Hydrol. Earth Syst. Sci. Discuss.,
doi:10.5194/hess-2016-589-RC2, 2017

Hydrology and Earth System Sciences
Discussions

Interactive comment on "Measurement and modelling of rainfall partitioning by deciduous *Potentilla fruticosa* shrub on the Qinghai-Tibet Plateau, China" *by* Si-Yi Zhang and Xiao-Yan Li

Anonymous Referee #2

Received and published: 3 March 2017

General comments:

The manuscript (ms) reports on measurement and modelling of rainfall interception by a deciduous shrub species. Although several studies have already been published on the rainfall interception by deciduous shrubs, only in a few modelling was done. The specific characteristics of these cover-types, with drastic seasonal changes in canopy structure, could make this study quite useful and liable to provide relevant contributions on the subject. However, I think that the ms has several important shortcomings in the present form and that its focus/rationale needs to be improved and clarified. In my opinion, the ms needs a major revision before it can be considered for publication in HESS.

Specific comments:

1. The English is poor and the ms does not read well (sometimes it is hard to understand what the authors are trying to say).
2. In some cases, standard terminology on rainfall interception is not used correctly by the authors. Usually, "interception" is used to describe the interaction process between rainfall and vegetation while "interception loss" refers to its evaporation component (the amount of water retained in plants surfaces that evaporates back into the atmosphere). The authors use the term "interception" with both meanings resulting in a confusing text (e.g., page 2, line 10−12, "The gross precipitation reaches the canopy is redistributed to interception, stemflow and free throughfall"; page 18, lines 12−14, "The stemflow are the part of interception that run down the stem, so if the interception reduces, the stemflow would reduce"). The authors should check all text and differentiate between concepts using the appropriate terminology.
3. The description of the experimental site and vegetation characteristics needs further information and to be reorganized. In page 5, lines 16−17, the authors say "The coverage of shrub is 26%, and the height of shrub is 35.4 cm". How was this cover fraction evaluated? Does this value correspondent to the (average?) cover fraction of an individual plant or to the total percentage of cover area in the experimental site? Is the given value for shrub height a mean? What about other characteristics of individual plants (average number of stems per plant, mean diameter of each stem,...)? Although some of this data is presented in the ms, it is dispersed across several sub-sections (e.g., page 13, line 7). All this disperse information should be gathered together.
4. Concerning the measurement of rainfall, throughfall, stemflow and micrometeo-

rological variables, important information is missing. Location and type of the different gauges (tipping bucket and/or bottles) are not given. How were rain gauge locations chosen? How far from the edge of the patches were they placed? Did the gauges/bottles stay in fixed positions or were moved to new random positions each time they were measured? At what height were the micrometeorological sensors installed? Where were these sensors installed: above a shrub patch or in open areas between patches? What is the footprint for these data? Although micrometeorological data is from a previous study, it should be briefly described here. All this information is relevant to the study (measurement and modelling of rainfall interception) and should be presented in the ms.

An aerial photography of the site with the location of the used devices (rainfall, throughfall and stemflow gauges and the Bowen ratio tower), would be helpful.

5. To extrapolate stemflow measurements to the total patch area the authors used a stepwise methodology to derive a regression model. Which were the independent variables considered in this analysis? Though the final model has only three variables (page 7, eq. 1), were other structural features/rainfall characteristic considered?

   One of the variables included in eq. 1 is q, "the number of rainfall events that generate stemflow" (page 7, lines 5−6). How was q evaluated? In page 6, lines 14−16, it is stated that "Because it is very difficult to collect stemflow in the remote area, we did not measure stemflow for each rainfall events, and we measured and recorded stemflow eight times during the study period". Given this, how do the authors know the number of rainfall events that generate stemflow in each period?

6. It seems to me that the authors do not totally understand the sparse version of Gash's analytical model.

   (a) They say that the model requires several parameters and refer that "the free

   throughfall coefficient (p) and the canopy coverage (c)" are two of them (page 9, line 19). In page 18, lines 16−17, they re-state that p is a parameter of the model. This is not correct! The sparse version of the Gash model only requires c, the proportion of covered area relative to the total area.

   (b) Although not acknowledged, the authors mix the sparse version of the Gash model proposed by Gash et al. (1995) with the slightly different version presented later by Valente et al. (1997) (e.g., the amount of rainfall required to saturate the trunks (P_g") is only defined by Valente et al. (1997)).

   (c) Two of the most important parameters of the sparse version of the Gash model are $\overline{R}$ and $\overline{E}_c$ (and not $\overline{E}$, as it is said in page 10, line 3). According to Gash et al. (1995), these parameters are the mean rainfall rate and the mean evaporation rate during saturated conditions, respectively, and should be representative for the whole modelling period. Following Gash (1979), the method usually used to derive $\overline{R}$ is the average of all hours with rainfall equal or greater than 0.5 mm (two bucket tips) for the whole modelling period. How did the authors calculate $\overline{R}$? Nothing is said about this. The same happens with $\overline{E}_c$. The authors say they used data obtained with the Bowen Ratio/Energy Balance method (BREB) (page 11, lines 10−11), but do not say how.

   (d) Besides, it seems that the authors do not fully understand the meaning of $\overline{E}_c$. It represents the evaporation rate at which intercepted water can evaporate from a fully saturated canopy. But the authors say that $\overline{E}_c = \overline{E}/c$ (page 10, line 8). What is the meaning of $\overline{E}$ in the context of the sparse version of the model? If $\overline{E}$ is the actual measured evaporation rate from a fully wet vegetation and it is assumed that the only water source is the studied wet vegetation then this relationship is correct. Otherwise, it is not. It seems to me that the authors did not get it correctly. In fact, the authors say (page 23, lines 12−13) that "the average evapotranspiration in *P. fruticosa* shrub meadow was 0.11 mm h^{-1} during the experimental period". How was this

calculated? They also refer that "the hourly evaporation varied greatly in different time, ranging from -0.04 to 0.87 mm h^{-1}, controlled mainly by radiation" (page 23, lines $16-17$). However, during rainy/cloudy conditions (when the canopy is saturated), radiation is typically low and evaporation rate should not change much. This may suggest that the aforementioned values include periods where the vegetation is not fully wet, possibly not representative of saturated canopy conditions.

(e) The authors present three equations (page 10) to calculate the different components of rainfall interception (interception loss, stemflow and throughfall). Although based in the model version proposed by Valente et al. (1997) (again not acknowledge here), these equations do not describe the sparse version of the Gash model. As the authors say (page 9, lines $11-13$), one of the assumptions of the model is that $\overline{E}_c$ and $\overline{R}$ are assumed constant over the whole modelling period. However, while gross precipitation seems to be constant (since the j index is missing in P_g), but should not, $\overline{E}_c$ and $\overline{R}$ can change from storm to storm (because they have a j index). Moreover and contrary to the current practice, trunk storage capacity (S_t) is expressed in mm on a projected cover area basis (that is why it is necessary to multiply S_t by c in eq. 4 and 5). Whenever the units of a parameter are water depth (e.g., mm), it should be clearly stated in the text what is the reference area (e.g., ground area, covered area, ...).

(f) The authors present a new version of this model to adapt it to the studied deciduous shrub (page 10, line 17 to page 12, line 10). They assume that the evaporation rates from all the vegetation components (canopy, stems and inter-patch herbs) are the same. I am not sure if this is a realist assumption, since roughness and/or the micrometeorological conditions are seldom similar. Nevertheless, the requirements of the energy and water balances should be met. When all the vegetation is saturated, the measured BREB values ($\overline{E}$) represent the evaporation of the total area and not just of the

wet shrubs cover (see my previous comment 6.(d)). It seems to me that the authors did not took into account the water balance equation in their new modelling proposal (page 12, eqs. 8 and 9 and Table 2). How were these new equations obtained? An explanation is needed.

(g) Another important missing information is the "time-step" used to run the model. Although the model is storm-based, it is usually run assuming that each rainday is an independent rainfall event. Which procedure did the authors used?

7. The authors present results on the water storage capacity of leaves and stems (page 13, lines $12-16$) but do not explain how they were obtained. Only the method used to measure branch water storage capacity is described. Furthermore, they do not explain how ml were converted into mm (page 13, line 18). What is the reference area in the latter?

   The method used to estimate another model parameter (p_t) is not also described in the text.

8. Considering the characteristics of the studied vegetation (deciduous), it would be expectable the presentation of data on the time evolution of some parameters, namely canopy cover, and canopy and stem storage capacities. This would provide support on the need of using time variable parameters instead of the usual constant values. Besides, as the authors used different $\overline{E}_c$ and $\overline{R}$, it would be relevant to have a graph of their values along the modelling period. Neither of these variable parameters, nor the constant ones needed to run the sparse version of the Gash model are given in the ms.

9. The performance of the tested models were only evaluated by the total error (EE). However, EE per se does not evaluate the quality of model performance throughout the simulation period. For that purpose, authors should have applied

some additional measure, such as modelling efficiency (see Mayer and Butler, 1993, Ecol. Modelling, 68: 21-32).

10. As in many other studies, the authors have conducted a sensibility analysis of the sparse version of the Gash model. The question is: what is new about this? If they have used their own model version this could be interesting. What has been done is just a repetition that does not bring any new insight on the subject. Furthermore, the presentation of the results and their discussion are incomplete. Why is not shown a positive change of c in the graphs (Fig. 2)?

    In what concerns canopy cover (S), model sensitivity to this parameter was found to be very small which is not in accordance with most previous findings. However, the authors state that "the results in this paper are in accordance with [the] results" of other studies and will not be discussed in the ms (page 17, lines 6−8). On the other hand, they state that "the canopy storage capacity is the most important parameter in the interception modelling" (page 19, lines 11−12) which is contradictory. In my opinion, the authors should focus their work in what is new and relevant to the subject (modelling the rainfall interception process in a deciduous shrub cover).

11. **Minor comments:**

    (a) Page 3, line 13 & page 4, line 6 − replace "Analytical" by "Conceptual". The Rutter model is not an "analytical model".

    (b) Page 4, line 6 − what are semi-constants?

    (c) Page 4, lines 16−20 & page 4, lines 1−6 − the objectives of the work should be presented in a concise way. This text should be simplified and avoid repetitions.

    (d) Page 6, line 6 − specify tip sensitivity of rainfall gauge.

    (e) Page 6, lines 16−17 − there are only seven periods with measurements. Data from the 17th July 2012 is missing (Tables 3, 4 and 5). Authors should mention that in the text.

    (f) Page 6, line 18 − are stem diameter units correct (mm)? A stem with 3.4 mm seems too small to support any collecting device to measure stemflow.

    (g) Page 7, line 19 − what is the meaning of "10 min frequency data"? Do the authors mean "10 min average data"?

    (h) Page 8, lines 15−17 − this sentence should go to the discussion section.

    (i) Page 9, line 6 − replace (Gash, 1975) by (Gash, 1979).

    (j) Page 11, line 10 − the acronym BREB should be previously defined.

    (k) Page 12, lines 1 and 4 − the subscript j is missing in the symbols.

    (l) Page 12, line 15 − do the authors mean a storm with 50 years' return period?

    (m) Page 13, line 9 − according to eqs. 1 and 10, symbol for stemflow should be SF_v, not SF_b.

    (n) Table 1 − please remove the reference to P_g; this variable is not in table.

    (o) Table 3 − table not referred in text.

    (p) Figure 1 b) − I do not understand this graph. What do the authors want to show with it? Please explain.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., doi:10.5194/hess-2016-589, 2016.