Reviewer 1

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.

Many thanks to the reviewer for giving time to this review and for their detailed comments on the manuscript. We have replied directly to these within the attached pdf but also make a reply to the more general comments below.

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 perhaps more replication of measurements would have been good, although we do need to keep in mind that budgets are limited.

We are grateful to the Reviewer for acknowledging the contribution that this data set, and its interpretation, can make to the literature.

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 recharge events in the summer cannot be reasonably explained by any other mechanism than preferential flow from the soil to the groundwater table. The comparative modeling demonstrates this. It would have been useful in hindsight to have carried out dye tracing and pit excavations but in spite of this lack of direct evidence, the data collected can be clearly explained and modeled using a macropore model.

To show clearly where the pressure data demonstrate macropore influences we propose to include additional graphical evidence that points specifically to the groundwater responses that are directly attributable to a preferential flow mechanism (see additional figure below).

Also, to make the revised manuscript clearer on this point we have pointed out in the attached pdf places where, in line with the Reviewer's comment, the phrase 'preferential flow pathway' is more appropriate than the term 'macropore' which we have currently used.

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.

The SR model has a different physical basis than the Richards equation or models related to it. For example: the SR model is based on free surface films, the triggering of flow by means other than pore-to-adjacent-pore transmission, and disequilibrium between domains. The RE in contrast is based on water conduits whose air-water surfaces are not free but have fixed geometry governed by capillary forces, flow triggering in any given pore based on what is happening in immediately adjacent pores, and the medium as a whole progressing through a series of states in which hydraulic conductivity equilibrates with capillary retention. Thus the SR model cannot be derived as a special case of RE model. It has utility as a supplement to the RE model (or in the case of the present manuscript the traditional soil-water balance model) in order to account for the water-transporting effects of processes that the RE model does not account for.

One important difference is that because the RE effectively ties the rate of flow through a pore or channel to the channel's dimensions, it allows fast flow only through large pores, and only when these pores are filled. Under this assumption fast flow is necessarily high-volume flow. And this makes it unable to represent a type of bypassing situation observed at our site and others, specifically that water can appear at a water table or a deep layer of soil under conditions where the flux is not high and where one or more layers are not close to saturation. Based on concepts of free-surface flow in films, the SR model divorces flow volume from flow speed. Thus its combination with the SMBM facilitates representation of processes that occur but are not accounted for by a traditional SMBM formulation.

In this manuscript we did not go into this matter in detail because it is covered elsewhere (Nimmo, 2010a; b; 2012). We did not consider it within the scope of this paper, but propose to add the following in a revised manuscript:

"Although it was developed around the concept of flow through macropores, the source-responsive model can also be useful for fingered, funneled, or other modes of preferential flow. Much as unsaturated-flow models based on straight capillary tubes can effectively represent flow through the grossly irregular pore space of soil, this model based on macropores can represent flow through geometrically different preferential flow paths. Thus its applicability, which depends on there being some form of preferential flow, does not strictly require that this be specifically macropore flow. "

Nimmo, J.R. 2010a. Theory for Source-Responsive and Free-Surface Film Modeling of Unsaturated Flow. Vadose Zone Journal 9(2):295–306. doi:10.2136/vzj2009.0085.

Nimmo, J.R., 2010b, Response to Germann's Comment on "Theory for Source-Responsive and Free-Surface Film Modeling of Unsaturated Flow": Vadose Zone Journal, v. 9, no. 4, p. 1102-1104, doi:10.2136/vzj2009.0085.

Nimmo, J.R., 2012, Response to Masciopinto's Comment on "Theory for Source-Responsive and Free-Surface Film Modeling of Unsaturated Flow": Vadose Zone Journal [in press], v. 11.

As a final point, the reviewer mentions MACRO, but we note that MACRO does not allow any upwards flow from the groundwater into the soil profile and as such would be an inappropriate tool to apply to these data. In any case the family of dual domain models to which MACRO belongs require many unconstrained parameters as we note in the manuscript, and the purpose here was to experiment with an alternative type of PF model, which would be more parsimonious while still having a physical basis.

Reviewer 2

The authors have presented a very well written research article on linking a SWBM with a SRPFM to estimate diffuse and preferential components of groundwater recharge. The paper has attempted to link local hydrologic data of a field site at various time scales to a "Parsimonious Modelling Approach", as coined by the authors. The paper has a significant merit in the detailed data collection from the field site as presented in Figures 3, 4 and 5. Field hydrologic data being scarce and difficult to collect, this data is of significant benefit to the hydrology community.

Thanks to the Reviewer for giving your time to this review and in acknowledging the contribution that this data set, and its interpretation, can make to the hydrology community.

While I may sound reserved in the following comments, but I feel I am doing so to help improve the clarity and also the quality of the modeling aspect of the paper. It took me a long time to put together the complete picture of the research description and to link it those to the results. The authors may want to simplify or modify the paper to provide those links to accomplish a better flow while reading through the material. What threw me off totally was I did not find a solid OBJECTIVE of the research at any point. Perhaps that was one reason why it took me a while to understand the goal of the research, although the title was quite clear.

In light of this comment we propose to amend the introductory section to make it more explicit what our objectives are as follows:

1. Evaluate soil-moisture dynamics at the Shropshire site, using the extensive data set available, to assess the type of diffuse and preferential flow processes that control recharge and its relation to soil moisture.
2. Test whether a traditional Darcy–Buckingham approach can adequately represent the recharge processes.
3. Develop a modification of the SMBM that uses a new preferential flow model to quantify the processes that are not accounted for in the traditional SMB approach.
4 Show whether the new combined model can perform adequately in the ways that the traditional approach cannot.

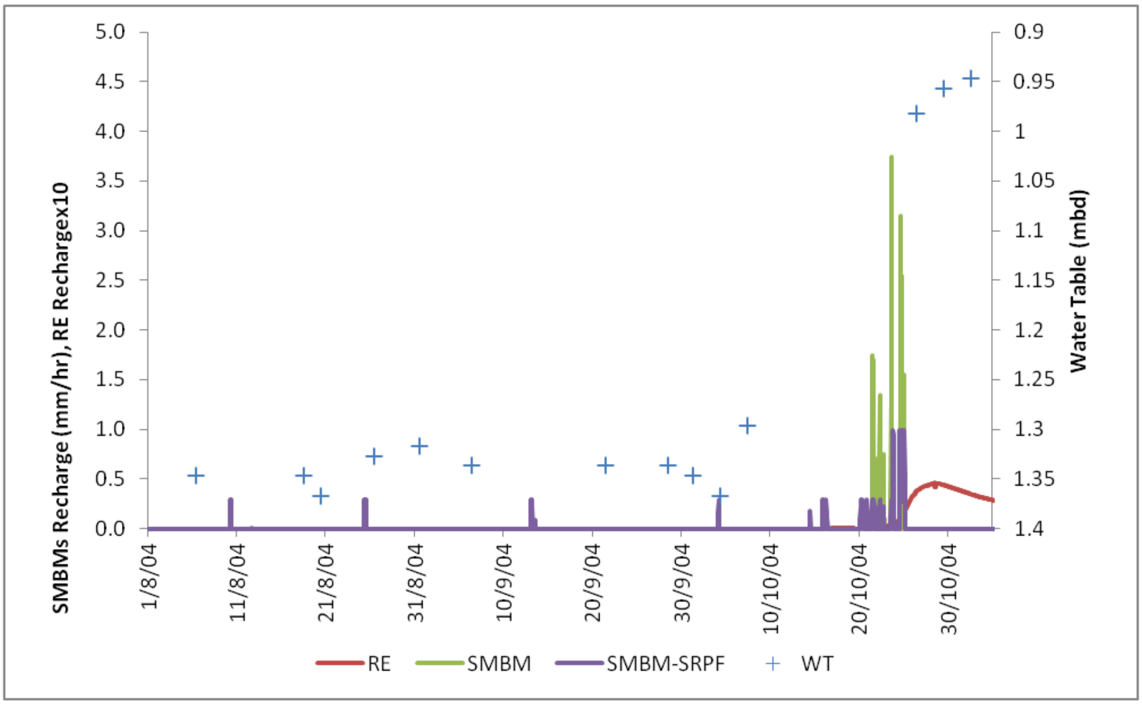
My first confusion began in the description of HYPOTHESIS TESTING section. The authors have used a one-D model FAT3D-UNSAT to simulate diffusion with gravity component, I presume, for a heterogeneous single porosity media. I did not understand the basis of soil moisture retention characteristics and why there are four layers instead of three that were used in the simulation as presented in Figure 7. Also, there is no statistical comparison between the observed and the simulated pressure at T1 or the changes in head in the piezometer BH6. My observation of the figure tells me that there was a close correspondence between the observed and the simulated heads in the time series presented. The simulated errors could be artifacts and/or limitations of the model. While I am certain that the authors have not overlooked the model accuracy and limitations, as they have made comparative analyses, however, I just want to ensure that their understanding derived from the simulations are not biased due to the modeling.

The soil moisture characteristics are derived through the calibration process. As indicated in the text of this section there are a large number of alternative parameter sets that perform well and this was one of them. At this stage there are few data that provide a means to guide the choice of the van Genuchten characteristics, so initial choices were made using simple comparisons between the soils here and the USDA standard soil classes. These were then varied to yield the final fits. The use of four layers for this exercise reflected the different visual descriptions for the upper and lower glacial outwash deposits used to guide the initial choice of van Genuchten parameters.

Statistical measures were not deemed to be useful for the present analysis since a full optimisation procedure has not been employed for the modelling. Visual fit was deemed to be good enough for the short calibration period used for this modelling exercise.
The single porosity modelling demonstrates that fitting certain features in the pore pressure data cannot be easily resolved using coarse layering. It also shows that a diffusion type model cannot reproduce rapid recharge events after dry spells that are observed at this site. These are only obvious if recharge responses are shown for the full year and a figure to illustrate this will be included in the final manuscript.

I admire the authors' brevity in using a SMBM for such complex processes of unsaturated zone. They have provided details on how they adjusted for the preferential flow component using the SRPF model. I see a close correspondence of observed GWL vs SMBM simulated GWL in Figure 8. What I did not find is a comparison of simulated results with and without the SRPF component. This alone will be more educational because it will provide the understanding of the impact of preferential flow in ground water recharge.

In response to this comment we have run a one layer SMBM without the SRPF modifications (SMBM) as well as a Richard's Equation model (RE) for the whole one year period we have data for, for comparison against the SMBM-SRPF model. This showed that while the overall estimation of recharge between the models only varies by around 15%, substantial differences in behavior are seen, particularly during dry periods. A brief additional section will be added to the revised manuscript to describe these results including a new figure as shown below. The figure shows the distinctively different behavior of each model during late summer 2004. The SMBM-SRPF model is the only model to be able to reproduce recharge events during this period seen in the water table record (although the data are not continuous for this part of the record, there are enough data to make this observation), similarly for the recharge event that occurs in June 2005. The RE response is highly and unrealistically smoothed and lagged in comparison with the SMBMs.



I also had another concern while going through the manuscript. The question that arose in my mind was, "why was the SMBM inadequate in accounting for the amount of recharge, when the site was so well instrumented?" Mass Balance is usually sufficient to account for major changes in a reservoir. We include Momentum Balance to account for rates of changes. In this case, the modeling was done at hourly intervals. If all hydrologic data collected could be accounted on an hourly basis, then the SMBM should be sufficient to simulate the recharge volume over each hour. What was the need for correcting the SMBM using SRPF model? Either I do not understand the implication from the study or it needs to be explained a bit more clearly.

The time and space resolution of the Shropshire field measurements, even though they are much better than what is usually available, still fall short of being able to represent the effects of preferential flow. For example, preferential flow can cause recharge at a meter or more in depth in less than one hour. The paths of such flow may be narrow and relatively fast flowing so as not to cause a sensible increase in water content in some of the layers the flow passes through. When such flow modes are active, as the evidence suggests for some of the Shropshire data, some other method needs to be used to account for them, for which we suggest the SRPF model.

I am still trying to understand the factor Mlim in the model proposed. The authors have commented, "The model works well for Mlim between 250 and 750 m-1." Isn't that what has been compared in Figure 9? In fact, Figure 9 perhaps suggests that SMBM without SRPF model should be able to account for the recharge volumes over a certain period of time.

Yes, you are correct that the recharge volumes, if aggregated over longer time steps, are well predicted by the SMBM in the winter period. However, the SRPF, via the variable Mlim, enables the timing of the flow from the topsoil to the water table to be better simulated over shorter time periods (as shown in Figure 9 – note, these are simply snapshots of the whole year model simulation, not calibrated against single events) and, during the summer, when preferential flow still occurs but abstraction from the matrix also occurs, water table rises also occur at the right time. This is something that the SMBM cannot do on its own as shown in the additional figure given above.

The conclusion section seems to be more of a summary rather than a "take home" message. I do believe that the authors could provide some additional evidence to strengthen their proposed model and also provide the possibility of its extension to other hydrologic regimes. I would also recommend modifying the abstract to reflect their objective and conclusions adequately.

We propose to modify the title of this section ‘Summary and Conclusions’ and to bring out the main points more clearly.

Please use SMBM or SMB throughout. Also, the manuscript may need a very few grammatical corrections.

We will ensure this is done in the revised manuscript.