

Hydrol. Earth Syst. Sci. Discuss., referee comment RC1
<https://doi.org/10.5194/hess-2021-292-RC1>, 2021
© Author(s) 2021. This work is distributed under
the Creative Commons Attribution 4.0 License.



Comment on hess-2021-292

Russell Scott (Referee)

Referee comment on "The effect of rainfall amount and timing on annual transpiration in a grazed savanna grassland" by Matti Räsänen et al., Hydrol. Earth Syst. Sci. Discuss., <https://doi.org/10.5194/hess-2021-292-RC1>, 2021

In this paper, the authors examine the transpiration of a grazed savanna in Africa to determine what are the controls on annual ET partitioning. They do this by using 6 yrs of eddy covariance data and three different partitioning techniques. They conclude that early season rainfall timing strongly controlled annual T/ET by affecting the growing season dynamics primarily of grasses rather than the trees.

I found this study very interesting and generally, sound. I think it will be potentially of great interest to the readers of this journal. However, I found the presentation of the results confusing at times and would recommend a thorough restructuring of them. Many of the authors' conclusions are conjectures about the grass functioning with little to back them (i.e., you've got T and ET but not T_{grass} and T_{tree}).

Here are a few suggestions and comments that hopefully may guide a restructuring of the paper:

- The paper needs a deeper look into the controls on the total T and T/ET. Ultimately, this has got to be about water availability, right? Rain event frequency is really an indirect way of looking at it. It says nothing about the total amount of water and where it is located (shallow or deep). Certainly, storm depth must be a critical factor in how frequency is translated into water availability. Since you've got the data to do it (E, T, GPP, LAI, soil moisture) can you better unpack the seasonal pattern, showing in greater detail how summed T and E and soil moisture evolve through the early to middle part of a growing season contrasting a normal year with a dry one? You could look at the monthly level data, but you should be able to do this on a daily scale for the TEA or Berkelhammer results if you wanted to show the finer dynamics. Also, what about the E dynamics? Does storm frequency have an influence in the amount of E?

- There are lots of inferences about grass and tree functioning, but little data about this is shown in the results. Is there a way you can use the remote sensing and the monthly data to make your case more strong? E.g., you write that there aren't many LAI changes for the trees so LAI is really indicative of the grass LAI. You're also saying that the C4 grasses control their water use by dying back or growing new leaves. If so, is there a way to use T/ET (or maybe better, just T) and LAI to show this more clearly?
- The introduction is underdeveloped. What is missing are the previous results that lead to expectations of what you might find here. There are quite a few studies cited for semi-arid systems but what have you learned from them that help guide this analysis?

Text specific suggestions and questions:

Title: Is it a savanna or grassland or both?

P1.

L 27-29. Is there data to support these claims about grass and tree functioning?

L30. What is an anomalous monthly T/ET relation?

L31. How can drought be reasonably described by P timing alone? Storm depth has got to play a role as it ultimately is about water availability and its timing.

P.2

L12. You might also consider this paper which also talks about monthly T/ET dynamics in semiarid systems: Scott, R. L., & Biederman, J. A. (2017). *Geophysical Research Letters*, 44(13), 6833-6840.

L23. Maybe say "partially decoupling" as 37% isn't huge.

P.3

L5. These papers that compared approaches could be cited here: Berkelhamer et al. 2016, Scott et al. 2021, Nelson et al. 2020. *Global change biology*, 26(12), 6916-6930.

L11-L13. I don't understand how using both LAI and EVI allowed you to quantify the dynamics of the grasses and trees.

L19. "farm" or "ranch"?

P.5

L20. "verified" or "computed"? Verified with what?

P.6

L10. averaged over what time period?

P.9

L11. You need to sum 1/2 hr T's and ET separately and then take their ratio. You can't just take the average T/ET.

L25. I'm wondering why this was done across all years. I would think that the same reasons that you fit the Berkelhamer approach yearly should apply equally here. Yearly changes in ecosystem structure/lai should warrant a yearly fit.

L27. "...of each month." This is confusing. I thought uWUEp was computed for the 6 yrs and uWUEa was computed monthly?

P10.

Table 1. As you've already described the methods in the text. This table is superfluous. Suggest omitting.

L14-16. Was there a reason for using both EVI and LAI? I've always found the essentially the same information content in each signal. For simplicity in the presentation of the results, you might consider using only LAI.

Table 2. For the T and E columns. What method is this or is this an average between the three? Also, see Fig. 5b...no method given.

P16.

L7-9. This is a possibility but isn't it also possible that T/ET in the late rainy season goes up as the soil dries and E becomes negligible?

L16. "due to a higher early-season precipitation frequency". Sorry to beleaguer the point, but the higher frequency may be a symptom rather than the cause of higher water availability.

L29. It gets awkward to use the inverse. Why not present the usual WUE metric instead, making these numbers readily comparable to previous studies?

P18.

L2-3. As this section jumps back into the site water balance shown in 3.1, I found it confusing. You might change the organization of the results to one being about the water balance (talking P, ET, T, interception, Esoil, deltaS etc. and their variability) and the other being ET partitioning. Also, maybe adding a section that talks about the grass/tree dynamics separately to better support your claims.

L11. See comment L16 above.

P19.

L10-13. In this summary of the results where is the evidence for this? I think this paper would really be improved if you could organize your results to better show this.

L22, Not clear what this sentence is here to address. On the surface, it says rainfall

frequency is not important.

L25. Where is this dieback - regrowth shown? Can you use EVI or LAI to show this?

P21.

L5. I would delete this comparison. Using a BR from a higher annual PPT site isn't appropriate. Also, in order to use the BR to estimate ET you need to rely on H which may or may not be subject to commensurate errors.

L4-18. I'd suggest also considering, Scott, R. L., & Biederman, J. A. (2019). *Water Resources Research*, 55(1), 574-588 here. To me, the fact that $ET \sim P$ is really solid evidence for the validity of your ET measurements so long as runoff (surface or deep) is negligible. Having an $ET = P$ seems quite appropriate especially if you have those deep-rooted trees to capture any deeper infiltration.

P22.

L 9-16. The Scott and Biederman 2017 paper using an entirely different method suggests a peak of $T/ET \sim 0.60 - 0.70$ for a drier savanna site, similar to the results you have here.

L20. This is a discussion point, not a conclusion that comes from this paper.