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Interactive Comment

Interactive comment on "Dynamical process upscaling for deriving catchment scale state variables and constitutive relations for meso-scale process models" *by* E. Zehe et al.

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I have actually commented on an earlier version of this paper (and that of Lee et al.) before it was submitted, and have also included some comments in my paper for the same special issue. However, although I appreciate that I saw and commented on the paper only shortly before it was to be submitted, I did expect to see at least some response to my comments in the submitted version (even if only to include some of the citations I suggested). It is therefore somewhat disappointing to find that this is not the case.

I think the approach to implementing the REW concepts taken in this paper is a real

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missed opportunity. I also think that it is unacceptable to just ignoring past work that is contradictory to the assumptions made in this paper approach (e.g. Binley et al., WRR 1989 study of whether effective parameters work at larger scales). Neglecting to mention past studies of using detailed modelling to find functional forms at larger scales (for example, the UP model of Ewen et al. HESS 1997; or the Topkapi model, e.g. Liu and Todini, HESS,2002) is also unfortunate.

After all the appeal to physics on which the REW concepts are based, it seems singularly perverse to then neglect the physics when choosing a representation at larger scales. The fact is that simple physical reasoning suggests that using local scale equations with average or effective parameters to include the effects of heterogeneity and macropores do not work – this was demonstrated more than 15 years ago and it has not been established that it will work in this paper. The main idea of the REA work (that representing the heterogeneity as a distribution of responses might still be important at larger scales even if the pattern is not) also seems to have also got forgotten.

Then there is the question of equifinality that is also neglected in these papers. OK, so the results presented in reproducing soil and discharge responses are "reasonable" (though they would not appear to be really acceptable yet in reproducing either discharges or soil water dynamics). But is it not a concern that there might be many other possible forms and parameter sets that would do equally well? And since the results are not actually that good, should there not be some concern about uncertainty in parameters (and, indeed, measurements)?

In this respect the authors have only taken part of the uniqueness of place argument on board. This did not say that uniqueness of place would preclude extrapolation of parameter sets as prior estimates. It did say that it should be expected that those prior estimates would be highly uncertain (partly because of interactions amongst parameters in the set means that there is a need to drift behavioural sets of parameters from gauged sites to ungauged sites) and that we should therefore aim to reduce that uncertainty by appropriate measurement where possible (and that some types of data collection might prove to be much more valuable than others in that respect).

It is these issues that are the really exciting essence of the potential advances to be made in the REW theory. The REW concepts should be used for formulating something new – NOT seeing if we can get away with doing the same thing over again (but in one sense badly in the sense that the current solutions in CREW etc will be a poor approximation to (and may not be convergent with) the true solution of a continuum Richards equation in a heterogeneous domain (before even considering preferential flow effects).Some further specific quantities:

p.3 state variables must be measurable quantities. ????in what sense is this possible for any distributed model element or REW???

p.3 using appropriate upscaling. ???how is this going to be possible. Upscaling will be predicated on knowledge of small scale properties that we will never ever have??

p.5. "texture" at the REW scale will certainly be important – but you then neglect any consideration of texture later in this paper where parameters are assumed to be homogeneous "effective" values.

p.8. simulated state variables are physically consistent with local observations. . . . . simulated time series of catchment average soil moisture and matric potential may be used as target measures for validation of meso-scale models. ???how can this be acceptable? Local soil moisture measurements are not commensurable with what you are predicting, nor is Figure 4 compelling as evidence of physical consistency – surely many other formulations would seem equally plausible (even a simple mean value!).

p.10. Equations 5. should there not be some consideration of hysteresis at larger scales (over an above that due to local scale hysteresis and the effects of macropores)??

p.11. There is a long history of work that suggests that infiltration excess overland flow

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cannot be predicted by homogeneous effective soil properties – trying to do so means that surface runoff contributing areas are either 0 or 100% of the REW.

p.15. the units of macropore volumes might be better stated as m^3m^{-2} if that is what the authors meant.

p.20. 95% confidence limits. Not clear how these are estimated (reference to Eqn. 5 misleading as these are not stochastic).

R^2 of 0.51 seems to be cited as 0.98 in Table 2 (or I may be missing something??)

p.23. This is a misrepresentation of the uniqueness of place arguments (see comments earlier). The authors need to think much more about the uncertainty with which the properties of any particular landscape can be estimated. In addition, the idea that landscapes are in equilibrium states cannot be supported by the evidence over large parts of the globe (surely including the Weiherbach catchment, where I would suspect that there is still evidence of glacial and periglacial processes affecting the soils and bedrock properties). This discussion should be cut.

In conclusion, I must emphasise that am not criticizing the REW concepts. I have argued strongly in several papers that the future of hydrological modeling lies with the REW concepts. This is because of the way in which the REW approach allows a new look at the problem of representing the effects of complexity at the scale of discrete REW landscape units (for which a representation of fluxes based on continuum mechanics will not be appropriate). There is still nothing wrong with the concepts, but, while recognizing that the implementation of closure schemes is a very difficult problem, there is a lot in this paper (and, it must be said, in other recent applications of the REW concepts) that is totally incompatible with the fundamental principles of the REW approach. The only justification for not requiring a major revision of this paper is the argument (made cogently by Siva elsewhere) that to learn from applying those concepts we have to start somewhere. That is a decision for the editors. My own contribution to the REW special issue (see HESSD-2006-0009) expresses the opinion that this is not a strong enough argument. It is analogous to saying that if we set off up a one way street in the wrong direction it will eventually get us to our destination. We can already perceive what that destination should look like, and I would suggest that this paper is not heading in the right direction.

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