Comments on "Mechanistic insights into tropical circulation and hydroclimate responses to future forest cover change" by Fahrenbach et al.

The manuscript presents an in-depth analysis of the biophysical effects of land-use changes, based on simulations conducted with several Earth System Models (ESMs) participating in CMIP6-LUMIP. The results presented in the main text and supplementary material provide valuable insights into model responses to (mostly) increases in forest fraction. These increases result from either more intensive afforestation or reduced (avoided) deforestation under scenario SSP1 compared to SSP3, with the largest changes occurring in tropical Africa. In addition to the modeled changes in water fluxes and other key variables, the study computes several metrics designed to help understand the mechanisms driving changes in the surface water balance (P − ET).

We thank the reviewer for their comments and helpful suggestions. We have uploaded a revised version as well as a version with track changes. Below, we address each of the comments (original comments in black and answers in blue). We identify 4 main points from the "main comments" section of the review and respond to them individually at the end.

<u>Main comments:</u>

As noted, the study is comprehensive and, based on the model intercomparison, provides clear conclusions regarding robust changes in precipitation (P), evapotranspiration (ET), and consequently P − ET (Conclusion 1), as well as on the independence of land use-induced effects from the background climate (Conclusion 2; note that it seems unusual not to include a figure in the main text to support this conclusion).

However, regarding the mechanisms of change (Conclusion 3) — where this study invests more effort and could be more innovative — the authors, in my view, overcomplicate the analysis, overlook well-known causal chains, and fail to provide a credible explanation. Understanding the biophysical effects of land cover changes is clearly not straightforward. The change in a given variable depends on (1) the direct impact of surface forcing (i.e., changes in land surface properties), which can involve various processes (e.g., changes in radiative or turbulent fluxes that alter the surface energy balance), and (2) the atmospheric responses to (1). The atmospheric response is key, as it feeds back onto surface variables, either amplifying or damping the initial effect (e.g., changes in water recycling), and can export the impact beyond the region initially perturbed. The resulting net effect of, for example, afforestation, depends largely on the region, climate, and the spatial scale of the modified area, among other factors.

Mechanisms of change may be analyzed from the different optics (more typically modification in water or energy budgets either at the surface or the atmosphere). This paper focuses on the atmospheric water balance, with some useful simplification and decomposition of it terms.

Starting the results description, it reads (lines 211-212): "This study seeks to identify the mechanisms driving the Δ(P −E) pattern over the tropics from a dynamics perspective and to reconcile the apparent mismatch between the tropical Δ(P − E) and ΔNEI". It is not clear what the "apparent mismatch" refers to. Figure 2 shows a clear (and expected) response to tropical afforestation: increased ET and concomitant surface cooling. In turn, this change supplies moisture and latent heat to the atmosphere (Figures S4 and S10). Figure S4 also shows that the increase in NEI is primarily due to latent heating, partially offset by other radiative effects. Why should we expect a different result in this case?

In several parts of Section 3, it is stated that dynamic effects in the lower troposphere dominate or explain the changes in P − ET (e.g., lines 238–239, 248, 293–294, 318–319), leading to Conclusion 3. As noted earlier, the atmospheric response is indeed key, but it does not explain the primary response of the models to tropical afforestation—namely, the increase in ET (leading to the reduction in P − ET). As shown by numerous previous studies—many of which are cited in the introduction—this increase in ET is a direct consequence of changes in surface properties such as increased LAI, canopy conductance, and turbulence. This pattern clearly dominates in this set of simulations.

This response is clearer during the dry season, as observed in central-southern Africa during the austral winter, where the change in P − E corresponds almost entirely to ΔET (Fig. 3). Naturally, a change rooted at the surface is then transmitted to the atmosphere, which can be analyzed through the water budget. In this case, increased ET leads to more humid air (Fig. S10), changes in atmospheric motion and moisture convergence, as illustrated by the omega approximation (Fig. 3f). However, this does not imply that changes in vertical motion and regional circulation are the primary causes of changes in P − ET, as the authors suggest; rather, these are atmospheric responses to surface forcing. I agree that the mechanisms discussed in the paper are relevant—particularly for explaining changes in P, when present—which may, in turn, modulate ΔET, but the explanation and conclusions should carefully follow a consistent causal chain and avoid reversing it.

Another interpretation that seem at least partially incorrect, yet presented as "true" throughout the paper, including in the conclusions and both abstracts, is that the reduction in (near-)surface wind is due to the drag effect of increased surface roughness. In contrast to the previous case, here the authors attribute an atmospheric response to afforestation entirely to a change in a surface property (i.e., roughness), without providing convincing evidence. While this should be a contributing factor, other well-known mechanisms could also contribute to—or even primarily drive—this response. One common mechanism involves temperature-induced changes in regional (monsoonal) circulation, which is completely overlooked in this case, despite all relevant indicators being present: a significant surface cooling and a concomitant sea-level pressure increase in central Africa (Fig. S10). Given that the mean pressure gradient and wind are directed toward the interior of the continent (Fig. 5), the resulting pressure increase would be expected to weaken the monsoonal circulation. The change in wind stress is not definitive evidence of the proposed mechanism, as it may instead result from changes in the low-level circulation. Moreover, the authors do not specify how wind stress was calculated.

These main comments affect a core conclusion of the paper, so the recommendation is for major revisions. However, all of the issues relate to the interpretation of results, many of which could be addressed through a re-assessment of the existing analyses.

We summarise the reviewer's main comments as follows and address them individually below:

1. The reviewer suggests including a figure in the main text to support Conclusion 2 regarding the independence of land use-induced effects from the background climate (paragraph 1).
2. The reviewer seeks clarification on the "apparent mismatch" between NEI and $\Delta(P - E)$ mentioned in the introduction and questions the expectation of a different result given the observed responses to tropical afforestation (paragraph 4).
3. The reviewer argues that the study primarily focuses on atmospheric dynamics as the primary driver of changes in P − ET (Conclusion 3). They assert that the increase in ET, a direct consequence of changes in surface properties, is the primary response to tropical afforestation and subsequently drives the reduction in P − ET (paragraphs 5-6).
4. The reviewer proposes an alternative or additional mechanism for the reduction in near-surface winds, suggesting that temperature-induced changes in regional (monsoonal) circulation, linked to surface cooling and pressure increases, should be considered alongside the drag effect of increased surface roughness (paragraph 7).
5. The reviewer requests clarification on the calculation of wind stress.

Regarding main comment 1: We understand the reviewer's comment but decided not to include Figure S3 in the main text since it shows very similar changes to Figure 2, which is included in the main text. In other words, there is minimal additional quantitative information in Figure S3 that isn't already included in Figure 2. We believe this is a good use case for a supplemental figure.

Regarding main comment 2: We appreciate the reviewer pointing out the lack of clarity regarding the "apparent mismatch" between the simulated $\Delta(P - E)$ and ΔNEI. Our statement refers to the expectation, based on established atmospheric dynamics, that an increase in Net Energy Input (NEI) over the tropics would typically drive a strengthening of the Hadley circulation and an overall increase in net precipitation (P−E) within the inner tropics. We include below a schematic representation of the moisture and energy transport in the Hadley circulation (Figure R1).

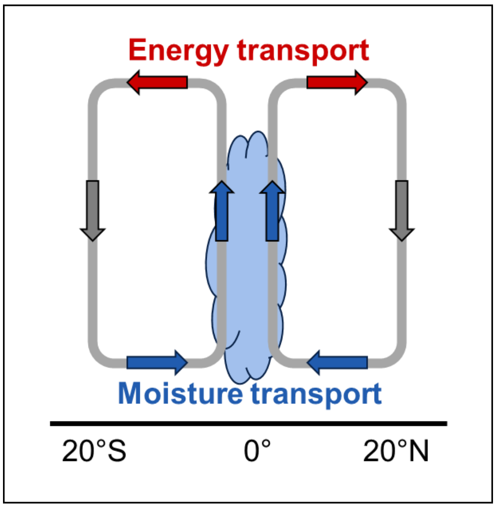

Fig. R1: Schematic illustration of the energy transport (red arrows) and moisture transport (blue arrows) in the overturning circulation in the tropics.

However, our simulations of tropical afforestation show an *increase* in NEI (primarily due to enhanced latent heat flux from the afforested areas) alongside a *reduction* in P−E in large areas of the inner tropics. This is the "apparent mismatch" we aimed to reconcile. While the southward shift and contraction of the ITCZ we observed are consistent with energetic frameworks linking NEI and ITCZ position (e.g. Byrne and Schneider 2016), as mentioned on Lines 208-210, the overall drying (P−E reduction) is not the dynamically expected response to a positive NEI anomaly in the tropics.

The reviewer correctly points out the increased ET and surface cooling, which supply moisture and latent heat to the atmosphere, contributing to the positive NEI. Our analysis delves into why this increased energy input does *not* translate into increased net precipitation in the core tropical region in our simulations, focusing on the role of altered moisture convergence patterns despite the enhanced energy.

To better clarify this, we have edited the text in the result section as follows (L. 203-210): "However, this response seems counterintuitive from an atmospheric dynamics perspective: Despite a simulated increase in NEI (given by the sum of the surface latent heat, surface sensible heat and net radiative energy into the atmospheric column; Eq. 6) over the tropics due to afforestation (Fig. 2e, S4), we observe an overall reduction in net precipitation. Based on atmospheric dynamics, we would expect that an increase in tropical NEI implies a strengthening of the overturning circulation and an increase in net precipitation throughout the inner tropics. While the southward shift and contraction towards the equator of the ITCZ are consistent with energetic frameworks based on NEI (Byrne and Schneider, 2014; Byrne and Schneider, 2016), the overall reduction of P−E is not."

Additionally, we changed L. 218-219 to "This study thus seeks to identify the mechanisms driving the Δ(P − E) pattern over the tropics from a dynamics perspective and to reconcile why the increase in NEI does not lead to the dynamically expected increase in (P – E)."

Regarding main comment 3: We fully agree with the reviewer that changes in the land surface properties following afforestation lead to direct changes in evapotranspiration.

Our focus in analyzing the dynamic effects (leading to Conclusion 3) stems from the question of *how* the increased evapotranspiration translates into the observed spatial patterns of Δ(P – E). We would like to clarify that if evapotranspiration would increase without any accompanying changes in atmospheric circulation, then precipitation would increase by the same magnitude through local recycling, resulting in net-zero changes in (P−E). This is true unless there is also a change in non-locally sourced precipitation, as would be needed to maintain a constant recycling ratio. A change in non-locally sourced precipitation requires a change in moisture-flux convergence, changes in which have been shown to be overwhelmingly driven by changes in atmospheric circulation (Chadwick et al. 2013; Wills et al. 2016; Fig. S9). Thus, to explain any non-zero changes in Δ(P – E), there must be changes in atmospheric circulation that redistribute moisture and alter precipitation patterns. Thus, while the initial change in evapotranspiration is land-driven, the resulting pattern of Δ(P − E) has to be caused by changes in atmospheric circulation. This necessitates a dynamics-focused analysis which we perform in Section 3.

We have added the following sentences to the introduction for clarification (L. 69-74): “Critically, if evapotranspiration were to increase without any accompanying changes in atmospheric circulation, precipitation would increase by the same magnitude through local recycling, resulting in net-zero changes in P − E. Therefore, to explain any non-zero changes in P − E, there must be changes in atmospheric circulation that redistribute moisture and alter precipitation patterns. Thus, changes in P − E are closely linked to atmospheric circulation changes, which drive large-scale redistribution of moisture and energy (e.g. Seager et al., 2010; Ma and Xie, 2013; Chadwick et al., 2013; Wills et al., 2016).”

Additionally, we also added the following explanation to the result section (L. 237-238): “Note that the non-zero changes in P−E imply that the afforestation in this region leads to changes in atmospheric circulation and moisture transport.”

Regarding main comment 4: We thank the reviewer for this comment. Indeed, the thermodynamic/energetic influences on the monsoon are an important factor in addition to the surface roughness changes, and we have already investigated it extensively in the manuscript, albeit not quite in the way the reviewer is describing. It is not near-surface temperature over land that determines the strength of the monsoon, but near-surface moist static energy (or moist entropy), as has been extensively documented in the literature (Emanuel 1995; Geen et al. 2020; Harrop, Lu and Leung 2019; Ma et al 2019). We have now added two additional subpanels to Figure S10 showing the increase in near-surface moist static energy over central Africa (reproduced below), which show that the near-surface MSE actually increases. This is opposite of the change in temperature due to the large increase in near-surface specific humidity. Therefore, this mechanism would actually weaken the monsoon. This perspective based on near-surface MSE is a complementary perspective to the perspective based on NEI discussed throughout the main text and summarized in the schematic (Figure 8). Relating back to the reviewer’s main comment 2, this is another way of explaining why the P-E change is counterintuitive, because it is opposite to the change in the monsoon that would be inferred from the change in near-surface MSE alone. We have added a sentence explaining that the same conclusions are reached when using near-surface MSE instead of NEI to investigate the energetic influences on the monsoon (L. 283-284).

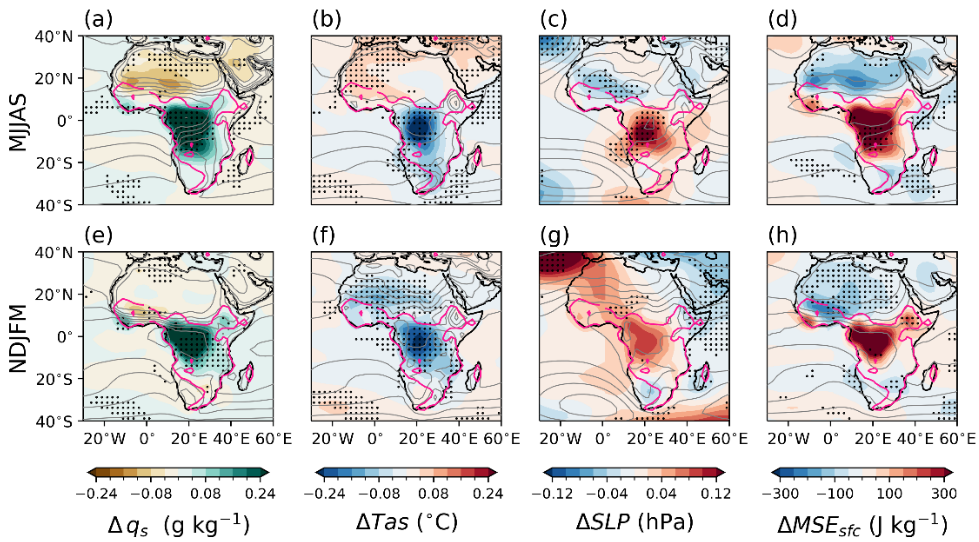

Fig S10: Spatial maps of changes in (a, e) surface specific humidity Δq_s, (b, f) near-surface temperature ΔTas, (c, g) sea level pressure Δ SLP and (d, h) near-surface moist static energy ΔMSE_{sfc} in MJJAS (upper row) and NDJFM (lower row) over Africa in AFFOREST. Contours show the BASE values with a spacing of 2.5 g kg^{-1} for q_s, 3°C for Tas, and 2.5 hPa for SLP. Pink contours enclose the regions where there are at least 5\% of afforestation. Stippling shows where 6 out of 7 ESMs agree on the sign of change.

As for the change in surface pressure, this is influenced by both mechanisms, because the link between anticyclonic motion and divergence operates through the near-surface vorticity balance, where wind-stress curl is proportional to mass convergence. The implications of this balance for tropical circulations and P-E is discussed extensively in Section 4 of Wills and Schneider 2015. Trees have a large influence on the surface roughness, which determines the relationship between the near-surface wind and surface wind stress, so the quantitative relationship between anticyclonic motion (as evident in SLP) and divergence will be modified by this change in surface roughness.

References:

- Emanuel, K. A. (1995). On thermally direct circulations in moist atmospheres. *Journal of the atmospheric sciences*, *52*(9), 1529-1534.
- Geen, R., Bordoni, S., Battisti, D. S., & Hui, K. (2020). Monsoons, ITCZs, and the concept of the global monsoon. *Reviews of Geophysics*, *58*(4), e2020RG000700.
- Harrop, B. E., Lu, J., & Leung, L. R. (2019). Sub-cloud moist entropy curvature as a predictor for changes in the seasonal cycle of tropical precipitation. *Climate Dynamics*, *53*, 3463-3479.
- Ma, D., Sobel, A. H., Kuang, Z., Singh, M. S., & Nie, J. (2019). A moist entropy budget view of the South Asian summer monsoon onset. *Geophysical Research Letters*, *46*(8), 4476-4484.

Regarding main comment 5: The wind stress is calculated as the magnitude of the eastward and northward wind stress (tauu and tauv) both of which are standard outputs from CMIP6.

<u>Some specific comments:</u>

- Lines 37–39: Canopy conductance/resistance is also a key factor.

  Thanks, we changed the sentence to "However, trees also enhance evapotranspiration through their larger leaf area and deeper root systems (Bonan, 2008), physiological control of transpiration through canopy conductance, as well as through the enhancement of turbulent fluxes by their influence on surface roughness." (L. 37-39)

- Line 66: Runoff is defined locally (or in a grid cell in a model). The integrated runoff over a basin leads to river streamflow.

  We have removed the parentheses after runoff in L. 67 to prevent confusion about the definition of runoff.

- Line 100: 1000 what? (Units are missing)

  Thanks for spotting this, we have changed it to 1000 Mha.

- Definitions in Section 2.2.1 (and throughout the text): Moisture, wind, and vertical velocity are, by definition, zero at the surface. These quantities must therefore be near-surface values. What level do they correspond to — 2 m, 10 m, or the lowest atmospheric model level? This is particularly relevant for the wind-based metrics used in the paper.

  We thank the reviewer for the comment. We have now specified for q_s and $\boldsymbol{u}_s$ that we mean near-surface values (e.g., changes in L. 168-170, 263, 268, 303, 308, Caption of figure 5-7). We have also added the following sentence "Note that near-surface temperature and humidity variables are defined at a height of 2 m, while near-surface wind variables are defined at 10 m, consistent with cmorized CMIP6 variables." (L. 168-170).

  Please also note that the near-surface vertical wind does not only come from convergence below 10 meters, but also due to flow parallel to the surface when the surface is slanted in pressure coordinates.

- Explicitly state pressure vertical velocity throughout the text, as it is omitted in several sections.

  Changed.

- Line 171: The phrase "three-dimensional" is unnecessary here.

  Changed.

- Lines 195–196: P – E defines surface runoff.

  We wrote that net precipitation "...is a key control of surface runoff" (L. 199-200) since there are other factors like the exchange with ground water or soil water storage which impact surface runoff.

- Line 258: Again, there is no such thing as "surface vertical wind". The level used for near-surface circulation analysis must be clearly defined.

  We change it to "near-surface vertical wind" and have now specified the levels at which the wind-related variables are defined in Section 2.2.1.

- Lines 263–266 and Fig. 5: This is very confusing. Why not directly use the pressure levels provided in the model outputs?

In order to illustrate the impact on surface values more clearly, we are interpolating the vertical velocity to sigma coordinates. In pressure coordinates, the fixed pressure levels represent different heights above the surface. In contrast, in sigma coordinates sigma = 1 is always at the surface which helps to understand the afforestation impact on near-surface winds. We opted to show the sigma coordinates in units of pressure to help the reader intuitively understand around which height in the atmosphere this relates to as well as to relate back to the pressure level determined to be relevant when looking at the omega scaling (around 700 hPa).

In order to explain this better we have edited the text as follows (L. 273-277): “Note that we interpolated the pressure vertical velocity profiles to sigma coordinates (defined as pressure normalized by the grid-cell surface pressure in the BASE dataset). This coordinate transformation was performed to provide a clearer representation of effects on surface values, as pressure coordinate surfaces vary in their height above the surface, while in sigma coordinates $\sigma = 1$ always corresponds to the surface itself. The resulting sigma coordinates are presented in units of pressure by multiplying by the area-averaged surface pressure from the BASE dataset.”

- Lines 422–424: I agree that having a large model ensemble allows for more robust conclusions, but model differences are also of great interest. The authors could elaborate more on this in the discussion.

  We have added the following sentence to the discussion: “While our large ensemble strengthens the robustness of our overall conclusions, the inter-model differences in the simulated hydroclimate and circulation responses also offer valuable insights into the uncertainties and complexities inherent in climate projections (Fig. S7, S8).” (L. 436-438)

- As noted in the manuscript, a large ensemble of simulations also allows for an increased signal-to-noise ratio. Yet, although scientifically relevant, the signal may not be particularly significant from the perspective of its impact on natural or human systems. In this sense, the authors could further discuss the intensity of the projected changes and their implications for, e.g, water availability, temperature, etc.

  We have calculated the fractional changes (AFFOREST/BASE) for evapotranspiration and precipitation and now mention the percentage changes in the text (L. 357-361 and Fig. R2). We further discuss qualitatively the potential impact for water resource management (L. 453-455) and on flooding (L. 455-457). However, we decided to make no additional quantitative statements about these effects since it will be highly dependent on the scale of afforestation / deforestation in these regions in the future.

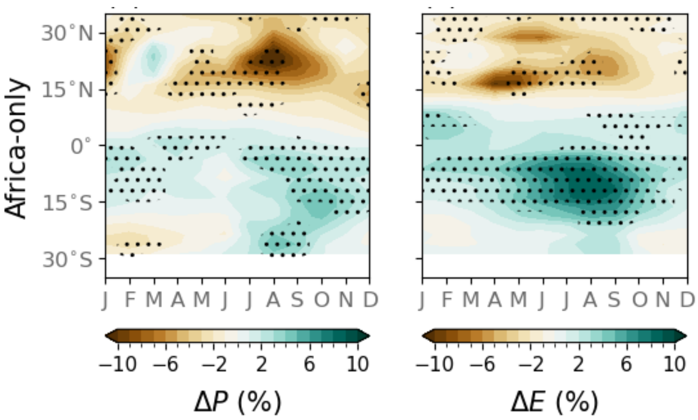

Fig. R2: Zonal-mean fractional changes in ΔP and ΔE over Africa (average over land area 20°W to 50°E). Stippling shows where 6 out of 7 ESMs agree on the sign of change. Note that we did not include the Δ(P-E) subplot as it saturates in all regions where the climatological P-E is near zero.

- Figure S6: This figure shows absolute values (not changes), correct? If so, the delta symbol should be omitted.

  Thanks for noting this, we changed the figure caption and colorbar labels.