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Comment on hess-2022-261

Anonymous Referee #3

Referee comment on "Estimating vadose zone water fluxes from soil water monitoring data: a comprehensive field study in Austria" by Marleen Schübl et al., Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2022-261-RC3, 2022

Dear Editor, dear authors,

Please find below my review of the paper entitled "From soil water monitoring data to vadose zone water fluxes: a comprehensive example of reverse hydrology" by Marleen Schübl, Giuseppe Brunetti, Gabriele Fuchs, and Christine Stumpp.

This article investigates the use of the Bayesian approach to invert water content profiles to derive the soil hydraulic parameters, including their statistical distribution and related statistical parameters. Based on this information, the authors compute the water cycle over large periods and quantify the groundwater recharge, its uncertainty, and its temporal variability at 14 sites in Austria. The authors conclude that there is a West-East gradient with more continuous groundwater recharge at mountainous sites with wetter climates versus seasonal lower groundwater recharge in the Eastern plain.

The article is well-organized, well-illustrated, and well-written. The scientific question is properly stated, the methodology to answer conclusions is clear and straightforward, and the conclusions are quite obvious. The paper addresses an important topic and deserves to be published in the HESS journal. However, I have several concerns that should be addressed prior to publication.

I am not very familiar with the Bayesian approach, and thus hope that my comments do not reveal my lack of expertise in this subject. However, I consider that any paper should be standalone and present clear facts understandable by any scientific reader. Several points deserve to be clarified:

- For the Bayesian approach, the choice of distributions needs to be clarified. If the errors between the modeled and observed data are expected to obey the normal law, no details are given about the laws of the soil hydraulic parameters. I expect most parameters to follow normal laws and hydraulic conductivity to follow a log-normal law. If I understand well, the Bayesian approach allows us to characterize the SHP laws. Then, why not show them in the Result section and state on the alignment of normal laws? Why not state on the multimodality features of the SHPs? In addition, do the SHPS distributions have any consequences on the Bayesian approach and the Monte Carlo method? Is the normality of errors between experimental and modeled data compatible with any statistical law for SHPs?
- The problem of equifinality and non-uniqueness needs to be addressed in the paper. The authors inverted all the SHPs, except the parameter "l" fixed at 0.5. However, we know that many parameters may be interrelated, and some may have a poor impact on water fluxes. In particular, the value of the residual water content has no effect (or very little on the water fluxes), so this parameter may not be reachable when inverting. A similar statement may apply to the saturated water content. What is the strategy of the authors regarding this aspect of non-uniqueness? Why not propose a sensitivity analysis that shows the most influential parameters and select those when inverting water content data while suggesting additional information for the others?

- I also have some concerns regarding the data inverted to derive the SHPs. In their study, the authors invert only water content profiles. However, if I remember well, they also have water pressure head profiles for some sites. I understand they selected the water content profiles because they had those data at their disposal at all sites. However, for a given site (with the two types of data), they could have compared the results when inverting water content and water pressure head. My feeling is that the authors may not have had the same results. Based on this comparison, they might validate the choice of water content for all sites and strengthen their conclusions. That could be the topic of further research.

- Lastly, I had some questions and concerns about the ACP proposed at the end of the result section. I was surprised by the plots of "individuals" (i.e., sites) and the "variables" on the same plots (Figure 4). Even after searching on R tutorials and finding these types of plots, I am not convinced that we have the right to do so. For ACP, variables and individuals don't have the same nature and should be plotted on separate plots. I also suggest plotting the correlation circles and commenting only on the vectors (variables) that are well represented on the maps, i.e., which vector is close to the correlation circle.

The authors will find an in-depth review in the enclosed file, with suggestions, comments, and proposals throughout the manuscript. Again, this paper is valuable and promising, and I have no doubts that it will be published after improvements.

Please also note the supplement to this comment:
https://hess.copernicus.org/preprints/hess-2022-261/hess-2022-261-RC3-supplement.pdf