Hydrol. Earth Syst. Sci. Discuss., 9, C3162–C3165, 2012
www.hydrol-earth-syst-sci-discuss.net/9/C3162/2012/

Hydrology and Earth System Sciences Discussions

Interactive comment on "Assessment of shallow subsurface characterization with non-invasive geophysical methods at the intermediate hill-slope scale" *by* S. Popp et al.

S. Popp et al.

steffen.popp@ufz.de

Received and published: 18 July 2012

Response: Thanks to the anonymous Reviewer. We think he/she addresses some points, which need consideration and clarification in order to avoid further misunderstandings regarding the purpose of the study, and which will help to improve the paper. A point-to-point response to the comments is included below:

This paper presents an interesting case study relevant to the estimation of soil water content at the hillslope scale. This is a very active field of research and all contributions can be considered valuable. However I have a few reservations about the substance

and the presentation of this case study. In particular, this seems to be an incomplete study. First of all, the dataset is definitely too small to allow for definitive conclusions about the catchment behavior in terms of soil water content changes. I acknowledge that the May-June period is indeed interesting in terms of snow melt effect. But such a short data collection period, in only one year, cannot be considered as sufficient to draw general conclusions on the catchment response.

Response: In this study, we focus on a general characterization of hill-slope underground which has resulted in a zonation based on the results of geophysical mapping. Even though we have given examples of EMI studies for soil-water estimations, we have not stated to solely determine or estimate soil-water contents. Rather, we wanted to assess the application of EMI and gamma spectroscopy at this particular site with its relatively wet and clay-rich soils. Our purpose was not a monitoring study even we visited the site twice. Rather we used data from two dates, which represent different site conditions according to the PCA analysis. You are right, for drawing conclusions on soil-moisture or catchment behavior you would need more data over a longer period. But as given in the title, we wanted to assess the application of common geophysical methods in order to obtain spatial information from a site, from which a couple of point measures cannot adequately image subsoil conditions entirely.

Second, why is moisture content only considered at 20 (or 18?) sample points? This is clearly a far too small dataset to draw final conclusions (at one time instant only, by the way). Why not using faster and more extensive TDR surveys?

Response: TDR does not work reliably at the site because of the high clay contents (silty loams to silty clay soil according to Lindenmaier et al. 2005). We tried to measure with TDR during the gamma survey and obtained very often values between 90 and 99.8 percent water content, which is definitively not true. However, if TDR would have worked, you could take the results from each of the 327 points for kriging interpolation of water contents instead of geophysics. TDR did not work, so we used other techniques. As already stated, we wanted to apply a fast and non-invasive technique for

HESSD

9, C3162–C3165, 2012

characterization of the hill-slope underground. For validation, we checked soil-water contents at few selected points by additional laboratory analysis of soil moisture because this important parameter is relatively easy to obtain. Of course, you could always put more effort into more detailed analysis, but this would go beyond the purpose of the study. We wanted to characterize the hillslope in a general manner and obtained finally a zonation of hill-slope area by clustering, which allows us to separate between slope areas with different properties regarding different ECa. This zonation can serve as the basis for further detailed soil analyses according to the targets.

Third, the lack of relationship between ECa and theta shown in Figure 3 would definitely call for an explanatory covariate to correct for. But no such explanatory variable is proposed and cannot be inferred from the only other data available (gamma emission) given the point below.

Response: For an explanatory covariate variable you would need more samples points and/or additional data. As already mentioned, we did not aim at detailed soil-water estimation by EMI at the site. Rather we wanted to show the complexity of EMI and gamma response to such heterogeneous soils, and the difficulties in data analysis arising from the circumstances. Then we have shown a possibility of analyzing the spatial data, the cluster approach and discussed the results. We did not focus on deriving a function for explaining ECa relationships, which we could not achieve with 18 soil samples at this large site. But also with more ground-truthing measures, there are likely still uncertainties in establishing explanatory covariates in heterogeneous environments, as Tromp van-Meerveld and McDonnell (2009) have shown.

Fourth: I find disturbing the confusion about the dependence of gamma emission on moisture content. On one hand, the authors only perform one gamma ray survey – under the assumption (evidently) that the dependence on moisture content is negligible, and the gamma emission is mainly informative of soil texture (why otherwise a single survey?). On the other hand, the authors conclude (page 2521 – line 27-28) that “the low radioactive emissions [. . .] are very likely the consequences of high soil-water

HESSD

9, C3162–C3165, 2012

contents". There is a clear inconsistency here. And more data shall be collected to prove/disprove such conclusions. I feel that studying the dependence of gamma emission on moisture content would be, at this site, an interesting study of its own.

Response: We do not think that there is a clear inconsistency. It is common that gamma spectroscopy gives information on both soil texture and soil moisture, as shown by several studies cited the text. We applied gamma spectroscopy primarily for exploring soil texture features, mainly clay content. However, with regard to the known high clay contents (see Lindenmaier et al. 2005 and references therein), gamma emissions were relatively low. We concluded that this could be the result of attenuation due to increased water contents, which is also a known effect that is even utilized for soil-moisture estimations (see references in the text). We cannot quantify this effect by a single field measurement. However, the heterogeneity of the analyzed soil samples provides a loose correlation or general trend between gamma emissions and water content (see Fig. 5) that supports our interpretation of an attenuation effect.

In summary, I feel the described study has some merit, but deserves a more substantial dataset (e.g. adding more measurements in time) to allow for conclusions to be drawn.

Response: Adding more measurements in time is useful when performing a monitoring study, which has not been the purpose of our study. Rather we focused on the application of common and applicable geophysical methods (GPR for example does not work at the site due to the conductive soils) and the possible implications which can be drawn from the obtained results by a simple but appropriate data analysis.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 9, 2511, 2012.