

## ***Interactive comment on “Modelling runoff at the plot scale taking into account rainfall partitioning by vegetation: application to stemflow of banana (*Musa spp.*) plant” by J.-B. Charlier et al.***

**J.-B. Charlier et al.**

jb.charlier@gmail.com

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We would like to thank the Editor and the three reviewers for their constructive comments regarding this manuscript. Enclosed, the author responses (AC) to each referee comment (RC).

A) Comments made by reviewer 1 (M. Sraj)

A.1) General comments

RC 1: The original contribution of the study needs to be clear in the Introduction and

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## Conclusions.

AC 1: Rainfall partitioning by plant canopy and its effect on runoff and infiltration has been largely studied experimentally. Under tropical climate, Cattán et al. (2007a, 2007b, 2009) have shown that the processes of interception and stemflow by banana plant fed preferential drainage and surface runoff pathways at the base of the plant. The interception and stemflow processes are rarely represented in distributed hydrological modelling. The interception is often modelled using complex approaches, as for example the Rutter (1971) model used to simulate rainfall interception in the physically-based distributed SHE model (Abbott et al., 1986), which needs meteorological data and structural parameters often unavailable due to the complexity of the conceptual scheme. In this setting, our aim is to develop a simple hydrological interception/stemflow model, especially adapted to the banana plant, based on physical and geometrical concepts (accounting for the structure and properties of the plant) rather than on empirical concepts (i.e. Rutter (1971) or Gash (1995) models), and having few parameters. For that, the MHYDAS model proposed in this paper has four parameters easily available on the field. The original contribution will be mentioned clearly in the Introduction and the Conclusion of the revised version of the manuscript.

RC 2: Some information about banana plants on the plot, such as plant density, plant height, stem diameter, LAI etc. might be useful for further comparisons. Are these characteristics changing with time? If yes, was that considered in the model?

AC 2: The characteristics and properties of banana plants given in section 2.1.2.3 will be detailed in the revised version : The banana plant has an impluvium shape. Its crown is made of verticillated leaves with a petiole and a midrib supporting two wide laminae. The Cavendish cultivar planted on the plot can reach 3 m in height; the average length and width of the leaves are 1.74 m and 0.72 m, respectively. As stated in section 2.1.2.3, on a banana plot planted in a square design (2.35 m × 2.35 m), the measured values of LAI and ASf given by Cattán et al. (2007a) for a banana plant were: LAI = 3.2 for a full-grown banana plant, and ASf = 0.047 m<sup>2</sup>. These characteristics do

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not change after flowering (around 6 months after plantation) when the banana canopy exhibited a maximal leaf area, the canopy from one banana plant overlapping with the adjacent plants. The model did not account for the lower stemflow effect before flowering because the selected rainfall events occurred after this period.

RC 3: I am missing uncertainties of the all reported results (e.g. standard deviations, standard errors) in the text and in the tables. I would also recommend to include model statistics such as the RMSE.

AC 3: We agree with this comment. The RMSE will be added in Table 3 in the revised version.

## A.2) Specific comments

RC 1: Page 4309, lines 18, 19: Definition of interception loss  $E_i$  "interception  $E_i$ , which is the water stored in the canopy and evaporated mainly before it reaches the soil" should be corrected – that water never reaches the soil, it completely evaporates. Otherwise the water balance equation (equation 1) is not valid.

AC 1: We agree with this comment, and the text will be modified as follows in the revised version : "interception  $E_i$ , which is the water stored in the canopy and completely evaporated before it reaches the soil".

RC 2: Page 4316, lines 6, 7: I suggest a short explanation of the assumption that soil is all the time close to saturation. Probably because of humid tropical climate as explained later in section 3.

AC 2: The chosen production function is only valid for soils under wet conditions. The text will be modified as follows in the revised version (Section 2.2.1): "Herein, we assume that the soil is close to saturation at the soil surface as often observed under wet climate or in wetland areas. In fact, in a permanent humid context, the initial soil moisture is always close to saturation. Therefore, we considered a constant infiltration capacity at the soil surface equal to  $K_s$ . In this model, the simple production function

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separates rainfall PR into surface runoff (or stormflow) SR and infiltration IR using the  $K_s$  threshold. Consequently, the production function is valid only for soils always close to the saturation state and without any influence of the rise of the water table; the following case study located under humid tropical climate respects these conditions."

RC 3: Page 4316, line 16: Equation 16 is incorrect. PR/ $K_s$  should be more than 1!

AC 3: We agree with this comment and the equation will be modified.

RC 4: Page 4317, lines 15, 16: The simulated discharge  $Q_s$  is not presented in Figure 3. Maybe do you mean Figure 2? Otherwise Figure 3 should be corrected.

AC 4: Figure 3 will be replaced by Figure 2 in the revised version.

RC 5: Page 4322, lines 14, 15: The citation Nash and Sutcliffe (1970) is not in the References.

AC 5: The following citation will be added: Nash JE, Sutcliffe JV.: River flow forecasting through conceptual models part I: a discussion of principles. J. Hydrol., 10(3), 282–290, 1970.

RC 6: Page 4346, Figure 7: The figure is not clear enough. It is too small. The differences between measured and simulated results are not evident enough.

AC 6: We agree with this comment. Figure 7 will be shared into two Figures 7a (for calibration) and 7b (for validation) on two pages in the new manuscript.

### A.3) Technical corrections

RC 1: Page 4310, line 17: Citation is not complete, it should be (Gash et al., 1995).

RC 2: Page 4312, lines 15, 16, 18, 20: Symbols e.g. AR ,  $P_{sf}$  , ANR etc. should stay after their explanations (e.g. area, fluxes, etc.). That should be corrected throughout the whole document, also in figure captions. RC 3: Page 4325, line 4 and 29 and page 4327, line 17 and page 4347, figure caption: Units  $L s^{-1}$  should be written as  $l s^{-1}$  (liters per second).

AC 1, 2, and 3: We agree with the three comments and the corresponding modifications will be made in the revised version.

B) Comments made by reviewer 2 (N. van de Giesen)

B.1) Major remarks

RC 1: Surface runoff is, as the authors emphasize, a complex process. What remains a bit questionable in the presented model is the routing part. It is assumed that all “produced” surface runoff also reaches the bottom of the lot. Given the steep slope, this may be a bit OK but it remains somewhat doubtful that water running off at the base of a banana stem makes it all the way to the outlet. Such water is likely to run on to patches where the infiltration capacity has not yet been reached unless there is a well developed network of rills connecting the stems to the outlet. Does such a network exist? Have the authors any qualitative observations on flowpaths or was that difficult due to the presence of litter, etc.? So the main question is how redistribution within the plot is/should/could be accounted for? I appreciate that a model needs to have focus and few parameters to have analytical value so I would not recommend a much more complex routing scheme but the issue should be addressed.

AC 1: Globally, the conceptual scheme of the model was based on observations of flowpaths carried out on the field as well as by video monitoring during rainfall events (see Cattán et al., 2009). In the case of a tilled plot, a developed network of rills connecting the stems to the outlet can be observed easily. In our case (no tillage), this network was less marked (because the network is more sinuous in the interrow, and thus less embedded), but was still observed. The network connects the zones of runoff propagation downstream of the pseudostem and also on drip zones between banana plants. In this setting, as stated by Reviewer 2, infiltration of runoff during the transit in the rill may occur. But it may occur only for the runoff volume for which runoff intensities are lower than the saturated hydraulic conductivity  $K_s$ , because, as assumed for the rest of the plot, the initial soil moisture state is always close to saturation (see

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Specific comment 2 of Reviewer 1 on soil saturation conditions). Thus, we have made the hypothesis that the runoff production follows the same process in the rill network and on the rest of the plot, leading to choose a lumped approach for modelling. We assume that our approach with MHYDAS model is well adapted to simulate runoff in such a context with a parsimonious model. This issue will be added in the revised version in order to justify the choice of a lumped modelling approach.

RC 2: Perhaps for this open discussion (not necessarily in the final paper), the authors may want to speculate on why banana has such a strong impluvial structure. Given the high demand bananas have for nutrients, perhaps concentrating water at the stem bottom increases weathering. In any case, it is difficult to imagine the advantages of such a structure unless there is also preferential infiltration along the root system. I hope the authors have sufficient direct observations to comment on whether preferential flow into the soil near the stem base occurs.

AC 2: Wild species of banana grow spontaneously in glade or at the edge of forest in warm and wet environment. We can speculate that the morphology of the banana plant is adapted to these conditions with a large canopy maximizing interception of light and notably water given the high need of the plant (This later effect was reported by Navar (1993) and recently by Li et al. (2008) in semi-arid region). An additional effect is probably to bring to the foot of the stem additional nutrients given the higher concentration of solutes in stemflow than in throughfall (Andre et al., 2008). Concerning banana plants, some authors have shown that preferential drainage of water occurred under the banana stem due to abundant stemflow (Cattan et al., 2007b), generating intensive leaching of nutrients (nitrate and potassium) for the root zone (Sansoulet et al., 2007).

## B.2) Minor remarks

RC 1: There are also other mechanisms that allow for runoff to occur while rainfall intensities are less than  $K_s$  measured at points during a dry period, such as air inclu-

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sion in larger pores and crust/mud formation during rainfall. These mechanisms are especially relevant under high intensity tropical rainstorms. The fact that in this case mainly during rainfall events of intermediate strength the runoff is higher than would be expected, points in the direction of stemflow. A brief discussion of the different mechanisms may be in place, however.

AC 1: Rainfall redistribution by plant canopy is not the only process involved in runoff when rainfall intensities are less than the  $K_s$  measured on the plot. Two other processes may occur. The first one is soil crusting which reduces  $K_s$  value between infiltration measurements and runoff assessment. The second is related to air trapped in the pores in the top soil since the runoff events in the channels are short and rapid. In our case on Andosol, given the high cohesion of soil aggregate, crusting was not observed on the plot. Concerning air inclusion, we think it was unlikely because additional observations during double ring infiltration measurement performed on the same type of soil in 2006 showed that permanent regime was obtained after a few minutes. However this later hypothesis should be investigated further. This brief discussion will be added in the revised version.

RC 2: The routing model chosen is linear whereas it is likely that non-linear effects are relevant. Could you elaborate briefly on the reason behind choosing a linear model and on the possible (dis)advantages?

AC 2: Generally, the full non linear equations of Saint-Venant are used to model flood routing. The choice of a simplification of Saint-Venant equation (kinematic or diffusive wave) is often made on pragmatic grounds in that a full Saint-Venant equations needs complex numerical approaches for the resolution of the differential equations, and would be too computationally intensive. The modeller encounters the questions of construction of finite-difference or finite-element systems (Marks and Bates, 2000) and methods for solving them (Cunge et al., 1980). In order to avoid numerical instabilities, the best compromise between the complexity of non-linear model and the simplicity of empirical ones, was a linear diffusive wave as a simplification of the full Saint-Venant

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equation. Generally, the diffusive wave model has been largely used for flood routing (see Moussa and Bocquillon (2009) for a review). These arguments will be given in the revised version to justify the choice of the transfer function.

RC 3: The optimization procedure follows a certain logic but it also seems somewhat arbitrary. Why was a manual two-step optimization used instead of, say, an exhaustive search over the low dimensional parameter space? Have other approaches been tried as well?

AC 3: In the literature, several approaches were developed to automatically calibrate a model. However, automatic calibration is generally used in conceptual models, where parameters cannot be measured or don't represent any physical measure. In our case, the two parameters  $K_s$  and  $\beta$  have a physical sense, thus a manually procedure allowed to better visualize the model behaviour, testing it on a acceptable range of values based on measurements. Moreover, performances of our model with this kind of manual procedure give good results ( $NS > 0.8$ ) and are equivalent to automatic procedure.

RC 4: The standard deviation given for  $K_s$  is extremely low: 7.6 mm/h for an average  $K_s$  of 75 mm/h, especially because the range is more as one would expect (33-200 or so mm/h). Please check.

AC 4: The numerical values of mean  $K_s$  deduced from a controlled suction disc infiltrometer on the plot are correct (75 mm/h with a standard deviation of 7.6 mm/h, from Cattani et al., 2006). This relative homogeneity is probably due to the  $K_s$  measurements which took place only few months after plantation. The suction disc infiltrometer only measures  $K_s$  on a small surface (8 cm diameter cylinder).  $K_s$  measurements performed using the double ring infiltration method (Bouwer, 1986) in 2006 on the same type of soil show a mean  $K_s$  value of 67 mm/h with a confidence interval of [50,85] mm/h. More generally, on Andosol, we can have usually a larger range of values of around (30 to 200 mm/h) according to agricultural management (Dorel et al. 2000). These precisions will be given in the revised version.



RC 5: The derivation of the model and its equations is sometimes a bit too pedestrian. A matter of taste but steps such as presented in eq. 11 can be omitted.

AC 5: Corresponding modifications will be made in the revised version.

RC 6: The text is well written but every now and then some typos seem to occur. The authors may want to go through the text carefully one more time. Examples: P4308 | 10: “related” instead of “relative” P4313 | 5: “partitioned” instead of “shared” P4324 | 23: “rainfall” or “runoff” would be better than “flood” (same in figures!)

AC 6: We agree with the comments, and the corresponding modifications will be made in the revised version.

C) Comments made by reviewer 3 (Dr. Kirnbauer)

C.1) Some suggestions for further improvement:

RC 1: Further information should be given on the plot. How old is the banana plantation? And what was the previous plant cover?

AC 1: Banana was planted on 21 February 2001 and the previous crop was banana, followed by 8-months fallow. This precision will be added in the revised version.

RC 2: How does the surface of the plot look like? (See the comment of N. van de Giesen and see some statements in the conclusion, p. 4332 line 15ff). What is the proportion of open soil to that covered by the leaves of the banana plants? (See the comment of M. Sraj)

AC 2: Please see our responses to the first comment of Reviewer 2, and to the second comment of Reviewer 1.

RC 3: Are there macropores or preferential pathways in or under the soil?

AC 3: Generally, Andosols may exhibit preferential flow patterns given the hydrophobic nature of their constituents (Clothier et al., 2000; Poulénard et al., 2004). In the studied

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plot, Andosols are strongly porous media with a total porosity reaching of 71 and 81% in hA and hB, respectively (Cattan et al., 2007b). Concerning water pathways under the soil, Charlier et al. (2008) have shown that in this same pedoclimatic environment, lateral subsurface flow is limited in favour of percolation through the water table. This is particularly true because Andosols in the studied zone are developed on a very porous formation of ashes mixed with lapillis.

C.2) Further information should be given on the measurements

RC 1: What was the registration interval of the rain gauge (if more than one, how many)?

AC 1: An error relative to the number of raingauge was done on the text p4324, line 11, because only one rain gauge was used to measure rainfall intensities on the plot. It was done using a tipping-bucket raingauge with a sensitivity of 0.2mm of rain per tip. This correction will be made in Section 3.2.

RC 2: Are long term observations of rain intensity available? And if yes, how do the calibration/verification events fit to the long term observations (maximum and mean intensity, probability distribution)?

AC 2: The probability distribution of rainfall depth and intensities are not available on the plot. However, throughout the experiment of Cattan et al. (2006) from which rainfall/runoff data presented in this paper are issued, there was a total of 2000 mm of rainfall depth during a cumulative period of 5.5 months (between September 2001 and April 2002); this period includes the main rainy period of a hydrological year. Those authors showed that rainfall events with a rainfall depth superior to 10 mm represented 4.5% of the 862 recorded events, and that rainfall events with a rainfall intensity superior to 72 mm/h (approximately equal to the mean Ks of the soil surface) represented 10.3%. Thus, we can see that the rainfall regime consisted of frequent light falls. For our paper, we selected events which were systematically superior to 10 mm depth, representing 530 mm of cumulative rainfall depth (i.e.  $\frac{1}{4}$  of the total rainfall depth of the

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period). Consequently, we assume that the calibration/validation events of our paper fit with the main rainfall events occurring in a hydrological year.

RC 3: In Table 2 an additional column “duration of event” or “mean rain intensity” should be given.

AC 3: These parameters will be given in the revised version.

### C.3) Specific comment

RC 1: On p. 4327 line 1 we find the statement “To improve the understanding of stemflow production . . . ” and in the following sections 4.1f the model behaviour is discussed. This improves the understanding of the model, not of stemflow production. If ever, the understanding of the process of stemflow production could be improved by stemflow measurements (as M. Saraj states). Maybe, these measurements could be made in a subsequent project.

AC 1: We agree with the comment. Indeed, results of this paper improve the understanding of stemflow modelling not of stemflow production, even if they are in accordance with the experimental studies of Cattani et al. (2007a, 2009) at the plant scale. This will be modified in the revised version.

RC 2: Typing error on page 4332 line 19: . . . should be taken into account (instead of taking). I strongly agree with M. Saraj’s statement that Figure 7 is too small!

AC 2: We agree with the comments and corresponding modifications will be made in the revised version (as explained in our comments for Reviewers 1 and 2).

### D) References

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