

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

After the first revision, the authors have implemented changes that improve the manuscript. However, they continue to present interpretations of model outputs and bold conclusions regarding the simulated response to afforestation that, in my opinion, are not fully supported by the interesting analyses provided.

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).

First, in general, concerning the role of atmospheric circulation in the reduction of $P - ET$:

Several statements throughout the manuscript indicate that the reduction in net precipitation is primarily driven by dynamical changes:

Abstract: "The increased surface roughness not only increases evaporation, but also surface momentum fluxes, thereby slowing near-surface winds and reducing the orographic net precipitation"

Short summary: "... reduce net precipitation (precipitation minus evaporation) in these regions, which determines water availability. This happens because trees slow near-surface winds, ..."

End of sect. 3.2.1: "Thus, the primary mechanism(s) explaining the net precipitation changes over Africa are of dynamic origin (i.e., related to changes in the time-mean circulation), rather than related to transient-eddy or thermodynamic changes."

Conclusion 3: "The changes in net precipitation over Africa are driven by the competing effects of surface drag-induced reduction of lower-tropospheric winds and net energy input-induced strengthening of deep-convective upper-tropospheric circulations."

While such mechanisms may be valid in explaining precipitation responses alone, they do not explain changes in $P - ET$, which is dominated by changes in evapotranspiration. The latter is primarily driven by altered surface properties due to afforestation, not by large-scale dynamic circulation changes. Thus, attributing changes in $P - ET$ to dynamical processes misrepresents the underlying mechanisms and could mislead readers regarding the role of surface processes. In their responses and in a newly added paragraph in the revised manuscript, the authors state: "We would like to clarify that if evapotranspiration were to 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 $(P - E)$."

Why should a change in ET automatically result in a precipitation increase of the same magnitude? This assertion is not supported by physical principles. While it is true that, to maintain water balance, increased ET should moisten the atmosphere, this does not necessarily lead to an increase in precipitation of the same magnitude. Even if the P/ET

recycling ratio remains constant, this does not imply that absolute changes in P and ET will be equal.

We thank the reviewer for asking for further clarification on the role of atmospheric circulation on the changes in P-E.

In a perfectly closed system with no horizontal moisture transport, an increase in E would lead to an equal increase in P over time. This happens because the water has nowhere else to go; while the air would first moisten, a new equilibrium would be reached where the higher precipitation rate balances the higher evaporation rate. The result would be a zero change in net precipitation (P-E). The only way to avoid a zero change in P-E is if atmospheric moisture transport, which is controlled by the atmospheric circulation, adjusts to modify the moisture transport; this is simply a statement of moisture conservation. To better explain this in the text, we have edited the above-mentioned sentence to (L69-73):

“Critically, if evapotranspiration were to increase without any accompanying changes in atmospheric **moisture transport**, precipitation would increase by the same magnitude through **conservation of moisture**, resulting in net-zero changes in (P – E). Therefore, to explain any non-zero changes in P – E, there must be changes in atmospheric moisture transport.”

However, our simulations show a robust, non-zero change in P-E, which demonstrates that the system is not closed. This non-zero change is direct evidence that large-scale atmospheric circulation has responded to the initial increase in E by redistributing moisture. We agree with the reviewer that the initial change in E is a land-driven input. However, changes in P-E require considering also how the atmospheric circulation and moisture transport adjust to this E change, since P-E is determined by moisture flux convergence through conservation of moisture. Thus, the moisture budget mechanisms discussed in the manuscript explain changes in P-E, not precipitation.

Importantly, our moisture-budget analysis shows that the P-E response is shaped by the dynamic circulation response. In particular, we show that the dynamic component of moisture flux changes, ΔMC_{Dyn} , is the dominant contributor to the total $\Delta(P-E)$ pattern over Africa. This provides direct, quantitative evidence that the net precipitation changes are not a passive consequence of changes in surface evapotranspiration but are instead a direct result of a dynamical atmospheric response.

In summary, while the increase in evapotranspiration is the initial physical forcing, it is the resulting change in atmospheric circulation that physically controls the spatial pattern and magnitude of the non-zero $\Delta(P-E)$ response. Our focus on the dynamics is therefore a necessary step to explain the robust and physically constrained changes observed in net precipitation (P-E) across the models, and we do not attempt to quantify the relative contributions to evapotranspiration changes in isolation, as discussed more below.

Second, with regard to the drag effect on evapotranspiration:

As shown by several studies, increased surface roughness is only one of several mechanisms through which forests can enhance turbulent fluxes. If this mechanism were dominant, we would expect both latent and sensible heat fluxes to increase due to reduced aerodynamic resistance. However, in these simulations, there is a clear increase in latent heat flux, while the response of sensible heat flux is weak and of different sign (Figure S4). This pattern—a decreasing Bowen ratio—suggests that higher ET is primarily driven by changes in surface plant properties such as canopy conductance, rooting depth, and LAI, rather than by increased roughness alone.

We agree with the reviewer that evapotranspiration changes can be related to several different changes in surface properties, including deeper root system, physiological control of transpiration (canopy conductance) and surface roughness changes. Our study does not claim to disentangle these different influences on evaporation, and the simulation setup would not allow us to do so. Rather, it focuses on P-E, which as discussed in our response above is determined by the atmospheric moisture budget. Our moisture budget analysis demonstrates that a slowdown of near-surface wind convergence is a primary factor reducing P-E, as can be seen in the ΔS term (Fig. 3, 4).

We believe that a sentence in the abstract might have mistakenly given the impression that we are saying surface roughness changes are the dominant influence on evaporation changes, and we have modified it such that it implicitly included changes in plant properties (which are further discussed in lines 36-38):

“Not only do forests increase evaporation, but they also increase surface momentum fluxes, thereby slowing near-surface winds and reducing orographic net precipitation.”

Third, with regard to the changes in circulation:

Surface drag certainly affects near-surface winds, but it is not the only mechanism at play—changes in pressure gradients also play a key role. In their response, the authors acknowledge and partially agree a previous comment regarding changes in monsoonal circulation. However, this is not discussed in the revised text, and they ultimately maintain the same conclusions regarding the causes of circulation changes.

I invite the authors to consider a hypothetical numerical experiment in which surface roughness is held constant, while still allowing for other afforestation-driven changes (e.g., increased ET and alterations in the surface energy balance). Would we not still expect changes in surface temperature—and consequently in pressure and wind fields—somehow similar to those shown in the current model simulations?

We thank the reviewer for addressing the role of pressure gradients. As we mentioned in our previous response, the link between surface pressure and circulation is itself modified by changes in surface drag. This is because near-surface anticyclonic motion and divergence

operate through the near-surface vorticity balance, where wind stress curl is proportional to mass convergence. The direct influence of trees on surface roughness alters this relationship, meaning that the quantitative link between surface pressure changes and moisture divergence is itself changed by forest-induced changes in surface roughness. Thus, our manuscript discusses the influences on the monsoon circulation based on the moist static energy budget. Critically, it already captures the mechanism discussed by the reviewer, where near-surface cooling slows the circulation; however, this mechanism is overwhelmed by near-surface moistening, as shown in Fig. S10.

The reviewer's thought experiment—where surface roughness is held constant while other surface energy balance changes can take place—would of course also induce changes in the circulation. However, it would only capture the energetic effect, while the “full” afforestation scenario in our simulations changes both the surface momentum balance and the surface energy balance. Our key finding is that these two changes in the surface property changes trigger two separate competing effects, namely the surface drag and energetic effect. Thus, while in the hypothetical experiment changes in surface temperature and wind fields would occur, they would not show the same changes as the ones analysed in our manuscript, as it is missing the second, important effect due to surface roughness changes. They would only see the strengthening of the circulation that would be induced by the increase in net energy input (Figs. 6b, 7b), or equivalently the increase in near-surface moist static energy (Fig. S10), whereas the slowing of the near-surface winds due to the roughness increase would be missing.