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The Cryosphere Discussions

Interactive comment on "Quasi-3-D resistivity imaging – mapping of heterogeneous frozen ground conditions using electrical resistivity tomography" *by* C. Kneisel et al.

Anonymous Referee #2

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The paper describes a quasi 3D electrical resistivity tomography approach to map small-scale mountain permafrost distribution and characteristics in challenging alpine terrain. Synthetic modelling is applied to analyse the performance of the quasi 3D approach and to optimise data acquisition. The presented data set is interesting and well suited to analyse the approach, and in principle I think that the approach and the data are well suited for publication in The Cryosphere.

GENERAL COMMENTS

However, although the authors obviously have conducted a systematic study of the applicability of their approach, they present an incomplete data set, which the reader

cannot judge on his own, because it lacks some important information (see my comments below).

Many of the statements made in the paper and in the conclusions are based on analyses which are not shown or discussed – so the reader cannot follow the argumentation (e.g. the last three points in the list of key results in the conclusion). Similarly, some results are presented in Figures, but are not discussed at all in the text. For example, neither the background for Figs. 4 and 5 is explained, nor the conclusion of this analysis: which model is more appropriate to represent the results and why? Also Figs. 7 and 8, which I expect are the main result of the study, are only discussed in a few lines. Concerning the interpretation, it is not clear to me whether the authors correctly differentiate between permafrost and ground ice. With ERT data, the specific amount of ground ice (and water content, soil texture, porosity etc...) makes a huge difference for the interpretation of the resistivity values, not only the presence or absence of the (temperature defined) permafrost.

The authors very often cite only own publications (11 out of 17 citations are from the authors themselves, with 3 out of 11 being only EGU abstracts), which is not representative – neither for the application of geoelectric surveys in permafrost, nor for 3D or quasi 3D geoelectric approaches, which are widely applied e.g. in hydrology or archaeology.

If a methodological study was intended more details on the results from the synthetic modelling have to be included (e.g. the synthetic model itself and the respective deviations from it have to be shown; as well as comparisons between the different array configurations and the so-called robust and least-square inversions). If a case study was intended, then the discussion and interpretation of the 3D data are sparse and the few presented results do not really match the high efforts for the acquisition and processing. However, extending the paper by a complete presentation of the methodological analysis could really provide a baseline for future applications of quasi 3D geoelectric surveys in mountain permafrost.

SPECIFIC COMMENTS

- The authors describe that extensive studies were made at the Muragl site prior to the quasi 3D survey (including 1D and 2D geoelectrical surveys and monitoring, refraction seismics, temperature loggers, a borehole, etc.). It becomes not clear to me, why the synthetic modelling was not conducted for the Muragl site itself, but for another site? What are the reasons for choosing the two test sites and how are they related to each other?

- A map with the location of all ERT profiles within the glacier forefield and the corresponding spatial dimensions would be useful.

- The discussion of the results is not comprehensible by the reader, as important data are not shown. To evaluate the performance of the inversion of a) synthetic data for different line spacing (Figs. 1a-c), and b) for the benefit of perpendicular tie lines (Figs. 2 and 3), the initial synthetic model should be shown, e.g. similar to the cube in Fig. 8. Without knowledge of the synthetic model, the results in Fig. 1b and Fig 2/3 appear to be largely comparable, and Fig. 1b (only parallel lines) may even produce more realistic results: compare upper panels in Fig. 1b and Fig. 2/3: I guess the synthetic model is rather smooth (as in Fig. 1) and not as patchy as in Figs. 2/3? How do you explain the patchy pattern? Why are the blue anomalies located between the ERT lines?

- I have the impression that the term permafrost is sometimes used synonymously with ground ice (e.g. "permafrost lenses" in lines 15, 17). Otherwise, if there are no other evidences for large permafrost-free zones in the glacier forefield, the interpretation of the data (page 903) seems to be ambiguous: Resistivity values higher or lower than a certain threshold are used as indicator for permafrost or non-permafrost areas. For example the statements in lines 9 ("lower boundary resistivity value representing permafrost conditions"), 20 ("isolated small permafrost lens"), and line 20 on page 904 ("in case of large resistivity contrast, reliable distinctions can be made between

permafrost and non-permafrost areas"), are only valid, if permafrost means relatively high ice contents, because permafrost in terms of frozen conditions with very low ice content cannot necessarily be differentiated from unfrozen coarse-grained material by ERT measurements. Since the authors state in the beginning that they want to investigate small-scale heterogeneities in permafrost conditions, the relations between a) frozen/unfrozen conditions, b) high/low ice contents, and c) differences in subsurface material should be discussed in more detail to avoid ambiguities.

- P. 903, line 19-23: How can you evaluate from ERT data whether the permafrost is in equilibrium with climate conditions? I wonder if the state of permafrost can be judged from single measurements, or whether a monitoring would be necessary to identify degrading permafrost?

MINOR COMMENTS

- The font size of the labelling in all figures is too small

- P. 897, line 7: better: "...able to resolve the structure of the shallow subsurface."?

- P. 899, line 1: please specify what you mean with "GIS-based models".

- P. 999, line 23: a specification of contact resistances with and without usage of sponges would be interesting

- P. 900, line 3: Res3Dinv instead of Res2Dinv?

- P. 900, line 14: "However, the best model from a geomorphological perspective might not be the one with the lowest possible RMS." Please explain this statement in more detail.

- P. 901, line 1-2: please specify what you mean with "fine", "medium", and "coarse"

- P. 901, line 24: a map with the position of the profiles would be desirable here

- P. 901, line 27: "The assumption, that properties (vertical and horizontal resolution,

depth of investigation) of different electrode spacing and array types can be transferred from 2-D ERT is confirmed." By what? Please add a citation here or give reasons for this statement.

- P. 902, line 14: see specific comments: without additional knowledge (synthetic model) it is not clear from this data, that perpendicular tie lines significantly improve the inversion result.

- P. 902, line 29: do you mean "...permafrost occurrence of ... m depth" or "thickness"?

- P. 903, line 5: could this not just be a variation in ice content, or do you have other indications which clearly point to unfrozen conditions? A variation in ice content could also explain the variations in resistivity in Fig. 6: according to the assumption of a heterogeneous subsurface material, the right part in Fig. 6 could (in my opinion) also represent a zone with low ice content rather than a deeper permafrost table below a 7 m thick active layer.

- P. 904, line 1: "Results from the field surveys confirm that good data quality and spatial resolution of the subsurface model can be achieved by using triple electrode spacing between parallel surveys in combination with perpendicular tie lines." These results are based on a synthetic model, which is not shown, so this statement is speculative.

- Table 1: why is the RMS error after 6 iterations given, when all the presented data are from the 3rd iteration? Which inversion scheme was applied?

- Figure 6: I do not agree with the interpretation (interface between active layer and frost table) of the seismic tomogram: the green zone is clearly a homogeneous zone (e.g. frozen debris?). Interfaces between different layers are rather indicated by gradual velocity changes than by homogeneous zones.

Interactive comment on The Cryosphere Discuss., 3, 895, 2009.