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Comment on hess-2022-59

Anonymous Referee #2

Referee comment on "Water partitioning in a Neotropical Savanna forest (Cerrado *s.s.*): interception responses at different time-scales using adapted versions of the Rutter and the Gash models" by Livia M. P. Rosalem et al., Hydrol. Earth Syst. Sci. Discuss., <https://doi.org/10.5194/hess-2022-59-RC2>, 2022

Comments on "Water partitioning in a Neotropical Savanna forest (Cerrado *s.s.*): interception responses at different time-scales using adapted versions of the Rutter and the Gash models" submitted to HESS-D by Rosalem et al.

[General comment]

This paper measured the interception processes including forest floor interception loss for a period of 32 months, and applied Rutter and Gash models. Although the methodology used in this study should be evaluated carefully, I recognized that the efforts to conduct measurements would be very intensive, and would agree the importance to understand interception phenomena in Cerrado region. However, the logic of this manuscript is not constructed well, so readers cannot understand what is the most important findings in this research. The main description seems to be the model performances for daily, biweekly and monthly basis. I understood the differences in performance among different time scales, but I do not think that the differences are important to clarify the interception process. The important thing must be what factors resulted in the differences and what processes affect the model performance. I would like to recommend that authors, at first, write a paper to show interception process in this site in detail based only on measurement data without applying any models. The interception loss in this site would be affected by rainfall characteristics (e.g., rainfall duration, rainfall amount, rainfall intensity, etc)? Or, other micrometeorological factors (e.g., wind speed, vapor pressure deficit, net radiation, etc) influence the interception process? Based on these knowledges, I believe that authors could develop suitable models to simulate the interception process. I hope my comments will help to do substantial revisions.

[Specific comments]

Could you add the LAI data? (line 92-93)

I cannot catch up how to calculate the spatially representative amount of throughfall. Is that the average of four Davis, five manual gutters and three automatic gutters? (line 100-119)

Because the canopy in this site is discontinuous (line 93), I could expect that the spatial heterogeneity of throughfall would be very high. Please show the differences in throughfall amount measured by four Davis, five manual gutters and three automatic gutters. The differences are related to the canopy openness? Also, a total of 12 measurements of throughfall are safely enough to obtain spatial representative value?

As Rosalem et al. (2018), published in *Ecohydrology*, pointed out, Davis gauge underestimates the inflow of water flux with increasing intensity (please see FIGURE 3 in Rosalem et al., 2018). I would like to confirm that authors applied the same correction to throughfall measurements in this paper. If not, application must be required.

Three gutters are connected to three Davis gauge? If so, I am wondering that the one tip amount of 0.048 mm is too small to detect the correct amount. As Rosalem et al. (2018) showed, the underestimation by Davis gauge is relatively high. How many pulses generated by the gauge were recorded in 10-min intervals? The time between tips, equivalent to 600 second divided by accumulated pulse count, should be more than 1.0-1.5 second. Please note that, if authors used other rainfall gauge, similar issue exists and should be investigated.

How did authors calculate stemflow amount in the stand scale?

Three automatic collectors of stemflow is connected to Davis gauge? The same issue mentioned above, underestimation of inflow with increasing intensity, must be checked.

Forest floor interception was measured by two LIDs (line 121-122). Please show the evidence indicating two LIDs could safely measure the spatial representative value of forest floor interception. This is very critical, because high spatial heterogeneity of throughfall could be expected from the disconnected canopy in this site.

Figure 2: The forest floor evaporation was calculated considering potential evaporation (E_p), but how did you calculate E_p above forest floor? Did you measure net radiation above the forest floor?

Please add the description to explain how to calculate the aerodynamic conductance above canopy and forest floor (line 214-215).

How did you obtain the interception ratio of 33%? (line 253) Throughfall was described as 70-72% (line 228), so 33% interception is too large. There is a description of 40% interception in conclusion section (line 481). Maybe the target rainfall events are different among parts, but it is difficult to understand.

I cannot understand why E_c is calculated as the difference P_g and the sum of F , T_s and E_f . I recommend that basic equation showing rainwater balance should be added in the M&M section.

Discussion about the stemflow is not directly related to this paper (line 300-313). If authors show the data of canopy structure, bark, and so on, it is useful. Unfortunately, the current MS did not include any data, so I recommend to remove this part.

I felt that the much sentences in the current MS are related to model performance (line 237-245, 268-273, 293-299, 320-328, 335-342, 368-380, 435-454, Table 6, 7, 8, 9). Similar descriptions are found among parts, so I recommend reconstruction of the logic. In my opinion, it would be better that "discussion" should be separated from the "result" section. Then, descriptions of model performance should move to discussion section, and more concise discussion is recommended. Rather than differences in model performance, the reason for the difference and factors affecting it are more important to understand the interception process in this site.

Looking at appendix C, there are high correlations between observation and model output for throughfall and stemflow. However, the correlation for interception loss is very low. Could you explain this point?

[Technical corrections]

Line 98: "average PAR (photosynthetic active radiation) of $1041.8 \pm 427.4 \mu\text{mol}\cdot\text{m}^{-2}\cdot\text{s}^{-1}$ ", I cannot understand the duration of average.

Line 199-200: "From June of 2019 to May of 2019, we used to calibrate our Rutter and Gash models, while the second part, from June of 2019 up to of 07th February of 2020, .."
Please check the consistency of months. The current description is strange.

Figure 3, y-axis title of the upper panel: "Potencial" should be "Potential".

Figure 3, lower panel: Is this net radiation? Solar radiation was measured at 2 m height (Table 1). More than 600 W m⁻² value is too high for solar radiation above the forest floor. Please check carefully.