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 #3

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

1 General Comments

1.1 Contents and Relevance

The manuscript assessed the sensitivity of spatial variability of soil moisture in modelled runoff from agricultural fields. The foundation of the presented study is a remarkable soil moisture dataset derived with proximal ground penetrating radar (GPR). Measured soil moisture was gridded into an original soil moisture raster. The values of the original raster were reorganised in space following six scenarios with different degree of spatial organisation. The primary focus of the manuscript is about how runoff simulations differ if the six soil moisture scenarios instead of the original soil moisture raster are used in a spatially distributed hydrologic model. The authors demonstrated that synthetically arranged soil moisture data could generate more feasible runoff simulations than a constant soil moisture grid. The manuscript has the potential to contribute new information within the scope of HESS: it is relevant how soil moisture could be incorporated in spatially distributed hydrologic models applied to agricultural fields with scarce soil moisture data. However, the methodology needs to be explained in more detail, and the discussion should give more credit to related work. These issues are addressed below.

1.2 Hydrological Processes and soil moisture in the model

Neither the actual hydrological processes on the assessed fields (or implicitly used perceptual models of the authors) nor the model structure are sufficiently described. Both were necessary to understand the sensitivity of runoff simulations to spatial soil moisture variability. Even though the model structure can be seen in cited literature (Laloy and Bielders, 2008; Laloy and Bielders, 2009), the authors should clearly point out the model components that incorporate soil moisture processes. A brief description of how soil water content is used in the relevant model components (percolation, evapotranspiration, infiltration model; depression storage?) would be helpful to understand the results of this manuscript.

The subsection “2.4 Hydrologic model” has been thoroughly rewritten in order to better describe the modelled relevant soil moisture processes. The high sensitivity of antecedent soil moisture to the runoff response in the hydrological model (see Laloy and Bielders, 2008) was outlined. Basically, soil moisture is only involved in the infiltration component of the CREHDYS model. Infiltration is computed using the Green-Ampt model (Green and Ampt, 1911) which assumes a uniform wetting front infiltrating vertically. For simplicity, no surface storage was considered in this study. At the event scale, no percolation and evapotranspiration were taken into account. More details are given in the subsection “2.4 Hydrologic model”. The soil moisture thresholds and the relation of soil moisture with the runoff generation were further discussed in the “Discussions” section.

In addition, more information was provided concerning the five fields (i.e., soil type, soil cover, elevation range) were added in Table 1 and in the new section “Agricultural fields”. The better descriptions of the fields help to understand the hydrological processes occurring in the fields.

1.3 Ground Penetrating Radar

The ground penetrating radar (GPR) derived soil moisture measurement provides a helpful foundation for the analysis. However, the long GPR description in section 2 is somewhat distracting. The technical GPR derivation details on GPR setup, data inversion, and acquisition are not of central relevance for the major objective of the study - that is, the sensitivity of spatial soil moisture distributions on runoff simulations. Could some details be referred to previous studies (e.g. Lambot et al., 2004; Lambot et al., 2008)?

The reviewer was right about the too prominent place that was taking the GPR description in this study. The subsection “Sensing of soil moisture by ground penetrating radar” was reduced and now focus on soil moisture characterisation issues in relation with the current study (e.g., penetration depth).

1.4 Study Sites: Replications in Space and Time

The effects of soil moisture on simulated runoff are compared among five agricultural fields. However, it remains unclear what the benefit from using five fields is. Moreover, the comparability of the simulations among fields might be limited. From the first author’s website (but not this manuscript) it can be seen that the fields are located close to each other in flat terrain. Therefore, potential differences of topography and grid cell size among fields might probably only have minor influences on the results. However, different measurement times (many measurements were taken in March, one in July) might reduce the comparability among fields.

One of the main objectives of this study is to observe the effect of antecedent soil moisture spatial variability on the runoff response specifically in various field conditions, in order to investigate in which extent findings of previous studies can be generalised. The five fields were better presented in a new subsection entitled “Agricultural fields” in the beginning of the “Materials and Methods” section. In particular, soil types were given, with textural information when available, as well as topography and land cover information. The Belgian fields (Burnia, Marbaix & Walhain) were similar in terms of soil type and all fields were quite similar in terms of topography. The influence of grid sizes was investigated in particular for Marbaix, 15 April 2009 and varying grid sizes did not drastically alter the hydrographs (see next comment’s answer). The particular behaviour of Walsdorf was discussed and mainly explained by its particular soil moisture pattern that may be influenced by the season (i.e., more influence of evaporation in summer).

1.5 Rastering Influence

The rasters were setup so that at least one measurement point fell within each grid cell (p8956, l13). However, there seems to be substantial scale dependence of the results (e.g. p8961, l11: ‘correlation may increase at a larger scale’), which seems to be logical since small scale variability averages out. How much is the comparability among fields reduced by different grid cell sizes? Furthermore, the original CREHDYS model was designed for 0.3m grid cells. What are the implications of using cell sizes up to a 15m this study?

The reviewer raised an important point about the resolution scale dependency of the results. It is worth noting that the statement “correlation may increase at a larger scale” was related to the correlation between measured soil moisture and the TWI, and not to the behaviour of the simulated discharge using the TWI-based scenario. To investigate the rastering influence, increasing grid sizes was investigated for Marbaix, 15 April 2009, which was chosen as it was conducted on the largest field at high resolution, maximising the grid size possibilities. Increasing grids sizes only slightly increases the correlation between soil moisture and TWI while there was no clear effect on simulated hydrographs and on the order of scenarios. The methodology with respect to the increasing grid sizes was explained at the end of the "Antecedent soil moisture scenarios" subsection. The related new results were included in the "Evaluation of soil moisture modelling scenarios" subsection and discussed in the "Discussions" section.

Lastly, the CREHDYS model was indeed designed for a 0.3 m grid. However we concur with Laloy and Bielders (2009) to consider that it can be used straightforward at the field scale. Indeed, the governing equations and hydrological processes of the CREHDYS model also apply at the field scale. For instance, the event-scale modelled processes of CREHDYS share a lot in common with the LISEM model of De Roo et al. (1996), which apply to field and small catchment scales. It is true, however, that values of the relevant hydrological submodel parameters may be different at the field scale than at the plot scale.

1.6 Nugget Effects and Scenarios

Substantial nugget effects have been observed (Table 2). However, I am not sure how much of the nugget effects can be explained by GPR measurement errors and microtopography (p8960, l24). Is it not more likely that the acquisition tracks dominate the nugget effect? Is it a good idea to apply an omnidirectional semivariogram to a clearly structured system (Figure 2)? This is of major importance, since the 'spatially structured' scenarios 6 and 7 actually exhibit low spatial structure (e.g. Figures 1e and 1f) due to the large nugget effect. This behaviour is correctly described in the manuscript (p8966, l14); however, does it even make sense to look at 'spatially structured' scenarios (e.g. Figures 6, 7, and 8) if they are close to random patterns?

The reviewer has raised an important limitation with respect to the computation of variograms that were used for *variogram* and *connected* scenarios. The uneven disposition of the measurement points (i.e., spacing along the acquisition tracks is 2 m whereas acquisition tracks are offset by 5 to 15 m) may overestimate the nugget effect when using omnidirectional variograms. New variograms were computed along the acquisition tracks only and the "line effect" was quantified, as it was already studied for a previous GPR field acquisition (see Minet et al. (2011)). The new variograms had reduced nugget variances and thus showed larger spatial coherence when used for soil moisture pattern simulation (see new Fig. 1, which is now Fig. 2). New hydrologic simulations using the new variograms were performed for all fields for *variogram* and *connected* scenarios. Following these new simulations, Table 2 to 4 and Figs. 3 to 8 were updated.

The new variogram for Marbaix, 15 April 2009 is presented in Fig. 3 and shows smaller nugget effect and larger range. The average *variogram* and *connected* hydrographs did not change a lot in Table 3 and Figs 4 to 8. Nevertheless, increase in spatial coherence of the antecedent soil moisture maps led to a larger variability in hydrographs for most of the field campaigns. The same order between the variograms was obtained and it did not change the interpretation. However, the discussion about the nugget effect was modified (i.e., smaller nugget effect were obtained).

1.7 Presentation Quality

The structure of the manuscript sections is not always satisfactory (e.g. method and discussion statements in the results section). Furthermore, there is too much stress on the soil moisture measurement campaign, which is of minor importance for the primary focus of the manuscript (i.e. model sensitivity to spatial soil moisture distributions). The discussion section needs to be extended with a critical assessment of the results based on previous research.

Figures are generally clear and tables are well structured. In particular Figure 1 helps to understand the manuscript. However, shorter table and figure captions are preferred. Furthermore, there is a lot of repetition in the text and captions (e.g. p8961, l25: study locations and dates; p8963, l7 to l10 and Figure 4: 1000 realisations; rainfall on second Y-axis).
The language is not always fluent and precise (e.g. p8962, l10 et seq.). The words ‘the’ and ‘very’ could often be omitted. Some sections (e.g. conclusions) are quite choppy. The manuscript would benefit from a review by a native English speaker.

The whole manuscript was revised according to these comments. In particular, the description of the GPR method for soil moisture sensing was simplified (see comment 1.3’s answer). The discussion was strongly improved by a better integration of the results with findings of previous researches and a deeper interpretation of the results. Figures and table captions were revisited. Finally, the manuscript was carefully re-read by all authors with a particular attention to language issues.

2 Specific Comments

Title: I suggest pointing out that the assessment of effects on runoff was based on simulated (!) data. Moreover, it is not the variability, but the spatial distribution of soil moisture that was studied. The abbreviation GPR is not necessary in the title since the soil moisture measurement method is not the part of research that is particularly novel. Consequently, it is suggested that the authors use a more accurate title (e.g. ‘sensitivity of spatial soil moisture distributions on runoff simulations’).

According to the comments of all reviewers, the title was modified as follows:
“Effect of high-resolution spatial soil moisture variability on the simulated runoff response using a distributed hydrologic model”

p8948, l21: The last two sentences of the abstract are fairly unspecific.

The last sentence was eliminated as the soil and rainfall conditions were not deeply investigated in this study. The second last sentence was modified as follows: “These observations generalised our current knowledge about the impact of antecedent soil moisture spatial variability on the field scale runoff.”

p8948, l26: The importance of soil moisture for real runoff generation processes and not only their representation in hydrologic models should be pointed out.

In our view, the first sentence of the “Introduction” section states the importance of antecedent soil moisture conditions on the runoff in real hydrological processes whereas the second sentence states that, as a result, soil moisture in an important variable in hydrological modelling.

p8949, l6 (and the following sections): Bulk reference lists are not helpful; please clearly provide which effects of soil moisture have been found in previous modelling studies. Furthermore, it were interesting to know why structured soil moisture patterns resulted sometimes in higher but sometimes in lower discharge compared to random soil moisture patterns. The authors should present dominating factors (e.g. climate, season, soil type, land use) and runoff generation mechanisms of previous research. This could help to understand and categorise previous research findings.

The references written in p8949, l6 were removed as they are all detailed in the two following paragraphs. The fact that structured soil moisture patterns resulted in lower discharge in Bronstert and Bardossy (1999) was attributed to the actual poor organisation of the structured pattern that was observed in that study because of dry conditions. The dominating factors on runoff in these studies were also better presented and compared to our results in the “Discussions” section.

p8949, l21: Not only infiltration, but also saturation excess overland flow is affected by antecedent soil moisture.

The sentence was modified as follows: “The large effect of the soil moisture variability on the runoff response is to be attributed to the prominent role of soil moisture in the runoff generation by either infiltration excess or saturation excess overland flows.”

p8949, l25: precipitation-dependence of soil moisture sensitivity in previous research is mentioned. Does the applied ‘typical’ rainfall of this study fall within rain intensities and durations for which high sensitivity of soil moisture on runoff generation can be expected?

In an early stage of the study, several measured rainfall events were tested. For much lower rainfall intensity, no runoff at the outlet could be observed (nor generated and transported to the outlet), which is quite obvious. For much larger rainfall intensity, no difference between scenarios would be observed, which is stated in p8950 l2-3 by the study of Castillo et al. (2003). The rainfall event used in this study was better defined in the “Hydrologic model” subsection and had a return period of 6 years.

p8950, l7: It should be clearly mentioned under which conditions (e.g. steep vs. flat topography; wet vs. dry periods) soil moisture was found to have a dominating effect on accuracy of runoff predictions. This is of central importance to understand the findings of this manuscript.

The following sentences were added:
“The effect of spatial variability of soil moisture seemed to be observed particularly in steep topography (Kuo et al., 1999; Castillo et al., 2003) that allows lateral redistribution of water over the catchment. It is also expected to be maximal in dry conditions as shown in Merz and Plate (1997) where two antecedent soil moisture conditions were compared. It is worth noting that wet conditions inherently exhibit low spatial variability because of the bounded behaviour of soil moisture by the saturation (Famiglietti et al., 2008).”

p8950, l25: The described scale gap increases the relevance of this manuscript, since hydrologic predictions often rely on soil moisture information at the missing scale. However, this has not been described in this section. And the scales (‘fine-scale’, ‘coursescale’, ‘large-scale’) should be described more precisely.

The relevance of the “scale-gap issue” for hydrologic modelling at an intermediate scale was emphasized. The same scale terminology (i.e., coarse-, and fine-scale) were used throughout the study and defined in terms of order of magnitude.

p8951, l1: The difference between the two objectives is not clear. It seems that there is only one objective (sensitivity of spatial soil moisture distributions on runoff simulations).

The first objective refers to the observed effect of soil moisture spatial variability on the runoff by testing seven antecedent soil moisture scenarios. The second objective refers to the comparison of these scenario effects between the fields. These two objectives were better phrased as follows: “(…) this paper aims to: 1) investigate the effect of different scenarios of the spatial structure of antecedent soil moisture on simulated runoff at the field scale and; 2) find the spatial structure of the within field soil moisture that the most closely approaches the measured soil moisture pattern in terms of hydrologic response.”

p8952, l2: No abbreviations in the title.

Corrected

p8953, l3: Equation or reference for the 3-D Maxwell’s equation missing.

The related information was withdrawn for the simplification of the “Sensing of soil moisture by ground penetrating radar” section.

p8953, l6: How is roughness height defined?

The related information was withdrawn for the simplification of the “Sensing of soil moisture by ground penetrating radar” section.

p8953, l9: Reference for the Levenberg-Marquardt algorithm missing.

Corrected

p8954, l6: It would be helpful to introduce the study region (five fields) earlier in a separate section.

We agree with the reviewer that a separate section presenting the fields more in details was missing. The first subsection entitled “Agricultural fields” of the “Materials and Methods” section now presents the 5 fields with respect to their geographic situations, topography, soil types and crops at the time of the surveys.

p8954, l12: It is not clear what ‘largest watershed’ means. In particular looking at Figure 2, it seems that the ‘watersheds’ are not clearly delineated. Are there ditches or agricultural roads around the fields?

The largest watersheds were delineated in each field surveyed by the GPR and the fields were considered as hydrologically isolated from the others. This was clarified in the new section “Agricultural fields”.

p8955, l1: The section title is a bit misleading; I suggest a title that points out that different scenarios were assessed (‘antecedent moisture scenarios’).

The subsection was renamed “Antecedent soil moisture scenarios”.

p8955, l11: Variograms and connectivity functions can also describe extreme patterns of organisation.

In this part, we distinguish the stochastic and deterministic variability of soil moisture, where the limit between the two can be defined by the necessity of introducing auxiliary spatial information or not. It is true that variograms and connectivity functions may apply for whole ranges of soil moisture organisation, except the extreme pure random pattern. The text was modified as follows:

“Between these two extremes, hydrological systems exhibit soil moisture conditions that can be modelled from pure random variability to highly structured soil moisture patterns, with intermediate degree of organisation (Western et al., 1999). However, the introduction of auxiliary spatial data (e.g., topography) to simulate soil moisture defines the limit between stochastic and deterministic variability. (…) Except the pure random case, soil moisture patterns can be captured using variograms or connectivity functions."

p8955, l20: The name 'true' is a bit misleading; another name ('measured', 'observed') is preferred.

The name "true" was replaced by "measured" throughout all this study. Confusion with the measured point measurements were carefully avoided in text by specifying "point measurements" when referring to raw GPR soil moisture measurements.

p8956, l9: Figure 2 should be introduced earlier (e.g. at the beginning of this section).

This figure was introduced before enumerating the seven scenarios as follows and thus Figs. 1 and 2 are now inverted:
"In this study, soil moisture scenarios are based on point measured data, which are displayed as an example for Marbaix, 15 April 2009 in Fig. 1."

p8956, l23: Why was a single direction algorithm used? With the given grid cell sizes between 7 and 15m D-infinity might be more realistic.

As suggested by the assigned editor, we tested other computations of TWI using the multiple flow direction (MD8) (Quinn et al., 1995) and the infinite flow direction (D-infinity) (Tarboton, 1997). The new computation using these two indices were not satisfactory in terms of correlation between measured soil moisture and TWI and because it did not result in the same determination of watersheds, especially for the D-infinity. Furthermore, a single direction method was used to compute the TWI in other studies (Merz & Plate, 1997; Merz & Bardossy, 1998) that are compared to our results.

p8957, l3: I do not agree that topographic indices have a high predictive power in small catchments. Catchment size is not a first order control on the performance of topographic indices; morphology, soil type or climate might be more important. In particular, the application of topographic indices in flat terrain is critical.

The reviewer points the limitations in the justification of the use of the TWI. The justification was modified as follows:
"The TWI was chosen for modelling structured soil moisture patterns because of the lack of other detailed sources of information for these fields (e.g., soil properties, vegetation) and for its high predictive power in wet conditions. (Western et al., 1999). The limited elevation range of the fields may however limit the redistribution of water according to the topography and restrain the explaining power of the TWI for soil moisture in these fields. Nevertheless, although high-resolution soil information at the field scale could have provided more insights for explaining moisture patterns, no high-resolution soil parameters could be found at the catchment scale (> 10 km). We thus investigated the use of topographically-derived indices (i.e., TWI) for soil moisture modelling in a data-scarcity context. As soils were bare or nearly-bare, the influence of vegetation heterogeneities on spatial soil moisture variability might be furthermore limited in our study. For larger catchment scale (>10 km), land cover differences among the fields may better explain soil moisture patterns (Western et al., 1999). For drier climatic conditions, when potential evapotranspiration exceeds precipitation, local controls as potential radiative indices have shown better correlations with observed soil moisture (Grayson et al., 1997). Some reviews about the predictive power of the TWI for soil moisture can be found in Western et al.

(1999) and Sørensen et al. (2006)."

p8957, l5: Reference missing.

The following reference was added:
Western, A. W.; Grayson, R. B.; Blöschl, G.; Willgoose, G. R. & McMahon, T. A. Observed spatial organization of soil moisture and its relation to terrain indices Water Resources Research, 1999, 35, 797-810

p8957, l7: evapotranspiration -> potential evapotranspiration.

Corrected

p8957, l15: More precise details are necessary to understand how the maps for scenario 6 were derived.

The following information was added:
"The sixth scenario (*variogram*, Fig. 1 (e)) maps were made by simulating gaussian soil moisture patterns using variograms describing the spatial dependence of soil moisture. Variograms were computed considering the spatial dependence of the data along the acquisition lines only, neglecting the spatial dependence of the data of adjacent lines (Minet et al., 2011). An exponential model accounting for a nugget effect was fitted for all the variograms. Zero-mean gaussian distributed values were then simulated in each grid pixel using an implementation of the sequential non conditional method."

p8958, l6 to l9: the terms 'statistical properties' and 'geostatistical properties' are too general.

The whole paragraph was rephrased as follows:
"It is worth noting that all scenarios have the same mean as the *measured* scenario, and that all scenarios, except the *constant* one, show exactly the same distribution as the measured scenario, owing to the ranking procedure. Moreover, the *measured, variogram* and *connected* maps were characterised by the same variogram. This allowed to truly compare the modelling discharge between the scenarios."

p8958, l21: please name the specific findings in previous research; no bulk reference citing.

All these studies studied the empirical relationship between mean and standard deviation of soil moisture using various soil moisture sources and for a wide variety of extent scales. The sentence was modified as follows (and moved to the "Discussions" section):
"In that respect, several authors have proposed empirical relationships between the mean soil moisture and its corresponding standard deviation for different extent scales using soil moisture data from remote sensing estimates and invasive sensors at various extent scales (Western et al., 2003; Vereecken et al., 2007; Famiglietti et al., 2008)."

p8959, l8: The model flow paths are derived from topography. How much does this interfere with the findings of this study (e.g. performances of soil moisture distributions according to the topographic index)?

As no large deviating structures were present in the field, due to their single tillage managements (i.e., one single crop per field), we can expect that flow paths were mainly governed by topography. Nevertheless, deviating structures as wheel tracks could slightly deviate the flow path in real conditions. This was noticed in the "Discussions" section:

“In the hydrologic simulations in this paper, flow paths are governed by topography, but it is worth noticing that in reality, deviating structure within (e.g., wheel tracks) and between (e.g., ditches, roads) fields may limit the use of solely topographically-driven hydrologic modelling. If not accounted for in real case experiment, it would reduce the relationship between the explaining power of the TWI for soil moisture and the runoff response using the *structured* scenario.”

p8959, l11: It would be helpful to have a table or flowchart with processes/parameters that are linked to soil moisture distribution.

The role of soil moisture within the CREHDYS model is now described in subsection 2.4 “Hydrologic model” (see also Comment 1.2’s response). At the event-scale, initial soil moisture distribution plays a role in the infiltration component of the model only, as no surface storage and evaporation are considered. Therefore, no table or flowchart would be necessary.

p8960, l27: In general, I agree with this statement (which should belong in the discussion section); however, it could be important to know if observed soil moisture of this study were too small to see the effect described by Grayson et al. (1997).

New variograms along the acquisition tracks were computed following the comment 1.6. When comparing new Nugget/Sill ratios with mean soil moisture, no clear relations appeared, except for Burnia where a decrease in the Nugget:Sill ratio is observed with increasing soil moisture. The lack of a full range of soil moisture conditions was underlined as a possible explanation for non-observing the relation between dry conditions and the nugget effect. This was moved to the “Discussions” section.

p8961, l4: Why is only scenario 3 (TWI) and no other scenario compared to measured soil moisture? Correlation coefficients could easily be calculated, and could give an idea of the deviation of all scenarios from measured soil moisture. And why is this section (evaluation of the TWI model) under the title ‘soil moisture measured by GPR acquisition’?

There was a confusion between the topographic wetness index (TWI) computed for each field and the results of the TWI-based soil moisture scenario. Because of this, we decided to call the two soil moisture scenarios based on the TWI the *structured* and the $structured_{inv}$ scenarios instead of the *TWI* and TWI_{inv} scenarios, respectively.

In addition, the subsection “Relation between topographic wetness index and measured soil moisture” was removed and the part concerning the observed TWI-θ relationship was moved to the “Surface soil moisture measured by ground penetrating radar” subsection. The part discussing the TWI-θ relationship was moved to the “Discussions” section.

p8961, l9: The authors should comment in the discussion section what the low to negative correlations between TWI and soil moisture mean, and why they are important for this manuscript. Considering the low correlation coefficients, the conclusions (TWIbased soil moisture is better than a constant value) should be critically reconsidered.

These important considerations were discussed in the “Discussions” section, including the relationship between the explaining power of the TWI for soil moisture and the good performance of TWI-based scenarios, even when measured soil moisture was poorly correlated to the TWI.

p8962, l17: These statements (and Figure 5) are interesting. However, how comparable are the measurement data from the five fields? Do they only differ by the mean soil water content or also other factors (topography, soil type, soil cultivation, land use, climate, day or season of measurement)?

The particular soil moisture patterns in each field might be the result of the topography, soil type, meteorological forcing, etc, but a comprehensive study of these soil moisture patterns is beyond the scope of the study. Nevertheless, it is worth mentioning that repeated measurements in Marbaix and Burnia exhibited a temporal stability of soil moisture patterns and also a better comparability in terms of runoff responses (it is now mentioned in this subsection and in the "Discussions" section). The particular case of Walsdorf, where the soil moisture was not related to the topography was also discussed in this subsection.

p8963, l9: How robust is the analysis with 1000 random replications? Even for the field with the lowest amount of grid cells (Keispelt, approx. 210 grid cells) 1000 replications seem to be fairly low considering the 210! (i.e. 10ˆ398) potential permutations. Therefore, it might not be surprising that the extreme scenarios are not represented with 1000 replications (Figure 6).

The limitation was emphasised in the "Antecedent soil moisture scenarios" subsection:
"But the probability that the *random* scenario yield a particular realization is drastically low, i.e., equals to 1/n!, where *n* is the number of pixels, and may not be encountered in our study. The number of 1000 realisations for the stochastic scenarios is thus a tradeoff between the computation time and the desirable variability among realisations."

p8963, l14: It is unclear where the different ranges among scenarios (Figures 6, 7, and 8) come from. Do they originate from the smaller amount of permutations given a certain spatial organisation compared to the 'random' scenario?

The different ranges among stochastic scenarios were discussed in the "Discussions" section:
"This larger variability is to be attributed to the spatial coherence of groups of non-contributing reinfiltrating pixels that can be placed on or completely outside the flow channel, resulting in a small or great discharge, respectively. The probability that numerous reinfiltrating pixels are present on the flow channel is smaller in the *random* scenario than in stochastic scenarios accounting for spatial coherence."

p8964, l14: Is it a good idea to normalise the Nash-Sutcliffe efficiency (NSE)? Advantage: Scenarios can be compared. Disadvantages: Limited comparison among watersheds and measurement season.

Following a comment of the second reviewer, we decided to present the non-normalised NSE in the table, whereas the mean and standard deviation between the fields were computed based on the normalised NSE. This permitted to increase the information content in the table.

p8965, l1: The content of Figure 9 could be meaningful, since it compares agreement of model input (soil moisture correlation) with agreement of model output (runoff NSE). However, this figure should be followed by a rigorous cause-and-effect discussion. It does not speak in the model's favour that negative correlation coefficients for Walhain correspond to NSE values above 0.9. How can this effect be explained?

Simulations using the stochastic soil moisture scenarios have shown that a wide variety of runoff responses can be obtained depending solely on the location of runoff contributing areas. In that respect, a soil moisture pattern which is poorly correlated with the TWI could result in a runoff response close to the one of the TWI-based soil moisture scenario. For instance, we can imagine a soil moisture pattern with contributing areas close to the outlet but just beside the flow channel, which results in a large discharge (as the TWI-based scenario) but a small TWI-θ correlation. In other words, a large number of antecedent soil moisture maps can result in the same hydrograph (non-unicity of model inputs with respect to the outputs).

This issue was widely discussed in the “Discussions” section (“Soil moisture patterns and its relation with topographic wetness index” subsection).

p8965, l15: Please be more specific about the ‘threshold behaviour’ of the hydrologic model. Do the authors mean the soil saturation deficit? And which modules of the hydrologic model are responsible for the substantial variability in runoff simulations given different spatial soil moisture distributions? Should there not more hydrograph clusters be seen in Figures 6 to 8 if the author’s assumption of ‘threshold behaviour’ were correct?

This first part of the “Discussions” section was improved with respect to the threshold behaviour and the runoff generation. In the hydrologic model, above a particular soil moisture threshold depending on the intensity of the rainfall with respect to infiltration capabilities, runoff is generated by infiltration excess overland flow. The hydrologic model was also better detailed with respect to the role of soil moisture in the “Hydrologic model” section. It is believed that no hydrographs clusters appeared in Figs. 6-8 because of the integrating effect of the simulation over numerous pixels, while it might appear using single-cell simulations.

p8965, l17: Do the authors mean saturation overland flow (SOF) instead of Hortonian infiltration overland flow (HOF)? This is of major relevance for this manuscript, since spatial distributions of antecedent soil moisture might have a higher impact on SOF than on HOF. Consequently, the model structure description and discussion of this manuscript should carefully delineate the relationship between soil moisture and relevant runoff generation mechanisms in the CREHDYS model.

The hydrologic model and especially the infiltration component were better detailed in the “Hydrologic model” section. Runoff is generated at a certain soil moisture threshold because rainfall intensity exceeds the effective infiltration capacity. This may occur either before the soil to be saturated (Hortonian overland flow) or after soil saturation (saturation overland flow). Note that the model discrimination between Hortonian and saturation overland flow is limited by the fact that a single effective hydraulic conductivity is assumed for both the eventually present thin surface crust and the larger sub-crust area. It is worth noting that both phenomena are modeled in CREHDYS using the same equations.

p8966, l4: Can evidence for the relationship between contributing areas and increasing soil moisture in your model be provided?

With minor modifications, the CREDHYS model could provide soil moisture as an output. But it is clear from the model equations that for increasing soil moisture, the runoff increases.

p8967, l5: ‘non-conditional’; conditional on what?

Non-conditioned on the soil moisture measurements. This sentence was removed from the “Discussions” section.

p8967, l7: ': : :as the simulated soil moisture patterns are not related to topography which is hydrologically determinant'. Is this an assumption? Is this a finding? If yes, please provide clear evidence.

Simulated soil moisture patterns were not specifically related to the topography. This sentence was removed from the "Discussions" section.

p8967, l21: 'large' predictions do not always mean 'safe' predictions.

It is true that the TWI-based scenario did even not give the overall largest predictions (it is the *variogram* scenario). Moreover, this sentence is referring to extreme hydrological events that were not studied here and that may not give rise to the same conclusions (see the Introduction and Noto et al. (2008)). Therefore, this sentence was removed.

p8968, l2: suggestion: 'spatial distribution' instead of 'variability' (could be spatial or temporal variablilty).

It was modified as follows: "spatial variability" (and in other parts of the manuscript)

p8968, l10: Constant soil moisture did not always result in lower discharge (e.g. compared to inverse TWI).

The sentence was modified as follows: "Spatially constant antecedent soil moisture conditions (*constant* scenario) resulted in a smaller discharge than scenarios exhibiting soil moisture spatial variability, except for the $\textit{structured}_{inv}$ scenario."

p8968, l19: ': : :explained in terms of contributing areas'. No evidence has been provided to support this statement.

The concept of contributing areas was largely developed in the revised "Discussions" section and explained most of the observed behaviours of the spatial variability of soil moisture on runoff response. This concept was also used in previous studies for similar explanations.

p8969, l5: 'good method': this was the best method, but is it a good method? I have major doubts if the TWI is an acceptable method in particular in flat terrain.

The sentence was modified as follows:
"In the absence of other detailed source of information, organising the soil moisture pattern accordingly to the TWI appeared to be the best soil moisture modelling method, even when TWI was poorly correlated to measured soil moisture."

Table 1: It would be interesting to know if there were any precipitation events before the measurement.

It is believed that the soil moisture measurements gave the best indication of the wetness state of the fields.

Table 1: Shorter table caption is preferred (e.g. 'soil moisture acquisition').

Table 1 was reorganized (i.e., the number of point and duration were moved to Table 2) and the caption was replaced as follows: “Description of the agricultural fields and resolutions used in hydrologic simulations”.

Figure 2: Projection and references should not be in the figure.

The projected coordinate system was referred in the figure caption.

Figure 3: Units of semivariance missing: either [-] or [%2].

Corrected

Figure 4: Only four out of five fields are displayed. Why is the Keispelt field not shown? It would be interesting to see the July simulation as well (in contrast to the spring simulations).

The 10 field campaigns hydrographs could not be presented as figures in the paper for brevity reasons. The reader is thus referred to Table 3 for knowing the simulations results for all the fields. The July simulation in Walsdorf was presented in Fig. 4 (c).

Figures 4a, b, c: The difference between ‘variogram’ and ‘connected’ is hard to see.

The hydrographs for the “connected” scenarios were drawn in another colour.

Figures 6, 7, 8: Why are all deterministic scenarios shown in the figures of the stochastic scenarios? TWI and inverse TWI might be enough.

We preferred to show all scenarios to facilitate the comparison with Fig. 4.

3 Technical Corrections
3.1 Structure Suggestions
p8951, l5 to approx. l20: The structure is not logical. At least the GPR description belongs in the methods section.

The last section of the introduction, presenting the objectives of the study, was clarified. Statements belonging to the “Materials and methods” section were moved to that section or deleted to avoid redundancies.

p8958, l15 to l26: This belongs rather in the discussion than the methods section. In particular the future benefit of the presented method should be part of the discussion or the conclusion section.
Corrected

p8959, l21: -> methods section.
Corrected

p8959, l14 to 19: -> discussion section; a figure of all fields could help to understand the topography-related discussion.

The line effect was already discussed in Minet et al. (2011), so the discussion about this line effect was removed. The reader is referred to that paper. The presentation of all 10 field campaigns would be too long.

p8959, l24 to 26: -> discussion section.
Corrected

p8960, l1: -> methods section.
This sentence was removed (repetition)

p8960, l27: -> discussion section.
Corrected

p8961, l12 to l18: -> discussion section.
Corrected

p8961, l21 -> methods section (repetition).
This sentence was removed (repetition)

p8962, l5 to l8 -> discussion section.
Corrected

p8963, l3 to l5 -> discussion section.
Corrected

p8963, l14 to l17 -> discussion section.
Corrected

p8964, l13 to l17 -> methods section.
Corrected

p8967, l18 to l24 -> conclusions section.
Corrected

3.2 Writing and Language Suggestions
p8948, l6: ‘most closely as’ needs to be paraphrased.
The end of the sentence was modified as follows: “(…) and at finding the structure of the soil moisture pattern that approaches the measured soil moisture pattern in terms of field scale runoff.”

p8961, l10: consistent use of abbreviations (‘Fig.’ in this line; ‘Figure’ in line 24).
According to the “Textual and Visual Conventions” of the HESS journal, “Figure” was abbreviated as “Fig.” when encountered within a sentence but was not abbreviated when starting a sentence.

p8969, l14 and l15: Journal abbreviations used or not? Please be consistent.
Corrected, except for “Hydrological Sciences Bulletin” (abbreviation not found).

p8951, l8: ‘correct estimation of the runoff’ -> ‘adequate runoff estimation’ (since runoff simulations are rarely ‘correct’).
p8954, l7: summarize -> summarise (consistent use of British English).
p8957, l7: exceed -> exceeds.

p8959, l2: CREDHYS -> CREHDYS.
p8963, l23: very wide -> wide.
p8964, l9: coefficient -> coefficients (suggestion: consistent use of Nash-Sutcliffe efficiencies instead of coefficients).
p8965, l18: introducing a spatial variability -> introducing spatial variability.
p8965, l21: pattern -> patterns.
p8967, l1: in average -> on average.
p8986, l6: organisations -> organisation.
Table 3: maximum runoff peak -> runoff peak.
Figure 4a: 04/07/2008 -> 07/04/2008.
Figures 7 and 8, captions: ‘plain line’ -> ‘dashed line’.
The word ‘the’ could often be omitted. A few examples: p8948, l5, p8949, l4 (twice), l20, l25 (twice), l26, p8950, l2, l12 (twice), l15, p8951, l1, l2, l22, p8955, l2, p8956, l6, p8965, l20, p8967, l6, l16.
These editorial comments were all accounted for.

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.

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