Hydrol. Earth Syst. Sci. Discuss., 9, C3155–C3161, 2012
www.hydrol-earth-syst-sci-discuss.net/9/C3155/2012/

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: We thank T.P.A. Ferre for his valuable comments on the manuscript and appreciate his knowledge on this topic. The discussion of open questions allows us to further clarify the aims of our study and to improve the manuscript. A point-to-point response to the comments is included below:

The authors have completed a thoughtful field investigation that has produced what promises to be a valuable data set. Unfortunately, I don't think that their analyses are sufficient to justify their conclusions. I have the following specific concerns regarding

the work, as presented.

The authors have adopted a very simple model of EMI depth sensitivity. This interpretation, which can be attributed to McNeil (I did not see this attribution in the text, although it is in the reference list), is based on simplifying assumptions that may not apply. We have published a more complete analysis (see Callegary publications) which should be considered, especially for the PR measure used here. In particular, the vertical and lateral sensitivities of the two orientations are quite different and can depend on the water content. With appropriate discussion of the limitations, I like the PR approach for this effort.

Response: For this study, we refer to the model of EMI depth sensitivity of McNeil 1980 and Geonics, described in section 2.2 on page 2517, lines two to five. We have adopted this model because it is commonly used for explaining ECa results in many EMI studies, which have been cited in the text. We know that this model is based on simplified assumptions. However, it is suitable for our purposes because we want to explore pattern of soil electrical conductivity at the entire hillslope. Even though we tested a possible relationship between ECa and soil-water content, we have not focus on detailed depth sensitivity analyses with regard to the location of soil sampling because this is hardly to assess at the heterogeneous site. Basically, you are right, that is a very important issue when soil-water contents are to be predicted with EMI, and the study published in Callegary et al. 2012 is very significant for this purpose. The study needs the attention and citation in each EMI study and we will regard and insert it into the text. On the other hand, the (3-D) analysis done in Callegary et al. 2012 is very complex, and goes far beyond the purpose of the paper. We have measured at a large and complex site, where spatial heterogeneity definitively exceeds those of the sample volume of the EMI device, independently if we adopt a 1-D or 3-D depth sensitivity model. Testing a possible correlation of ECa with soil-water contents at 18 points was done in order to check if there would be any trend between both parameters. If there would have been a clear trend or other relationship, a detailed analysis of sensitivities would be justified. However, a detailed testing at the site would mean to increase the number soil-water measurements significantly (by one order of magnitude) in order to capture the entire heterogeneity of the hill-slope, which goes really far beyond the purpose of the study. Tromp-van-Meerveld and McDonnel (2009) gave a nice example of the effort and the results of assessing the use of EMI for soil-water estimations at a much smaller hillslope. Instead, we were interested in a general and primary characterization of the hill-slope area, and used kriging interpolation for both 'on-the-go' and point measurements for identifying different sub-areas based on interpolated ECa values and the simplified, but common depth sensitivity model of McNeill (1980).

Related to the previous point, the authors have not made sufficient effort to consider the impacts of the differing support volumes of the EMI and gamma methods. I believe that this reduces the value of their data set.

Response: We are aware of the different support volumes of EMI and gamma methods. In section 2.2 and 2.3 we described the support volumes of EMI and gamma spectroscopy, respectively. Since gamma spectroscopy is mainly limited to the upper 30 cm of soil column, we considered only EC values from the horizontal dipole (EC_h) with a similar depth sensitivity (according to the depth sensitivity model of McNeill, see section above) for joint cluster analysis.

The authors should be aware of the work of Qing et al., Robinson et al., and Franz et al. all of whom have shown large scale applications of EMI for hydrologic mapping. Qing actually mapped a larger area than was considered in this study. I think that rather than claim uniqueness of the study based on difficulty of access or scale of measurement, the authors may want to highlight their cluster-based analysis.

Response: Thank you for providing additional references of EMI measurements at larger scales. The work of Qing et al. and Franz et al. we really did not know. However, with the exception of the study site of Quing et al. (19.5 ha), the other sites are smaller with sizes of 4 and 6.25 ha, and there are only few EMI studies at sites larger than 10

ha. We just want to show that we have investigated a very complex landscape unit, comprising both a rough surface with a difference of altitude of more than 200 m and a difficult underground that hampers a straightforward interpretation of geoelectrical applications. Ground penetrating radar, for example, does not work at the site because of the conductive soils due to the high clay content and relatively high soil-water contents. In our opinion, this is a special environment compared to other studies sites, which is not unique but a challenge, and has not been reported yet. Related to these environmental conditions, we have shown a way to handle the ambiguous results of EMI (and gamma spectroscopy) by cluster analysis. So, the cluster approach is the consequence of the site-specific conditions, and sure, we want to highlight this approach.

The authors visited the site twice, assuming that the water content changed. But, there are no data presented to assess whether the water content was different. Rather, readers are shown cluster maps based on a correlation made under the later condition and asked to accept that they show texture/water content from time lapse measurements. The addition of confirmatory water content measurements under different conditions seems critical to publishing this work. Similarly, the authors claim that the gamma response is due to water content variations, but there appears to be a lack of data to support this critical conclusion. (Could this be tested by measuring on site under dry conditions, then sequentially adding water?)

Response: You are right, that is a critical point. First of all, it is fair to assume that the upper soil-water content has changed over the time shortly after the snow melt in May and one month later in June. In the study of Lindenmaier et al. (2005) it is described that subsoil water content is close to saturation throughout the whole year and that variations occur in the upper 20 cm of the soil in the range of 10 % only. Water content was determined to be more than 60 vol%. An own field experiment by FDR probes along a limited transect between June and August in the previous year showed similar results. Water contents of the upper soil varied only between 51 and 55 percent. Another field test was about measuring variations in ECa before and after 10-litre of saline solution was given to the soil at a local point. By means of EMI (with EM38DD), we could not measure any significant variation in ECa within one hour. As described by Lindenmayer et al. (2005), infiltration at this heterogeneous site with clay-rich soils is low, as well as soils never dry up due to the high precipitation rates and water-holding capacity. Thus, determining soil-water changes at a specific point or single soil depth and to correlate these changes with EMI results is hardly to assess in this environment. Soil temperature was regarded during data analysis. Giving that porosity, clay mineral content and soil salinity has not changed within one month, detected changes in ECa at the hill-slope scale can be very likely linked to changes in soil-water content in the upper soil column. In other words, changes in soil-water/and/or ECa at the point scale are often less significant (depending on location) or detectable than changes at the spatial scale of this particular hillslope.

The authors should be careful of using terms like '10% of seasonal variation in soil moisture'. Does this mean a variation of 0.1 in water content? Or, does it refer to 10% variation in the water content value?

Response: The term '10% of seasonal variation in soil moisture' is cited from Lindenmaier et al. (2005) and means 10% variation in the water content value.

The strongest part of the paper is the cluster analysis. The authors appear to have selected a useful approach and used this approach appropriately and thoughtfully. The authors are also too loose with conclusions like 'spatial pattern at the hillslope scale similar to those of ECa'. A quantifiable measure of similarity should be used here.

Response: We just compare the partitioning of the hillslope based on clustering and the previous obtained hillslope zonation by ecological mapping. This is a qualitative assessment, and for this first step, we do not think that we need a quantifiable measure of similarity.

Too often, it seemed that the similarity was overstated. This is particularly true when considering Figure 8. To me, it appears that the clusters are not able to discriminate

among the ECv and ECh values. This suggests that the clustering is dominated by the gamma data. Even these data, when artificial fill sites are removed, does not seem to separate clusters well. I was left feeing that the conclusions were overreaching and not sufficiently supported by the data or the analyses.

Response: In Fig. 8, only the ECh and gamma total counts are shown because these values have been used for cluster analysis only (because of the similar depth range). So, clusters cannot discriminate between ECh and ECv values because there were no ECv values incorporated. ECh values from May and June surveys were chosen because they represent two different soil states, and gamma values are an additional and independent variable, as described in section 3.3, page 2522 up to line 5 on page 2523. In the end, maybe we have been too optimistic when comparing the cluster map and the ecological map concerning similarity. However, we are still happy with the results of geophysics and clustering compared to the map of ecological moisture. Please keep in mind that we compare the results of two completely different approaches, as discussed in the last paragraph of section 3.3 on page 2524. Similarities of spatial sub-groups (cluster) are evident, as well as differences, of course.

I would encourage the authors to take one more trip to the field and follow the procedures of their second field trip. Perhaps they could conduct some other field calibrations and investigations of instrument sensitivity at the same time. I understand that this represents a major effort. But, without the addition of confirmatory data under both conditions, the conclusions are unfounded.

Response: As described, we did not focus on soil-moisture determination by EMI in detail. We know that we cannot achieve this without significant effort for soil-moisture determination. One may ask why we still use geophysics when we take 100 or more soil samples for water analyses. If you have an adequate number of soil samples, you could use these results for kriging. Our purpose was a primary characterization of a complex landscape unit in a qualitative manner by non-invasive methods as basic work for any further detailed measurements. And we wanted to show difficulties you have to face, and opportunities of data analysis for a feasible interpretation of results. Maybe this purpose has to emphasize more in detail.

Tromp-van Meerveld, H. J. and McDonnell, J. J.: Assessment of multi-frequency electromagnetic induction for determining soil moisture patterns at the hillslope scale, Journal of Hydrology, 368(1-4), 56-67, 2009.

Lindenmaier, F., Zehe, E., Dittfurth, A., and Ihringer, J.: Process identification at a slow-moving landslide in the Vorarlberg Alps, Hydrological Processes, 19(8), 1635-1651, 2005.

McNeill, J. D.: Electromagnetic terrain conductivity measurements at low induction numbers, Technical Note TN-6, Geonics Ltd. Mississauga, Ontario, 15p, 1980.

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