**Hydrology and
Earth System
Sciences
Discussions**

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 #2

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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. 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.

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.

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.

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.

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.

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.

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

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