Hydrology and Earth System Sciences

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Discussions

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

F. Ries et al.

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We appreciate the thoughtful comments in the anonymous review of referee 1. To simplify communication, we split the referee's general comments into various parts and address them separately.

Comment 1: 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

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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.

Reply: We are thankful for this statement and we will clarify the intended use of HYDRUS in our case, which indeed was misleading. In our application HYDRUS is not meant to be a physically-based soil water model but rather a tool to estimate vertical water fluxes from continuous and long term soil water content measurements. To limit model complexity and equifinality we kept the model structure as simple as possible and used the SCEM method to find the best parameter set. We will discuss the resulting parameter sets accordingly and will also compare our results with literature. Among others we will address the incommensurability problem (Beven, 2006): Scale effects, heterogeneities, measurement technique problems and other factors can lead to discrepancies between values of variables predicted by a model and "real" parameters. We are working in an area where data is extremely scarce but sound knowledge on the local water balance, i.e. groundwater recharge, is of vital importance. Among others, a harsh climate, logistic and political factors are main reasons. Many researchers working in semi-arid and developing regions are facing similar problems. Compared to tensiometers, the installation of soil moisture sensors is relatively easy and straightforward under these conditions. Lysimeters are limited to long-term research projects with intense and expensive measurement infrastructure that are rarely available.

Comment 2: 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.

Reply: We are thankful for this detailed analysis and will use it in our modified manuscript version where we will discuss the limited physical meaning of the optimized parameter sets: Our optimized "effective" model parameter sets include factors like varying stone content, vegetation influences in the uppermost soil layer and last but not least preferential flow in soil macropores. Indeed, our K_s values could be indicative for preferential flow and alpha for the matrix, a nice example that parameters of a unimodal HYDRUS application should not be compared to parameters of a real clay soil. We will also compare our results to the large scatter of K_s) values for clay soils in the UNSODA database (Nemes et al., 2001). Furthermore we will show the differences between our model parameter sets and "real" soil physical parameters by additional information: We took undisturbed soil samples (200 cm^3) from different locations within our study site and determined soil hydraulic parameters by means of multistep-outflow (MSO) experiments (Puhlmann et al., 2009). Soil hydraulic parameters from MSO were in the range of our parameter sets but did not account for scale effects like stoniness or vegetation influences.

Comment 3: 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.

Reply: Thanks for this comment: We will also discuss the "L"-problem in our revised manuscript. Others faced similar problems as the obtained negative L values for finer textured soils (e.g. Schaap and Leij, 2000). We will also test our parameters against the parameter constraints postulated by Peters et al. (2011).

Comment 4: 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.

Reply: As stated above we will stress in our revised manuscript that we are aware of data limitation, which is a typical problem in the area we are working. The strength of our data is the high temporal resolution and long time span, but we cannot cover the entire spatial variability. We will also discuss a possible dual permeability model like the bimodal model as suggested by Durner 1994. In accordance with the reviewer we

will state that more degrees of freedom do not necessarily mean a better and more realistic model result.

Comment 5: 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.

Reply: We are thankful for this comment, because similar to the reviewer we were encouraged by the temperature response of the groundwater that matched up with our major percolation events. We will provide additional data of the groundwater level that will show the magnitude of groundwater level rise, although this is highly influenced by pumping. And, as stated above, we will modify our manuscript to make it clear that we do not claim the physical basis of our approach but rather see it as an empirical model.

Comment 6: 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.

Reply: As stated above, we are aware of limitations in our data set and in the simplified, empirical model we used for our analysis. We will discuss this in our modi-

fied manuscript accordingly also with the help of additional data from multistep-outflow (MSO) experiments from the lab. But the temporal fit of our results to groundwater temperature and accompanied level rises encourages us that our approach is an appropriate compromise between data limitation and the urgent need to estimate soil water percolation in Mediterranean, semi-arid karst regions.

Comment 7: 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.

Reply: We will check all figures for readability in the revised manuscript and assure that they are appropriately scaled.

References cited in this reply:

Beven, K.: A manifesto for the equifinality thesis, J. Hydrol., 320, 18–36, 2006.

Durner, W.: Hydraulic conductivity estimation for soils with heterogeneous pore structure, Water Resour. Res., 32, 211–223, 1994.

Nemes, A., Schaap, M. G., Leij, F. J., and Wösten, J. H. M.: Description of the unsaturated soil hydraulic database UNSODA version 2.0, J. Hydrol., 251, 151–162, 2001

Peters, A., Durner, W., and Wessolek, G.: Consistent parameter constraints for soil hydraulic functions, Adv. in Water Resour., 34, 1352–1365, 2011.

Puhlmann, H., Von Wilpert, K., Lukes, M. and Dröge, W.: Multistep outflow experiments to derive a soil hydraulic database for forest soils, Eur. J. Soil Sci., 60, 792–806, 2009.

Schaap, M. G., and Leij, F. J.: Improved prediction of unsaturated hydraulic conductivity

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with the Mualem-van Genuchten model, Soil Sci. Soc. Am. J., 64, 843–851, 2000.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 11, 8803, 2014.

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