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Hydrology and
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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.

H. Lee et al.

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The authors appreciate the valuable comments from the referee H. Vogel for the improvement and thorough discussion of the materials presented in the paper. We present our response to the each comment made by H.-J. Vogel in the following lines.

Comment 1: 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 945; and 946; 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.

Response 1: We can imagine that for a soil physicist a hydrological model such as CREW relies on a “bunch” of empirical parameters that cannot really be determined at the scale of interest. However, the reviewer should take into account that normally hydrological models for the mesoscale are purely conceptual models, where all parameters and states are non-physical! The REW concept was invented in 1998/99 to overcome this problem. Related models such as CREW suffer certainly from a lot of drawbacks, however, from our perspective they do point in the right direction. We agree especially that the many of the alpha-type parameters are simply calibration parameters and that one should (as we did in studies that will hopefully appear in near future Advances in Water Resources, e.g., Lee et al., 2006) investigate whether these parameters could be simply omitted to end with a model that is more parsimoneous.

However, our present study intends to show mainly that models based on the REW approach may be used to model real world catchments (something that was doubted for quite a long time in the hydrological community). The Weiherbach catchment is of special interest in this case, as it was extensively investigated and offers a set of distributed soil moisture observations based on TDR, additionally to streamflow data etc. Furthermore the model CATFLOW is available for this catchment (the model subdivides the catchment into 169 2d hillslopes with an interrelated drainage network, process representations are 2 d Richards-eq., Penman Monteith for ET, Diffusion-Wave approximation of Saint Venant for overland flow and river flow). By representing the typical spatial patterns of soils, landuse, macroporosity CATFLOW has been shown to yield simultaneously reasonable predictions of streamflow, soil moisture dynamics (square correlation coefficients between 0.6 and 0.7) and ET for a simulation of app. 1.5 years (Zehe et al., 2001, Zehe and Blöschl, 2004, Zehe et al., 2005). Figure 11 in the old version (= Figure 9(b) in the new version) (which compares point measure-

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ments of soil moisture and soil moisture simulated with CREW) should be judged together with Figure 9 in the old version (= Figure 9(a) in the new version) that compares the time series of volume averaged soil moisture from the simulation with CATFLOW with the time series of CREW. The time series of averaged soil moisture simulated with CATFLOW is the best guess regarding how the true average soil moisture in the Weiherbach may have evolved during this period (as it is simply the average from the distributed model structure that reproduced observed discharge, ET and large parts of the point scale soil moisture data at 61 sites, and the model represents the spatial information we have there and is driven by observed boundary conditions). These two graphs together hint that the time series of average soil moisture simulated with CREW is at least consistent with the finer scale model. Provided one accepts that CATFLOW gives the best estimate of how the true average soil moisture may have evolved at that scale, then our study suggests that CREW does a reasonable job in reproducing the same patterns. The new manuscript better explains our reasoning in this context, especially when read together with the companion paper (Zehe et al, 2006).

The beta-parameters are related to REW scale capillary pressure-saturation relations and an REW scale unsaturated hydraulic conductivity curve. Both relations are necessary for describing capillarity driven processes such as capillary rise but also recharge to the saturated zone at that scale. We agree with Hans-Joerg Vogel that with current measurement techniques these curves cannot determined experimentally yet. We selected a Brooks and Corey functional form because this is the most parsimoneous approach. In the companion study Zehe et al. (2006) suggest that we might assess the parameters of these relations based on numerically simulated drainage and wetting experiments at the REW scale. The idea is to represent the typical structures and patterns (i.e. the soil catena with REV scale soil hydraulic parameters, spatial distribution of macropores) in a 2d simulation domain and impose either an increasing suction starting from a saturated domain (drainage) or a constant head of zero starting from a "dry" domain (wetting case). The next step is to link the spatially averaged outflow/inflow to volume saturation for deriving the unsaturated hydraulic conductivity curve and volume integrated matric potential to volume integrated saturation. They showed (a) that even in strongly heterogeneous media homogenous inflow/outflow was observed after an averaging length of 3 time the correlation length and (b) different patterns of soils inside the domain yield different parameters for the REW scale capillary pressure-saturation relation and the unsaturated hydraulic conductivity curve. They showed furthermore that the beta parameters obtained for the setup of a typical Weiherbach hillslope were close to those Lee et al. (this paper) obtained during the manual calibration. Although this approach might only work for weakly heterogeneous media, we think that the idea of using REV scale distributed models to support this upscaling procedure is reasonable and currently the only way because (a) we might use all the data obtained for the REV scale, (b) represent important structures and patterns and (c) do not average REV scale parameters but fit parameters such that they reproduce simulated average dynamics. This is of course not an adequate substitute for real-world observations at that scale, but we think it is currently the only way. We agree with Hans- Joerg Vigel that most of the other parameters have to be calibrated manually or estimated using Monte Carlo techniques, but such is life in hydrological modeling at this time! Nevertheless, the REW approach has the potential to move away from this calibration based approach, as soon as measurements at the REW scale become available.

Comment 2: 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 considered 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.

Response 2: The authors agree with the referee that the preferential flow paths are structural units at the REW scale. As such, we need to introduce another zone for the preferential flow paths with new mass and force balance equations to describe the interrelation of mass fluxes and forces among neighbouring zones explicitly. However, the main aim of the paper with respect to the hydraulic conductivity function is to come up with the functional relationship for the hydraulic conductivity (defined at the REW scale) as a function of soil moisture content (defined at the REW scale) that incorporates subgrid heterogeneity, including the presence of macropores, as specified above. In their study on flow and transport in the Weiherbach catchment Zehe et al. 2001 showed that macropore flow affects infiltration/runoff generation at the plot, slope and catchment scales in the Weiherbach and included the effect of spatially distributed macroporosity on infiltration with a simplified threshold approach. In this study and in their derivation of REW scale constitutive relations for capillary pressure-saturation (Zehe et al., 2006), they included information on the typical spatial distribution of macropores at the slope scale in the same manner. By the way, the inclusion of another zone for the preferential flow paths in a REW is the topic of another paper by Zhang et al. (2006) that already appears in the same special issue as this one.

Comment 3: 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?

Response 3: We agree with the referee that this is a weakness of the model. To answer this question we must again refer to the history of the REW approach. In the original papers the u-zone is just a single control volume. The aim of this paper was to develop the necessary relationships for the implementation of the REW balance equations starting from the original formulations made by Reggiani et al. (1998, 1999). Within future studies one should at least subdivide the u-zone into something like an upper root zone and a sub soil zone. This subdivision of the unsaturated zone into several layers along the vertical direction would take the effect of both the capacity of water storage in the top soil and the attenuation of the input signal caused by precipitation events at the soil surface into account. This will definitely improve the model performance by better describing the processes happening in the unsaturated zone (controlling the time delay of the water flow at unsaturated zone, different water uptake rate by the roots with the depth and so on). At the present stage, the referee's suggestion is a valuable one but is beyond the scope of this paper. In passing, we state that the subdivision of the unsaturated zone into layering has already been completed in the context of an ecohydrological study that also uses the REW model, which is soon to be submitted for publication in Water Resources Research (Schymanski et al., 2006).

Comment 4: 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 demonstrated 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.

Response 4: Equation (13) is based on the standard Green-Ampt equation as shown in the text. We want to remind the reader that many field measurement approaches (used by constant head permeameter community) are based on the assumption that the end -infiltration rate is mainly determined by the saturated hydraulic conductivity, so our approach cannot be too bad and is in fact widely used in hydrology. For the case of a fully saturated soil, the reviewer is right that infiltration rates will be much smaller than Ks, if there is bedrock with lower conductivity below, otherwise not. However, in the case of a fully saturated u-zone in the REW, eq. 13 does not determine infiltration any more (depth of u-zone yu =0). As water is incompressible, infiltration is due to mass conservation determined by the recharge rate and this in turn will be determined by the exfiltration rate to the stream. So it could even go down to zero.

Comment 5: Evaporation in eq. 15: is the factor (1-M) missing before ep? and in eq. 16: what exactly are the parameters m, c and d and where do they come from?

Response 5: (1-M) is not missed before ep, because we assume that there is evaporation from the soil over the vegetated area. So, the authors think the exclusion of vegetated area in the calculation of probable maximum evaporation rate from the soil is not right.

m, c, and d are pore size distribution index, pore disconnectedness index, and diffusivity index respectively and they are from Brooks and Corey (1966) and Eagleson (1978). m is from the Brooks and Corey (1966) expressions relating the capillary pressure in the unsaturated zone to the saturation: Psi=Psib*(su)**(-1/m) (see eq. 4 at Eagleson 1978) where Psi is soil matric potential head of unsaturated zone, Psib bubbling pressure head, and su saturation. c defines the relation between hydraulic conductivity K and the soil saturation su: K=Ks(su)**c (see eq. 7 at Eagleson 1978) where Ks is saturated hydraulic conductivity. d=c-(1/m)-1 (see eq. 12 at Eagleson 1978).

Comment 6: I do not see how the parameters auc and auwg are linked to the spatial variability of the conductivity.

Response 6: For auwg, if Ks (saturated hydraulic conductivity) is assumed to follow a lognormal distribution as discussed at Appendix A, then, by following the steps presented at Appendix A, auwg =2*pe/pi *exp(-0.25*sigma**2) where sigma**2 is the variance of logarithm of Ks and pe is the dimensionless exfiltration diffusivity. Based on auwg =2* pe / pi *exp(-0.25*sigma**2), auwg is linked to the spatial variability of the hydraulic conductivity.

For auc, the similar derivation procedure that applied to auwg has been adopted by assuming a lognormal distribution for Ks. The details are presented at Rogers (1992), so the derivation procedure is not presented in the paper. auc is linked to the spatial variability of the conductivity in the same way as auwg is linked to that.

Comment 7: Saturated surface area: Is there any intuition for the meaning of the different beta in eq. 26?

Response 7: The differentiation among beta1wo, beta2wo and beta3wo in relation to the physical and/or landscape properties has not been examined yet. The three betas together define the relation between saturated area and the depth of saturated zone. It may not be possible to look at them separately and estimate them separately. At the moment, we don't have any intuition for the meaning of the different betas.

Comment 8: Hydraulic conductivity: I do not see the relation between the different averaging results (Fig. 6) and equation 28.

Response 8: The three different averages are three different ways estimating REW scale average saturated hydraulic conductivity (first factor in eq. 28) from the point scale values within the simulation domain.

Comment 9: Simulations: It is not completely clear how you get the values for all alpha and beta '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).

Response 9: We agree with Hans-Joerg Vogel that this has to be clarified. The upscaling procedure in this study and introduced in the companion study (Zehe et al., 2006, HESS) are both based on the same idea, to employ a fine scale model and associated functional forms and derive the parameters such that they fit the average simulated dynamics. In this approach we used natural boundary conditions (precipitation climate series) i.e. the upper boundary condition was atmospheric and there was free drainage the lower boundary. For the hydraulic conductivity curve we used different averages of REV scale values as estimators. The parameters obtained within this approach were not acceptable during the simulation. In his study Zehe et al (2006, this issue) used artificial boundary conditions to run the numerical drainage and wetting experiments, which are much better defined and avoid the internal dynamics switches from wetting to drainage. Their approach yielded much smoother curves (although they are hysteretic) and parameters that were close to the ones obtained here using manual calibration. This fact is explained better in the new manuscript. Additionally we will add the following statements to clarify:

- For the Weiherbach catchment, aro is 1.0 from Figure 3. (after Eq. 18 in section 3.2.4.)

- For the Weiherbach catchment, estimated values for a1os, a2os, a3os are 0.01, 0.60, and 0.31 respectively. (after Eq. 24 in section 3.2.5.)

- For the Weiherbach catchment, beta1wo=0.71, beta2wo=1.79, beta3wo=0.92 from Figure 5. (after Eq. 26 in section 3.3.1.)

- For the Weiherbach catchment, beta1Psi=0.97, beta2Psi=0.64 from Figure 6(a). (after Eq. 27 in section 3.3.2.)

- From Figure 6(b), estimated beta1K and beta2K values for the Weiherbach catchment are 3.0 x 10-6, and 1.68 for KGEO, 8.0 x 10-7, and 1.63 for KHA, and 7.0 x 10-7, and 1.49 for KAH. (after Eq. 28 in section 3.3.3.)

Comment 10: Manual Calibration: Is this mainly governed by ‘Event 2’ (Fig 10)? because this seems to be the most important day within a one-years period where the model parameters are most sensitive.

Response 10: The manual calibration generating QsimM has been focused on capturing the biggest flow (Event 1) and the base flow over the whole year. So, Event 1 could be the most important event for the parameter estimation during manual calibration.

Event 2 is the second largest rainfall runoff event observed from 1991 to 1998 in the Weiherbach catchment. Hence, it may be regarded as a “ validation” event.

Comment 11: 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. Fig 4a: use different font. Fig. 4b: what are the different symbols?

Response 11: We will endeavour to improve Fig. 1 and 4. In the caption of Fig. 4b, the following words have been added: “where eos, S, and Psi are seepage outflow, saturation degree over the entire volume of soil, and the average matric potential head of the soil over the entire unsaturated zone.”

Comment 12; Use spell checker - done

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