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Comment on hess-2022-59

Anonymous Referee #1

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 Lívia M. P. Rosalem et al., Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2022-59-RC1, 2022

This manuscript describes measurement and modeling of interception loss from the canopy and forest floor. As is typical in published applications of Rutter-Gash models, the models performed poorly at the scale of individual events and better when accumulated over longer timescales as presented in Table 5. The manuscript also presents some potentially important modifications to the standard Rutter-Gash approach by modifying the model of canopy storage, modifying the evaporation rate from trunks, and adding explicit measurement of evaporation from wet forest floor. The manuscript does not evaluate the behavior of the models in relation to the standard ones, so the importance of the modifications is difficult to understand. In addition, the manuscript omits some details about parameter estimation and presents some model modifications vaguely and in terms of programming techniques, so the methods are not possible to follow comprehensively. The discussion touches on some important questions pertaining to the applicability of the modeling assumptions, such as time scale of forest-floor evaporation and assumptions embedded in the Gash model, and mismatches between data needs and data availability, such as radiation budgets relevant for the forest floor. However, the manuscript mostly disregards these important points and the poor by-storm performance, and makes detailed interpretation of the effects of seasonal variation of parameters. Given that the models mostly cannot reproduce per-storm interception loss, and the time-integrated performance is driven by essentially the regression between rainfall and throughfall expressed in the calibrated canopy storage capacity, I do not agree with the conclusions that the modeling is useful for understanding canopy processes. These are not unique problems for this study and field experience is valuable for developing the next generation of models, but the modeling presented in this manuscript is not well described or developed.

This manuscript needs a thorough edit for English: there are many errors in word choice, grammar, and spelling.

L34 these are rainforest numbers that are not likely relevant to the field site here

L74 The logic of the theoretical work should be justified here. The objective based on the study site, L72, is not as important as the theoretical implications of behavior of the model, and there are modifications to the basic models that are not explained here at all. Similarly, the model introduction L55-64 should focus on the shortcomings of existing models that need to be overcome, not on basics.

L94 15,000 stems per ha? This is 1.5 trees per square meter; is that correct?

L98 What use is PAR for this study?

L114 why “also”?

L119 according to DBH, but by what criteria and why?

L120 this reference does not provide details of this device. A few critical details about how a LID works are needed for this part to be understandable

L123 But there was only one August during the study, correct?

L137 upper envelopes and linear regressions are not the same. Stem storage capacity has generally not been estimated by this technique in the past for several important reasons. This needs justification and explanation.

L150 These two ad-hoc modifications to the Rutter model are not sufficiently described or justified. There is no way to understand exactly what "priority" means in terms of the water and energy balances.

L152 What about the Tf records indicate dynamic storage?

L153 this term needs explicit definition. I do not know what Cc is supposed to represent

L156 What is the purpose of Ccmax exactly and how was it implemented? Rutter's model explicitly allows storage C to exceed storage capacity S, with no explicit upper limit. Adding a cap here appears to reduce the dynamic storage, not enable it. Why only "before

the end of the storm"?

L159 what is Cmax. the same as Ccmax?

L160 I don't understand this parameter identification technique

L176 I do not understand how data were used to estimate Cfmax

L179 Most of the parameters referred to in this sentence are not defined

L185 I can't find where these parameters were estimated

L199 something is wrong. This describes an experiment 8 months long but fig 3 says 3 years

L204 The original Gash model was applied at the monthly time scale. There is no time scale assumed by Gash

L214 This conflicts slightly with L145. I assume this means stomatal resistance was zero. There are no details about assumptions and about parameters relating to aerodynamic resistance

L220 Fig 3: L218 told us to expect net radiation here

L224 more similar than what?

L242 Why does the Gash model predict essentially the same canopy evaporation every storm except for very small storms?

L264 I think it is unlikely that measurements of forest-floor evaporation were better than the throughfall measurements.

L265 It is not obvious to me that canopy evaporation is modeled better.

L280 As described earlier, it did exactly the opposite: it prevented accumulation

L284 How? Shouldn't mean daily Rutter be the same as daily Gash?

L344 This problem is severe for the modeling assumptions and needs detailed theoretical discussion

L348 I do not understand this sentence

L353 I agree. I think this makes the Gash model irrelevant for this process. Lack of data to estimate energy budgets and vapor exchange at the scale of the forest floor are also major problems for applying a Rutter model. Given that Rutter does not seem to be helpful for estimating canopy evaporation on a storm scale, it seems even less likely to be helpful for forest-floor evaporation.

L397 I don’t think the evaporation model was useful for small storms, either. There was not systematic negative bias in estimates, so that means the small storms were overestimated.

L425 I would agree if the models were useful at short timescales, but they are not. If the processes are not correctly modeled, how can we extrapolate using the model?

L510 I do not agree. The models get major masses correct, but due to calibration (e.g, of canopy storage). The models are not likely to be of any predictive value should, for example, vegetation structure change.

The journal names are missing for many References.