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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 #1

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General comments:

This paper introduces an interesting attempt of electrical resistivity survey in mountain permafrost environments and I'm really impressed by the authors' effort to collect a number of data of 2D ERT profiles on a rugged glacial deposit and a steep talus slope.

However, unfortunately, the paper is not well organised.

First of all, I'd like to ask the authors' main intension. Do the authors want to appeal the original tactics of the quasi-3D ERT to the scientific community?

If so, the presentation, explanation and discussion of the forward modelling must be

too poor as I point out later. The authors cited Schwindt and Kneisel (2009) instead. However, it is only a non-peer-reviewed abstract of an EGU meeting, thus this citation is too weak to skip detailed discussion.

If the answer is no, it would be better to focus the results on the glacier forefield (Figs 6, 7 and 8) as a case study (it suits a Brief Communication: 2–4 journal pages, a maximum of three figures and/or tables, maximum 20 references, and an abstract length not exceeding 100 words). In the latter case, the paper may be re-organised to compare the data with the micro topography, surface flow field, 1D and 2D geoelectrical data, ground surface temperatures, etc. in the same site.

Specific comments:

Sites:

It would be better to briefly note that 2 sites were chosen for this study at the beginning of the chapter.

Although the authors mentioned that both sites have a lot of previous studies, the site characteristics revealed by these studies were hardly introduced. For example, the advance and retreat of Muragl glacier during the Holocene mainly control the horizontal and vertical distribution of the permafrost in the forefield. Such a note probably helps readers to know the reasons and significance of the site selection.

p. 899, ll. 1-4

"... a high number of parallel, overlapping and perpendicular ERT profiles ..."

Are there really 'a high number of ERT profiles' in the site? Only 3 profiles were indicated in the cited papers.

ll. 5-6

If no location map was presented, the authors should notice the latitude and longitude of the site. In addition, it may be better to replace 'trough-shaped' with 'U-shaped'.

l. 7

I think that the sentence "Both the north- ..." is unnecessary.

Methods:

p. 901, ll. 4-7

"..., based on ... (Kneisel and Schwindt, 2008) indicate that the most important factors influencing data quality are parallel spacing and the information of perpendicular crossing profiles ..."

This sentence indicates that the tactics of the quasi-3D imaging has been already established, although that is not true (the cited paper does not show the quasi-3D imaging).

ll. 7-9

It would be better to write down why the often recommended way can hardly be applied in mountainous regions.

ll. 11-12

"Design of the synthetic profiles was geared on the surveys measured in the Bever Valley."

This sentence needs a reference and it would be Kneisel and Schwindt (2008).

ll. 13-16

"Synthetic quasi-3-D images were modelled using different array types (...), electrode spacing (...) and ...."

This is not true for this paper, because only one array type and one electrode spacing were presented for readers.

Results and discussion:

p. 901, ll. 20-24

The first sentence of the chapter is not true for this paper. It would be, "In 2008 permafrost occurrence with fine- to medium-grained surface material was investigated using 17 ERT profiles."

In addition, such information presented in the first paragraph should be mentioned in the method chapter rather than that for the results and discussion.

p.901, l. 27 to p. 902, l. 2

"The assumption, that properties (...) of different electrode spacing and array types can be transferred from 2D ERT is confirmed."

Where is data? How did the authors analyse? After all, is this really assumption? Did the authors check whether 'the assumption' can be predicted from the software algorism to generate quasi-3D dataset from modelled 2D profiles?

p. 902, ll. 2-3

"Choice of electrode spacing and array type depend on site characteristics and objectives of the project."

Is this a result of this study? If so, it should be explained much more specifically.

ll. 3-5

"The application of only parallel arrays results in ... and loss of information value with larger parallel spacings (cf. Fig. 1a, b, c)."

It should be specified that the Fig. 1 indicates either collated values of 2D apparent resistivity forwarded models or (real) resistivity models by the following inversion. I suppose the figure shows the latter case. Is this supposition correct?

Readers can find the difference between Fig. 1a, 1b and 1c but CANNOT judge which part loses the originally given resistivity, because the authors did not show the initial

(starting) model of resistivity distribution which was prepared for the forward modelling. Thus, we cannot evaluate whether the larger spacing is worse or not.

In addition, in contrast to the authors' insistence, the resistivity models at shallow depths appear to show more unrealistic liner anomalies for smaller spacings.

ll. 6-11

It is questionable that the effectiveness of perpendicular tie lines was checked properly. To check effectiveness of tie lines, all pairs of longitudinal (Y direction) profiles between the two cases should have same apparent resistivity distribution. Then, incorporation of minor difference only detectable in tie lines should be tested.

However, the authors gave large difference in the starting models as indicated in Figs 4 and 5, which probably results in different resistivity distribution between the two cases even though only parallel lines derived from the given models are collated and inverted.

ll. 16-21

To lead this paragraph, thorough revision will be required as I mention above.

p. 903, l. 8

"... in combination with results of time-lapse geoelectrical monitoring, ..."

This sentence needs a reference.

ll. 26-28

"Advantages and disadvantages of different electrode spacing and array types can be transferred from 2-D ERT."

Nearly nothing about such topics is discussed in this paper.

Conclusions and perspectives:

p. 904, ll. 10-14

Nothing to lead this conclusion is discussed in this paper.

ll. 16-18

This is not one of the conclusions from the data and discussion presented in this paper but that of Schwindt and Kneisel (2009).

l. 19

This conclusion is still questionable.

ll. 21-22.

"..., however as for 2-D surveys the reliability of the inversion results diminishes at greater depth."

This topic is not discussed in this paper, either. At least, it would be better to replace it with, "the reliability ... results is expected to diminish ...."

Figures:

It would be better to use the identical colour scale for Figs. 6 and 7.

Technical corrections:

p. 901, ll. 4-7

"..., based on ... (Kneisel and Schwindt, 2008) indicate that the most important factors influencing data quality are parallel spacing and the information of perpendicular crossing profiles ..."

A comma must be required after the parentheses.

p.901, l. 27 to p. 902, l. 2

"The assumption, that properties (...) of different electrode spacing and array types can be transferred from 2D ERT is confirmed."

A comma must be required after "ERT".

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