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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Recharge estimation and soil moisture dynamics in a Mediterranean karst aquifer" *by* F. Ries et al.

Anonymous Referee #1

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This manuscript presents a 3-year dataset (soil moisture and meteorology) from a Mediterranean karst location, an inverse modeling procedure to estimate soil hydraulic parameters, use of the optimized parameter values to compute soil-moisture balances and percolation fluxes during the period of the data, and an illustrative application of the results to a 62-year period of meteorological data. The results are interpreted with implications for the relation between recharge and storm size, timing, and other factors related to the variability of events that generate recharge.

The implications and conclusions about what causes or relates to recharge in this type of location appear at face value to be interesting and important. Unfortunately, however, they are arrived at through a flawed analysis. The main problems are that the data set is too limited and specialized, and the physical model based on Richards' equation

and unimodal soil hydraulic properties is too simplistic, to support the ambitious goals of modeling percolation in a complex soil. Since the conclusions mainly concern water fluxes and the data reflect only water content and not fluxes, the modeling problem is very difficult, and probably not approachable with any widely used quantitative model of soil water flow. The effort described here achieves plausible conclusions about recharge because it has a large number of fitted parameters that are adjusted freely without regard to what could physically characterize a real soil. The analysis does not represent a physically realistic relationship between the input data and the predictions, but rather an artificial mathematical relationship.

The physical plausibility of the soil hydraulic properties from the optimization (table 3) is not discussed in the paper but it is very important and forms the basis for taking the further results seriously. The reason may be that the data for calibration are insufficient or the quantitative model (meaning Richards' equation implemented through Hydrus 1D) is inappropriate, or both.

One indication is that clay and bulk density increase with depth. This suggests Ks should decrease with depth, but values in table 3 show lowest Ks near the surface, and greater Ks at lower depths. Also, the parameters assigned to each layer do not combine plausibly to describe a real soil. For example the values assigned to layer 4 at SM-3 include alpha = 0.001 mm-1, which implies an air-entry pressure around 100 cm-H2O and therefore an upper pore-size limit around 15 microns or so. This suggests a tight silt or clay texture, and Ks of maybe a few tens of mm/d. But Ks is given as about 6000, too high by a factor of 100 or so. In other words, these values indicate large pores to get the listed Ks but small pores to get alpha. So it doesn't correspond to a physically plausible medium and definitely not a common soil type.

It should also be noted that the parameter L listed in table 2 is controversial in its relation to tortuosity. It cannot be interpreted as tortuosity when given negative values, as for many cases in table 3. It then is just an empirical fitting parameter. It should be given a fixed positive value if it is to say something about a physical property of soil.

Concerning the data set, it is a difficult problem to constrain a dynamic soil-moisture flow model with data representing only water contents, not fluxes or other flow-rate indications. The measurement of 4 depths at each location has no replicates or additional installations to indicate spatial variability. There are no flux or matric suction measurements. This is a sparse data set for the task of finding values for 6 parameters of the Mualem-van Genuchten formulas.

Part of this problem is acknowledged in the discussion section, 8818/28 – 8819/2, in noting that a unimodal Mualem-van Genuchten fit may not be suitable for this heterogeneous structured soil. Indeed a bimodal fit or a dual permeability model might be more realistic, but would increase the number of parameters to be fit. It would then be even more difficult to get physically realistic estimates of parameter values using the data set that consists only of water contents.

The most impressive result from the model is how well its major percolation events match up with the temperature data from the well (fig. 7). This result suggests that the parameter values obtained constitute an empirical model that predicts some of the system hydraulics, even though they are not realistic. The evaluation with the 62-year data set and analysis of implications for recharge related to various factors are highly appropriate ways to make use of a predictive model, though I do not see them as justified results because of the faulty parameterization.

What I suggest if the authors want to resubmit a paper like this is one of two alternatives. The first is to obtain a larger and more diverse data set (including tensiometer measurements and maybe lysimeter measurements of soil-water fluxes) and use them with a model that is capable of representing the different types of flow that can occur in a soil with complex structure. The second is to adopt more modest objectives appropriate to the available data. Perhaps the data could be used to investigate characteristic soil-moisture sequences that correspond to different meteorological events.

Although in this review I am not emphasizing minor changes, I also note that many

figures, especially fig 4, are too small to be read without additional magnification.

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