

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

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Reply to editor:

Dear Editor,

The objective of our contribution was to introduce a new application - the so-called quasi-3D resistivity imaging- of the established 2D ERT imaging method for mapping permafrost conditions tested at a site with well known heterogeneous permafrost distribution (known from our previous research results, e.g. numerous 2D ERT surveys, refraction seismic surveys etc.).

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We intended to introduce this new application to a broader community in a concise form presenting results of the successful application and briefly describing the chosen setup which is not based on trial and error but confirmed with the synthetic forward modeling approach.

In our opinion the technical details and results of the synthetic modeling as requested by the reviewers are beyond the scope of this contribution and will be subject of a planned manuscript that will be submitted in the near future to a journal more specialized on geophysics, herein, the constructive reviewer comments concerning the synthetic modeling approach will be included.

We will follow yours and the reviewer's suggestion and will submit a revised version of our manuscript as a brief communication introducing the new application quasi-3D resistivity imaging method, omitting the part of the forward modeling, hence, our reply to the reviewer comments will focus on those comments relevant for the revised version to be submitted as short communication.

We thank all reviewers and the editor for the constructive comments on our discussion paper.

Best regards C. Kneisel, A. Bast, D. Schwindt

Authors' response

First of all, we would like to express our thanks to the three anonymous reviewers for their constructive and useful comments. Below, we describe the general changes we will apply to the manuscript to improve the paper, followed by more detailed replies to the individual points raised by the reviewers.

Overall remarks

Based on the comments by the editor and the three anonymous referees we will apply the following modifications to improve the manuscript. For the preparation of the revised manuscript we will consider all minor comments which are relevant for the

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modified content of the brief communication concerning grammatical comments, suggestions on re-formulation, font-size of figures etc. Furthermore, in the brief communication we will focus on the Muragl site, exclude the site description for the Bever site and will omit the forward modeling approach. A photograph of the measured grid in the Muragl glacier forefield will be included. The manuscript will be revised and several sections will be rewritten. We will extend the discussion and interpretation of the presented data. The conclusions will be rewritten, as a large part is relating to the results of forward modeling.

Anonymous Referee #1

“The paper is not well organized. 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?”

We don’t completely agree with you, regarding our main intention, as mentioned above, we think the structure is traceable and the paper is well organized,

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

That’s correct, but for the brief communication we will focus on one site only.

“Although the authors mentioned that both sites have a lot of previous studies, the site characteristics revealed by these studies were hardly introduced.”

Correct, this was a bit short in the original manuscript and will be extended.

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

In the paper you are referring to, 3 profiles are shown representing three different resistivity classes, however, 45 profiles were measured to infer the synthesis map.

“(p. 901, ll. 4-7 ”)...., based on ... (Kneisel and Schwindt, 2008) indicate that the most

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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)."

This statement is based on previous studies performed in the Bever Valley and was confirmed by forward modeling and recently measured quasi-3D grids. It is true, that the cited paper does not show the quasi-3D image. In Kneisel and Schwindt (2008) only the location of the measured 2D ERT surveys are presented.

"(Il. 7-9) It would be better to write down why the often recommended way can hardly be applied in mountainous regions."

The phrasing will be changed and extended to make the statement clearer. It is possible to apply quasi-3D ERT with less than double electrode spacing, which would be, in case of steep alpine terrain and logistical reasons (e.g. time-consuming electrode coupling, battery capacity, efficiently using good weather conditions) not really efficient.

"(Il. 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)."

We agree, the missing reference will be added.

"(Il. 13-16) "Synthetic quasi-3D 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."

This is correct, but as our intention was to write a concise paper of the quasi-3D approach in alpine environment with forward modeling supporting the field measurements, this statement was based on our experience with this method. As the Wenner-Schlumberger array was used for the quasi-3D imaging in the Muragl Valley the same was chosen for the synthetic dataset.

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“(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.”

We agree, this will be modified

“(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 algorithm to generate quasi-3D dataset from modelled 2D profiles?”

As mentioned above, our aim was to present a concise paper of our experience with the quasi-3D imaging. The statement was based on analyses that are not objective of this paper, that’s why this paragraph will not be included in the short communication.

“(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.”

This is not a result of the study. The quasi-3D imaging is based on the combination of several 2D ERT transects, hence, the advantages and disadvantages of different setups can be transferred. We will rephrase and include a reference for the 2D ERT case.

“(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?”

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This is correct, but the figures will not be included in the short communication.

“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.”

This is correct, but the figures will not be part of the short communication. The effect of linear anomalies, due to higher noise at shallow depth is not discussed sufficiently in the paper. Technical details on the forward modeling approach including the initial model will be presented in a special publication.

“(II. 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.”

We think that the effectiveness of tie lines was checked properly, both in forward modeling and in field measurements. The explanation of our approach was maybe too brief. We will consider your suggestion for optimization, even though the figures will not be part of the short communication.

“(p. 903, I. 8)”... in combination with results of time-lapse geoelectrical monitoring, ...” This sentence needs a reference.”

Yes, we will add the missing reference.

“(II. 26-28) "Advantages and disadvantages of different electrode spacing and array

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types can be transferred from 2-D ERT." Nearly nothing about such topics is discussed in this paper."

and

"(p. 904, ll. 10-14) Nothing to lead this conclusion is discussed in this paper."

The quasi-3D imaging is based on the combination of several 2D ERT transects, hence, the known advantages and disadvantages of different setups can be transferred. We will rephrase and include a reference for the 2D ERT case.

"(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)."

That's true. We will not discuss it in the brief communication but in a further manuscript that will be submitted in the near future.

"(l. 19) This conclusion is still questionable."

We don't agree that this conclusion is questionable but will prepare an additional figure for a better illustration.

"(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"

We will alter it.

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

The color scale is identical, for better visualization different contour intervals were used.

Anonymous Referee #2

"Many of the statements made in the paper and in the conclusions are based on analyses which are not shown or discussed (e.g. the last three points in the list of key results in the conclusion)"

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The statements in the paper concerning the forward modeling approach will be omitted within the brief communication, the conclusions will be revised

“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”

These figures will not be relevant for the content of the brief communication.

“Figs. 7 and 8, which I expect are the main result of the study, are only discussed in a few lines.”

The assumption, that figures 7 and 8 represent the main results of the study is correct. A more detailed interpretation on these figures will be added

“it is not clear to me whether the authors correctly differentiate between permafrost and ground ice”

See reply further below

“The authors very often cite only own publications”

That’s right, the reason is that the new application was tested at our site where we have numerous results from different applied methods, to our knowledge no other applications from periglacial mountain environments with permafrost in unconsolidated sediments are available for comparison so far.

“ If a methodological study was intended more details on the results from the synthetic modelling have to be included”

and

“ 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.”

Initially we intended to provide a combination of both as mentioned above. We cer-

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tainly do not agree that the results do not match the high efforts for acquisition and processing, but we will follow your suggestion and extend the discussion and interpretation of the 3D data set. We still consider this application as a further milestone in the application of near surface geophysics in mountain permafrost environments.

“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.”

We appreciate your appraisal, but like already stated, the aim of this paper was to give a brief overlook on our current results. We will be glad to consider your constructive suggestions for our planned publication on the methodological aspects of quasi-3D imaging to provide a baseline for future applications of quasi-3D imaging in mountain permafrost.

“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?”

We developed the application within our team, with first approaches at a permafrost site below timberline in the Bever Valley. Then, after gaining experience extending towards the discontinuous permafrost in the glacier forefield. Of course the two sites are not related to each other. However, in our opinion the results of forward modeling concerning electrode spacing, parallel spacing, influence of perpendicular tie lines and the application of different array types should be generally valid. Our aim was not to create a synthetic dataset of the permafrost occurrence in the Muragl Valley. Nonetheless, this paragraph will be deleted for the brief communication.

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

For the brief communication we will provide a photograph with the measured grid.

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“ The discussion of the results is not comprehensible by the reader, as important data are not shown.”

We will revise the relevant parts and extend the discussion of the results for the brief communication.

“ I have the impression that the term permafrost is sometimes used synonymously with ground ice”

We are aware of the meaning of both terms and that permafrost in terms of frozen conditions with very low ice content cannot necessarily be differentiated from unfrozen coarse-grained material by ERT measurements. We will include more discussion on a) frozen and unfrozen conditions and b) high/low ice content. Differences in subsurface material are also difficult to delineate from ERT alone.

“(P. 903, line 19-23) How can you evaluate from ERT data whether the permafrost is in equilibrium with climate conditions?”

This statement is not based on ERT alone, but also on previous measurements as shown in the cited publications (Kneisel, 2004; Kneisel and Kääb 2007). We will rephrase and add more details on this.

“(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.“

We will include a reference

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

We will specify different substratum classes in the revised manuscript.

“(P. 901, line 24): a map with the position of the profiles would be desirable here”

As stated above we will provide a photograph with the measured grid in the brief com-

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munication.

“(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.”

and

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

Tests confirming these statements were applied during the forward modeling process and confirmed by field measurements. Due to the decision that this paper will be resubmitted as a short communication these results will be presented in a planned special publication.

“(P. 902, line 29): do you mean "...permafrost occurrence of ... m depth" or "thickness" ;’

This is true; “depth” will be corrected towards “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 7m thick active layer.”

In consideration of one ERT measurement only, this interpretation could be possible. Our interpretation is based on additional results from adjacent ERT surveys, ERT monitoring, repeated BTS measurements, surface temperature logging (miniature temperature dataloggers), borehole data up to 8m depth in direct adjacency as well as geomorphological findings and a GIS-modeled PISR (potential incoming solar radiation) map. Hence, we will add more details to that topic in the brief communication to substantiate

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our interpretation.

“(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.”

We suppose the phrasing must have been not precise enough, as the statement was not concerning the synthetic model, but the field survey. The outline was related to the quasi-3D image measured in the Muragl glacier forefield where we achieved a good data quality and resolution using the mentioned configuration. However, we will rephrase and prepare an additional figure for illustration.

“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?”

Table 1 gives statistical details of the measured quasi-3D image only, and not of the synthetic data. We will remove the table for the brief communication and will show some of these details in combination with Fig. 7.

“(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.”

Yes, the green zone represents frozen debris and is shown in fig. 6 as a more or less homogenous zone. The permafrost body in that part of the glacier forefield is characterized by comparatively “warm” temperatures with presumably high water content in the segregated ice lenses. The transition between the warmer outer parts of the permafrost body and the slightly colder inner parts is not abrupt but is rather performing a slow change. Hence, a hard velocity change can not to be expected for this special

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permafrost site. We will modify figure 6 to clarify the comprehensibility.

Anonymous Referee #3

“the outline of the forward modelling approach could be improved.(...) The paper should, thus, more explicitly focus on the technical performance of 3D inversion and forward modelling and should discuss technical approaches and problems in more detail.”

The objective of our contribution was to introduce the quasi-3D resistivity approach as a new method for mapping permafrost conditions tested at a site with well known heterogeneous permafrost distribution. In our opinion the technical details and results of the synthetic modeling are beyond the scope of this contribution and will be subject of a planned manuscript that will be submitted in the near future. However, for the brief communication we will focus on the results obtained at the Muragl site.

“The Introduction should shortly refer to advancements in 3D-ERT modelling outside the permafrost community.“

We will include some approaches in our introduction outside the focus of cryospheric sciences, e.g. archaeological studies.

"The Introduction could be more instructive about the problem why real 3D modelling (dipole/pole) is often difficult in permafrost environments – e.g. error levels of dipole arrays, signal to noise ratios”

Yes, you are right and we will incorporate it in the brief communication.

“The Introduction could refer to other 2.5D work in the permafrost community”

As far as we know findings from other quasi-3D approaches in unconsolidated sediments with permafrost are not available, but we will check the literature again

“The Introduction should include a paragraph on what questions could in future be addressed by 3D techniques that are not covered by 2D techniques”

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A paragraph with possible future applications of quasi-3D ERT will be included.

“I think, the “High-altitude alpine permafrost environments often exhibit ...“ section, that is presently implemented in the conclusion, rather belongs to the introduction.”

This is correct. We will include it in the introduction and remove it from the conclusions.

“It would be nice to give a concise statement on why it is important to understand the small-scale heterogeneity and what methods are presently at hand to do this.”

Yes, we will include it.

“Please state in some detail what are your systemic research questions that you aim to answer with your 3D-ERT approach.”

Our aim is to link the subsurface permafrost characteristics to surface textural characteristics, geomorphology, and, in the future, to other site characteristics (e.g. snow cover thickness and duration). We will present more details on that topic in the revised paper.

“Clarify the input data of the forward modelling: it would be nice to include a separate paragraph that explicitly describes the input data and forward modelling settings that were used to create Fig. 1 and Fig. 2.”

That’s correct, but this will be subject of a more technical manuscript but not for the brief communication.

“Be more precise on the inversion modelling parameters you use and give a short statement on whether you applied – robust inversion?, reduced effect of side blocks?, altered damping factors? . . . or not and why.”

Yes, we will include these details.

“(P 902 L3/Fig 1,a,c) I am not quite sure whether this is because of the data coverage and density (as you outline) or just an interpolation problem of RES3DInv”

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and

“The problem of single lenses is especially apparent in Figs. 1,2,3. It would be interesting to see whether the implementation of the original 2D data could change this in the forward modelling process, too,”

Again we will consider your suggestion for a more technical manuscript but not in the brief communication.

“The conclusion is too long and includes parts that belong to the Introduction”

and

“(P 905 L1-7) I would include that in the introduction to state the major focus of your present work”.

See above.

“The major outcome of the forward modelling is still a little bit unclear. This partly belongs to the fact that the input data is not explained in detail. However, the input data has a high impact on the "statement 1: transect spacing“ and on the "statement 2: importance of cross profiles“. I would suggest to rethink your message: "At the given heterogeneity of resistivity a transect spacing of max. 3 times the electrode spacing becomes necessary“ but this message makes an exact description of the implemented data sets necessary.“

Due to the fact, that we will remove the forward modeling part, we will reorganize and revise our conclusions for the short communication.

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

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