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Hydrology and Earth System Sciences Discussions

Interactive comment on "Predictions of rainfall-runoff response and soil moisture dynamics in a microscale catchment using the CREW model" *by* H. Lee et al.

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The authors present a model which aims to describe water dynamics at the catchment scale. The model is based on the idea to aggregate small scale heterogeneities to homogeneous entities at the larger scale where it is assumed that meaningful effective properties can be found. The catchment is subdivided in blocks of REWs each composed of distinct regions: channel-, saturated-, unsaturated zone with given geometry The related balance equations for mass and momentum are formulated and the main task is to find suitable 'closure relations' which integrate small scale parameters to simpler descriptions and which establish the relation between different state variables in order to reduce the number of unknowns to makes the model operational. The model parameters are calibrated using a distributed small scale model for the Weiherbach catchment and/or direct measurements of water contents and stream flow.

My own experience is more at the smaller scale and I think it is obvious that at the catchment scale the model structure has to be more holistic. I appreciate that the authors meet the challenge to go for a model which is thought to be consistent while the model parameters are thought to be related to some physical properties of the investigated material.

As with all models, the question is where can we find the required parameters and what is the predictive power of the model. I have a few more general remarks in this direction:

- The model relies on an enormous bunch of empirical parameters where many of them have no clear physical meaning. They have to be obtained by calibration based either on measured time series of state variables or on the results of a detailed small scale model. Actually the authors did both and the discrepancies especially of the various α and β demonstrate that these parameters can hardly be identified. This should be critical for the predictive power of CREW. The validation based on the soil moisture data in Fig.11 is actually not really convincing since the measured water saturations cover almost the complete possible range
- The discussion of preferential pathways and their representation at the larger scale through effective or 'textural' properties (page 9,10) and the introduction of a macroporosity factor (page 18) suggests that the effects of preferential flow is captured by the model. However, this is actually not the case since the hydraulic properties in the unsaturated zone are chosen to be homogeneous. The deformation of the hydraulic conductivity function near saturation does not imply preferential flow. I think that preferential flow paths are structural units at the REW scale which are still relevant for mass fluxes and hence they should be consid-

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ered explicitly in the model. In this way they could actually behave like preferential flow paths which is not the case when they are lumped into an effective hydraulic description. It would be worthwhile to discuss this point in more detail.

- In the presented model the unsaturated zone is represented by one single block with an effective hydraulic conductivity which is considered to be a function of the water saturation averaged over the whole block. Doing so, I think some essential characteristics of the unsaturated zone are just dropped: The capacity of water storage in the top soil and the attenuation of the input signal caused by precipitation events at the soil surface. To preserve these essential features, I think it is indispensable to represent the unsaturated zone with a certain spatial resolution at least in the vertical direction. In the most simple case this should be possible without increasing the total number of parameters of the present model. So why don't you do it?

Detailed remarks

1. Infiltration   With equation (13) it is assumed that the minimum of infiltration capacity is the saturated hydraulic conductivity of the material and this is also demonstarted in the first numerical test further below. Is this realistic? I think in most natural systems the infiltration capacity decreases significantly as soon as the soil is completely saturated.
2. Evaporation   in eq.15: is the factor $(1 - M)$ missing before e_p? and in eq.16: what exactly are the parameters m, c and d and where do they come from?
3. I do not see how the parameters α^{uc} and α^{u}_{wg} are linked to the spatial variability of the conductivity.
4. Saturated surface area: Is there any intuition for the meaning of the different β in eq.26?

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5. Hydraulic conductivity: I do not see the relation between the different averaging results (Fig.6) and equation 28.

6. Simulations: It is not completely clear how you get the values for all α and β 'during the upscaling procedure'. Probably it would be helpful to explain this already at the point where these parameters are introduced (and where the reader is puzzled the first time).

7. Manual Calibration: Is this mainly governed by 'Event 2' (Fig10)? because this seems to be the most important day within a one-years period where the model parameters are most sensitive.

8. Some of the figures have to be reworked so that they are better readable. In Fig. 1 super scripts are not clear, perhaps you can add the different y here. There is still room to enlarge this important figure. Fig4a: use different font. Fig.4b: what are the different symbols?

9. Use spell checker

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 3, 1667, 2006.