

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.

Anonymous Referee #2

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This paper presents one possible implementation of the REW concepts into a working hydrological model. It is closely linked to another paper in this special issue by Erwin Zehe et al. and this review should be read in conjunction with my review of that paper. Both reviews were, I think, written before final submission of the papers.

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

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physics tells us that this simply does not average simply in heterogeneous domains because of the nonlinear dynamics.

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 predictions at time step of model could not be presented?????)

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.

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, introducing 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.

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

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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).

Even more contentious is the suggestion that follows that the upscaling procedure could be used as a “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 K_s ??? effect of macropores??), and that the representation of the closure fluxes is correct (which it is not).

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.

Some minor points of detail

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

p.30 I.5 experimental

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

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