Hydrol. Earth Syst. Sci. Discuss., 5, S59–S63, 2008
www.hydrol-earth-syst-sci-discuss.net/5/S59/2008/

Hydrology and Earth System Sciences Discussions

Interactive comment on "Inverse modeling of soil characteristics from surface soil moisture observations: potential and limitations" *by* A. Loew and W. Mauser

Anonymous Referee #1

Received and published: 2 March 2008

Land surface models (LSMs) require specification of the soil hydraulic properties to simulate latent and sensible heat fluxes. Unfortunately, at the spatial scales of a LSM, little compelling information is usually available to parameterize the water retention and unsaturated hydraulic conductivity function. Therefore, there is considerable uncertainty associated with the choice of these functions, and thus significant uncertainty with the simulated sensible and latent heat fluxes. In this paper the authors use inverse modeling to back out the relevant soil hydraulic parameters in a LSM using observed soil moisture content data of the topsoil, pedotransfer functions and soil texture information.

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

Evaluation: This contribution addresses an important topic in hydrologic / hydrometeorologic modeling, and I enjoyed reading the manuscript. Certainly this work deserves publication, but is not very novel. Some comments below:

MAJOR COMMENTS:

1. The authors outline a Bayesian approach to estimating the soil hydraulic parameters in a LSM. After explicating the necessary theory, they then continue with estimating the maximum likelihood estimate of the prescribed Gaussian density function with the Nelder-Simplex method, without recourse to explicitly estimating parameter uncertainty. Given the scope of the current paper, and Bayesian approach outlined, I find it fundamentally more natural to use Markov Chain Monte Carlo (MCMC) simulation to converge to the entire posterior probability density function (pdf) of the parameters, rather than a single best estimate as is done now. This would not only provide much more information about the information content of the water retention data, and sensitivity to the individual soil parameters, but also make a more novel contribution.

2. The authors highlight that an appropriate initial guess of the optimized parameters was needed for their Nelder-Mead method otherwise this search method was susceptible to getting stuck in a local solution. This begs the question of how valid the presented results are? Do they correspond to the best attainable parameter values or just a local solution? To inspire more confidence in the findings of this study, the authors need to present evidence that their optimized values are indeed representative. Note that MCMC simulation will likely not get stuck in a local solution, because it will always maintain diversity in the parameter space enabling movement out of local basin of attraction. This is another advantage that the authors need to consider in their work.

3. In their approach, the authors use a prior distribution for the individual model parameters that discourages the optimized parameters to be well removed from their prior estimates. The sensitivity of the optimized parameters to the choice of the covariance matrix, G needs to be discussed. This would provide better insights into how flexible

HESSD

5, S59–S63, 2008

the optimization can be in the choice of the parameters or whether it is completely dictated by the definition of p0. If the latter is the case then the final optimized parameters will be very similar to their prior estimates, making the parameter estimation analysis presented in this paper unnecessary.

4. The authors discuss three different scenarios (A,B and C) in their inverse modeling studies, but do not adhere to this order in the discussion and presentation of their results. Perhaps I misunderstand the presentation.

5. How realistic are the error properties of the soil water content data considered in this paper, and do the results transfer to real world situations with structural and forcing error that cannot be adequately captured in simple additive error models? Please comment.

OTHER COMMENTS:

The use of "soil characteristics" in the title is not very informative.

Abstract: replace "unique" with "unprecedented"?

Page 97, Line 11 - 14: The word "define"; is used twice; better to use the word "using"; instead of the first define

Page 97, Line 25: The Vrugt et al. 2005 paper does not consider LSMs but a subsurface flow and transport model.

Page 98, Line 1: What causes the optimized parameters to take physically unrealistic values? This begs the question why a classical inverse modeling approach as done in this paper is used? Please comment. I believe that ignoring of model structural, and input forcing error forces the model parameters to compensate for this, and thus to take unrealistic values that do not correspond with the properties of the soil.

Page 98, Line 10: Replace pedotransfer functions with PTFs.

Page 98, Line 20: Similar comment as (4)

HESSD

5, S59–S63, 2008

Page 99, Line 9: Please define rmse

Page 99, Line 13: 1 to 10 cm

Page 99, Line 18: Please add reference to Loew, 2008 to the reference list, and appropriately place this between the brackets when referring to this paper.

Page 100, Line 17: How is the random process noise mathematically defined, and shouldn’t this be added to the state vector of the model instead of input variable to represent structural and forcing error?

Page 102, Line 6: Why is theta_s not used for saturation? This is consistent with the soil physics literature.

Page 102, Line 7: subscript s is missing after "K". K_s is the saturated soil hydraulic conductivity.

Page 102, Line 8: ... is also referred to as the soil water retention curve.

Page 102, Line 17: It might therefore be reasonable ...

Page 105, Sectoin 3.3: These measures do not provide information about the accuracy of the model, but about the predictive performance of the PROMET model. Accuracy can be obtained when doing MCMC simulation as discussed above.

Page 106, Line 18: This begs the question whether a single set of parameters is used for the whole profile, or whether these parameters vary with the three layers?

Page 108, Line 1 - 2: This confirms that forcing error should be taken explicitly into account when doing inverse modeling of LSMs! One way of treating forcing error is to do sequential data assimilation simultaneously with MCMC simulation.

18. Page 109, Section 5: I think that it would be good to add a Table with parameters that are be calibrated with their prior ranges, and initial estimate, po.

Page 111, Line 5: How is this information used to derive G?

HESSD

5, S59–S63, 2008

Page 111, Line 10 - 13: Why are a different number of ensemble members obtained for the individual parameters?

Page 111, Line 18: "accuracy assessment" or "predictive evaluation"?

Page 113, Line: 8: Again, are the parameters optimized for each layer, or just for the whole profile. I believe it is probably the latter.

Page 113, Scenarios: It might be good to also explicate in the text how many parameters are considered in each optimization?

Page 115, Error Analysis: Again, MCMC simulation could provide better insights into the posterior pdf for different properties of the error model.

Page 117, Line 5 - 10: See major comment (2). This necessitates the use of a more robust optimizer!

Page 120, Line 2 - 4: See major comment (2).

Page 121, Line 8 - 10 Here the reference to Vrugt et al. 2005 would be appropriate. Their work uses the EnKF in combination with MCMC to handle various error sources explicitly in the context of groundwater tracer modeling.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 5, 95, 2008.