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Hydrology and Earth System Sciences Discussions

Interactive comment on "Physically-based modelling of hydrological processes in a tropical headwater catchment in Benin (West Africa) – process representation and multi-criteria validation" *by* S. Giertz et al.

Anonymous Referee #1

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I have read the reply to my review but am not sure if the authors plan to adjust anything in the article itself to accommodate the comments, which still is the idea behind peer review. Basically, we are looking at a model with way too many parameters. This may be justifiable if it serves a learning purpose but it would not be acceptable if the model is published as if it really properly represents all major hydrological processes in the catchment. The two main issues I had with the article should be addressed, which I will repeat here.

1) Where do all parameter values come from?

To maintain some scientific rigor, please note for each parameter how it was arrived at. Was it measured? If so, how? If it was taken from the literature, please provide sources so we know if it are effective parameters or plant/soil specific parameters. If they were obtained through calibration, what was the range of allowed values and how did the calibration take place? Temporal and spatial resolution, especially for rainfall intensity?

2) Surface runoff can not be modeled in the way presented here.

The authors assume the reviewer does not understand but I am not sure that is such a good assumption. The slope units are treated as one dimensional but still have lengths of tens to hundreds of meters. Therefore runoff dynamics can not be neglected.

- Runoff can simply not be averaged over all Ksat values in the way it is done here. One has to take the spatial organization into account. Only when Ksat values remain the same along the flowpaths, which is definitely not the case, can one average in the way as done here.
- The main model improvement seems to be that 1-D SIMULAT is linked across the catchment units. But if one unit is taken as a point, any runoff from that point is immediate and, as such, flows immediately into the lower unit. If a delay is built into the model, it should be given and justified.
- Most important is that no account is given of re-infiltration within one unit. When you stand on a slope during the passage of a line squall (quite an interesting thing to do), you see water everywhere on the surface. Air is locked in the macropores and on top of the matrix is a sludge that hardly infiltrates. But as soon as the squall has passed, it is as if a switch is turned on as macropores become active and the full matrix conducts water. All the water standing on the surface at that moment (10-20 mm), hardly makes it a few decimeters further down the slope. These dynamics are not at all captured here.

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- When we compare the independent measurement of the surface runoff component (based on eq 12), with the modeled runoff, we find a R2 of 0.02, hardly a cause to cling to the presented representation. An average runoff coefficient would give equally good results. Perhaps simply acknowledging that surface runoff is not captured satisfactorily by the model would be better.

In summary, one can not brush aside a peer review with the simple comments given by the authors. The article will have to be adjusted or good arguments have to be given why no changes need to be made. The limits of this modeling exercise should be made explicit (not good at deeper soil moisture, does not really represent surface runoff properly).

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 3, 595, 2006.