Authors Response: report of Referee #2

To the editor:

Thank you for giving us the chance to explain our reasoning for adding the large rain events.

As suggested by the editor, we added our reasoning in the manuscript in both the M&M section (P12L11-16) and also in the result section (P13L8-P14L5). An extra appendix (B) is added to give the reader more insight in the events in question.

Full comment Referee:

In my original review, I was lost in the excess information and missed a crucial problem: all net precipitation data have been assumed for all events larger than 7.5 mm because of instrument failure (P10L10). Because of this, there are not enough actual data to support empirical estimates of canopy storage and all data-based estimation of canopy storage has been abandoned in this revision in favor of a purely empirical equation derived off site. These problems defeat most of the purpose of the experiment. By assuming storage capacity and also assuming that total storm evaporation loss is equal to that storage capacity, the experiment is left with insufficient data on canopy interception by which to calibrate models. Canopy storage capacity is one of the most important variables in the Rutter-Gash formulation, so those models cannot be used. The consequences for the assumptions are also obvious in the data such as figure 5, where there is a clear inflection point near 7.5 mm. The assumptions thus clearly drive the results, and the conclusions cannot be considered supported by data.

We would like to thank the reviewer for critically reviewing our manuscript. We understand his concern but disagree on the idea that our method is assumption driven, as we back up the assumptions with our measurements. We try to respond to the reviewer's reasoning below. Please let us know if any doubts about our approach remain and what would be needed to improve our manuscript.

"all net precipitation data have been assumed for all events larger than 7.5 mm because of instrument failure (P12L10). "

It is correct that when cumulative precipitation exceeded 7.5 mm, we assumed that all subsequent rain would be converted to through fall or stem flow (net precipitation). This is not due to instrument failure but due to a limitation in the size of our rainfall collection system. The reviewer is correct in his concerns that if intra-event evaporation occurred (during intermitted short periods of drought in a rain event) after this 7.5 mm threshold, this intra-event evaporation would not be detected with our measurement system, which would give rise to an underestimation of canopy storage and an overestimation of through fall. However all these large events were thoroughly checked on the possibility of intra event evaporation. The amount of precipitation significantly exceeded 7.5 mm in 10 out of 64 rain events. In Appendix B of the revised MS, the time-intensity and cumulative graphs of these 10 events are depicted to detect intermittent dry periods (ranked largest – smallest event). For some events with dry periods occurring late in the rain event, we also added humidity and temperature graphs to provide extra information. These intermitted dry periods would be problematic if they occurred during sunlight hours and late in the rain event when the threshold of 7.5 mm is exceeded. This is the case in 2 out of 10 events (event 57 and event 33). However only 0.33 mm and 0.23 mm of rain fell after the intermittent drought period of these events. This amount is, in our opinion, negligible in comparison with the total amount of PP that has fallen during these events (17,78 mm and 10.81 mm which makes the rain fallen after the drought period 1.8% and 2,1% of total PP respectively).

Because of this, we think our assumption for larger events is correct and that they can be used in the analysis.

"Because of this, there are not enough actual data to support empirical estimates of canopy storage and all data-based estimation of canopy storage has been abandoned in this revision in favor of a purely empirical equation derived off site. "

It is true that we abandoned the Leyton method (data-based estimation) for defining the canopy storage. After the first review, where we received comments from both reviewers about the definition of the inflection point, we decided to remove the Leyton analysis from the manuscript. The main reasons are the subjectivity of the method (Klaassen, 1998; Link et al.,2004) and the fact that seasonal changes are not taken into account. Therefore we decide to use the empirical equation of Gomez. It is true that the Gomez equation has been derived off-site but it is based on the leaf area index which we measured for all trees and seasons. Several authors (De Jong & Jetten, 2007; Galdos et al., 2012) point out the importance of using leaf area index to estimate interception storage capacity. We therefore don't agree that we assume interception storage capacity as it is based on an equation using measured leaf area index values from our trees of interest. The Gomez equation has been used for broadleaf trees in other publications (Verbeiren et al., 2016 & Wirion et al., 2017).

"These problems defeat most of the purpose of the experiment. By assuming storage capacity and also assuming that total storm evaporation loss is equal to that storage capacity, the experiment is left with insufficient data on canopy interception by which to calibrate models. "

As we stated above, only two out of 10 events larger than 7.5 mm had intra event dry periods during sunlight hours after the 7.5 mm threshold was reached. Because very little rain fell after these drought periods, the error margin of our assumption is very low.

"Canopy storage capacity is one of the most important variables in the Rutter-Gash formulation, so those models cannot be used. The consequences for the assumptions are also obvious in the data such as figure 5, where there is a clear inflection point near 7.5 mm. The assumptions thus clearly drive the results, and the conclusions cannot be considered supported by data…."

We are not sure to which figure 5 the reviewer refers to. If it is figure 5 (Figure 5: Example of the Leyton method from (Sadgehi, 2015).) of the first version of our manuscript, it is a literature based description of the Leyton method and is not at all related to our data. I agree with the reviewer that we can see an inflection point at the 7.5mm threshold however there is no relation at all to our dataset. If the reviewer refers to the figure 5 (Figure 5: Interception storage vs gross precipitation for all events for both trees.) of the revised manuscript, we don't understand that comment as there is no inflection point on that figure. This figure shows a larger variation in interception storage with larger events.

Thank you very much for considering our explanations.

Best regards,

Vincent and Charlotte

References:

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