Response to Referee #1

After going through the revisions of "The effect of rainfall amount and timing on annual transpiration in a grazed savanna grassland", I find that the uncertainties and limitations to the study have still not been adequately addressed. In particular, estimation of ecosystem transpiration from all methods is difficult, and methods relying on eddy covariance methods, while improving in recent years, are still uncertain (Stoy et al 2019, Nelson et al 2020, Scott et al 2020, Hu and Lei 2021). In depth analysis from sites with many quality measurements and expert knowledge (such as what is presented in this manuscript) are vital to understanding both the true ecosystem transpiration dynamics, as well as uncertainties in the transpiration estimation methodologies. Therefore, utilization of multiple methods for estimating transpiration with different underlying assumptions is important to understand if the patters observed, such as the findings here that annual T and T/ET are linearly related to the early season precipitation, are robust.

We have provided the results for all three methods in the manuscript and in the supplement. The N18 method result is now included in fig. 7. In addition, the supplementary fig. 9, 10 and 11 show relation to all rainfall variables and EVI for all methods. Result section now reads:

The annual T/ET and T were linearly related to early wet-season storm frequency for the B16 and Z16 methods (Fig. 7), while the relation of early season P, mid wet season P, annual P or mean annual EVI with annual T /ET, and T were more scattered (Fig. S9, S10, and S11). For the N18 method, the annual T was linearly related to mid wet season P (Fig. S10), and annual T and T/ET were linearly related to mean EVI (Fig. S11).

The manuscript uses three ET partitioning methods, with the Berkelhammer and uWUE methods being very similar both in calculation and underlying assumptions. The third method (TEA), also shares many assumptions (particularly that T=ET during some periods), but is the most different of the three. The previous version dismissed the uWUE and TEA methods stating that " The T/ET values in the late wet season of 2015 based on the TEA and uWUE methods are likely overestimates, given the decrease in GPP and low EVI values during this drought year", and did not present findings based on the other methods in many of the results, making it difficult to understand the robustness of the findings and conclusions. I think it is very important to show all the results, even if in the supplementary materials, particularly Figure 7 which is the main finding. However, while all three methods are now presented in many of the plots, the TEA method is still excluded in the key findings.

We agree – not showing the results from all methods in Fig. 7 was an oversight. As mentioned above we have now included results for all methods. The water balance components for Z16 and N18 are already presented in Table S2 in the latest supplement.

The argumentation used for dismissing the TEA method is now based on a soil evaporation model, with the derived parameter of De for the Berkelhammer method being closes to those reported in other literature, particularly Hu and Lei, 2021. However, the Hu and Lei study also compared seven different ET partitioning methods (including the uWUE and TEA methods) and found that the TEA method performed the best, which is a direct contradiction of what is reported here.

The issue here is not which method is correct or incorrect, because all the partitioning methods are all wrong in some way. I strongly advise the authors to revise the results here to take this uncertainty into account and understand how the differences in partitioning methods may impact the interpretation, or at the very least report the findings from all the methods.

As we have state above, we now provide the results from all the methods in this major revision.

Our analysis does not intend to dismiss the N18 method. Indeed, we have no intention of giving such impression as a reason for our analyses. We have provided all the results for the N18 method and we do give reasons why it is likely an overestimate at this site. Given that, we focus on what we consider more reliable estimates of transpiration to evaluate controls of this flux in our study site. Including N18 results in all discussion points would render the manuscript incomprehensible and will draw attention away from the novel results.

Overall, the reasons why the *T* from N18 method is likely an overestimate at this site are: (1) The estimated soil desorptivity (D_e) values are much lower than what is reported in the literature or in the Hu and Lei (2021) study, meaning that evaporation is probably underestimated and transpiration overestimated after rain events at daily scale (Fig. 4 in the manuscript). (2) The range of annual ratio of transpiration to ET (T_{N18}/ET) is from 0.54 to 0.65 which is much higher range than from the other methods and higher than the reported range from the grassland in Arizona (Scott et al, 2021) and from the Nylsvley savanna with 30% tree cover (Scholes and Walker, 1993). (3) When using N18 to estimate total annual T, and subtracting the estimated grass transpiration, the difference which is an estimate of annual tree transpiration is 193 mm averaged over the three wet years. This estimate is much higher than reported in the literature at similar sites. Taken together, we consider this a strong evidence that N18 overestimate transpiration at this site. We have added this point to the discussion.

We prefer not to delve further into assessment of the potential causes for the N18 method to generate overestimation of *T*; this will be speculative, beyond the scope of our study, and will draw attention away from the focus of the study. We note, however, that the likely reason transpiration was not overestimated in Hu and Lei (2021) study is that their study site is an irrigated field that does not experience long periods of transpiration reducing drought.

Hu, Xingyu, and Huimin Lei. "Evapotranspiration Partitioning and Its Interannual Variability over a Winter Wheat-Summer Maize Rotation System in the North China Plain." Agricultural and Forest Meteorology 310 (November 2021): 108635. https://doi.org/10.1016/j.agrformet.2021.108635.

Nelson, Jacob A., Oscar Pérez-Priego, Sha Zhou, Rafael Poyatos, Yao Zhang, Peter D. Blanken, Teresa E. Gimeno, et al. "Ecosystem Transpiration and Evaporation: Insights from Three Water Flux Partitioning Methods across FLUXNET Sites." Global Change Biology, October 6, 2020. https://doi.org/10.1111/gcb.15314.

Scott, Russell L., John F. Knowles, Jacob A. Nelson, Pierre Gentine, Xi Li, Greg Barron-Gafford, Ross Bryant, and Joel A. Biederman. "Water Availability Impacts on Evapotranspiration Partitioning." Agricultural and Forest Meteorology, November 2020, 108251. https://doi.org/10.1016/j.agrformet.2020.108251.

Stoy, Paul C., Tarek S. El-Madany, Joshua B. Fisher, Pierre Gentine, Tobias Gerken, Stephen P. Good, Anne Klosterhalfen, et al. "Reviews and Syntheses: Turning the Challenges of Partitioning Ecosystem Evaporation and Transpiration into Opportunities." Biogeosciences 16, no. 19 (October 1, 2019): 3747–75. https://doi.org/10.5194/bg-16-3747-2019.

Response to Referee #2

This is my second review of the manuscript. The authors did a good job of addressing my concerns with the first version, and the paper reads much better now. Thank you for considering my comments and suggestions.

I have only a few small comments that you might want to address:

1. L22. What area? Have you introduced what area you're talking about?

Southern African drylands. We have rephrased the sentence.

2. P.6, L3-5. This is so great to see you considering this. So many studies don't.

Thank you for this comment.

3. P. 12 section 2.8. This model of grass T seems to have some assumptions that are worth justifying here. The first being that there is no drainage below 60 cm, the second being that E is constant which would underestimate E and overestimate grass T after a rain event and may underestimate grass T later into an interstorm period. I'm confused why a dynamic soil E was not used.

The algorithm does not explicitly estimate drainage. The main purpose of using the model instead of relying on difference of soil moisture values between layers is to allow quantification of the characteristic declines in soil moisture corresponding to RWU events. The periods when a certain soil moisture level reflects percolation are excluded by the algorithm. The soil moisture values at the bottom layer (100 cm deep) are constant during the measurement period, suggesting that drainage is small.

We did test the use of the dynamic *E* from each model, but the EC estimated *E*'s are over 60% of grass *T*, which is unrealistically high amount. This is because the soil moisture measurements may not fully capture the soil evaporation of the EC footprint. The estimated grass *T* is in general low immediately after large rain events due to the exclusion of percolation events. For these reasons, the constant *E* was used, resulting in a reasonable estimate of annual grass *T*.

4. P.23, L13-15. What about early season or total growing season amounts as a predictor of T and T/ET? I don't recall you considering this, but in a water-limited system I would think the amount of rainfall would be an important control.

We have now added supplementary figures for relations between *T* and *T*/ET and early wet season *P*, mid-wet season *P,* annual *P* and mean annual EVI, for all methods. These predictors have more scattered relation with *T* and *T*/ET for the B16 and Z16 methods. We do emphasize the location (highveld grasslands) because the result is supported by the other experimental study from a similar highveld grassland (Swemmer et al., 2007).