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 #1

Received and published: 5 January 2011

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

General Comments:

This work presents an established proximal GPR methodology to determine the soil moisture distribution across five fields, creates seven different soil moisture patterns (called scenarios, including the measured one) classified as deterministic or stochastic, and subjects the fields and associated scenarios to a rainfall event typical for Belgium. The response to the rainfall event is simulated with a distributed hydrological model. No validation data for soil moisture or catchment discharge are included in this work. The paper is well-organized, with clear tables and figures.

This work is similar to the work cited by Marz and Plate (1997), Merz and Bardossy (1998) and Bronstert and Bardossy (1999). The work presented by previous authors was more informed by observed data and details about the catchments in question than this study, but this study advances the works cited by having high-resolution soil moisture data and by having 10 field acquisitions, at different average moisture conditions. With these two additions, this work does not provide a radically new contribution, but confirms the findings of previous works.

While the use of GPR is a novel approach to collecting many points in the field, this methodology in itself is not a novel contribution to this work. For example, a field campaign using TDR, while more time consuming, would also have provided the same true map of soil moisture. The title of this study may be a bit misleading to imply that the GPR technique is somehow necessary for better determination of soil moisture fields. This work can be better informed with information about topography, the soil types and associated soil physical parameters, and antecedent precipitation. Knowing the porosity, field capacity and wilting point vol. moisture contents would help interpret the GPR soil moisture data. Knowing the topography would help understand the likelihood of existing contributing areas, and thus inform the reader about the likelihood that the TWI (or any of the stochastic patterns) is a good distribution for the right reason. Typically, the TWI works when soil moisture redistribution occurs in the subsurface. Do these soils allow for such redistribution? Also, the GPR moisture readings are only for near surface soil moisture content (the reader should be told how deep and at what accuracy), so how do these represent the soil moisture at greater depth?

These general comments were addressed in the revised version of the manuscript, as many of them were raised by the other referees.

We totally agree that the GPR method used in this study is not a novel contribution, as this was raised by the two other referees. The section “Sensing of soil moisture by GPR” was reduced and now focus on soil moisture characterisation issues in relation with the current study (e.g., penetration depth). The reference to the GPR in the title was also dropped. Nevertheless, we are still convinced that the main novelty is to benefit from a large high-resolution soil moisture dataset in numerous fields that would be cumbersome to collect with other techniques such as TDR or soil sampling, as outlined in the abstract. Indeed, surface soil moisture may rapidly vary over time (scale of a few hours) and the proposed GPR technique permits to obtain surface soil moisture maps with an unprecedented resolution within a short time (> 1000 points per hour).

The five fields were better presented in a new subsection entitled “Agricultural fields” at the beginning of the “Materials and Methods” section. In particular, soil types were given, with textural information when available. Unfortunately, no field measurements of soil physical parameters such as porosity and hydraulic conductivity were done during the field experiments. As these soil properties are varying with time, especially in agricultural fields, they cannot be determined *a posteriori*.

Additional information about the topography was given in Table 1 (elevation range) and the justification of the use of the TWI was improved (in the “Antecedent soil moisture scenarios” section). Although small, the slopes are believed to be sufficient to allow for subsurface soil moisture redistribution. It is worth noting that generated runoff that flows in the channel network also contributes to increase soil moisture in flow accumulation zones that are outlined by the TWI. Furthermore, as vegetation heterogeneities within the field were limited (because of bare soils, single management of the plots, etc.), the TWI might be the primary factor of soil moisture variability in our study.

Finally, the limitation with respect to the rather shallow penetration depth of the soil moisture sensing by GPR was discussed in the “Sensing of soil moisture by GPR” section. The accuracy and precision of the GPR method for soil moisture were discussed in Jadoon et al. (2010) and a RMSE of 0.025 m^3/m^3 was found when comparing with TDR measurements.

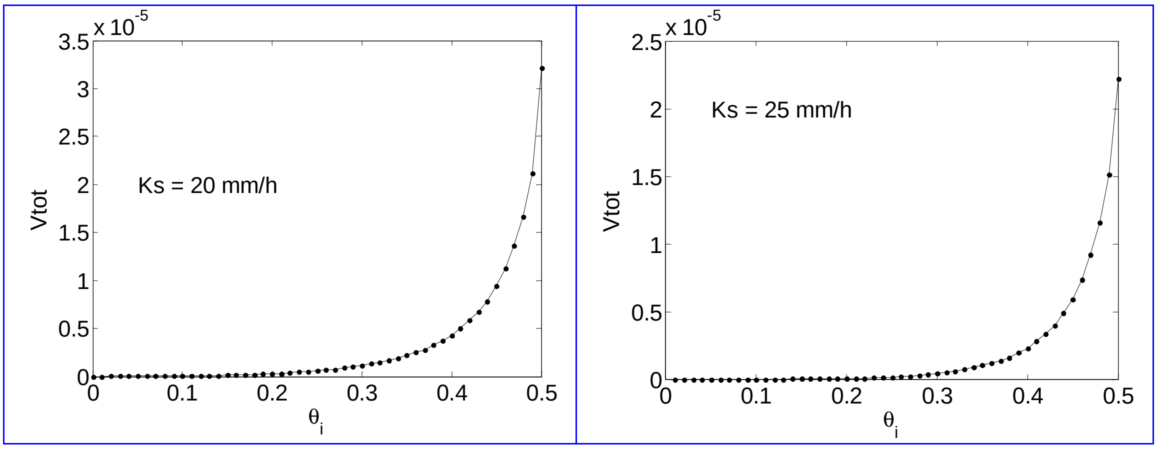
Detailed comments:

I like the use of threshold moisture contents that trigger runoff generation. Authors should provide an approximate value for this threshold moisture content (by field), based on simulated results. The importance of the threshold moisture content should be emphasized in the abstract of the paper.

The reviewer is thanked for that comment that permitted to better explain the runoff generation in our simulations. Soil moisture thresholds are given and discussed in the “Discussions” section. The infiltration component of the CREHDYS hydrologic model is also better described in the “Hydrologic model” section. Finally, the importance of these thresholds is also better emphasized in the abstract. See below for further explanations:

In the CREHDYS hydrologic model, the infiltration is modelled using the Green-Ampt approach (Green and Ampt, 1911), please see the revised manuscript, in the "Hydrologic model" section for further explanation. Runoff generation is expected to be caused by Hortonian process (i.e., infiltration excess overland flow) at the beginning of the simulation and by saturation excess flow when the soil moisture reaches the soil moisture at saturation. Nevertheless, both phenomena are modeled in CREHDYS using the same equations.

In order to observe the threshold antecedent soil moisture that triggered runoff, simulations were conducted in a single cell model with varying antecedent soil moisture values. In this study, the same parameterization was used for all the field campaigns, except for the initial soil saturated hydraulic conductivity (KS) that was set to 25 mm/h for Burnia and to 20 mm/h for the other fields. In the figure below we present the total runoff volume as a function of the antecedent soil moisture for the two different parameterisations, i.e., KS=20 mm/h and KS=25 mm/h. It can be observed that the thresholds are approximately equal to 0.20 m^3/m^3 and 0.25 m^3/m^3 for the two parameterisations, respectively. Runoff is generated at a certain soil moisture threshold because rainfall intensity exceeds the effective infiltration capacity. It is worth noting that these values depend on the rainfall that is used in the simulations. These soil moisture thresholds were discussed in the "Discussions" section and also permitted to better explain Fig. 5, where the variability between extreme scenarios was found to be larger for field campaigns with dry antecedent soil moisture conditions.



Please consider rewording two statements on Page 8958: 1) "fine-scale statistical properties of soil moisture can be found in the literature" – this seems very optimistic; and 2) "even without high- resolution soil moisture data, similar scenarios of high-resolution soil moisture maps as in this paper could be constructed using solely a mean soil moisture, topography data and adequate soil moisture statistical relationships" – this likely is not true for areas especially where the TWI is not a correct predictor.

The whole paragraph was revised accordingly and moved to the "Discussions" section. The paragraph now presents more generally the possibility of the disaggregation of coarse-scale remotely-sensed soil moisture data when fine-scale soil moisture patterns can be explained by other sources of fine-scale information (e.g., digital elevation model, soil information).

The correlation between soil moisture and TWI was pretty small (Table 2). The justification for using TWI despite this low correlation is addressed in section 3.2.1, but the speculation as to the applicability of the TWI is still a little weak to justify its use in this study. Line 20 in the abstract is therefore not based on the actual data, but on speculation. As noted above, if subsurface redistribution is a factor, it would be another strengthening argument for its use.

The reviewer raised an important point concerning the choice of the antecedent soil moisture scenarios. Actually, in a preliminary stage of the study, other topographic indices were tested: the curvature of DEM, the hill-shade as well as the computation of the TWI based on the multiple flow direction (MD8) (Quinn et al., 1995) and the infinite flow direction (Tarboton, 1997). None of these indices outperformed the TWI (based on a single direction flow accumulation algorithm) for all the campaigns nor explained better soil moisture patterns. Unfortunately, the lack of spatially-detailed soil information precludes the use of a soil-based index, which may be more relevant for explaining soil moisture patterns, especially for relatively flat fields. All the same, as this paper attempts to generalise the findings of Merz & Plate (1997) and Merz & Bardossy (1998), we decided to use the same method of computation of the TWI as in these studies.

The justification of the use of the TWI was improved in the “Antecedent soil moisture scenarios” section (see explanations above). The correlation between measured soil moisture and TWI and its relation with the performance of the TWI-based scenario were largely discussed in the “Discussions” section.

Line 20 of the abstract was modified as follows:
The most efficient scenario for modeling the within field spatial structure of soil moisture appeared to be when soil moisture is directly arranged according to the TWI, especially when measured soil moisture and TWI were correlated.

Specific (editorial) comments:

Page 8948, line 5: remove ‘the’ before ‘runoff’
Corrected

The idea of the ‘best scenario’ is confusing in introduction.
The last sentence of the introduction was removed, as this was redundant with the objectives of the study presented above.

Page 8949, lines 1-3: This statement requires a citation.
The work of “Zehe, E. & Blöschl, G. Predictability of hydrologic response at the plot and catchment scales: Role of initial conditions Water Resources Research, 2004, 40, W10202” was cited.

Page 8949, lines 4-24: replace larger/largest discharge with greater/greatest discharge. As I interpret discharge as an instantaneous volume/time, do you mean peak discharge or overall discharge and thus refer to an elevated hydrograph?
The term “runoff” was used instead of “discharge”, which was more in accordance with the beginning of the introduction.

Page 8949, line 25: remove ‘on’; remove ‘moreover’
Corrected

Page 8950, line 12: remove ‘moreover’
Corrected

Page 8950, line 17: replace ‘In a near future’ with ‘In the near future’ or with ‘In the future’
Corrected

Page 8950, line 18: replace ‘largely’ with ‘greatly’
Corrected

Page 8950, line 22/23: replace ‘potentialities’ with ‘potential’; remove ‘the’ before ‘soil moisture’; remove ‘a’ before ‘high resolution’
Corrected

Page 8950, line 29: remove ‘a’ before ‘particular interest’
Corrected

Page 8951, line 2/3: why use a new term ‘soil moisture organisation’ and ‘soil moisture scenario’ here, while above you only refer to soil moisture variability or soil moisture pattern?
Corrected

Page 8951, line 18: remove ‘moreover’
Corrected

Page 8954, line 2: replace ‘has’ with ‘was’
“has driven along parallel tracks” was replaced by “followed parallel tracks”

Page 8955, line 17/18: remove ‘the’ before ‘hydrologic modelling’; write out 7 as ‘seven’
Corrected

Page 8955, line 24: ‘permuted’ must be ‘permutated’?
Corrected

Page 8956, line 2/3: remove ‘The’ and start sentence with ‘Scenarios’; remove ‘the’ before ‘scenarios’

Corrected

Page 8956, line 4: replace ‘performed’ with ‘created’?
“performed” was replaced by “produced”

Page 8956, line 14/15: replace ‘maximal’ with ‘maximum’; add ‘having an’ as in ‘that avoids having an empty pixel’
Corrected

Page 8956, line 25: same resolution as : : :?
Corrected as: “same resolution that was set in the first scenario.”

Page 8958, line 8/9: place ‘moreover’ at the beginning of the sentence as: ‘Moreover, the true: : :’
Corrected

Page 8961, line 13: replace ‘has’ with ‘have’
Corrected

Page 8967, line 1: replace ‘in average’ with ‘on average’
Corrected

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

References

Green, W. and Ampt, G.: Studies on soil physics: 1, flow of air and water through soils, J. Agr. Sci., 4, 1–24, 1911.

Quinn, P. F.; Beven, K. J. & Lamb, R. The LN(A/tan-BETA) index - How to calculate it and how to use it within the TOPMODEL framework Hydrological Processes, 1995, 9, 161-182

Tarboton, D. A new method for the determination of flow directions and upslope areas in grid digital elevation models Water Resources Research, 1997, 33, 309-319

Zehe, E. & Blöschl, G. Predictability of hydrologic response at the plot and catchment scales: Role of initial conditions Water Resources Research, 2004, 40, W10202