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Hydrology and Earth System Sciences Discussions

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 #2

Received and published: 19 May 2011

Review of "Bayesian inverse modeling of in situ soil water dynamics: using prior information about the soil hydraulic properties" by Scharnagl, Vrugt, Vereecken and Herbst:

General Comments

The paper presents an inverse modeling framework that makes use of Bayesian concepts. The main focus is in the usefulness of prior information. The framework is applied within a variably saturated soil. It effectiveness is tested against synthetic data and real data. A positive and interesting aspect of the work is in the use of real data together with the Rosetta platform. In my opinion, the strength of the manuscript lies

in the methodological aspects. However, from the conceptual and theoretical point of view, the paper does not bring any new insights. Use of informative prior has been studied in the past (see works by Ulrych and Woodbury in the nineties) and successfully applied to the vadose zone (e.g., see work by Hou and Rubin 2005). Therefore, in my view, the contribution of this paper is purely methodological and this needs to be made clear in the abstract, introduction and conclusion. The material should be suitable for publication after some points are clarified (minor to moderated revision).

1. Does the paper address relevant scientific questions within the scope of HESS?

I would say yes. It fits with the scope of HESS and the questions are relevant in the field of hydrology.

2. Does the paper present novel concepts, ideas, tools, or data?

Conceptually, I would say no. Many of the issues raised by the paper (e.g., use of Bayesian theory and relevance of priors in predictions) has been tackled in the past. I would say that the main strength of the manuscript is the methodological tool.

3. Are substantial conclusions reached?

The conclusions support the efficiency of the methodology (especially against synthetic data). However, no substantial conclusions with conceptual substance were reached.

4. Are the scientific methods and assumptions valid and clearly outlined?

Yes.

5. Are the results sufficient to support the interpretations and conclusions?

Yes.

6. Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)?

Yes. In fact, the paper is well written and clear to follow.

7. Do the authors give proper credit to related work and clearly indicate their own new/original contribution?

Yes.

8. Does the title clearly reflect the contents of the paper?

Yes.

9. Does the abstract provide a concise and complete summary?

Yes.

10. Is the overall presentation well structured and clear?

Yes.

11. Is the language fluent and precise?

Yes.

12. Are mathematical formulae, symbols, abbreviations, and units correctly defined and used?

Yes.

13. Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated?

In general, the paper reads really well.

14. Are the number and quality of references appropriate?

Yes.

Specific comments

Comment 1: p. 2021, L. 15-20: The authors explain the discrepancy between laboratory and field scales however, the authors did not mention anything related to scales

of heterogeneity. Perhaps a sentence or two could be added for completeness with a significant reference supporting the statement.

Comment 2: p. 2023, paragraph around lines 15 - 25 ("In summary, the current. . .uncertainty"): This paragraph deserves special attention. The authors mention the use of prior information and its usefulness to reduce the uncertainty of the soil hydraulic properties'. They also refer how this prior information could be used to formulate an informative prior (line 25, p. 2023). Also, they mention that the "current practice" is to use a uniform prior distribution (around line 28, p. 2023).

A lot of these issues mentioned in this paragraph are not novel and have been tackled in the past. For instance, Woodbury and co-authors developed rational approaches to infer informative priors based on limited amount of information using the concept of minimum relative entropy (MRE). MRE provides a rational approach for modeling the PDF from information that characterizes the probability distribution incompletely. Such information could be given in terms of statistical moments or bounds. The main point is that the principle of MRE selects the distribution with the highest information entropy with respect to the prior information while satisfying a given information allowing us to employ the distribution model that is least subjective (see details in Chapter 13 of Rubin 2003 book "Applied Stochastic Hydrogeology").

I suggest the authors to read (and cite) the following reference:

Hou, Z., and Y. Rubin, 2005, On MRE Concepts and Prior Compatibility Issues in Vadose Zone Inverse and Forward Modeling, Water Resour. Res., 41, W12425, doi:10.1029/2005WR004082.

The reference above applies the concept of MRE in vadose zone inverse modeling and shows in a clear way how to develop a consistent and informative prior based on subjective opinion. I believe this work needs to be cited in the manuscript under revision. Furthermore, I suggest that the authors look deep in the works published by Woodbury and Ulrych, for example:

Woodbury and Ulrych, Minimum relative entropy inversion: Theory and application to recovering the release history of a groundwater contaminant, Water Resources Research 32(9), 1996

The main point is that there is a significant body of literature dedicated in studying prior information and how to make best use of it. Reading this paragraph cited in this comment gives the "impression" that the topic is being treated for the first time. I believe that the novelty of the work is not on the treatment of prior information but on the methodological methods used to approach the problem. Even because the authors assumed a model shape for the priors. On the contrary, MRE infers the model shape based on the information at hand.

Comment 3:

p. 2031, lines 9-11, "...Yet, the main thrust of the current paper is to show that we can do better than this and formulated informative prior distributions that are specified from soft data..."

Approaches for formulating an informative prior was already shown in the literature (e.g., Woodbury & Ulrych and Hou & Rubin, see my previous comment) and, therefore not a novelty. The above sentence gives the impression of novelty and I believe the authors should consider re-phrasing it.

Also, for the sake of completeness of the manuscript, I encourage the authors to define "soft data" with a reference.

Comment 4:

p. 2036, section 2.4.3 entitled "Defining the prior distribution of soil hydraulic parameters":

The authors tested 3 different formulations of prior distribution for Bayesian inverse modeling (multivariate uniform distribution, multivariate normal distribution, and multivariate normal distribution with correlation). This is fine under the context of the

manuscript. The authors justify the choice for the multivariate uniform distribution model (what they denote as prior 1) however justifications are still needed for prior 2 and 3.

Also, I repeat that the authors need to refer to the approaches that allow one to estimate a rational and informative prior distribution (instead of assuming a pdf model shape). As listed previously, there is a series of papers, e.g., on MRE, available in the literature. Prior information can be defined in a rational manner.

Comment 5:

p. 2044, line 6: "...This finding is not new but has been reported in the many other studies..." Please add some references to support this statement.

Comment 6: Although out of the scope of the paper, I think the authors should consider adding a sentence or two (perhaps expanding the discussion) related to the use of prior information to address issues related to model uncertainty. They use the prior information to estimate soil hydraulic properties (VG model parameters). How can the framework proposed be used to infer conceptual models? What are the implications in the computational costs? How can the framework adopt the ideas published by Neuman and co-workers (see Neuman's Bayesian model averaging work published in SERRA, 2003)?

Minor Comments

Comment 1

p. 2022, line 15: Please provide the year of the reference "Ritter et al." in the text.

Comment 2

p. 2031, line 2031: The sentence "...In addition, we can infer predictive uncertainty by propagating each realization of the posterior distribution...." is a bit confusing. Did the authors mean "propagating each realization GENERATED BY THE posterior distribu-

tion”? Please clarify.

Comment 3

p. 2039, lines 1 & 2: What does it mean “create the prior pdfs”? Does it mean inferred? The term “create” seems a bit vague. From the manuscript, I thought that the priors were assumed models (prior 1, 2 & 3) and not “created”.

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