

Interactive comment on “Linking soil moisture balance and source-responsive models to estimate diffuse and preferential components of groundwater recharge” by M. O. Cuthbert et al.

Anonymous Referee #1

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I have put some comments directly onto the PDF of the manuscript. The comments are related to the text and to some of the figures as well.

The idea of the manuscript is well-developed, but overall the execution of the ideas leave much to be desired. As the authors note there has been a lot of research involving the processes of water flow in dual-porosity soils. They present experimental evidence for preferential flow and recharge to a shallow water table. I think they do well in their explanation of the data and describing potential flow mechanisms as inferred from the data derived from the tensiometers and the piezometer. This data set is good however

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perhaps more replication of measurements would have been good, although we do need to keep in mind that budgets are limited. I am surprised that more evidence is not given to show that there actually is some macropore continuity in the upper horizon as well as in the lower horizon. The authors point to the preferential flow in the lower horizon as being either macropore flow or finger flow, but no evidence is given for this. This part is a big shortcoming in my view. Without that evidence their conclusion that preferential flow is occurring is not substantiated by direct observation; it is all based on tensiometer and piezometer response to rainfall.

The authors then go on to define a parsimonious model based on a soil water balance with a model for water fluxes representing preferential flow processes. I like the idea of simple models like the one they describe, but I also think they should have tested out a physically-based model other than just the single porosity Richards equation model. They should have presented the results from a dual-porosity model and try to fit the data and use the model to describe the ongoing processes. (I am actually surprised that the authors do not even mention the model, MACRO.) I believe that the parsimonious model can be derived from an analysis of the more complex and complete physically-base model, rather than to just define a bunch of parameters in some simple representation of the processes. In this way one could still keep the model parsimonious while also keeping the model more physically-based.

In a revision of the manuscript the more physically-base model should be applied to test the ideas of what processes are occurring, and to attempt to mimic the measured data. While I would like to see the parameters of the parsimonious model linked more to the physically-based model, it might be too much at this point to go that far within this manuscript.

Please also note the supplement to this comment:

<http://www.hydrol-earth-syst-sci-discuss.net/9/C4175/2012/hessd-9-C4175-2012-supplement.pdf>

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