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Hydrology and Earth System Sciences Discussions

Interactive comment on "Hillslope characteristics as controls of subsurface flow variability" *by* S. Bachmair and M. Weiler

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Thank you for the numerous useful review comments. We revised the manuscript according to the review comments.

RESPONSE TO GENERAL COMMENTS

The paper by Bachmair and Weiler searches for the controls of subsurface flow variability on hillslopes. They carried out an intense field study to obtain information on SSF on a grass field, and two forest sites. From the field data they derived response and predictor characteristics. In an elaborative statistical analysis they tested the partial correlation between the response variables and the predictor characteristics. Fur-

thermore, the authors applied the Random Forest approach to analyse the explained variance of the predictor variable. The study is very relevant and interesting for hillslope hydrology. However the study limits itself by only looking at one predictive characteristic at a time, while maybe combinations of predictive characteristics can better explain the variance in SSF. I think this point should be discussed. The paper is well written and well structured. Sometimes the paper is difficult to follow, because of the often used acronyms. It would be very helpful if an additional list of acronyms will be added, with a short explanation of the meaning and the units.

–> We discuss the issue about looking at one predictive characteristic at a time in the specific comments section. The partial correlation analysis indeed only shows the explanatory power of one predictive characteristic at a time. The random forest approach, in contrast, does account for interactions. This is why we chose this approach. The list of acronyms, also suggested by the second referee, was added.

RESPONSE TO SPECIFIC COMMENTS

P6893 L03: you may add [Gerrits, A.M.J., Pfister, L., Savenije, H.H.G. (2010): Spatial and temporal variability of canopy and forest floor interception in a beech forest, Hydrological Processes, Vol 24, 3011–3025.] as an additional reference for persistence of throughfall patterns. –> added

P6895 L04: Opening bracket is missing near Koeppen classification. –> added

P6895 L09: Order figures in text is order appearance of figures. Hence change order figure 2 and 3. –> changed

P6896 L05: Change sentence into: "However, many wells are shallower, due to: : :" –> changed

P6897 L01: What was the collecting area of the totalizators? –> The area of the funnel is 222 cm^2.

P6898 L05: None of the five selected events cover a winter period. Was there a specific reason for selected those five events? Please explain. –> We did not include winter events out of two reasons: First, we wanted to exclude winter processes that add even more complexity to the system (snow cover, snow melt, rain on snow, or frozen soil). Second, there were very few events during winter that a) were not influenced by such processes, b) showed a significant response in many wells (e.g. many events during which < 10% of wells responded), and c) had good data availability (e.g. some events with a high number of probe failures).

P6898 L07: I do not understand the meaning of AREANORM. What do you mean by area below the water table time series? Is this simply the average water level over a certain time (normalized to well depth)? What is the unit of AREANORM? –> Thank you for raising this question. We changed the acronym AREA_NORM to WTNORM. At first, we calculated the area below the water table time series curve over a certain time (the integral). However, due to the problem of time series with partially missing data (see methods section for handling of missing data) we standardized the calculated area to the amount of time steps for comparability. You are right that the response variable AREA_NORM actually represents the mean water table height for a certain time (normalized to well depth). We therefore changed it to WTNORM. Note that WTNORM is unit less since we divided it by the well depth times valid time steps.

P6898 L12: Please indicate better that 'index range is 0-1' is referring to INDEXACTI and INDEX150. –> Due to the normalization of WTNORM (previously AREA_NORM) all response variables (WTNORM, INDEXACTI, and INDEX150) range from 0-1. We rewrote this sentence. Note that we omitted INDEX_150 in the revised manuscript (see response to comment 2 by referee 2).

P6898 L25: Make a separate heading/section for 'missing data'. And improve layout of the subsections ' seasonal scale', 'event scale', and 'entire time series'. –> We prefer to not add another subsection since the handling of missing data only refers to the determination of the response variables. We improved the layout of the subsection and hope this satisfies the referee (better indicated header and bullet points).

P6899 S2.4: It will highly help the reader if an overview (list of acronyms) of all assessed characteristics is given with short definition and units. –> Good suggestion; we added a list of acronyms.

P6899 S2.4: I think the predictor variable 'soil depth' is an important control for SSF that is missing in the analysis (Hopp & McDonnell, 2009). I recommend to add this, although I understand that this parameter is difficult to determine. –> We assume the predictor well depth serves as a good proxy for soil depth. As stated in the manuscript, "We aimed to drill to a maximum depth of 2 m. However, many wells are shallower due to resistance in the periglacial drift cover or the bedrock. The majority of wells end in dense layers of periglacial drift cover, since the actual bedrock is mostly located far beneath the drift cover." Clearly, hitting a rock during drilling may result in shallower well depth than actual soil depth. With current methods we do not see how to determine soil depth more accurately. Excavations and soil pits for each well or several hand auger measurements per well were simply not manageable given the available resources. We would also like to mention that soils developing above periglacial drift can often not be clearly distinguished from the drift material and therefore it is much more difficult to determine soil depth in our environment compared to soils developing above granite (e.g. Panola hillslope).

P6899 L17: I assume that THROUGHF is defined as percentage of rainfall? –> We define THROUGHF as the mean percentage of maximum throughfall/rainfall (whichever is higher) of eight events per well during the leaf season in 2010/2011. Maximum throughfall was usually higher than open area rainfall. THROUGHF at the forested hillslopes thus represents mean percentage of maximum throughfall; at the grassland hillslope THROUGHF represents open area rainfall, which results in THROUGHF < 100% at the grassland hillslope. We rewrote this section in the manuscript to make it clearer.

P6901 L4-17: As correctly discussed by the authors, the STEMF index is arbitrary. Main missing characteristic in STEMF is the tree type. In general, deciduous trees have higher stemflow rates than coniferous, due to the tree architecture (upwards or downwards pointing branches). The latter prefer to drain the water along the branches to the edge of the canopy, while deciduous trees prefer to drain the water like a funnel to the bark of the tree. –> We do include tree type in our stemflow index by introducing a factor representing the bark type. . . ."Next, the stemflow index per tree was calculated by adding up normalized tree distance (normalized to maximum distance of 3 m), normalized DBH (normalized to maximum DBH), and a factor representing type of tree (rough-barked spruce/fir: 0.5, smooth-barked beech/ash tree: 1). These factors were set according to literature values of stemflow for different forest types (Levia et al., 2011)." (see p. 6901). Clearly, these factors for rough-barked spruce/fir and smooth-barked beech/ash tree are just an attempt to quantify actual stemflow volumes and they do not account for tree architecture. Nevertheless, the general trend that is suggested by the referee ("deciduous trees have higher stemflow rates than coniferous") is represented in the current stemflow index.

P6901 L23: Change sentence to ": : :. for 82% of the wells; the rest of the wells have: : :" –> changed

P6904 L04: What is mtry? –> mtry is the amount of randomly selected selected predictor variables for the random forest construction, as defined in the manuscript: "For random forest construction a subset of predictor variables is randomly chosen (amount of randomly selected predictor variables (mtry) specified by user)."

P6904 L24: To me the spatial pattern of AREANORM does not differ that much between seasons and events. Only season 3 for the coniferous forest differs significantly. –> A previous analysis of spatial patterns of water table dynamics for 45 events (Bachmair et al. 2012, WRR) showed that there is a trend of spatial variation with time (in wet fall/winter/spring predominantly saturation of the lower part of the hillslope, weaker water table response, and slower response times; under dry summer conditions whole-hillslope activation but higher spatial variability, generally stronger water table dynamics, and quicker response times). You are right that this trend of event-scale patterns is

not clearly discernible for the seasonal scale (we chose to not show the event scale to avoid doubling of information already presented in Bachmair et al. 2012, WRR). The lower spatial variability over time at the seasonal scale coincides with a higher explainability by the measured hillslope characteristics than at the event scale. The spatial patterns of well behavior monitored over three months are less variable and therefore better predictable than for single events. We now rewrote the sentence: "There is also variation in spatial patterns over time, which is more pronounced at the event scale than at the seasonal scale." We think it is important to point out that there is some spatial variation, which already implies that the fixed predictor variables cannot fully explain the observed spatial variability.

P6905 L3-6: Define when a correlation is weak, moderate or high. –> Since there is no standard definition we roughly use this definition: $0 < |r| < .3$ weak/low correlation $.3 < |r| < .7$ moderate/medium correlation $|r| > 0.7$ strong/high correlation When we try to extract trends in data and some of the data are slightly below or above these thresholds we still call it a "weak", "moderate" or "high" correlation to reach a summarizing conclusion.

P6905 S3.2: Define when a correlation is considered low, moderate or high. –> see comment above

P6905 L12: Define AWI as Antecendent Wetness Index in the manuscript. –> Definition added to the manuscript and also added to the list of acronyms.

P6905 L11-13: I do not understand this indicative conclusion. Please elaborate. –> "This indicates that the mapped predictors explain the observed water table response for time periods with high rainfall intensity and low AWI (antecedent wetness index) to a smaller degree." We found a trend that for time periods with higher mean/maximum rainfall intensity (and lower AWI) (summer periods, see Figure 2) the partial correlation was slightly lower or not significant than for time periods with lower mean/maximum rainfall intensity (and higher AWI). Table 5 shows that the partial r of some predictors (only predictors showing a significant partial r for most temporal scales were chosen for correlation (SLOPE, SLUG_LOW, WDEPTH, LULC, TRANS)) is anti-correlated with rainfall intensity (higher intensity resulting in lower partial r) and positively (yet in most cases not significantly) correlated with AWI (lower AWI resulting in lower partial r). The interpretation that "This indicates that the mapped predictors explain the observed water table response for time periods with high rainfall intensity and low AWI (antecedent wetness index) to a smaller degree." seems valid to us. Clearly, correlation coefficients do not prove causality. Also, a larger n would be desirable for statistical analysis. Nevertheless, it seems very reasonable to assume that high rainfall intensity and low antecedent wetness trigger different runoff generation mechanisms (e.g. onset of preferential pathways as discussed in section 4.2).

P6906 L14-21: Add the not shown data in a supplement. –> There are many tables and figures that were used for interpretation that we prefer to not show since we want to focus on the key tables/figures (e.g. for grassland this table shows in about 90% of the cases a non-significant r). If we include this table (showing partial r for each predictor for the forest and the grassland separately) we should also to include two figures showing the differences of the RF variable importance plots grassland vs. forest to be comprehensive. If the second referee and the editor agree on the issue raised by referee 1 we will add this table to the supplement, otherwise we prefer to keep the number of tables small.

P6906 L21: What is the base case? –> 90 wells (we added the definition to the manuscript)

P6907 L18-27: How is it possible that the explained variance of all wells (n=90) is higher than the forest (n=60) and grass (n=30) separately (related to fig 4)? –> The variance of the two subsets of the sample population (grassland vs. forest) may differ from the variance of the entire sample population.

P6909 L25-29: Why is THROUGHF not equal to 100% at the grass site? –> See re-

sponse above; THOUGHF is percentage of maximum throughfall/rainfall (whichever is higher). Maximum throughfall was usually higher than open area rainfall. At the grassland hillslope THROUGHF represents open area rainfall, which results in THROUGHF < 100% at the grassland hillslope.

P6910 L16-19: The study reveals that the hillslope characteristics only explain the spatial variability of the water table to some extent. This is true if one considers the predictive characteristics separately. But very likely certain combinations of occurring predictive characteristics will better describe the water table. For example: a thin soil layer in combination with a throughfall hotspot can cause high SSF. But if either the soil layer is not thin or there does not exist a throughfall hotspot high SSF might not occur. I think this is the main limitation of the study. Some discussion on this topic is recommended.

–> The partial correlation analysis indeed only shows the explanatory power of one predictive characteristic at a time. The random forest approach, in contrast, does account for interactions. This is why we chose this approach. A single regression tree more intuitively shows the interplay of different predictors and how the nodes are split. Before applying the RF method we started out with single trees, but a disadvantage of single tree is instability, since small changes in input data can produce highly divergent trees (Prasad et al., 2006). For instance, if the two main explanatory variables were soil thickness and throughfall, soil thickness could be a first split variable (resulting in two more homogenous nodes with lower and higher WTNORM due to thin/thick soil), and throughfall could further partition the one sub-node (thin soil) (resulting in two more homogenous nodes with lower and higher WTNORM due to high/low throughfall). The presented results of the ensemble tree approach (percentage of explained variance, variable importance) do not show the interactions as well. Important is, however, that all predictor variables taken together do not sufficiently explain the variance (see Figure 4). In our case due to the rather thick soil throughfall hotspots are likely blurred. We do not think any other analysis technique could account for such an effect. As already pointed out in the discussion, it would be interesting to assess whether the same analysis for hillslopes with shallow soil provides a different picture regarding the effect of vegetation.

P6911 L19-29: same comment as previous. I think it is too strong to conclude that vegetation is of minor importance for SSF based on this study. –> We slightly changed the sentence to "Our hypothesis that vegetation exerts a major control on spatially variable SSF dynamics AT OUR STUDY SITE has to be rejected." As pointed out, this result is surprising to us as well. We do not conclude that vegetation generally has a minor effect on SSF variability, yet at our study site we could not identify a stronger effect. As later discussed, this may go back to the comparably thick soil, which damps the effect of throughfall hotspots.

P6912 L07: Replace Gerrits, 2010 by Coenders-Gerrits et al, 2012. In this study I also found that throughfall has a minor effect on SSF and that the influence on soil moisture is only for a short period of time (it's a balance between throughfall pattern and bedrock topography). Hence it is not contradictive. We thought it was contradictive since in Gerrits, 2010 we read that ..."The virtual experiment shows that spatial variable throughfall has a large impact on subsurface storm flow behaviour. It appears that the spatial pattern affects both the SSF generation and the spatial variability along the hillslope, but only marginally the total SSF amount." We altered this section in the manuscript.

P6930 F2: Keep legend in the same range to make a comparison easier. –> This will suppress the discernibility of the spatial variability for some time periods (e.g. the spatial variability of plot 3_2010 will not be well discernible if the range of 1_2011 is applied). If the second referee and the editor agree on the suggestion by referee 1 we will change this, otherwise we prefer to keep the figure as it currently appears.

Change Gerrits et al, 2009 into: A.M.J. Coenders-Gerrits, A.M.J, Hopp, L., Savenije, H.H.G. and Pfister, L. (2012) The effect of spatial throughfall patterns on soil moisture

patterns at the hillslope scale. accepted for publication in HESS –> changed

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