widespread.

I think that adding a bar plots with temperatures and cloud cover changes with associated uncertainties (in alternative boxplots) across the 7 reforested areas could be useful too as some changes are very local.

Lines 160-161: The results are consistent with evaporative/cloud-driven cooling in low latitudes and albedo-driven warming at high latitudes, but these mechanisms are still not quantified at this stage. Consider replacing "highlight" with a more cautious term such as "suggest".

Lines 165-169: The authors should better explain why local responses appear stronger in the sustainable scenario, explicitly relating these differences to the different density of reforestation in the two experiments. In addition, the discussion of the underlying energy-balance mechanisms remains largely qualitative. A more quantitative description of the main energy-budget contributions would be helpful, particularly since these fluxes are already presented in Fig. S3. I suggest adding a few sentences summarizing their relative roles in the cooling or warming effects.

Line 169: Fig. S3 is mentioned in the text before Figure S2

Figure S3: I do not get the point to not use the same scale for latent heat fluxes.

Hydrological cycles

Lines 205–209: In contrast to the temperature response discussed above, the reversed scenario appears to produce stronger but more spatially localized hydrological anomalies. This difference between the thermal and hydrological responses is interesting and deserves further discussion. The authors should explain why the reversed reforestation pattern leads to a more localized hydrological response despite producing a more spatially extensive temperature signal.

Figure 3: The current continuous color scale tends to mask weaker anomalies. A non-linear color normalization (or discretized contour intervals) could help highlight small but potentially meaningful changes.

Lines 230–238:

Looking at both the annual maps and the zonal means, I do not see clear evidence of a common pattern of modest soil-moisture reduction across subtropical regions, except perhaps in parts of the Southern Hemisphere under the reversed scenario. Could the authors clarify how this conclusion was derived and specify the regions supporting this interpretation? Conversely, a noticeable increase in soil moisture is present over parts of North America, but this feature is not discussed. The seasonal responses shown in Fig. S6 do not appear to be uniformly characterized by drying.

Furthermore, the attribution of the soil-moisture reductions to increased evaporative demand under warmer conditions is plausible, but it is not fully supported by the analyses presented. In particular, it is not clear how this explanation reconciles with regions such as North America, where soil moisture increases despite warming. Soil-moisture changes may also result from enhanced transpiration and other hydrological adjustments associated with reforestation, as authors argue before. Please provide additional evidence supporting the proposed mechanism or adopt a more cautious interpretation.

Lines 232-234: Indication of the reference figure is missing. I think authors are referring to Figure S6, but it should be Figure S4. Please, be consistent with the order of the mention in the main text.

Lines 242-243: The figure reference appears incorrect. Figure S4 shows seasonal runoff changes, whereas the text is discussing precipitation changes. Please check and correct the figure citation.

Lines 240–243: This paragraph would benefit from substantial clarification. The discussion starts by describing responses outside the tropics but then abruptly shifts to a narrow tropical precipitation increase. The logical connection between these statements is unclear. Furthermore, it is not obvious why the precipitation changes between ~10°S and 10°N should be interpreted as remote effects, given that part of the reforestation occurs within tropical regions. Please consider rewriting this paragraph in a clearer and more coherent way, explicitly distinguishing local and remote responses and explaining the physical interpretation of the reported anomalies.

Line 244: “There is little evidence of ITCZ displacement or hemispheric asymmetry in the precipitation field (Fig. S4).”

Please verify the figure reference, as Figure S4 appears to show runoff anomalies rather than precipitation. In addition, the statement that there is little evidence of an ITCZ displacement could be fully substantiated by the analyses presented. Could the authors provide the climatological ITCZ position (e.g., as contours or an additional diagnostic) to better support this conclusion?

Line 246: “and reduced rainfall across 0-10°N”

The statement “reduced rainfall across 0–10°N” is difficult to identify from the zonal mean shown. Could the authors clarify which regions contribute to this signal and provide additional evidence supporting this interpretation?

Figure S5: This is a nice figure. Please, avoid overlapping of legend with bars. You can add a single bigger legend in the empty part of the plot.

Line 255 and Figure 4: Since the authors explicitly introduce the terrestrial water budget equation, it would be useful to show the resulting water-budget terms directly in Figure 4, rather than only its individual components. This would provide a more integrated view of the hydrological response to reforestation and help the reader assess the overall changes in water availability. The discussion should also focus more on the resulting water-budget changes rather than primarily on the individual contributions

Lines 265–267: Looking at Figure 4, negative P−ET anomalies also appear in South Asia and Europe under the reversed scenario. Please clarify this statement.

Figure S7: I wonder why the authors diagnose the AMOC using the overturning streamfunction, while the Hadley circulation is not presented using the corresponding atmospheric mass streamfunction. The atmospheric mass streamfunction is commonly used for assessing changes in the intensity, width, and latitudinal position of the Hadley cell (e.g., Lionello et al., 2024). Presenting the Hadley circulation in terms of the streamfunction would allow a more robust assessment of the claimed circulation changes.

Furthermore, since the discussion increasingly relies on changes in the Hadley circulation, ITCZ position, and AMOC to explain the long-term hydroclimatic response, I strongly suggest moving Figure S7 to the main manuscript. Please, increase the size of the ticks and ticklabels.

Lionello, P., D'Agostino, R., Ferreira, D., Nguyen, H., & Singh, M. S. (2024). The Hadley circulation in a changing climate. Ann NY Acad Sci., 1534, 69–93. https://doi.org/10.1111/nyas.15114

Lines 319–320: The manuscript states that the Hadley cell undergoes both a southward displacement and a broadening of approximately 5–10°. While a southward shift may be consistent with the proposed ITCZ adjustment, the physical basis for the broadening is not discussed. Please explain in more detail how the broadening was diagnosed and provide a physical interpretation of this response.

Long-term effects: If I correctly interpret the results, the warming response simulated over parts of the middle and high latitudes around 2100 is followed by a reversal of the signal after 2100. This appears to be one of the most interesting findings of the study, yet it is not sufficiently highlighted or discussed in the manuscript. Please clarify this transition and its underlying mechanisms. In addition, I strongly suggest including a spatial map of the long-term temperature response, analogous to Figure 2. Such a figure would facilitate the interpretation of the reported changes and help identify the regions contributing to the apparent reversal of the signal.

Figure 5: For consistency with the previous figures, consider showing long-term changes relative to the 2100 state in addition to the current representation. This would facilitate the comparison between near-term and long-term responses.

Discussion

The Discussion is generally well structured and representative of the main findings, policy implications, and limitations of the study. However, the elements of novelty are not sufficiently emphasized. I encourage the authors to more clearly articulate the key scientific advances provided by this work relative to the existing literature