

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

### **Anonymous Referee #3**

Received and published: 29 February 2008

This paper analyzes whether it is possible to determine soil characteristics from surface soil moisture observations. Since remote sensing measurements of surface soil moisture are one of the few data sources available on the large scale, this question is certainly relevant in hydrology and fits well within the scope of the journal.

My first concern with this manuscript is that several similar studies have been presented in recent times (e.g. de Lannoy et al., 2006; Santanello et al., 2007). It is not clear to me what the additional contribution of this paper is compared to these papers. This should be made clear in the introduction.

A second concern is related to the erratic impact of sampling interval and measurement

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



error in figure 7 (and other figures). Why is this not more consistent? I wonder what would happen if you use several different error realizations for each pixel in figure 7. I think this would remove the fluctuations in RMSE with increasing error level. I know that this involves a lot of additional simulations, but at the moment the results seems quite random and strongly influenced by how the model can compensate the added error. To make this necessary revision feasible, the authors could consider reducing the amount of simulation for the sampling interval.

In soil physics, several authors have argued that soil water content time series alone are not sufficient to determine all hydraulic parameters (Russo et al., 1991; Si and Kachanoski, 2000). To investigate this issue, 2D plots of the error surface would be helpful. I suspect that these will show quite wide elongated valleys, indicating uncertain parameters and high parameter correlations. A similar analysis for scenario B and/or C would also be interesting to see whether the functional relationships contained in the pedotransfer functions overcome this limitation. Further discussion of this issue is required.

I also have doubts about the generality of the recommendation in the paper. It seems to me that the recommendations for sampling interval and measurement accuracy are completely dependent of the soil water dynamics in the observation period. This should be made clearer in the paper.

In general, the paper is well structured. However, there were several occasions where information was unnecessarily repeated (see specific comments). In addition, I have noted grammar errors and typos in the specific comments below. Finally, I had a hard time reading Figure 4 because it was too small. A different design would have been appropriate to fit the unorthodox design of HESSD.

In summary, I recommend that moderate to major revisions are required as outlined in this review. If the revisions are adequately done, I am looking forward to reading the paper in HESS.

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

## SPECIFIC COMMENTS

Page 96, line 4. Use biogeochemistry.

Page 96, line 15. Use 'are evaluated';

Page 96, line 24-26. Is this not a bit exaggerated? How many LS models consider chemical and biological processes in the unsaturated zone?

Page 97, line 25. Did any of the studies mentioned here actually use a land-surface model?

Page 98, line 3. Use 'coincide' here.

Page 98, line 16. Map scales (e.g. 1:10000) not useful here. The smallest unit represented on the map is of interest (resolution).

Page 100, line 5-18. This paragraph repeats previous statements. Can be removed without loss of information.

Page 100, line 12-14. The inverse modelling approach is introduced twice. Improve formulation.

Page 100, Line 15. Title does not seem very appropriate for the content of this section. Numbering does not make sense either in this section.

Page 102, Line 10-14. Previous statements are repeated here. Can be removed without loss of information.

Page 103, line 13-16. It would be more elegant to mention these cases in the order that they are discussed in the paper. This also applies to the definitions of the scenarios later on.

Page 105, Line 10ff. Why present both the RMSE and E? They are perfectly correlated. RMSE would be sufficient for me.

Page 106, line 17-20. Because Richards equation was presented in section 2.1, the

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



impression was created that PROMET numerically solves this equation. I gather from your remark here that PROMET uses an analytical approximation instead of a numerical solution. Please clarify. It might be appropriate to remove section 2.1 and include a short description of soil water movement in PROMET at this point. How do the assumptions to derive the analytical expressions influence the results of this study, in particular for the deeper soil layers where PROMET is very coarse?

Page 107, line 8. Use 'remote sensing information'.

Page 107, line 8. Who is W.?

Page 107, line 12-20. The soil information presented in section 4.2.4 can be integrated in this paragraph. In addition, a general profile description of the soil would be appropriate. PROMET assumes a homogeneous soil profile. How realistic is this for the site under consideration?

Page 107, line 24. This is an interval, not a frequency. In addition, use accurate instead of best.

Page 108, line 20. Use % or volumetric units (i.e. 24% water content or 0.24 cm<sup>3</sup>/cm<sup>3</sup>).

Page 109, line 12-14. This the third or fourth time this is mentioned. Can be removed without loss of information.

Page 110, line 3-4. Why use the TDR measurement at 25 cm only. It would have been more appropriate to use the average of the 15 and 25 cm TDR measurement to compare with the second layer of the PROMET model? Please motivate this decision.

Page 110, line 6. I think these errors are on the low side, also considering that the field is cropped. Adding the error levels 8 and 10 Vol% seems appropriate.

Page 113, line 1-3. Redundant information. You already mentioned that H has this form in case of direct measurements.

Page 119, Line 16-23. A discussion of another submitted paper in the conclusions

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

does not seem to very appropriate. In addition, this was already mentioned before.

## REFERENCES

Russo, D., Bresler, E., Shani, U. and Parker, J.C., 1991. Analyses of Infiltration Events in Relation to Determining Soil Hydraulic-Properties by Inverse Problem Methodology. *Water Resources Research*, 27(6): 1361-1373.

Si, B.C. and Kachanoski, R.G., 2000. Estimating soil hydraulic properties during constant flux infiltration: Inverse procedures. *Soil Science Society of America Journal*, 64(2): 439-449.

---

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

# HESD

5, S54–S58, 2008

---

Interactive  
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

