

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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General Comments

This study addresses important and challenging scaling issues related to hydrology. Specifically, it deals with upscaling of both hydrologic state variables and effective hydraulic properties at catchment/REW scales. The authors derived the time series of catchment-scale average soil saturation and hillslope effective hydraulic functions. They also found that the dominant patterns of soil heterogeneity and macroporosity (mainly macroporosity in my opinion after I read that paper) are enough to represent the main aspects of the catchment scale hydrologic processes. Overall, it is a well

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written and organized paper, although there seem to be some repetitions of verbiage from time to time that can be reduced in the revised version. However, I have some concerns that I will elaborate below.

Major Technical Comments

I have four major comments about this paper.

1) The linkage between the two major objectives of this study

I found the linkage is not clear and needs to be better established. When generating times series of catchment-scale average soil saturations in the unsaturated zone by averaging the corresponding distributed model output, does the unsaturated zone have the same processes as the modeling that is used to generate the hillslope scale soil hydraulic functions. As I image the first part should be done in a more complete processes that also include the other hydrologic processes, such as the saturated zone and the concentrated overland flow zone etc. It is not very clear to me that if this modeling also uses the same set up as the one that is used to obtain the upascaled hydraulic functions. In other words, are the two main exercises in this study really tied? Is the process used in determining the effective hydraulic functions an integral part of the one used to generate times series of catchment-scale average soil saturations? If a different process is used to obtain the effective hydraulic functions, will the effective hydraulic function results be the same?

2) The average saturated hydraulic conductivity

The authors state that as expected, in homogeneous soils the average saturated hydraulic conductivity determined from a sufficiently large sample of point measurements is a good estimate for the hillslope scale/REW scale saturated hydraulic conductivity. I found this is quite strong statement and needs to be substantiated. I can only assume that the homogeneous soils here have to be texturally heterogeneous soils as opposed to the case study which is dominantly structurally heterogeneous. Otherwise

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it does not make sense to even mention the average saturated hydraulic conductivity determined from a sufficiently large sample of point measurements since the saturated hydraulic conductivity would be a constant for homogeneous soils. If my assumption does reflect what the authors meant, I would argue that the statement by the authors needs to be elaborated. For the drainage scenarios depicted in this study, the simple average is probably not a good effective parameter estimate, unless it is really a mildly heterogeneous case.

3) The inconsistent use of hydraulic functions

The soil hydraulic properties after van Genuchten and Mualem were measured in the laboratory using undisturbed soil samples. But for REW-scale soil effective parameters, the parametric relationships of power-law type as shown in Eq. (5), similar to the Brooks-Corey model [Brooks and Corey, 1964], were assumed. I found that it will make more sense to use a consistent model. It is even more desirable to relate input hydraulic parameter structure to the upscaled parameters.

4) The dominant heterogeneities that dictate the hydrologic processes

After reading this paper, the take home message seems to be that the macroporosity heterogeneities and patterns dominate both the hydrological processes and the effective hydraulic properties for the catchment (REW). In other words, the critical subscale soil heterogeneities that actually impact hydrologic processes at next higher scale level are related to the macropores. At the end of this paper, the authors postulate that a set of typical closure relations exists for each landscape. I feel this hypothesis might be a natural extension (step) of what being studied in this paper, i.e., structural heterogeneities. For textural type of heterogeneities, the degree of heterogeneities and spatial correlations rather landscape might be more dominant in determining parameterizations of hydrological processes at the next higher scale. My question is: will a set of typical closure relations always exist for various heterogeneous scenarios, such as a mixture of both structural and statistical (textural) heterogeneities?

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Specific Minor Comments

Page 1630, line 26. The word “hillslope” appeared suddenly. Does it imply that what the authors summarized earlier is not related to “hillslope”?

Page 1633, line 22. “is” should be “are”.

Page 1635, line 19. The authors stated that the approach is similar to the perturbation methods. In what sense is the approach similar to the methods? Does the approach have the same limitation of being applicable to mildly heterogeneous media? If this is the case, I think the authors should be more specific about it.

Page 1638, section 2.3. We focus on the exchange term e_{us} , which denotes ground-water recharge or capillary rise. My question is: why was only this exchange term used in deriving the catchment-scale hydraulic functions? While this term is certainly influenced by hydraulic properties mostly, all other terms will also carry fingerprints of hydraulic properties in the catchment, in my opinion. This comment is somehow related to my major comment 1).

Page 1638, line 18. “?” needs to be deleted.

Page 1650, line 16 - 18. It should be noted here that the time series of the upscaled catchment scale soil moisture are not just simply the arithmetic averages of the observations. Then what is it? I also have some comments on the bottom panel of Figure 4. From the way it is presented right now, it does not seem to tell us anything. While the authors declare that the average soil moisture simulated by the landscape and process compatible model structure falls in the range of the observed soil values, I feel it might be more appropriate to show comparison with the average observations, such as some type of area weighted average if the observation points are not evenly distributed spatially.

Page 1653, line 9. Delete “I”.

Page 1653, line 26. Delete “not”.

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References

Brooks, R. H., and A. T. Corey. 1964. Hydraulic properties of porous media, Colorado State Univ., Hydrology Paper No. 3, 27pp.

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