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Discussions

Interactive comment on "Fracture network characterisation of a landslide by electrical resistivity tomography" *by* S. Szalai etal.

Anonymous Referee #2

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Comments on the paper entitled "Fracture network characterisation of a landslide by electrical resistivity tomography" by Szalai et al. Manuscript number: NHESS-2014-130

General comments

The paper by Szalai et al. aims at imaging a network of fractures affecting a loess plateau in Hungary. This plateau is prone to collapse and the presence of tension cracks is supposed to weaken the soil strength and facilitate landsliding. The authors use Electrical Resistivity Tomography (ERT) to characterize the geometry of the fracture network along the Vár Hill. The first chapter introduces the work and details a part of the numerous techniques which can be used to characterize landslides. The

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second chapter exposes the geological and geomorphological setting of the study site and the third chapter details the previous studies conducted in the area. The fourth section succinctly presents the method used (ERT). In the fifth section, the authors expose their conceptual model of the area with respect to the electrical properties of the ground. The sixth chapter is dedicated to the presentation of the results and to their interpretation. A conclusion is drawn in a seventh section.

This work presents an application of ERT technique to hazard mapping and, more specifically, to fracture mapping in an area subject to landslides. The subject is not specifically new and/or original in terms of fracture characterization. However, it presents an interesting case study of the application of ERT to the characterization of the structural heterogeneity of the top of a landslide. Nevertheless the paper suffers several critical points which prevent from publication in its present form: 1) the paper is not clearly written and some sentences are very hard to understand (some words seem to be missing). However, since I am not a native English speaker I cannot make any fruitful comment to enhance the quality of the writing; 2) the state-of-the-art regarding methods to characterize landslides might be improved and the order of the paragraphs also needs to be rearranged. The authors should focus on methods devoted to the study of fine-grained landslides. For instance, I think it is of no use to present the Ground-Penetrating Radar (GPR) technique since it will very probably be inoperative in clayey loess horizons (electrical resistivity too low) below a few dm. On the contrary, other geophysical techniques such as seismic refraction and reflection, surface wave inversion, seismic cross-correlation and time-domain electromagnetic techniques have been successfully applied in fine-grained landslides (for depths generally greater than a few m); 3) contrarily to the authors' statement, several research papers expose geophysical studies devoted to the internal characterization of fine-grained landslides and not only to slip surface detection. Among many others: Hibert et al. (2012), Travelletti et al. (2013), Marescot et al. (2008). Recent reviews by Loke et al. (2013), Perrone et al. (2014) detail specific application of ERT to landslide characterization. The paper by Bièvre et al. (2012) is especially dedicated to fissure characterization within

NHESSD

2, C1916–C1926, 2014

a fine-grained landslide, using ERT and seismic techniques. Papers dealing with the characterization of cracks with ERT within other fine-grained structures might also be relevant (e.g. Jones et al. 2012, Jones et al. 2014); 4) a geomorphological map of the study area presenting the location of fractures visible at surface would facilitate the comparison between ERT results and the geotechnical interpretation; 5) the methodology part should be much more detailed and the quality of the experimental data needs to be assessed; 6) the conceptual model of the area regarding its geophysical setting is an interesting section. It should however be accompanied by a parametric forward modelling study. This would provide informations regarding the ability of ERT to detect tension cracks as a function of their depth, width, filling, etc., but also as a function of the arrays used and of the electrode spacing; 7) Results: 7a) this section would gain to be rewritten and rearranged to ease the reading; 7b) the quality of the experimental data needs to be assessed and discussed. Further comments are provided in the "specific comments" section; 7c) the strong hypothesis that resistive anomalies are caused by tension cracks only might be regarded as very doubtful. The geophysical results indicate a more or less horizontally stratified and more or less resistive horizon between 0.5m and 1.5m depth, with very few vertical anomalies reaching the surface. The paper by Ujvari et al. (2009) shows hollows at the study site (their fig. 4b), the geometries of which more adequately match the shape of ERT anomalies as well as their resistivity. These authors also showed tension cracks (their fig. 4a; which depth below the top of the plateau?) which were almost closed and which might hardly lead to any electrical contrast considering the resolution of the ERT technique. Since the paper is based on the hypothesis that resistive anomalies are caused only by vertical tension cracks and fractures, it should be much more thoroughly justified and supported by direct evidence. I also do not understand why some resistive anomalies are interpreted as cracks and some adjacent others are not; 8) the authors eventually interpret the continuity of the identified cracks along the four ERT profiles and propose a relative timing for future collapses along these continuous fractures. This very daring part of the paper is supported by no data except the authors' own interpretation of the indirect geophysical profiles. Without any further observation/measurement, such as displacement velocity, I strongly recommend the authors remove this part of section 6 from the paper; 9) figures and figure captions need improvement (except captions of figures 1 and 2 which come from Ujvari et al. (2009)); 10) finally, the bibliography contains nearly 100 references, nearly one third of which consist of national papers or unpublished reports; furthermore, a major part of them are written in Magyar. Some of these references might be very difficult to access.

Specific comments

The unit for resistivity is ohm.m and not ohmm. Resistivity scales should be the same in order to compare the results of the different protocols. Figures should be scaled and oriented.

1. Introduction See above.

2. Geological and geomorphological settings

Figure 2: according to the interpretation of ERT profiles, the uppermost loess horizon (between 110 and 142 m asl) contains a perched water table. This should be drawn on the figure.

Figure 3: pictures should be oriented and scaled. What are profiles S4-10 and line S10?

Figure 4: the figure should be oriented and scaled. Since there is a visible fracture in the field, why was profile P4 implanted apart from it?

3. Former researches in the area This section should be merged with the previous one.

4. Method

p. 3975 The different protocols provide varying vertical and/or horizontal resolution and penetration depths. Did the authors try to jointly invert them in order to obtain images with both good vertical and horizontal resolution as well as good penetration depth?

NHESSD
2, C1916–C1926, 2014

line 15: the CP distance is referred to as n, while in figure 5 it is referred to as m.

p. 3976 lines 7 to 12 & figure 5: what are the units in the tables and in the upper part of the sections? The red lines in figure 5 show that an important part of the data, at the bottom of the profiles, is not used. Why did the authors choose not to use the whole experimental dataset? Is it only due to the lack of information they bring or is it partly related to signal-to-noise ratio issues? Furthermore, it is frequently said along the paper that resolution does not perfectly allow to distinguish between adjacent cracks. Acquiring ERT profiles with smaller electrode spacing would have provided higher resolution images along with less penetration depth, allowing the whole experimental dataset to be used. Finally, the figure should not present the number of acquired data but the number of experimental data used for inversion and interpretation on the final images. Dp-Dp protocol is used with a single electrode spacing (n factor of 0.5). Why did the authors use only one inter-electrode spacing for measurements? Doing so, they do not take the full advantage of Dp-Dp possibilities along with a 10-channel resistivimeter in order to get high resolution images. Using a single electrode spacing of 0.5 m for Dp-Dp measurements, it is shown in figure 5 that the m factor ranges between 0.5 (one electrode spacing) and 34.5 (69 electrode spacing). Because of the geometrical configuration of Dp-Dp measurements, it is well-established that the measured voltages are much lower than for classical protocols with potential electrodes located between the current electrodes. This results in a much lower signal-to-noise ratio and consequently much noisier data. It is then generally recommended to use m factors no more than 5 to 6 times the n factor (e.g. Dahlin & Zhou 2004). Consequently, with a factor n = 0.5, the m factor should not be set to a distance higher than around 3.5 m. In this work, the m factor is up to 34.5 m, i.e. 10 times the generally recommended maximum length. Can the authors discuss these points and justify their choice for this very particular use of the Dp-Dp protocol with regard to the data quality estimation (data with very low noise level, reciprocal measurements, etc.)? Moreover, using larger n factors would allow to record more experimental data with less noise. Figure 5 shows that the Stummer protocol does not contain any experimental data between 1.5 m and

3 m depth. This depth is typically of interest for the purpose of the study. The images are however further interpreted. How were interpreted these parts of the images which contain no data?

Lines 12 to 14: for P-Dp protocols, the infinite electrodes were located 60-90 m from the western end of the profiles, which is in agreement with the very recent findings by Razafindratsima & Lataste (2014). The importance of the angle of this infinite electrode relative to the profile sense as also been noticed by this research. Can the authors specify this point? The location of infinite electrodes should also be drawn in figure 4. Finally, were “forward” and “reverse” measurements acquired?

lines 15 to 16: a paragraph cannot be made of a single sentence. How many electrodes were used? What were the parameters used for the measurements (e.g. voltage, number of “stacks”, dispersion threshold, etc.)? A discussion on the quality of the experimental data seems compulsory since this work makes a highly detailed use of the electrical images. This is also necessary with respect to the specific Dp-Dp protocol used in this work. Were reciprocal measurements acquired to assess the overall quality of the data? In the same way, the data processing and inversion strategy should be detailed. How were bad data detected and filtered? How many experimental points were removed prior to inversion? How many experimental points were finally used after the data below the red line (in figure 5) were removed? What was the grid mesh size for forward modelling and inversion (i.e. what is the resolution of the final images)? How was the number of iterations decided?

Lines 17 to 29: the authors should detail what are “Initial Lagrange multiplier”, “roughness factor”, “Initial damping factor of resistivity” as well as the “Vertical/Horizontal roughness ratio”. They also should justify why they set these parameters to these specific values. Since resistivity variations between soil and fractures is expected to show a rather high contrast, why was L2 norm (least square; which provides smooth variations) preferred to L1 norm (least absolute values; which tends to provide sharper boundaries along with a slightly more realistic geometry)?

5. Model of the study area

The first sentence of this section should be supported by field data and/or references. A synthetic and parametric forward modelling study would also support the conceptual model and allow to estimate the size (depth, width) and nature (filling) of the anomalies which can be imaged with ERT and with the different protocols used.

6. Results & interpretation

The authors use an empirical relationship provided by Rinaldi (in the book edited by Caicedo et al., 2013 [accessed via Scholar]) which links resistivity to the water content of some Argentinian loess soil. The relationship proposed by Rinaldi is based on the long-known empirical Archie's relationship (Archie 1942). However the direct application of this relationship (initially defined for clean sands) to Hungarian loess soil suffers several drawbacks. Among others: 1) it has been widely demonstrated that this relationship is site-dependant (electrical conductivity of the fluid, bulk density, nature and electrical properties of the solid matrix, etc.) and is not a "universal" relationship (i.e. parameters defined at one site are not transferable to another one); 2) loess soils contain clayey particles, the nature and proportion of which influence the resistivity distribution. This is why a clayey term has been added to the relationship in order to use it in fine-grained horizons. This modified relationship is consequently referred to as "the modified Archie's law" (e.g. Mavko et al. 2009). This clayey term however does not seem to have been used in the work by Rinaldi.

The authors then cannot refer to this law and need to experimentally determine this relationship on their study site whenever they want to establish a (semi)quantitative relationship between resistivity and water content.

The authors state that the anomaly in the Dp-Dp profile in figure 8 might be linked to an inversion artefact. Considering the aforementioned remarks regarding the Dp-Dp protocol, it seems that this artefact might also be explained by very noisy experimental data. This is partially confirmed by the high RMS error (12.18%), indicating that the inverse model is not able to satisfactorily explain the experimental data. It would therefore be interesting to show apparent resistivity pseudo-sections of each protocol in figure 8, along with the inverted sections.

The number of iterations varies between 1 and 8 (figs. 8 to 14). The authors should detail why they chose so much varying number of iterations and what criteria they used to stop the inversion process.

In figure 8, depth, distance and resistivity numbers should be harmonized between the four images to ease the comparison (this remark also applies to figures 9 to 14). The resistive anomaly of zone 3 (12 to 12.5 m along the profiles) seems to be the only one reaching the surface for profiles Sch, P-Dp and Dp-Dp. Every other interpreted fractures do not. On the contrary, there is always a shallow layer (resistivity around 100 âĎę.m and thickness of around 0.5 m) covering the interpreted fractures and cracks. How do the authors interpret this?

p. 3980, lines 11 to 13: it is stated that most of the anomalies reach the surface for the Stummer profile. Analysing figure 8, I do not agree with this statement: most of the anomalies do not reach the surface.

p. 3983 Profile P4 has been acquired in an area where no fracture has been identified in the field. The authors state that this area seems to be stable (line 4). The electrical image in figure 11 however shows the presence of a layering very comparable to the three other profiles. This might be an argument not to interpret this resistive horizon only in terms of cracks. Lines 5 to 7: the resistivity scale of the Sch profile cannot be used as an argument for the overall stability of this area. It indicates that, from an electrical point of view, the prospected area is rather homogeneous. Furthermore, a RMS error greater than 15% should prevent from any interpretation of the geophysical data without any direct evidence to support it. Also, the origin of such strong resistivity differences between the four protocols (acquired at exactly the same place without removing the electrodes) should be discussed: profiles P1 to P3 do not exhibit such strong differences, in agreement with the theory. Finally, the Sch and St profiles show resistive anomalies reaching the surface, contrarily to profiles P-Dp and Dp-Dp. What could be the origin of such differences? Lines 8 to 10: this sentence should be removed. Assessing a potential hazard on the basis of indirect, fundamentally non-linear, and non-calibrated geophysical data makes no sense.

p. 3984 Lines 26-27: since direct observations of fractures at many positions have been made over the study area, a final synthetic map representing fissures mapped in the field along with fissures interpreted with ERT would help to support the discussion of the results. Has any direct prospecting (e.g. digging) been carried out to support and confirm these results and interpretations?

Figure 9: what are the numbers in red?

p. 3986 The fractures, previously interpreted independently in each ERT profile, are correlated between the profiles. How is the quality of these correlations assessed (i.e. how to be sure that ERT anomalies on each profile correspond to the same fracture)? A hazard assessment is then established as well as a relative timing for future failures: “any time edge”, “near-future edge”, “future edge”. This part of the paper is, for the most, very daring: 1) these assumptions are based on indirect, non-linear and non-calibrated geophysical data; 2) the overall quality of the geophysical data is non-assessed and then questionable (no measurements in the centre of the Stummer protocols, possibly very noisy Dp-Dp data); 3) the interpretation of resistive anomalies in terms of cracks is not supported by direct evidence, except for the MF structure which is visible on the field. On the contrary the paper by Ujvari et al. (2009) suggests that these resistive anomalies could be interpreted in terms of hollows visible in the field.

7. Conclusions The first sentence is not specific enough. Many works using geophysical methods were devoted to the characterization of the internal structures of landslides. There are very strong differences between images obtained with SCH, P-Dp

and Dp-Dp arrays and with the Stummer configuration. It is however stated that this last configuration is efficient. It is said that "large fractures have been found in the still passive area demonstrating its dangerousness" (p. 3988, lines 16-17). This interpretation is not calibrated nor supported by other data. This furthermore seems to be in contradiction with lines 21 and 22: the authors say that the southern and northern parts of the study site, which contain fractures as well, are "not as much endangered".

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NHESSD

2, C1916–C1926, 2014

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