

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 reviewer is thanked for its review. We have made the changes as suggested and the answers to the comments are detailed below.

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 in 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 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.

We totally agree with the reviewer about the fact that the GPR soil moisture sensing method, including the 4-wheel motor-cycle, and the hydrologic model were already presented in other publications and therefore do not constitute technical innovation in this paper (only new application results). Therefore, as outlined in the introduction, the novelty of this paper is to build on an extended high-resolution soil moisture database based on 5 different fields and 10 dates for the investigation of the soil moisture variability on the runoff response. This paper aims thus at generalising the findings of Merz and Plate (1997), Merz and Bardossy (1998) and Bronstert and Bardossy (1999) for various field and moisture conditions. To the best of our knowledge, no studies about the effect of antecedent soil moisture on the runoff response have relied on so numerous high-resolution measured soil moisture data. With the proposed GPR method, an unprecedented resolution and acquisition rate were achieved.

The description of the soil moisture sensing by GPR in “Materials and methods” was strongly simplified to focus on the main goal of the paper. Please refer the revised manuscript.

Some detailed comments:

Title: The title should be better phrased: “. . . on the simulated runoff response . . .”

The title was modified as follows:

*Effect of high-resolution spatial soil moisture variability on the simulated runoff response using a distributed hydrologic model*

The reference to the GPR was removed for the sake of brevity and for insisting on the novelties presented in this paper.

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.

The issue of relating surface (e.g., from remote sensing measurements) and subsurface soil moisture (for use in hydrological models) is widely acknowledged in hydrology (Capehart & Carlson, 1997; Vereecken et al., 2008). Subsurface soil moisture was inferred from remotely-sensed surface measurements in many studies, using a wide range of methods ranging from simple statistical relationships to physically-based hydrodynamic models (see Wagner et al., 2007 for a review about these methods).

It is worth pointing that the penetration depth of the GPR system we used is larger than common remote-sensing instruments, owing to the smaller operating frequency (0.2 – 0.8 GHz) and corresponding larger wavelength. The retrieval of two-layered or continuously-varying soil moisture profile from frequency GPR data was also investigated in Minet et al. (2011).

In this paper, it is assumed that the spatial variability of the surface soil moisture is at least representative of the hydrological active soil layer variability. Nevertheless, this limitation is now emphasised in the revised paper in the “Sensing of soil moisture by ground penetrating radar” subsection.

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.

The soil parameters that are used in this study were determined for a typical Belgian loamy soil (Laloy and Bielders, 2008) and thus apply to the majority of the fields in this study, i.e., Walhain, Marbaix and Burnia, (8 field campaigns on 10) that are located in the loess belt area. In Laloy and Bielders, (2008), these parameters were determined by literature review, field measurements and hydrologic model calibration. As no runoff measurements were available for the fields in this study, we could not use specific calibrated field parameters so the same parameterisation as used in that study was used here. Moreover, using the same parameters for the hydrologic model in similar soil conditions helped to compare the effect the antecedent soil moisture conditions between the fields according to the different soil moisture scenarios.

Moreover, in a downscaling perspective, no high-resolution soil parameters would be available over large catchments (>10 km). We thus investigated the use of topographically-derived indices (i.e., TWI) or stochastic scenarios for soil moisture disaggregating in a data-scarcity context. Only structured

scenarios based on topography were used in this study as topography is a first-order control on soil moisture distribution and owing to the topographic data availability at high resolution.

Fields were further described in the “Agricultural fields” subsection in the “Materials and methods” section, with respect to the soil type (textural information when available), soil cover and topography. The justification of the use of the TWI (data-scarcity) was reworded in the “Antecedent soil moisture scenarios” subsection.

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.

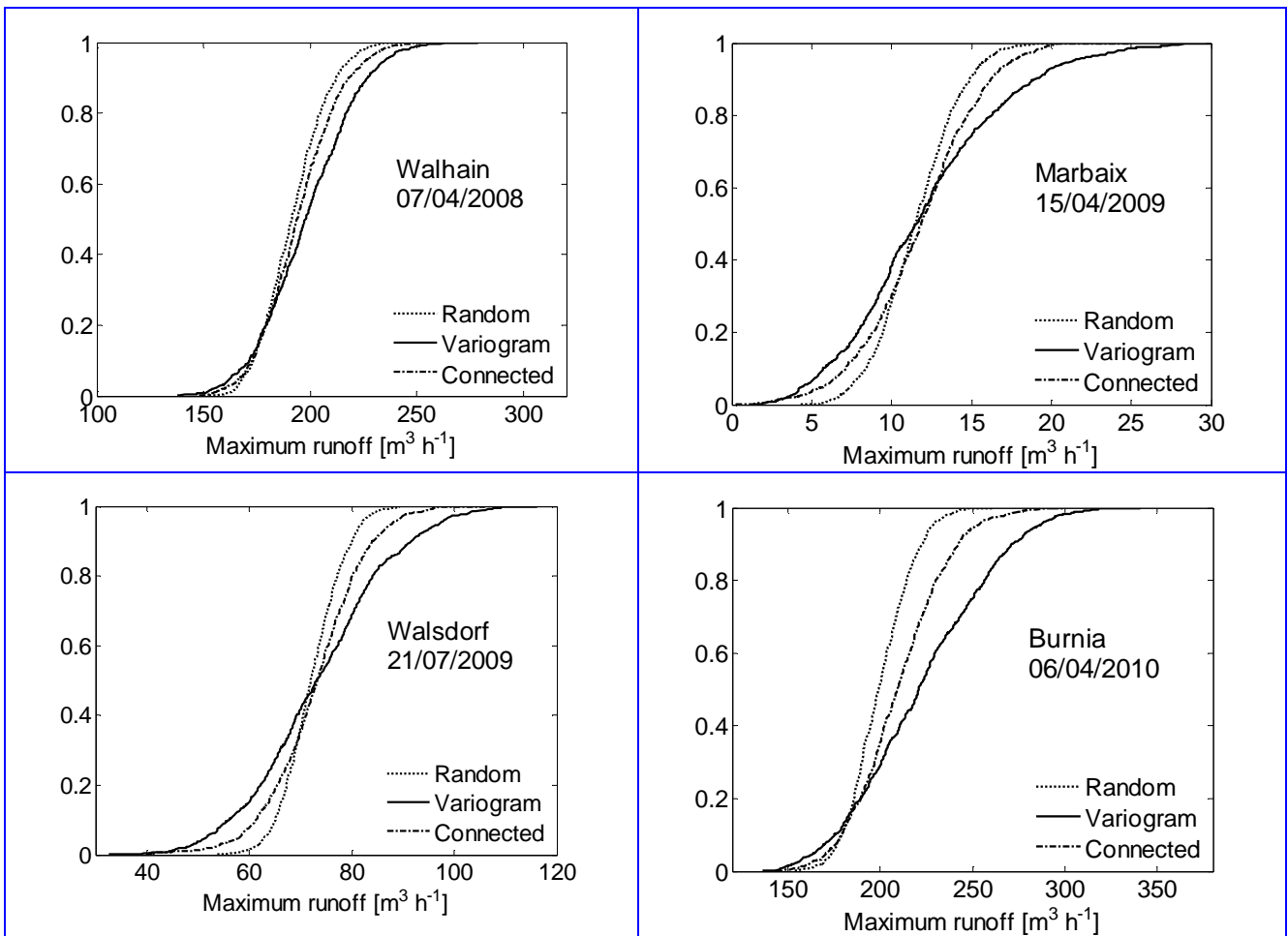
We agree that the non-normalised NS-coefficients are better informative than the normalised ones and they were replaced in the table instead of the normalised coefficients for each field campaigns. However, the mean and the standard deviation for all the 10 field campaigns computed with the normalised NS-coefficients permit to better compare the soil moisture scenarios (i.e., by statistical tests), so we decided to keep it in the table. The table caption and the text were clarified according to these modifications.

P 8965, L15: What kind of threshold behavior are you referring to? Infiltration? How is this realized in your model?

This important aspect concerning the runoff generation in the hydrologic modelling was also pointed by the other reviewers. This threshold refers to the threshold behaviour of the hydrologic model response to antecedent soil moisture. Under a particular rainfall and model parameterisation, runoff is triggered at a certain soil moisture threshold because of exceeding infiltration capacities. These soil moisture thresholds were computed from single-cell hydrological simulations and discussed in the “Discussions” section. In addition, the infiltration component of the CREHDYS model was better described in the “Hydrologic model” subsection.

P 8966 (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.

You pointed an interesting way to represent these stochastic data. Cumulative distribution functions were derived from the maximum runoff discharge for the stochastic scenarios for the 4 fields presented in Fig. 4 (see figure below). These cdf permitted to point that the most variable scenario in terms of hydrographs were the *variogram*, the *connected*, and then the *random* scenario, respectively. The distributions of the maximum runoff discharges appeared to be rather gaussian.



Nevertheless, we think that this figure does not bring new outcomes, as the larger variability of the variogram scenario was already observed in the Table 3 and in Figs. 6 to 8 and discussed in the text. Figures 6 to 8 also permitted to better visualise the hydrographs variability, not only for the maximum runoff but for the whole hydrographs.

Thank you again for your constructive comments. I hope that these answers and the modifications in the paper may meet your requests. Do not hesitate to contact me for further clarifications and enhancements.

Julien Minet

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