Anonymous REFEREE #1

The anonymous referee #1 pointed out different concerns about the paper that we try to address as follows:

- Hence, after long thinking, my suggestion is still that the authors provide a numerical study where they investigate the actual uncertainty in their results. Here they can provide 3 to 5 scenarios with varying ice/rock/air/water content and maybe lateral variations. I would recommend using exactly the same settings (quadrupoles and gather shots) as they use for the inversion of their results (i.e., after removal of outliers and geophones with poor coupling). Considering that the authors will have a real value (i.e., the numerical scenario), they can prove how good or bad their measuring setting can resolve different subsurface conditions, regarding both the geophysical heterogeneity and the air/water/ice/rock contents. The authors should also consider adding different noise levels.

  The numerical study could investigate the ability of the geometry used to collect the data to recover for instance the geometry of the rock glacier bottom (i.e., the contact to the bedrock) and the depth of the active layer. This is mentioned by the authors in line 27-28 as critical parts of the investigation of rock glaciers. Hence, the authors could establish a numerical study (with lateral variations in both the depth of the permafrost and the rock glacier) where ice content are defined as well defined anomalies and investigate how well the given geometry could recover such values. This may address the uncertainty in the ice and water content estimates. Maybe also to address components regarding the regularization that was mentioned during the very first version of the manuscript.

We now add a paragraph in the appendix (A2. results uncertainties) where we presents the results of a numerical simulation of the response of ERT and RST data to a known distribution of air/water/rock/ice for both field data settings and their petrophysical inversions. Here we present a quantification of the model results uncertainties accordingly to different noise levels in the simulated datasets.

In this new paragraph we address also the text-related specific comments of lines line 56-57, 73 and 284.

- The authors could also address the uncertainty associated to defining incorrect values for the Archie parameters (i.e., a, m n, and conductivity of the pore water).

As address in details later in the lines comment 225 and 241, these uncertainty and dependence on Archie parameters and conductivity of pore-water (in terms of validity of Archie's law depending on it) are addressed and discussed in the appendix (section A1 y A2). Also, in section A1 we discuss the regularization implemented in case of petrophysical joint inversion and we refer to Figure 3 and correspondent text for the regularization choice made in case of individual dataset inversion.

- Line 56 – 57 are incorrect and need to be addressed. The inversion is an ill-posed problem, not only affected by non-uniqueness. The presence of small uncertainty in the data generates large uncertainties in the inversion results. This is poorly addressed by the authors. The authors only discuss non-uniqueness. However, the influence of poor data quality has not been mentioned as a further source of error. Thus, I still believe that a numerical error could provide clear insights on the limitations of the results presented, especially the estimation of rock, air, water and ice content.

We modified the referred lines as follow:

"Once the geophysical data have been collected, the information contained in these needs to be interpreted: geophysical inversion seeks to provide quantitative information about physical properties from indirect geophysical observations. This is an ill-posed problem whose solution is neither unique nor stable (Backus and Gilbert, 1970). Thus, if any set of model parameters can be found that is able to explain the observations, then an infinite number of parameters-sets would exist and arbitrarily small errors in the measurement data may lead to indefinitely large errors in the solutions (Kabanikhin 2008)"

- Line 73: The joint inversion does not reduce the uncertainty. There is no computation of the uncertainty within the inversion algorithm. It helps to solve for a consistent model for the resistivity and the seismic data. Moreover, the solution converges to values that provide plausible estimations of rock, ice and air water content (as mentioned in the line 73 of the manuscript). However, there is no computation of uncertainties within the approach; this is still a deterministic solution. Hence, I believe that a numerical investigation based on their actual configuration (the measurements collected after removal of outliers) and simulating different conditions of the subsurface could really provide a estimation of the uncertainty in the results presented.

We follow the reviewer suggestions and re-write the sentence as:

"joint inversion scheme aims solve a model that consistently explains both resistivity and seismic data sets and better constrains the components percentages in order to avoid nonphysical results"

- Line 140 – 145 need to be carefully written. I provided some suggestions.

We follow the reviewer suggestions and re-write the sentence as:

"In most rocks and soils, electrical current is carried by movements of ions in the pore water (electrolyte conduction) and by the movement of mobile ions in an electrical diffuse layer at the grain-fluid interface (surface conduction - Revil and Glover, 1997), with the mineral matrix generally characterized by high resistivity, unless electrical conductors are present within it…"

- Line 151 – what is the contact resistivity? This needs to be corrected

Modified as *contact resistance*

- Line 175 – repeating and stacking sound redundant in the sentence. Maybe the authors could rephrase it?

We now deleted stacking

- Line 183 contains technical errors that need to be addressed

We modify the sentence as following:

"…the dipole lengths for the potential measurements were of 5, 25 and 45 m, while for the current injections where between 15 and 235 m with intervals of 10 m…"

- Line 203: Please provide clear description of the methods used to estimate the error parameters? Also about the parameters used to detect and remove outliers?

We do explain how the error parameters are estimate for both methods at lines 204-207 of the previously submitted manuscript:

"…the error models resulted in 1.2% relative error for El Jote and 15% error for El Ternero, in the first case, the error was obtained from the average of the standard deviation for measured apparent resistivities whereas in the second case such average resulted in 11.4% but it was subsequently inflated to obtain a satisfactory inversion convergence. For the RST, an absolute error of 0.001 seconds was considered, estimated from the average variability of the first arrival picking. "

Whereas we do explain the data processing in *section 3*.3 Data processing and Inversion:

"The ERT observation were automatically filtered using the acquisition software for a standard deviation larger than 25%, while for the seismic refraction travel time we manually picked the first arrivals after applying a gain to the seismic traces, therefore the traces were filtered according to our ability of identify the first arrival times."

- Line 225: which petrophysical constants values were used in the inversion of the data (i.e., a, m n, and conductivity of the pore water)? – was there any analysis on the dependence of the proposed results to those values?

We refer to section *A1 Inversion parameters for El Jote and EL Ternero* in the appendix, where we discuss the dependence of the inversion results on the Archie´s parameters *m* and *n* and on the initial porosity. As for the tortuosity factor *a* we followed the formulation fro Wagner et. Al (2019) and implement a modification of Archie's second law within the four phase petrophysical joint inversion (equation A3) where this parameter doesn't appears. We kindly refer to the comment regarding line 241 for the effects of a change in the resistivity model implemented within the petrophysical joint inversion.

- Line 241: if there are effects due to surface conductivity, could the authors discuss the applicability of the petrophysical joint inversion? – in the joint inversion deployed, the underlying model is only related to electrolytic conduction through Archie's model; thus, neglecting surface conductivity. If surface conductivity is dominating at the site, the estimates of rock, ice, water and air are incorrect . For instances, high surface conductivity are then interpreted as high porosity and water content. Yet this could be also related to lower porosity (i.e., due to the presence of fine grains), or higher ice content and the polarization of the EDL at the ice-water contact as mentioned in the line 242. This is an interesting point that the authors need to develop. However, it is only mentioned here to interpret the results and not addressed in the manuscript (i.e., lines 243 – 253).

We refer to section *A2 Methodology limitations in the appendix* (section A3 in the new submitted manuscript), where we describe the results of the analysis done by Mollaret et al. (2020). As explained in this section, they show that by using Archie's law, Archie's law with surface conduction and surface conduction model for the electrical resistivity petrophysical model, the inversion results are largely comparable and mainly depend on the porosity estimation, although they are based on theoretically different electrical conduction processes (due to the lack of field calibration of the respective electrical material parameters included in the equations, so that theses parameters are similarly determined by minimizing the data misfit).

- Line 284: what the authors imply with the marked sentence in the PDF? The P-wave velocity is defined by the ray paths within the inversion, not by other data. Maybe the authors mean that there are poorly resolved areas? – in such case, this needs to be rephrased. Is the poor coverage due to poor contact geophone-ground or due to the filtering within the processing of the data? – Maybe the numerical investigation can address for the particular measuring setting in this study (after removal of noisy measurements) and the ability of the data to detect anomalies within the poorly covered area are resolved?

We rephrase the sentence as follow:

"the model parameter domains shown in the individual P-wave velocity inversion results and in the petrophysical joint inversion results (Figs. 4c, 6c, 5 and 7) are geometrically delimited by the lowermost ray path but in the P-wave velocity model presented there are poorly resolved areas due to the limited ray-coverage within the displayed area."

- Line 347: I think the authors start an interesting topic by mentioning that "in many cases the implementation of petrophysical joint inversion can be limited by lack of proper petrophysical models". Could the authors develop further in this? - Do the authors consider that the 4-phase model implemented is not adequate? What petrophysical model needs to be implemented?

The quoted line is a general statement and doesn´t refer to the specific petrophysical model used in the study. Nevertheless we discuss its limitations in section A2 (section A3 in the new submitted manuscript) of the appendix

- Line 349-350: in which cases the proposed petrophysical inversion is not working? The authors should provide clear information. Address this directly with their data.

Please refer to comment above.

- Figure 6: the raypaths in the RST data could be plotted in a better y-axis to permit a better visualization of the data.

Modified accordingly

Anonymous REFEREE #2

The anonymous referee #2 pointed out different concerns about the paper that we try to address as follows:

- Title: As the study only presents data from one relict and one intact rock glacier, the title should be adjusted to: “Contrasting geophysical signature of a relict and an intact Andean rock glacier”

We modify the title accordingly.

- Line 1: Rock glaciers are not special types of glaciers as such it is scientifically wrong to say “other glacier types”

We modify the sentence as follow:

“In semi-arid Chile, rock glaciers cover more surface area than glaciers and are potentially important water reserves”

- Line 19: Herrington et al. (2018) did not write that the flow is coming from the rock glacier as the current wording implies. They have shown that water is passing through the rock glacier, which had an impact on water temperature and in consequence on the aquatic life. Please have a careful read at what Herrington et al. (2018) concluded.

We modified the sentence as following:

“Harrington et al. (2018) investigated the hydrogeological characteristics of an inactive rock glacier in the Canadian Rockies, showing that the coarse blocky sediments forming the rock glacier allow the rapid infiltration of snowmelt and rain water to an unconfined aquifer above the bedrock surface. The water flowing through the aquifer is eventually routed via an internal channel parallel to the front of the rock glacier to a spring, which contributes up to 50% of basin streamflow during summer baseflow periods and up to 100% of basin streamflow over winter.”

- Line 32: What do you mean by “often unfrozen pore water”?

Water in liquid form, we now simplify this as: *“water”*

- Line 33: Cogley et al., 2011 is a really bad reference for that, as they only say: "Rock glacier: A mass of rock fragments and finer material in a matrix of ice, showing evidence of past or present flow."

We now removed this reference from the list and add *Jones, Darren B., et al. "Rock glaciers and mountain hydrology: A review." Earth-Science Reviews 193 (2019): 66-90.*

- Line 40: I encourage the authors to cite the IPA Rock Glacier Action Group documents: https://www.unifr.ch/geo/geomorphology/en/research/ipa-action-group-rock-glacier/

We now add the suggested reference

- Line 90: why is this unpublished? The inventory is publicly available.

This inventory has been published. We have now included the web address where this inventory can be downloaded in the text (Available from: https://dga.mop.gob.cl/estudiospublicaciones/mapoteca/Paginas/Mapoteca-Digital.aspx)

- Line 95: I suggest deleting “mining” since the mining industry actually doesn't need as much water as other industries. I therefore suggest to simply combining all industries and state “industry”.

Nevertheless it is the main industry present in the region (and with the agriculture sector the main economical activity too) therefore we would like to leave the specification

- Line 148: The inclusion to Table 1 isn’t needed. Those are values cited many times in other references and as such a cross reference will just do.

We now modify the sentence as follow and delete Table 1:

“Relevant values for electrical resistivity in rock glacier environments may be found in Maurer and Hauck (2007) and Hauck and Kneisel (2008)”

- Line 164: Delete the reference to Table 1

Modified accordingly.

- Line 246: Make sure you use sediments and not rocks: “unconsolidated sediments”. This not only applies in this line, i.e., check the whole manuscript.

As commented in a previous round of revision, we would like to keep the terms rock for coherence with the original four phase model formulation afterwards used for the petrophysical joint inversion.

- Line 249: It should be “debris”, not “debrids”. This typo also appears at other places, so please check the manuscript.

Modified accordingly

- Line 269: Based on the electrode spacing I assume that error in the estimation of the active layer thickness is at least 25%. Is that correct, and if so, please add the error to the estimate.

We now add this information within the text.

- Line 341: “Dos Lenguas rock glacier”

Modified accordingly

- Line 342: “volumetric ice content”

Modified accordingly

- Line 342: Delete the % sign from the text. A unit sign (keep in mind % is a unit) only appears behind a number. If you want to use it in the text, write “percentage”. Check the whole manuscript as you can probably delete the symbol in most places anyway.

Modified accordingly along the manuscript.

- Line 373: This Section 5.5 does not provide value and should be deleted. The authors should focus on the objective of the manuscript, which is the geophysical investigation. The discussion on hydrogeology, in fact it is a discussion on hydrology only, is completely insufficient. Many key references are missing, and such a discussion would need to be expanded significantly. If this is an important topic for the authors, I encourage them to partner with a hydrologist and a Hydrogeologists and write an independent paper that is dedicated to that topic. In its current form it is inadequate and should not be accepted. It’s telling that this aspect isn’t reflected in the conclusion or outlook section. Not even the key sentence from Section 5.5 (Line 387-388).

We now deleted section 5.5 from the paper

- Line 406: replace “complicated” with “challenging”.

Modified accordingly

- Figure 1: Elevations in masl.

Modified accordingly also in figure 2

- Figure 9: Middle image should be b).

Modified accordingly