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

The paper presents a novel analysis on how tropical circulation changes can explain changes in the net precipitation over Africa under climate change scenarios. The paper argues for a different control between changes in surface drag induced reductions in moisture convergence, versus increases in evaporation due to increases in latent heat due to afforestation. Authors find that changes are largely independent of the background climate under low and medium warming scenarios.

I find your paper a very interesting and original piece of work that might help to understand that changes in the P-E due to afforestation might be mainly driven by surface drag as opposed to other processes associated to physiology like roots, LAI, etc that increase Evaporation but may increase precipitation in a similar amount. Same as with one the previous reviewer, I still remain unconvinced by your claims on being surface drag the main driver of changes in P-E over certain regions and areas (what you call Western and Southern Africa in MJJAS), but I believe that even simple back-of-the-envelope calculations could go a long way in convincing readers that this is indeed the case in your simulations. Please see my major comments below.

We thank the editor for taking the time to read our manuscript in detail, and for his comments and helpful suggestions to improve our manuscript. 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). Additionally, we did another minor round of editing in the text.

Major comments

Line 246: “Transient eddy changes are also smaller than dynamic DMC changes …” I am not entirely convinced this is true for continental regions. For instance, in Figure 3.e transient eddy changes in western equatorial Africa are large and opposite as changes in DMC over the same region, consistent perhaps with small changes in D(P-E) in Fig. 3c over the same region. Similarly, changes in DTE in Southeastern tropical Africa are higher than changes in DMC over that region, and consistent with positive changes in D(P-E). Certainly over the ocean near that ITCZ DMC dominates clearly over DTE. To what extent the fact that the TE changes are comparable with the MC changes, modifies the analysis, in particular, how appropriate is then to consider that all changes can be approximated by the changes that come from Wills and Schneider scaling? Please be very clear as to how the further analysis in the paper relies on DMC being the dominant change.

We thank the editor for his comment. We agree that in western equatorial Africa dTE is locally comparable in magnitude to dMC (Fig. 3e). However, due to the opposite signs of dTE and dMC this leads to net zero changes in d(P-E). This counterbalancing effect is important, showing that while the absolute magnitude of dTE is comparable to dMC, this effect does not show up in the net precipitation pattern. Similarly, we agree on the dTE contribution to the drying in southern Africa, a feature we had already highlighted in the original manuscript text. Nonetheless, our analysis shows that the primary mechanism determining the distinct features of the overall P-E pattern – specifically the drying over western and southern Africa and wettening over central Africa – is related to the dMC term in MJJAS and NDJFM. To better point out the nuanced effects of dTE and the main contribution of dMC to the net precipitation pattern, we have edited Lines 246-253 as follows:

"The transient-eddy changes are generally smaller in magnitude than the mean-circulation-related changes and do not primarily determine the overall pattern across Africa. However, changes can be locally important, such as the contribution to the net precipitation drying in southeastern Africa in MJJAS (Fig. 3e) and wettening over central Africa in NDJFM (Fig. 4e). Furthermore, in specific regions like western equatorial Africa, large and opposing ΔTE and ΔMC anomalies can result in near-zero net change (Fig. 3c-e), highlighting local compensations. Nonetheless, the primary mechanism(s) determining the structure of the overall pattern in $\Delta(P - E)$ 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.

Regarding the empirical omega scale derived from Wills and Schneider 2015 and 2016: This dynamic scaling remains appropriate for our analysis as the dMC term is the most important influence on the d(P-E) pattern as discussed above. This scaling then gives an approximation of the dMC term, separately of changes in dTE. In Lines 160-162, we mention that "this approximation gives only a qualitative description of $\Delta(P - E)$ since it neglects the effect of transient eddies and horizontal advection, and is not defined in regions where the surface extends above 700 hPa". However, since this scaling captures the main large-scale features of the dMC and thus d(P-E) pattern with the dry-wet-dry dipole across Africa in both seasons justifies these simplifications and confirms the utility of the scaling for isolating the main physical mechanism.

Lines 333-334: "Surface drag effect is the dominant ..." The wording here implies that we have a large confidence that the surface drag is the main driver of the decrease in P-E which goes back to the main concern of the previous reviewer.

We thank the editor for his comment. We used the term "dominant driver" as our analysis demonstrates that the surface drag effect has a large influence on the P-E pattern. By analysing surface drag-induced changes in P-E through changes in the surface term dS (Fig. 3f, 4f), we find a large reduction in coastal regions of Africa and large model agreement regarding this effect. This strong signal is clearly linked to the afforestation-induced increase in surface drag, which reduces near-surface wind speed, reduces the near-surface vertical velocity (Fig 5) and subsequently leads to a reduction of topographically-induced P-E.

However, in order to better point out what we mean by the term "dominant driver" as well as to underline the modulation of the surface drag and energetic effects by other, concurrent changes, we have edited Lines 337-343 as follows: "Both the energetic and the surface-drag effect are active over the entire African domain to afforestation, however the resulting net precipitation response depends on which effect is dominant (i.e., has the larger influence on vertical velocity at 700 hPa and thus $P - E$) over a certain region. The surface drag effect is the dominant driver of the net precipitation decrease over western and southeastern Africa during MJJAS and NDJFM, although the magnitude of change is modulated by the energetic effect. Conversely, the energetic effect is the dominant driver of the net precipitation increase over central Africa, though the surface drag effect still influences near-surface pressure vertical velocity."

Again in lines 260-265 the main decrease of the DS term along the West and East coast of Africa is attributed to an increase of surface drag due to afforestation, however, and as pointed out previously by one reviewer, the decrease in the flow might be due to the increase in latent heating over the continental region which tends to produce a decrease in net precipitation. Surely part of this decrease might have to do with changes in the surface drag, however, part of this decrease might be entirely due to other changes in the plant physiology that lead to a higher latent heat in the simulations, a net precipitation decrease and therefore a weaker monsoon circulation towards the continent with the concomitant decrease in the across isobar flow near the surface. Lacking a dedicated simulation that controls for changes are exclusively due to surface drag, I fail to see how the authors can claim a preferred way in which this effect reduces surface convergence. Again, the fact that other people: Bell et al, Lague et al, have not been able to make clean simulation changes to isolate this effect, does not preclude the authors to be clear on how they think such simulation should be conducted in order to isolate the effect, and what would be the results if their preferred mechanism is to be found. This can help others to clarify this point, which does not seem so far clear neither in the present paper nor in the existing literature.

The diagnostics already presented in the supplementary material—such as the inland increase in sea-level pressure, the rise in near-surface specific humidity, and the gain in moist static energy—are entirely consistent with the interpretation that drag weakens low-level convergence and precipitation, even under a more favorable thermodynamic environment. This provides a solid basis for your argument, and highlighting it clearly will strengthen the paper. In contrast, the evidence suggests that physiology, albedo, and roots primarily raise evaporation while simultaneously enhancing precipitation through recycling, leaving only a minor residual effect. If I am understanding the paper correctly, making this distinction explicit, and clarifying that you are using "drag" strictly in the aerodynamic sense of momentum sink and reduced aerodynamic resistance, will help sharpen the main message. In short, I encourage you to reinforce this interesting and important conclusion with the quantitative and diagnostic evidence you already have at hand, as doing so will greatly increase the impact and persuasiveness of your manuscript.

We thank the editor for his encouragement to clarify the evidence supporting our conclusion that the decrease in P – E via the dS term is primarily driven by an increase in aerodynamic surface drag due to afforestation, while also highlighting the consistency of the pressure and moist static energy changes with our existing mechanistic explanations using NEI.

We concur that the presented diagnostics are highly consistent with the aerodynamic drag interpretation. The inland increase in sea-level pressure (SLP) (Figure S10c, g) implies a weakened near-surface circulation despite a more favorable thermodynamic environment for monsoonal circulations provided by the increase in MSE (Figure S10d, h), which results from the rise in near-surface specific humidity (Fig. S10a, e) despite a broad scale cooling (Fig. S10b, f). This implies that there must be an additional influence on the near-surface circulation, because the thermodynamic environment would lead to an increase in circulation strength and P – E, which is not what is found. This can be understood based on our analysis in Figs. 5, 6c-e, and 7c-e showing that mechanical, dynamic weakening of low-level winds and convergence by increased aerodynamic drag is overpowering the opposing, positive influence of the thermodynamic environment (i.e., higher near-surface MSE/NEI/latent heat). The reduction in flow occurs *despite* the thermodynamically favorable latent heat increase, which is supporting our conclusions. We have incorporated this more explicitly by adding the following text (Line 283-

287): “The overall decrease in net precipitation occurs despite a thermodynamically more favorable environment for precipitation, marked by an increase in near-surface specific humidity and MSE (Fig. S10). The influence of the thermodynamic environment will be investigated with an atmospheric energy budget perspective in the next section, but the fact that the net precipitation decrease still occurs suggests the importance of the mechanical weakening in opposing the influence of the latent heating and near-surface MSE changes.”

We have also clarified our methodology to explicitly state that we are referring to aerodynamic drag, which is strictly the momentum sink term resulting from changes in surface roughness and wind speed. We have added the following sentence to the method section (Line 167-169): “In our results, the change in ΔS primarily reflects changes in aerodynamic drag on the near-surface wind (u_s) caused by the alteration of surface roughness.”

We agree with the editor that a dedicated, idealized simulation would be the definitive way to isolate the pure surface aerodynamic drag effect. Previous studies which tried to examine the impact of surface roughness alone (as in studies like Bell et al. (2015) and Lague et al. (2019)) changed the vegetation height, which simultaneously modifies momentum, heat, and moisture fluxes. Even in simulations where only the roughness length parameter in the surface bulk formula is changed (rather than the vegetation height as a proxy) this would lead to changes in evaporation. This can be explained by changes in the roughness length for momentum (z_{0m}) typically being coupled with changes in the roughness lengths for heat and moisture (z_{0h} and z_{0q}), thereby altering latent heat flux. Thus, we propose that the ideal conceptual simulation to isolate the pure aerodynamic drag (momentum sink) effect would be one where z_{0m} is perturbed while keeping the roughness lengths for heat and moisture (z_{0h} and z_{0q}) fixed at their original values. This methodology leverages the distinct parameterizations for momentum and scalar transfer. By altering only z_{0m}, the change in near-surface wind speed is a pure momentum sink effect (aerodynamic drag), while the direct change in the aerodynamic resistance for evaporation (governed by z_{0q}) is eliminated. The resulting thermodynamic changes would thus only be due to indirect effects (e.g., advection, large-scale circulation changes), allowing for a clearer focus on the primary role of the momentum-drag anomaly. We have now added this description of the proposed experiments to the discussion section (Lines 468-471): “Instead, we propose running simulations where the roughness length for momentum is modified independently of the roughness lengths for heat and moisture. This simulation setup would allow the isolation of the pure surface (aerodynamic) drag effect on near-surface wind speed from the aerodynamic influences on evaporation and sensible heat flux.” We acknowledge that these idealised experiments are beyond the scope of our current analysis.

Minor comments:

Figure 6: Please check with some trusted source the name of the regions in Africa. West Equatorial Africa seems more appropriate than Central Africa, also East Central Africa, more appropriate than Southern Africa. Notice a typo in panels c,d and e in Vertical. Same in Figure 7. This can be a problem for the interpretation of the results as many will trust on the name you provide the regions on the abstract of the paper, without necessarily looking at the maps. Please make sure you are using the most appropriate name for each of the regions.

We have corrected the typo in Figure 6 and 7. We understand the editor's concern regarding the region names. However, since these regions in Africa are defined based on P-E anomalies rather than commonly used regions like for instance from the IPCC, there is no exact naming convention. Nonetheless, we tried to comply with the standard geographical naming of regions in Africa (e.g. Regions Of Africa), where central Africa is more located to the west of the continent. However, we have now renamed Southern Africa to Southeastern Africa to better describe this region (also with regards to the IPCC land regions ESAF and SEAF).

Line 170: cmorized, could be written as CMORized for better understanding?

Thanks, we changed it.

line 191: perhaps be more clear that all three processes mentioned lead to an increase in the latent heat flux.

We have reformulated the sentence (Line 189-191) to "In response to the afforestation, the zonal-mean evapotranspiration increases in the tropics due to enhanced transpiration through a larger leaf area, increased evaporative capacity due to deeper root systems and increased surface roughness (Fig. 2b; Bonan 2008)."

Line 341: "which undergoes" instead of "which is undergoes"

Thanks for spotting this mistake.