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Hydrology and Earth System Sciences Discussions

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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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 simu-

lations 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.

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)?

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.

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?

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 micro-topography (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?

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.

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').

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

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

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.

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

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?

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.

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', 'course-scale', 'large-scale') should be described more precisely.

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).

p8952, l2: No abbreviations in the title.

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

p8953, l6: How is roughness height defined?

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

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

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?

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

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

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

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

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.

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.

p8957, l5: Reference missing.

p8957, l7: evapotranspiration -> potential evapotranspiration.

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

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

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

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)?

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

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).

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’?

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 (TWI-based soil moisture is better than a constant value) should be critically reconsidered.

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)?

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 repli-

cations 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).

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?

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.

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?

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?

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.

p8965, l4: Can evidence for the relationship between contributing areas and increasing soil moisture in your model be provided?

p8967, l5: 'non-conditional'; conditional on what?

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.

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

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

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

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

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.

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

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

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

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

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).

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

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

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.

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.

p8959, l21: -> methods section.

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

p8959, l24 to 26: -> discussion section.

p8960, l1: -> methods section.

p8960, l27: -> discussion section.

p8961, l12 to l18: -> discussion section.

p8961, l21 -> methods section (repetition).

p8962, l5 to l8 -> discussion section.

p8963, l3 to l5 -> discussion section.

p8963, l14 to l17 -> discussion section.

p8964, l13 to l17 -> methods section.

p8967, l18 to l24 -> conclusions section.

3.2 Writing and Language Suggestions

p8948, l6: ‘most closely as’ needs to be paraphrased.

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.

p8961, l10: consistent use of abbreviations (‘Fig.’ in this line; ‘Figure’ in line 24).

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.

p8969, l14 and l15: Journal abbreviations used or not? Please be consistent.

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.

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