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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 many useful review comments. We much appreciate all ideas and we incorporated most of them.

RESPONSE TO GENERAL COMMENTS

1. Line 25, pg. 6906. I am not sure I understand how the explained variance, as produced by the random forest analysis can produce negative results? Does the author mean zero values here, or actual negative values? –> Negative values are possible and represent a very poor fit. "The percent variance explained is viewed as a pseudo r-square. It is pseudo in the sense that it can take negative values when the ratio of

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the mean squared error and the variance of the response is greater than 1." (Pang et al. 2008; In: Dehmer, M. and Emmert-Streib, F. (Eds.) Analysis of Microarray Data).

2. I do not have a clear understanding of the chosen water table response variables. I understand AREA_NORM to be a fraction of the area underneath each well that is saturated. Is this correct? INDEX_ACTI is the fraction of time which the water table is active, as defined as being above 2cm in height. However, in the field methods it is suggested that all measurements below 13cm height are unreliable and discounted? If this is the case, how can this be reliably quantified? For INDEX_150 why is 150cm chosen as the threshold? Again, can this be reliably quantified given the issue with measurements in the lower 13cm of the wells? For the deeper wells I can see this wouldn't be an issue, but if it is for the shallower wells, then may that not add structured errors to the data? –> Concerning AREA_NORM: Since referee 1 posed a similar question about the index AREA_NORM we changed the acronym AREA_NORM to WTNORM. The index is not a fraction of the area underneath each well that is saturated; sorry if the description was misleading. It is 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. The index thus actually represents the mean water table height over a certain time (normalized to well depth). Concerning INDEXACTI: This was a misunderstanding. As activated we understand a water table > 15 cm from the bottom of the well. Data in the lower 13 cm of the wells were found to be unreliable. Therefore we only accounted for a water table > 13 cm; by activation we meant a water table > 2 cm in the zone above uncertain data (hence >15 cm). We changed the definition in the manuscript to make it clear. Concerning INDEX_150: You are right that 150 cm below the soil surface is an arbitrary threshold. We started out with several response variables as exploratory analysis. It is true that INDEX_150 does not add to a better understanding (and is highly correlated as well to the other response variables). We therefore omitted it from the manuscript, rewrote the respective sections and altered the figures/tables. Thank you for this remark.

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3. There is not much discussion on how the three main response variables differ in their explainability for each slope. Were the two additional response variables for the event scale response mentioned after the introduction? Perhaps these could be drawn together in the discussion? –> We did not intend to explain the differences in explainability per slope, we only wanted to investigate the effect of grassland vs. forested hillslopes. A previous analysis (Bachmair et al. 2012, WRR) showed significant differences in well activation between grassland vs. forest but not between mixed forest vs. coniferous forest. You are right that the two additional response variables at the event scale are hardly mentioned in the results section since they did not provide too much insights or trends. We therefore chose to omit them from the manuscript (see also response to comment 2 regarding response variables). Thank you for this suggestion.

4. A more extensive examination at the event scale might be beneficial. How are the five chosen events representative of all the events? –> See also response to comment by referee one (why no winter events). The choice of events is rather arbitrary. We solely wanted to cover different rainfall characteristics and antecedent wetness states. In a previous analysis (Bachmair et al. 2012, WRR) 45 events were analyzed regarding spatial patterns of water table dynamics. We found high spatial variability yet trends during wet fall/winter/spring versus drier summer conditions. This is why we chose events that cover both wetter and drier conditions (May/Nov. wetter conditions and lower rainfall intensity, June/July/Sept. event drier conditions and higher rainfall intensity). We do agree that analyzing more data may yield more information, but will also make the manuscript more difficult to read. It is also much more time consuming, since we already analyzed 12 cases (entire time series, 6x seasonal scale, 5 events), 3 different response variables, 2 different analysis methods (partial correlation and random forest), and two different sub-cases (grassland vs. forest). We believe the chosen events do provide valuable first insights and prefer to leave a further analysis of more events for a follow-up study.

5. Does the relative proximity of the grassland slope to the valley region and stream

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change the water table behavior in comparison to the forested slopes? –> The lower transect of wells at the grassland hillslope is part of the hillslope and not attributable to the riparian zone. When we installed the transects we aimed for the same distance from the lower end of the hillslope (excluding the very narrow riparian zone) to the furthest left well of the lower transect (looking upslope). Due to differences in the width of the riparian zone, different slope, and partially unfavorable conditions (e.g. depressions due to fallen tree stumps) the distance is yet not the same. However, none of the lower transect wells is attributable to riparian zone. We believe the water table behavior is thus comparable. In fact, the lower transect of the coniferous forest hillslope shows a more continuous activation of wells (further away from the stream) than at the lower transect at the grassland hillslope. Nevertheless, we could have included the distance to stream as further topography predictor. On the other hand, there is spatial variability within a transect (e.g. at the lower transect at the coniferous forest hillslope the distance to stream should be very comparable within one transect), which shows this likely does not explain SSF variability.

6. Are snow melt or overland flows significant contributors to run-off generation at this site? If so, one would imagine that their influence would differ between the plots? Would this influence the results? –> This is a good point; we measured overland flow (OF) with one overland flow collector per hillslope. Overland flow is not a significant contributor to runoff generation but the beech leaf layer in the mixed forest does produce OF. There are also differences among hillslopes (mixed forest hillslope » grassland hillslope, no OF at the coniferous forest hillslope). However, we do not have information about the spatial variability of OF. There is a large body of literature on overland flow and its controls, and high spatial variability is a common observation (e.g. Godsey et al., 2004; Gomi et al., 2008; Loos and Elsenbeer, 2011; Orchard et al., 2012; Sidle et al., 2007). We discussed that there must be additional important drivers not captured by our measurements of the hillslope characteristics to explain the spatial variability of SSF. One such driver may be spatial variability in OF due to hydrophobicity, topsoil compaction etc. In the manuscript we now state that “Further controls could

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also be spatially variable surface conditions and thus overland flow due to litter, soil compaction, and hydrophobicity."

7. A figure illustrating what is meant by SLUG_LOW and SLUG_HIGH and how it is calculated might be useful, particularly as SLUG_LOW was identified as one of the highest scoring predictors. –> See response to the comment about an extra table by referee 1. We prefer to focus on the key tables/figures. How we calculated both predictor variables is described in detail (in a whole section) on page 6900. We simply determine the mean slope of the fast and slow part of the recession curve after slug injection. If the first referee and the editor agree with the second referee we will add this figure, otherwise we prefer to keep the number of figures small.

RESPONSE TO SPECIFIC COMMENTS

1. Pg. 6895, line 4: opening bracket missing. –> added 2. Pg. 6898, line25: Unusual use of colons here. –> changed 3. Pg. 6906, line 27: Unusual use of colons again. 4. Pg. 6909, line 1-3: Sentence here is unclear. –> I do not see why? 5. Pg. 6909, line 4: 'stick out' is a little colloquial. –> changed 6. Pg. 6912, line 24: Colon use. –> colon exchanged 7. Pg. 6913, line 1: Sentence unclear. –> reworded

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Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 9, 6889, 2012.