

Interactive comment on “Internal structure of two alpine rockglaciers investigated by quasi-3D electrical resistivity imaging (ERI)” by Adrian Emmert and Christof Kneisel

Anonymous Referee #2

Received and published: 27 September 2016

This manuscript describes the field investigation of the internal structure of two alpine rockglaciers using quasi-3D ERI. Also, this study compares these results with 2D SRT and ERT transects, and with thermistor chain data. The subject of this study is of great interest for the Cryosphere community. I enjoyed reading it. Although I recommend this manuscript for publication, I suggest some moderate revisions.

Major Comments:

1) The use of quasi-3D inversion is great and very valuable. However, I think its value cannot be really evaluated here because Figure 4 is not sufficient to analyze results. I suggest that the author also show a fence-type figure displaying various transects in 3D. This would help to see the variations in both depth and elevations. Also comparing

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results from a few transect inverted separately vs together would be useful to help the reader understand the value of quasi-3D ERI.

2) The description of the Figure 4 (5 and 6) and discussion in the manuscript is great (including the discussion of past studies). However, it is very hard to follow the discussion given the limited information in the figures. Topography is likely a key property in such analysis and has been measured (as visible in Figure 4) (I suggest that the authors add a sentence to explain how they measured it in more details). It would be great to have a map of the topography so that below structure could be more easily compared to surface variability (topography and micro-topography). Figure 4 shows that topography has been included in inversion but currently it is hard to observe and compare surface and subsurface features. Also a geomorphological map or any other information about the surface properties could significantly strengthen the paper.

3) I agree that the interpretation of thermistor data, EMI and SRT is generally difficult at temperature around 0 C. I think it would be very useful to see temperature profiles at other times of the year too for enhanced understanding of where the system is expected to be more dynamic. Please consider adding such a dataset in a figure.

4) There are several locations where wording could be possibly improved. E.g., “Its” or “their” on Line 6 (what does the “its” refer to) ?; “and hence the performance”(Line 10) sounds awkward; “homogeneous decrease” or “constant decrease” (Line 12) ?; etc.

Minor Comments:

L. 28 on page 2: Consider rewording last sentence.

L.31 on page 2: “best investigated” in the world ? Please be more specific

L.2 on page 3: “boreholes” at each site ? one at each site ? please clarify. I also suggest that authors name the two sites in this section.

L.21, page 4: what is the inter-electrode spacing (2m ?)

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L.28, page 4: standard deviation larger than 5%. Please provide information on how this standard deviation is measured, by the acquisition system alone ? or using reciprocal measurements ?

L.30 page 4: "collated". Consider using "merged"

L. 17, page 5: the shots were all located on the transects. Please discuss shortly why no shot have been performed outside the geophone interval (as often done in seismic refraction).

Section 4.1: I got lost in this section because I did not understand what was discussed first. I suggest that the author clearly state that this section compare single ERT transect and SRT. I think personally that this section should be placed after the quasi-3D inversion and be only related to comparison between seismic and ERT.

Line 5, page 6: "last two sensors". At which depths ? The sentence is confusing, although I agree that temperature close to 0 C means that comparison between various dataset is difficult, as well as the assessment of permafrost presence or not.

Line 5 on page 10: "removed" using which criteria

Line 9 on page 10: 'cut-off' value. Please clarify process. Although it is hard to evaluate penetration depth without knowing the acquisition geometry (a and n spacings), it seems to me that the bottom part in Figure 3 is not necessarily real. Please clarify your thoughts about this zone in the manuscript.

L.15, p.9: "could not rediscover" or "did not rediscover (or observe)". Please clarify the potential reason. Is it because environmental/climate change, thermal state during survey, difference in acquisition geometry ?

L24, page 13 "with the observed surface ridge structure", as well as in result and discussion section. It would be great to have more visual information (aerial image, geomorphological map, topography) to discuss these links.

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Figure 2: all the other figures have similar range of values (colors) for ERT, except that one. For better comparison between the various figures, I suggest that the authors trim this color scale and clarify this in the caption.

Figure 6: there are some longitudinal features that can be clearly identified and discussed in the manuscript. It would be nice to see them in a fence diagram too (ERT transects reposition in space) (see major comment). This would also show the reader that these features are observed in multiple cross-sections.

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