

***Interactive comment on* “Effect of GPR-derived within-field soil moisture variability on the runoff response using a distributed hydrologic model” by J. Minet et al.**

Anonymous Referee #2

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The manuscript reports on field-scale measurements of near-surface soil moisture and on the subsequent results of distributed simulation experiments referring the simulated runoff depending of different scenarios/patterns of the soil moisture as initial conditions for the simulations.

To my impression, the reported work has several novelties: 1. A new device (a 4-wheel motor-cycle) on which a GPR system was installed I order to drive over fields and collect soil moisture data. The main novelty here is the combination of the motor cycle with the GPR system. 2. High-density measurements of GPR-derived near-surface soil

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moisture for 4 fields, 10 dates, respectively. 3. “Scenarios” of soil moisture variability, i.e., 4 deterministic and 3 stochastic soil moisture pattern. The stochastic patterns contain 1000 realizations each. 4. Application of a distributed hydrological model on the four fields, while the soil-moisture scenarios and a high intensity rain event are used as initial conditions and boundary condition, respectively. 5. The runoff resulting from these simulations is discussed and compared with each other. No measured runoff data are available. The rainfall event is not related to some of the soil moisture pattern.

The author team (or parts of it) has published parts of that work before, in particular concerning the measurement device and the soil moisture measurement results. The model itself has also been described before. To conclude, the real novelties of this manuscript are the scenarios of soil moisture and the comparison of the simulation results. I feel that this is not necessarily enough for a publication of a new paper. Thus, I can not recommend its publication.

Some detailed comments:

Title: The title should be better phrased: “. . . on the simulated runoff response . . .” P 8954, L 19/20: it is assumed that the measured soil moisture (i.e. in the upper 5-10cm) “reliably reflects the soil moisture in the hydrological active soil layer . . .”. I assume that the authors applied this assumption, because they did not have information about deeper soil moisture and they assigned the same soil moisture as in the upper 10 cm for the whole depth. This is a very strong assumption. And a rather non-realistic one. With realistic variations over depth, the simulated runoff would have looked rather different. P8959 L 9,10: why did the authors use “typical” soil data and not real (measured) ones? And the same soil data for all fields? I guess that soil parameters are known for each field. The whole study becomes a bit virtual by applying non-field parameters. P 8964 L 9: Why did you show the normalized NS-coefficient? It is better to give the real coefficients, because then one can see not only the difference to the best simulations but also the absolute performance. P 8965, L15: What kind of threshold behavior are you referring to? Infiltration? How is this realized in your model? P 8966

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(and elsewhere): I doubt that it is a good idea to show the averages of the stochastic hydrographs. Maybe it would be better to derive a probability distribution from these results.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 7, 8947, 2010.

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