

Interactive comment on “Modelling subsurface storm flow with the Representative Elementary Watershed (REW) approach: application to the Alzette River Basin” by G. P. Zhang et al.

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I read the above manuscript with a lot of interest. Clearly the authors have done a considerable amount of work: 1) introducing a macropore domain and associated balance equations and closure relations, 2) using observed groundwater levels in the validation of the resulting model. Both are potential areas of innovation. I therefore am favourably disposed eventual publication of the paper.

However, I have quite a few serious concerns and would like the authors to address these seriously.

1) The paper introduces a macropore domain into the REW formulation. I really do not

understand the model schematic presented in Figure 1. The macropore domain takes up part of the unsaturated zone - I have a real problem with this. I do not believe the macropores take up such a large surface area - they are almost line segments and I do not see how they can take up so much area.

I also do not understand the definition of the variables presented in Eq. 1. In fact I do not understand at all the description in the paragraph that follows Eq. 1. The same comments apply to the paragraph that follows Eq. 4. The macropore description has to be better

motivated - it seems very artificial and ad hoc. I have real problems with this type of description.

2. If they introduce a new flow region, they should include not only new mass balance equation and also a new momentum balance equation. To my understanding this is not done - this means the whole formulation may be theoretically flawed. The impact of the new domain on the whole momentum balance must surely be looked at. I need reassurance that this has been done or that the authors have found a way out of this. If not, this is a fundamental problem, in that the physical basis of the REWASH model has been compromised. In fact the paper does not even present the set of momentum balance equations that underlie REWASH.

3) I would like the assurance that the REWASH model without these additions could not predict the observed runoff well with the appropriate choice of parameter values. In particular, I would like the assurance that the incorporation of macropore flow was absolutely essential to reproduce the observations.

4) I have a problem with the authors identification of the macropore flow domain as that of fast subsurface flow domain. I would like better justification of this association.

In summary I would like a thorough discussion of the above points, especially motivation for the way that the authors have conceptualized the macroporous region, the

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derivation of the balance equations for mass and momentum, and the justification for the inclusion of the macropore region in the model based on analysis of field data. Clearly this paper must undergo major revisions to satisfactorily respond to these comments and criticisms

I hope this helps, and my sincerest apologies to the authors for the delay in my responses.

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