

Interactive comment on “Bayesian inverse modelling of in situ soil water dynamics: using prior information about the soil hydraulic properties” by B. Scharnagl et al.

Anonymous Referee #3

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1. As a general comment I should say that this paper deals with issues that were dealt extensively in previous works, but without providing proper references. At the very least, this is a disservice to the reader, who may end up devoid of a complete perspective. I will provide specific examples below.

2. The paper also has the tendency to make self-serving comments that are at times breathtaking in their audacity. Again, I will provide a few examples below. Finally, the underlying physics of the problem is treated poorly, and the sense one gets is that this paper is yet another one in an indiscriminant application of an algorithm

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3. By the authors account, the paper tries to address the following 3 questions (page 2024):

“(i) What is the effect of prior information on the identifiability of the VGM parameters? (ii) How much prior information is needed to get well-defined soil hydraulic parameters? (iii) Is the Bayesian approach sufficiently robust in case of a biased prior distribution?”

Going over these items one by one, I do not get the sense that these goals are worthy of pursuing because (1) they were answered in a broad context of statistics and the answers are well known; (2) I do not think that there is a question about “how much prior” makes sense as presented. One will use whatever one has, the more the better, and one should expect to quantify uncertainty accurately. Whether or not that is sufficient, that depends on the applications (as shown in several papers, e.g., F. P. J. de Barros, Y. Rubin, R. M. Maxwell, The concept of comparative information yield curves and its application to risk-based site characterization, *Water Resour. Res.* 45 (W06401) (2009) doi:10.1029/2008WR007324. And F. P. J. de Barros, Y. Rubin, A risk-driven approach for subsurface site characterization, *Water Resour. Res.* 44 (W01414) (2008). doi:10.1029/2007WR006081.

The issue of prior information in hydrology was treated extensively in hydrology by Woodbury and coworkers and by Rubin and coworkers, e.g.:

Y. Rubin, X. Chen, H. Murakami, M. Hahn, A Bayesian approach for inverse modeling, data assimilation, and conditional simulation of spatial random fields, *Water Resources Research* 46 (9) (2010) W09520.

And

Z. Hou, Y. Rubin, On minimum relative entropy concepts and prior compatibility issues in vadose zone inverse and forward modeling, *Water Resour. Res.* 41 (W12425) (2005) doi:10.1029/2005WR004082.

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And

A. Woodbury, T. Ulrych, Minimum relative entropy and probabilistic inversion in groundwater hydrology, *Stochastic Hydrology and Hydraulics* 12 (5) (1998)

And

A. Woodbury, Y. Rubin, A full-Bayesian approach to parameter inference from tracer travel time moments and investigation of scale effects at the Cape Cod experimental site, *Water resources research* 36 (1) (2000)

Bayesian concepts have also been employed extensively by Neuman and coworkers. Issues of data worth were dealt with as early as 1994, in B. James, S. Gorelick, When enough is enough: the worth of monitoring data in aquifer remediation design, *Water Resour. Res.* 30 (12) (1994).

Extensive discussion of priors is provided in: Rubin, Y., *Applied Stochastic Hydrogeology*, Oxford University Press (2003), including extensive compilation of data provided in earlier works by Kitandis and Gelhar and coworkers.

4. The following statement (page 2023) is problematic:

“In summary, the current state of knowledge is that in situ measurements of soil water dynamics contain insufficient information to warrant a reliable estimation of the soil hydraulic properties.”

It is misleading and unfair because of the enormous body of work existing in this area that is ignored here. Many works have in fact produced high-quality estimates, such as:

Kowalsky, M.B., S. Finsterle, J. Peterson, S. Hubbard, Y. Rubin, E. Majer, A. Ward, and G. Gee, 2005. Estimation of field-scale soil hydraulic and dielectric parameters through joint inversion of GPR and hydrological data, *Water Resour. Res.*, 41, W11425, doi:10.1029/2005WR004237

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Next, how does one define “sufficient”? and what is meant by “reliable estimates”? Estimates are reliable in my opinion as long as they are associated with accurate quantification of uncertainty. So this has nothing to do with prior and everything to do with the goal of the characterization effort and the parameters of the problem..

5. On p. 2023 we find the statement:

“The current practice is to use a uniform prior distribution of the soil hydraulic parameters and let the observational data speak and determine parameter and predictive uncertainty”

That's grossly inaccurate. See my comments above and references regarding rational selection of priors. The body of literature in statistics on the proper choice of priors is huge. Many additional references are provided in the papers listed above.

6. On page 2026 the authors state:

“Because our interest is to estimate effective soil hydraulic properties, we arithmetically averaged the soil moisture data from the different measurement locations..”

This statement does not define what is meant by effective properties, except to suggest that they are related to the arithmetic averages of the soil moisture. In other words, the van Genuchten model is expected to describe the behavior of the average soil moisture. This is not a trivial assumption. Effective properties have a very clear definition in physics, and to my understanding, there are no effective properties for transient flow in soils. There is also no indication, as far as I know, that the van Genuchten model applies for arithmetic averages of the moisture. There is an extensive body of literature on this topic by G. Dagan and G. Sposito (see references in Rubin, 2003). Along this line, equation (1) is not applicable for the description of the average soil moisture. For that one would need to use the effective conductivity (see Rubin, 2003, *Applied Stochastic Hydrogeology*, Chapter 11). Yeh showed that effective properties could be defined only for very limited cases (e.g., steady-state, 1D). Additional discussion can be found at

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Vereecken et al., VZJ, doi: 10.2136/vzj2006.0055Vol. 6 No. 1, p. 1-28. The flow equation needs to be upscaled in order to describe average soil moisture. See also Milly, Transport in Porous Media Volume 3, Number 5, 491-514, DOI: 10.1007/BF00138613 . So, in this regard, I believe, the authors made a basic error in assuming that they could derive effective properties the way they describe in the paper.

7. The paper provides only limited information on the site. It also does not provide information that could be used to justify the assumption of one-dimensional flow.

8. Focusing on near surface measurements (6 cm depth) is problematic on several counts: (1) soil is quite disturbed (root and biological activity and weather) and it questionable whether the priors are at all applicable (2) effects of weather (like temp variations) are ignored, and there is no doubt that at such depths they must be recognized.

9. Page 2030 line 16, what is the justification for this assumption? In fact, it is not applicable generally. What would be the impact of this assumption on the results?

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