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Interactive comment on "The influence of long-term changes in canopy structure on rainfall interception loss: a case study in Speulderbos, the Netherlands" *by* César Cisneros Vaca et al.

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This manuscript reports on wet-canopy evaporation research conducted in a well instrumented site that has also been the site of important work on the same topic in previous decades. In general the data and analyses appear to be of high quality. The manuscript is rich in detail, but in places it focuses on presenting details at the expense of a comprehensive logically coherent examination of the objectives.

We sincerely thank Reviewer#2 for the detailed comments and valuable suggestions which helped us very much to improve the manuscript. In the following, we respond to the comments one by one.

The objectives themselves would benefit from some clarification. I think objective ii could be phrased better, in that the source of the latent heat flux is known but the real question is the source of available energy to drive that latent flux. Objectives iii and iv are too general to be useful. Aligning objectives with motivating statements would also improve clarity. For example, P2L31 suggests the objectives will include analysis of how canopy variation affects wet-canopy evaporation over time, and indeed there are some such comparisons in the discussion, but there are no explicit objectives pertaining to this goal, and only one very general conclusion about it.

This is a very good suggestion indeed, and we thank Reviewer#2 for this. We have rephrased the objective (ii). Moreover, the objectives (iii) and (iv) have been revised to make them more clear and specific. The revised objectives are now properly aligned with the motivating statement in the introduction section. The revised objectives read:

i) assess two indirect methods for estimating canopy storage capacity;

ii) quantify the sources of energy that drive the latent heat flux involved in the evaporation of intercepted rainfall;

iii) examine the effect of long-term changes in canopy structure on the rainfall interception losses;

iv) explore the relative importance of climatic and forest structural factors to overall rainfall interception loss using a physically based interception model.

One aspect of the work that I think warrants more robust treatment is the role of the canopy in supplying available energy for evaporation. There is little discussion of the sensitivity of various assumptions needed to support the estimations of this energy, yet it ends up being a relatively large proportion of the total budget.

Indeed, one of the important sources of energy for evaporation of intercepted rainfall is the energy stored in the canopy. Although it is not the largest one (only represents 15 % of the total energy involved), in the revised version, we have now discussed the sensitivity of the canopy biomass released energy to the assumptions considered in our calculations.

We have done an error propagation exercise, applied to Eq. 3 which embrace most of our assumptions. The uncertainty will be the quadratic sum of the relative errors δm_{bio} , δc_v, and $\delta \Delta T_{bio}$. The δm_{bio} is related mainly to the uncertainty of allometric equations (18% for n = 23, Chave et al., 2004) in combination with the uncertainty in the assumed moisture content (ranging from 44 % to 55 %). Then the combined uncertainty for δm_{bio} would be about 27 %. Regarding c_v, the range of values used in studies with similar species is from 2400 to 2928 (J kg-1 K-1) (Oliphant et al., 2004), which means an uncertainty of 22%. The uncertainty of ΔT_{bio} assumed to be equal to ΔT_{air} would be the largest one. Based on data presented by Meesters and Vugts (1996) (their Fig.6) the difference in temperature amplitude between T_{air} and T_{bio} would yield to an uncertainty of about 40 %. Then the error propagation of the product of the three variables will yield a 53 % of uncertainty for Q_{bio}.

There is quite a bit of duplicative text. For example, the insensitivity of interception models to in-storm vs. post-storm evaporation (i.e., Fig. 10) is mentioned at least four separate times in the introduction, results, and discussion. I think perhaps 3-5 tables and figures could be eliminated to reduce the overwhelming detail (some candidates: F2, F3, F9, combine F4 and F5; T3, T4 to text).

We have improved the readability of the manuscript by taking out any duplication and reducing the number of figures and tables.

Following the comments and suggestions from Reviewer 2, we have removed Figures 2, 3 and 9. Similarly, we have combined Figures 4 and 5 into a single figure (Figure 2 in the revised version). As for the tables, we have removed the Table 4 but have decided to keep the Table 3 for readability as it contains the main equations used in the Gash model to quantify the different components of interception loss.

P6L13-14 both citations of tree properties should be to the primary sources rather than these secondary works.

Thank you for pointing this out, in the revised version we have corrected the citation for specific heat of biomass (c_v), the primary source is indeed Michiles and Gielow (2008). And for the value of moisture content, we have verified and properly cited the work of Nord-Larsen and Nielsen (2015) that is the primary source.

P7 section 2.3 needs a name like “modeling,” or perhaps it should not exist and section 2.3.1 should be 2.2.8.

We agree and have renamed the section headings: 2.3 Modelling rainfall interception and 2.3.1 The Gash rainfall interception model

P7L25 does this mean data outside these two windows were completely omitted from all analyses? It doesn’t seem so.

Data outside those two windows were not omitted from all analysis. They were omitted only for the modelling purposes. We have now devoted some texts in the modelling section to clarify this issue.

P8L3 and P9L6 the meaning of “was preferred” is not clear in either place; please state exactly what was done instead of what was desired. Does this mean that Ebar is not average as stated P9L4?

Thank you for pointing this out. We have removed that sentence from P8L3 to avoid duplicative text. In P9L6, We have revised the paragraph. It now reads: “Because the distribution of rainfall intensity was highly skewed, we used the median rainfall intensity following the recommendations of Schellekens et al. (1999). The thus derived value of $\bar{E}$ will henceforth be referred to as the ‘water balance based’ evaporation rate.” As for the last part of the comment, we have elaborated the waterbalance approach (P9L4) to make this clear.

P11L3 this information duplicates the methods.

We have removed the sentence in the revised version.

P11L5 how can SE be calculated from accuracy?

This was a mistake, and we thank Reviewer#2 for pointing this out. In the revised version we have calculated the SE for our Sf measurements correctly and have revised the sentence accordingly.

P11L29 it is not clear what the denominator is in this percentage

The denominator is Rn-G-Q, the available energy for the turbulent heat fluxes H +LE. In fact, the percentage was calculated as 100% *slope, where the slope was derived from the regression of H+LE versus Rn-G-Q. In the revised version, we will keep only the slope value as an indicator of the energy balance closure to avoid any misunderstanding.

P12L12 is this not the "'water balance based' evaporation rate" promised P9L7?

Yes, that is water balance based evaporation rate as mentioned in P12L9. We have revised the sentence for greater clarity. It now reads: "The parameter $\bar{E}/\bar{R}$, multiplied by the median R of 0.82 mm h-1, results in an estimated water-balance based evaporation rate of 0.19 mm h-1."

P13L30-31 redundant with figure caption

We have revised the sentence. It now reads: "The sensitivity analysis of the Gash model shows that parameter equifinality (Beven, 1993) occurs between S and $\bar{E}/\bar{R}$ (van Dijk et al., 2015), which implies in this case that an underestimation of S is likely to be compensated by overestimation of $\bar{E}/\bar{R}$

P16L30 the splash droplet hypothesis does not depend on high rainfall intensities. Its mention here is also unrelated to the rest of the paragraph.

We apologise for not having been clear enough in the original version of our manuscript. What we wanted to say in P6L30 was enhanced evaporation of rain droplets splashed from the tree canopy. And it is well known that the specific number and the average size of raindrops increase with rainfall intensity (Murakami, 2006). We have revised the sentence, it now reads: “Likewise, enhanced evaporation of rain droplets splashed from the tree canopy has been invoked as a possible mechanism allowing high interception losses (Murakami, 2006) but given the low rainfall intensities prevailing in the study area this is not likely to be important.”

Table 2 first column heading is mistakenly labelled “data-logger”

We have corrected this in the revised version.

Table 7, Fig 10 the meaning of Run 1 and Run 2 must be specified here

We have described Run 1 and Run 2 in the respective figure/table captions of the revised manuscript.

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