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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.

H. Lee et al.

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The authors would like to thank the referee Keith Beven for the critical and valuable comments on the submitted manuscript - the discussion of which will assist in better communication of the aims and methods of the submitted paper, and have also helped us to improve it. The response to each of his comments is given below:

Comment 1: The authors recognize that "closure relations are the best mechanism to ground the REW theory to reality (p.5)" but then, this paper continues to use closure relationships based on local scale equations and effective parameter values. I cannot see, for example, how the concept of capillary pressure has any value at all in representing the unsaturated zone at the REW scale. As also noted in the review of Zehe et al., the physics tells us that this simply does not average simply in heterogeneous domains because of the nonlinear dynamics.

Response 1: The reviewer is correct in stating that averaging is not simple in heterogeneous domains, as he and Andrew Binley showed in their 1989 WRR papers. In our case, firstly the Weiherbach soils are deemed weakly heterogeneous. As shown in the studies of Zehe et al. 2001, Zehe et al. 2005 it was found sufficient to represent the Weiherbach as consisting of typical hillslopes with Calcaric Regosol ranging from the hill crest down to 0.8 of the slope length and Colluvisol in the lower 20%. Both soils are assumed to be homogeneous in those early simulations and parameterized using a typical average parameter set, yielding good predictions at the catchment scale. Secondly, as already pointed out in our response to Keith Beven's review of our companion paper in HESS (Zehe et al. 2006), we are concerned with using the REW as a means for separating scales. If this were not the case, the REW concept would be totally useless because values of state variables would change, even if we slightly modified the size of the control volume, and we would lose the R (representative) nature of the REW! If one accepts this, then we can either assess closure relations by performing measurements at scales similar to or larger than the REW scale or, as we do not have those yet, by using finer scale models that have been shown to work for the place of interest. The revised version of the companion paper, Zehe et al. (2006, this issue), shows that even a strongly heterogeneous hillslope that contains a population of highly connective macropores, drains as a homogeneous porous medium when the averaging length scale is 3 times large than the correlation length of the heterogeneities. This should hold even better for the weakly heterogeneous simulation domain, such as in this case, considering that it was shown to be sufficient to model the Weiherbach with CATFLOW. The proposed approach links average outflow to volume integrated state variables by fitting nonlinear functions. It is able to account for the nonlinear effects pointed out by Keith Beven, as it yields different parameters (which may be regarded as REW scale textural parameters) for different structures inside the domain. Let us

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therefore reiterate that the approach adopted is not a simple straight forward averaging of REV scale parameters, as the reviewer suggests.

Comment 2: Quite apart from heterogeneity issues, by deriving functional forms on the basis of recession behaviour, the authors are also neglecting the effects of time delays in the response during storm conditions. This has to become important at larger scales (is this why they chose to only represent discharge predictions at daily time scale when they have 6 minute data really suggests something is being hidden if prediction at time step of model could not be presented?????)

Response 2: If the process of interest is recession, we think that one should use recession analysis to derive the corresponding closure relation. As can be seen, closure addresses many exchange fluxes in the balance equations, and is not restricted to discharge/drainage relations as Keith Beven claims in his own paper that appears in this special issue of HESS (Beven, 2006, HESS this issue). Timing is indeed important at the larger scale, and aspects such as how to estimate the average slope, play a dominant role in this context.

Concerning the closure relation for seepage outflow, indeed the effect of time delays was not considered in the closure relation for seepage outflow (eos=wo*a1os*(Ks**a2os)*((yu*su*wu+ys)/(Z*Psi))**a3os, eq. (24)). Regarding the time delays in subsurface zone, we argue that the force balance equation defined for the unsaturated zone (vuz=K/yu*su*(Psi-0.5yu), eq. (34)) is able to take into accounts the the effects of time delays in the unsaturated zone; the same holds for the force balance equation in the channel zone. The relative magnitude of suction pressure and gravity with respect to the saturation at the given time step determines the vertical velocity. For example, the recharge rate to saturated zone is (eus=aus*wu*vuz, eq. (17)).

For the simulation of Weiherbach using CREW, we used hourly scale data by integrating 6 minutes data and represent discharge predictions at daily time scale for presentation purposes only, as explained at the text. The paper does not focus on presenting the examination of model performance with the high resolution data, which will require consideration of additional aspects if we are to capture streamflow signatures with acceptable accuracy. To see the first application of CREW to the real catchment (Weiherbach) and also to check the appropriateness of developed closure relations, we argue that the hourly scale data is sufficient for the purpose of the paper. The test of the newly developed model should be started with the coarse temporal scale data, and the examination of model performance with high resolution data (e.g., 6 min data) can be done subsequently. Definitely, 6 minute data contain more information than hourly or daily scale data which CREW may not capture in any such future application, and if so then appropriate measures should be taken. Such an application is yet to be completed; however, the use of the high resolution data is not that demanding. We did not set out to hide anything here by only using low resolution data. If CREW does not capture certain properties in any of future applications, then surely CREW should be improved to describe the part.

Comment 3: We should also require that the closure schemes be consistent. This is not always the case here. For example, the geometric relationship for saturated area is based on a topographic wetness index that assumes an exponential decline in transmissivity with decreasing storage; the drainage curves are based on a power function of storage.

Response 3: The drainage curves for seepage flow is based on the detailed simulation using CREW with a detailed hillslope setting as close as those published at Zehe and Blöschl (2004) and Zehe et al. (2001). This setup of a hillslope has been, as stated. above, the dominant and typical structures in the Weiherbach (and the landscape where the Weiherbach belongs to) and this structure was sufficient to yield a good performance of CATFLOW at this catchment. So it is consistent with the landscape and process consistent with the model that is used for deriving this relation within a numerical simulation. The power law between seepage outflow and storage

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gave the best results for this case. However, if this is against the assumption adopted for the geometric relationship for saturated area, then the further effort has to be given to make them consistent. We thank the reviewer for the comments on this finding and do appreciate that there is still room for further improvement.

Comment 4: There is the general issue of comparing model results with model results as if the more complex model was actually true (and where the closures in the simpler model have been derived in part from the results of running the more complex model, introducting a certain circularity). There is a comparison with discharge observation, but only for two hydrographs and, strangely, the a daily time step noted above; whereas the comparison of Figure 11 suggests that a sine curve, with opposite gradients to those predicted by the model, would be a better predictor than CREW (- or even a straight line at a soil moisture content of 0.3). Not comment is made about the lack of commensurability between point measured soil moisture values and model predicted REW averaged values.

Response 4: We agree that this has to be improved. Surely, point measured soil moisture is not directly comparable to REW averaged values from CREW. However, point measured ones can provide the vertical limits at the given time step for the REW averaged values. This is why such a comparison is worthwhile. Figure 11 in the old version (= Figure 9(b) in the new version) (which compares point measurements 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 about how the true average soil moisture in the Weiherbach evolved during this period (as it simply the average of 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

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average soil moisture simulated with CREW is at least consistent with the finer scale model. Accepting that CATFLOW gives the best estimate of how the true average soil moisture evolved at that scale, this suggests that CREW has done a reasonable job in reproducing the average soil moistures. The new manuscript better explains our reasoning in this context, especially in combination with the companion paper (Zehe et al, 2006).

Comment 5: The authors do investigate the results from the two storms to suggest that although a reasonable estimate of the peak flow is obtained in the second storm is obtained the process mechanism predicted may not be correct. They suggest revisiting the infiltration excess closure scheme (spatially integrated form of the Green-Ampt does not perhaps reflect effect of heterogeneity adequately) but also fall back on suggesting that the whole parameter space was not fully searched for the assessment of model performance (p.29) (implying that there may be a more accurate parameter set out there somewhere, despite the simplifications inherent in REW scale closure schemes???? - but why should this type of model, with its multiple parameter values that must be defined and inherent errors in boundary condition data, be any more robust to equifinality/ non-uniqueness of acceptable parameter sets than any other model).

Response 5: The major goal of this paper was to find out to which extent we can employ a REV scale model, which has been shown to work in a catchment of interest may be employed to derive closure relations and assess the necessary parameters. If one is in the lucky situation to have all the necessary data, one should use as much of them as possible. Furthermore, what is important to note in this context is that the spatial extent of the CATFLOW application and of CREW are the same, but the grain is totally different. This allows important comparisons of model results which is normally never possible. In this context, we did not address the equifinality problem, as we would simply lose focus. Hence, the statement of having not fully explored the parameter space should not be understood to refer to equifinality in this context.

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We did not want to assess all acceptable models for the Weiherbach, but only those which are compatible with our data and knowledge about this place (In Weiherbach, infiltration excess overland flow is an important runoff generation mechanism based on the previous study on this catchment (Zehe et al., 2005; Zehe and Blöschl, 2004). In this regard, we respectfully suggest that the improvement of infiltration closure relation could be a key to capturing internal processes right as well as streamflows.

Equifinality is a very important issue to discuss in the context of the REW approach and is addressed in a different study that is submitted to Advances in Water Resources journal (Lee et al., 2006).

Comment 6: Even more contentious is the suggestion that follows that the upscaling procedure could be used as "parameter estimation methodology to reduce the amount of necessary calibration by estimating parameter values prior to calibration" (p.30 - this could perhaps be phrased a little better). Does this not presuppose (a) that relevant small scale characteristics are known a priori (distribution of Ks??? Effect of macropores??), and that the representation of the closure fluxes is correct (which it is not).

Response 6: We agree that the formulation of using parameters "a priori" does not make sense, as they have to be measured once. We will reword this passage, in the sense that our approach might allow the use of REV scale to estimate model parameters at the REW scale. We acknowledge that the reviewer Keith Beven is not too optimistic that one can bridge this scale gap, but we think that the two companion papers (this one, and the one by Zehe et al., 2006, HESS) show evidence that it is partly possible at least in weakly heterogeneous media. Neglecting this possibility means to throw away (a) all data from these scales (we usually do our measurements there) and (b) REV scale physically based models which can help in this context.

Concerning the word correct: Correct is something that is behavioral, nobody knows this better than the reviewer, Keith Beven. The model structure and the model results are behavioral as they are consistent with all that we know about the catchment ge-

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ometry and soils as well as with the observed fluxes and states. We agree that the information on states maybe not as sharp as it should be, but it is much more than we usually have! TOPMODEL has often been shown to be behavioral, but we doubt that anybody has ever shown that it is correct.

Comment 7: I must emphasise again that, as with the Zehe et al. paper, I not criticizing the REW concepts. I have argued strongly in several papers that the future of hydrological modeling lies with the REW concepts. I am prepared to accept the argument (made cogently by Siva elsewhere) that to learn from applying those concepts we have to start somewhere, but the authors, particularly in the last section are implying much much more than that, in a way that I do not think is justified. We are nowhere near having reasonable closure relationships yet, and it is false to suggest that we are. In this respect, in referring to my Alternative Blueprint paper, the authors seem to have missed one of its most important points. This argued that the REW was a useful framework within which to develop future hydrological modeling concepts, but that this needed to be done in a way that tested those concepts as hypotheses, taking account of the inherent uncertainties in the process. This they have failed to do. But hopefully might consider taking on board in future.

Response 7: We reiterate the reviewer's comments: one has to start somewhere, and we are far away from claiming that we have the ultimate model and the ultimate closure relations. The model and the relations could be improved or simplified. But the main point of this paper is that we have made important progress: we have shown that the REW scale models work, and that they can produce results of similar quality to an REV scale resolved model at the same scale, and at least produce dynamics of internal states that do not contradict observations and the averaged model results. This is a promising development for the future and gives further motivation to carry on with the work on closure relations. Again, we do agree with the reviewer that what we all need in this context are better observations!

- Some minor points of detail

Abstract. L.3. requirements - done

p.61 Caption to Fig 10. “simulated hydrograph with both closure parameters as well as manually calibrated ones, respectively” is not clear. ->“simulated hydrograph with both closure parameters and manually calibrated ones, respectively”

p.30 l.5 experimental - done

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