

## ***Interactive comment on “Active and inactive Andean rock glacier geophysical signatures by comparing 2D joint inversion routines of electrical resistivity and refraction seismic tomography” by Giulia de Pasquale et al.***

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First, I'd like to apologize for the delay in reviewing the manuscript. It has been an extremely challenging year for everyone, I assume. De Pasquale and co-authors present a manuscript in which they compare 2D joint inversion routines of electrical resistivity and refraction seismic tomography (ERT / RST) from an active and an inactive rock glacier located in the Andes. Considering the challenging circumstances for collecting the data, the results of the geophysical survey are of interest to the readers of this Journal. However, the paper lacks a clear focus as the authors are

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trying to cram too many ideas and thoughts, some supported by good evidence and others purely speculative. Therefore, I suggest that the manuscript should not be published in its current form and major revisions are required. As a supplement, the authors will find an annotated version that many contains specific comments and questions as well as editorial suggestions. My comments here are therefore only of general nature. I understand that the novel joint inversion is the key of the research and as such, the authors should focus on those measurements and results. The discussion on the hydrological significance is not essential for this publication and in fact distracting. In addition, there seems to be several misconceptions regarding the hydrogeology. For example, the authors imply that water in the watershed must originate from a cryoform. That is incorrect and I think the measurements seem to indicate that there are relatively shallow groundwater aquifers likely below the base of the rock glacier. A proper understanding of the hydrogeology would be needed prior to drawing the conclusions presented, but the authors do not provide any information on the hydrogeology. The measurements also do not support a discussion on the periglacial hydrology as presented, and it would really be better to completely delete these sections. I would also encourage the authors to carefully read some of the articles referenced so that they accurately cite these references and not just sentences that may be read out of a general context. After reading the manuscript I'm still confused about the El Jote Rock Glacier. Is it now an inactive rock glacier, or is it a relict rock glacier? Based on the inversion results it seems that the average (!) volumetric (I assume it is volumetric) ground ice content is 1 – 2%. Unfortunately, the authors did not provide any error ranges for their outputs (something that must be added in the revised version), but even if the error is +/- 5%, which would be very good, this landform is more likely to not contain any ground ice anymore. This means, the probability for the El Jote Rock Glacier being a relict rock glacier, i.e. there is no permafrost left, is significantly higher than it being an intact rock glacier (active or inactive). The new inversion presented seems reasonable, however, there is very little evidence for it to be accurate because there are no in-situ data available, as the

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authors indicate. I'd like to remind the authors that geophysical investigations have been completed by others for which data from boreholes are available. The authors are therefore encouraged to first test their new approach for a well-known site and once confirmed that the methodology is accurately working, applying it to a site for which no information is available can be done. I was also surprised why the authors did not collect any soil samples from the front of the rock glaciers to at least get an idea of the potential gradation of the soil material and some of its characteristics, but instead they rely on references from the Alps. It also would have been helpful if the authors had extended their lines past the edge of the cryofoms and carried out additional lines perpendicular to the only one they completed, which would have allowed them to measure the ERT and RST characteristics of the natural terrain without a rock glacier as well as providing a cross calibration point. This is a fundamental step when completing geophysics on rock glaciers without any other information. Finally, I'm very surprised by the depth of the surveys. The authors managed to go much deeper than most ERT and RST surveys using similar configurations and I could not find an explanation for that. It is important that the authors better acknowledge the very limited data they have. It is understood that the measurements are challenging to complete, but this major limitation must be reflected in the interpretation of the results, the discussion and ultimately in the conclusions drawn from the two, very different surveys. Finally, there are several conceptual problems in the manuscript, such as when it comes to the origin of the water, or calling the form El Ternero glacier, instead of El Ternero rock glacier, saying that the rock glacier surface is below a layer of rocks, setting the permafrost table equal with the top of an ice-rich layer, or implying that an inactive rock glacier must be in a degrading state, etc.. While some of these mistakes may sound minor, they are indicative for not taking proper care of the science and rushing through arguments without taking care of every single sentence and word written. In summary, the measurements are worth to be published in the Cryosphere, but a major revision of the manuscript is strongly recommended for which the authors should focus on the novelty and refrain from speculations.

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Please also note the supplement to this comment:  
<https://tc.copernicus.org/preprints/tc-2020-306/tc-2020-306-RC3-supplement.pdf>

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Interactive comment on The Cryosphere Discuss., <https://doi.org/10.5194/tc-2020-306>, 2020.

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