The Cryosphere Discuss.,
https://doi.org/10.5194/tc-2020-306-RC1, 2020

The Cryosphere Discussions
Open Access
TCD
Interactive comment

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.

Anonymous Referee #1

Received and published: 11 December 2020

Dear authors, dear Editor,

The manuscript "Active and inactive Andean rock glacier geophysical signatures by comparing 2D joint inversion routines of electrical resistivity and refraction seismic tomography" presents the application of seismic refraction (SRT) and electrical resistivity tomography (ERT) methods for the investigation of two block glaciers. The objective of the manuscript is to gain information about the internal structure of the glaciers, in particular the water and ice content. The authors use two different joint-inversion algorithms that permit to resolve for a subsurface model (define by physical properties, i.e., electrical resistivity and changes in seismic velocities) that simultaneously explain the data collected with the two different geophysical methods. This is a study in line with the interest of “The Cryosphere”. Although the use of SRT and ERT for the characterization of a rock glacier has been addressed before, the use of two joint-inversion algorithms offers an interesting perspective and make the study relevant beyond other case-studies. Thus, I recommend the publication of the manuscript. However, I think that the paper could be significantly improved in its structure and the actual discussion of the results. I am here attaching a marked .PDF where i pointed to some lines that I think need to be evaluated and corrected by the authors. There point to formulations that need to be improved to avoid misunderstandings or possible technical errors in their descriptions. However, while readings the manuscript I found eight major concerns, which I think the authors should address: 1) I feel the tittle to be too provocative and/or to certain extent misleading. I understand that the authors want to stress the comparison of results obtained through two different inversion algorithms. However, I am not sure that the comparison is valid taking into account that one refers to a spatial regularization schemes and the other one aims at solving a set of petrophysical models. Moreover, the joint-petrophysical inversion uses a set of calibration parameters (presented in Table 3 and Table 4) to permit the computation of air, ice and water content. However, in the present study, such parameters are taken from the literature, even if these are calibration values than need to be adjusted to each site (see the works from Archie and the reference to the studies by Glover et al., (2000) and Glover (2009) – full references below). In this regard, the comparison is unfair and technically limited. Would not be better to change the tittle to something like “extended interpretation based on the application of two joint-inversion algorithms”? 2) If the authors decide that the comparison of the results is relevant, then I suggest that the authors provide a quantitative comparison of the parameters obtained through the joint inversion, i.e., the seismic velocities and electrical resistivity resolved from the 2 inversion algorithms. Right now, the authors present only the plot of the inversion results and force the readers to compare those results by means of color-coded images in different pages and sizes. I feel such comparison to be at best qualitative and open to debate. It would be better if the authors plot the parameters solved for both strategies (for example, Vp from the joint-petrophysical inversion vs. Vp from the structural joint inversion). In this case, deviations between both approaches could be quantified. Moreover, such analysis would be also convenient within a numerical study (with Gaussian error), where deviations from the truth model can be also quantified. 3) In order to perform a proper analysis of the two algorithms, the authors should investigate the variations in the retrieved models after testing different parameters used for the inversion. In this regard, the authors could investigate the resulting seismic velocities and electrical resistivity values after testing a few parameters in the petrophysical joint inversion and a few combination of the scale-length correlations for the joint-structural inversion. Right now, the study runs a set of inversions with some values extracted from the literature (for the petrophysical inversion), and based on the slope (for the structural inversion. Are we expecting the models to be comparable? – actually we are forcing the joint inversion to converge with some predefined settings that might not accurately describe the field conditions. In this regard, the users might be causing larger uncertainties in the inversion than just solving for a smooth-constraint independent inversion of the different data sets. I think that the proper comparison of the different joint-inversion algorithms needs to address the use of adequate parameters, or at least assess the deviations in the retrieved models by an inadequate selection of the inversion settings. The use of joint-inversion schemes has been largely investigated in geophysical studies, still they are not widely-accepted as they rely on the use of site-specific models or require of a correlation between the different parameters that might not exists. I think the authors point out to this problem. The use of a numerical study would be also a good option to extend the analysis to quantify deviations from the truth model. 4) Regarding the correlation of the seismic and electrical parameters, I find Figure 9 quite intriguing. Actually, the authors demonstrate no correlation between the seismic velocities and the electrical resistivity. I can see a cloud of points with a large variance and to pattern. However, the authors describe a correlation and quantify a model linking both of the properties. However, the authors do not present the correlation coefficients that actually quantify the actual correlation. The actual lack of correlation observed in Figure 9 is especially disturbing taking into account that the use of the joint-structural constraints aims at improving such correlation. Is such poor correlation due to the inadequate correlation lengths selected in this study? Is this a problem of poor data quality? If the authors cannot address this question in detail, I think that the authors should completely remove Figure 9. If the authors decide to keep the figure, please write explicitly the correlation coefficient and address in detail the lack of correlation. 5) I would like to get further information regarding the reasons to select the correlation-lengths used in the joint-structural inversion. Did I understood correctly that the values selected are related to the profile inclination (i.e., the slope)? I think the authors should investigate this in detail. Such value has no statistical-meaning regarding the correlation of the two geophysical parameters. Would not be more convenient to investigate the variograms of the measured data? Or, at least from the two independent inversions (following the smooth-constrained algorithm)? I might be misunderstanding this point, but the main inclination of the profile is not an argument to define the correlation lengths in this inversion. If the authors are really using the slope of the profile as a correlation length-scale, would not be expected then that this inversion provides practically the same inversion result than the smoothness-constraint? Finally, both approaches would be controlled by a lineal increase in the seismic velocities, which is in both cases forces to the plane defined by the surface geophones. 6) I also think that the authors should present information about the data-error. It would be convenient to see the pseudosection of the resistivity data, and maybe the travel times of the seismic measurements to assess the data quality. Maybe the small variations observed between different inversion algorithms result only by fitting the same data to the low error parameters defined by the authors. In this regard, I would be very interested to see more details of the normal-reciprocal analysis conducted by the authors. Just based on the principle of the error model, I would like to understand how can the authors solve for a relative error of 1% as mentioned in their manuscript. Such error is too low for the high resistivity solved in the inversion. Such low relative error is not consistent with the description of the authors regarding the high contact resistances and the problems setting the measurements. Is the analyses of the data based on the misfit between normal and reciprocals or the fractional error? Which analysis was used to define the 300 ms error parameter defined in the inversion of the seismic data? I just find such values extremely low and would be critical to understand how were such values quantified. What were the steps used for the identification and removal of erroneous measurements and outliers? I think that the authors could then present the L-curve for their independent inversions (for such low error parameters) as this would make the study more complete. This could also alleviate concerns regarding the accuracy of the fitting in the inversion and remove the redundant Figures 5 and 8. 7) I think that the authors could improve the figures presented. I read the manuscript printed in hard copy and it was just impossible to read Figure 1 and the color bars (especially in Figure 2). It is clearly needed to read the digital file and zoom-in. Moreover, if the authors decide to keep the visual inspection/comparison of their results, it would be more convenient to have all results for one glacier plotted in a single figure (independent, joint petrophysical and joint structural inversion). Maybe the plot of the air/ice/water fraction resolved for both glaciers (after the joint-petrophysical inversion) could be plotted together. In this regard, it is possible to compare the resistivity and velocity models obtained by different inversions in a single figure and the retrieved parameters for both glaciers (regarding the discrimination between active and passive). I also do not understand the sense of Figure 5 and Figure 8, as the authors refer to the RMSE and chi-square obtained in the inversion and the values are acceptable. I am not sure which extra details we can obtain from the relative residuals. In this regard, (and although it was already mentioned above), maybe it is still more convenient for the authors to address the data quality and quantification of data-error in detail, as well as to investigate the actual statistical correlation between the data and the effect in the retrieved models for different petrophysical parameters than those presented in Table 3 and Table 4. Maybe the authors can use my suggestions. I hope that my comments help the authors to re-structure and define the presentation of their results. I believe that an adequate analysis of the concerns mentioned above could also sharp the identity of the manuscript, right now it is not a case-study but it is also not a methodological paper. Maybe the investigation of the different aspects mentioned above could strength the relevance of the study as a case-study with an improved interpretation of the geophysical signatures through the combined used of different joint inversion strategies. Also, I recommend the authors to revise their conclusions and just list the main outcome of this study, avoiding speculations or points already addressed in previous studies.

References mentioned in this revision: âĂć Glover, P.W., Hole, M.J. and Pous, J., 2000. A modified Archie's law for two conducting phases. Earth and Planetary Science Letters, 180(3-4), pp.369-383. âĂć Glover, P., 2009. What is the cementation exponent? A new interpretation. The Leading Edge, 28(1), pp.82-85.

Please also note the supplement to this comment:
https://tc.copernicus.org/preprints/tc-2020-306/tc-2020-306-RC1-supplement.pdf

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