Major concern indicated by Referee #1:

1) I feel the title 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 title to something like "extended interpretation based on the application of two joint-inversion algorithms"?

Reviewers 2 and 3 commented that the manuscript was lacking a clear focus, required a better structure and according to this review it does not sufficiently investigated the comparison between the two joint inversion results. In an attempt to respond to all of these comments we decided to change the focus of the manuscript to the geophysical signature difference between active and inactive rock glacier and have updated the title accordingly. The new proposed title is now "Geophysical signature of two contrasting Andean rock glacier". In the new version of the manuscript, we focus on the individual inversion results and present the petrophysical joint inversion to aid the interpretation of the differences in the geophysical signature of the two rock glaciers and completely delete the structural joint inversion approach.

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 colour-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.

We agree with the referee that such analysis is necessary for a clear comparison of the two joint inversion schemes but we feel that for this paper such a detailed quantitative comparison would make the paper too dense and we have opted to focus on the geophysical signature of the two rock glaciers as stated before. A possible solution as suggested by reviewer 2 would be a follow up paper with a more detailed analysis of the two inversion schemes, where we could focus as well on a numerical study to better quantify the accuracy of the two inversion schemes.

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 combinations 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.

The revised version of the paper does not compare the two algorithms, so this reviewer comment is no longer relevant (see comments above).

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.

For the comparison and analysis of the geophysical signature we have modified Fig.9 with a density plot of the resistivity and velocity model inversion parameters (nowFig.8). The corresponding section in the discussion has been changed as follows:

5.4 Towards a diagnostic model representation for the ice presence in rock glaciers.
The results from the petrophysical joint inversion help quantify the volume content of air, water, ice and rock and identify El Jote as relict and El Ternero as intact rock glaciers. However, in many cases such an interpretation is limited by the lack of proper petrophysical models (or parameters). When petrophysical model coupling is not possible, the comparison of velocity and resistivity model inversion results can still deliver plenty of information about the rock-glacier's internal structure. In Fig.8 we show resistivity-velocity density plots for each rock glacier, built from the individual model inversion results of figures 4(c),(d) and 6(c),(d). The differences between the two rock glaciers are clearly noticeable, with relatively low resistivity and low velocity clusters for the relict rock glacier, while the intact one is associated with higher velocities and resistivities.

The relatively low resistivities and low velocities (Fig. 8a) are in agreement with air filled unconsolidated sediments inferred through the petrophysical joint inversion results (Figs. 5e,f). The lowest resistivities may be associated with liquid water and/or a proglacial aquifer (Fig. 5c; section 5.2).

The gradual increase in resistivity and velocity (Fig. 8b) are evidence of material consolidation such as bedrock or ice-rich layers. Given the very high resistivities (over $10^5 \Omega$ m) our interpretation is that these are ice rich layers (Table 1, resistivity values), which agrees with the petrophysical joint inversion results (Fig. 7d).

The rather different appearance of the two density plots (Fig. 8a and b) can be used as an indicator of the distinct nature of the two rock glaciers: overall, the stagnant rock glacier is characterized by lower resistivities and velocities while the intact rock glacier is indicated by higher resistivity and velocity values, reflecting the ice rich layer. The schematic plot (Fig. 8c) summarizes the findings for our two end-member rock glaciers and could be useful for identifying ice-rich landforms using seismic and electrical resistivity methods.

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.

We agree with the referee that a better study needs to be done in order to include the structural joint inversion; therefore we avoid presenting these results in the paper which are not relevant considering its new focus.

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 inversions 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.

In responding to this comment we realized that the definition of the error model in the original manuscript was incorrect and it has since been modified. The errors were computed for ERT data using the mean standard deviation of the observations which was of 1.2 % in case of El Jote and 11.4 % in case of El Ternero. These were the actual values used for the inversion schemes and we modified the text and table accordingly. For the error of seismic data we calculated an average error of 0.001 s as an estimate of the average variability in our picking of the first arrival traveltime. In the new proposed manuscript we also present a new section of the Methods: 3.3 Data processing and Inversion where. Here we clarify how the filtering happens:
"The ERT observation were automatically filtered by the acquisition software which did not take measurements when the contact resistance was too high, while for the seismic refraction traveltime, we manually picked the first arrivals after applying a gain to the seismic traces and therefore the traces were filtered according to our ability of identify the first arrival times."
Also, we eliminated Figure 5 and 8 and added a new Figure 3 with the L-curve analysis for the individual inversions. Moreover, we provided images of the datasets for both rock glaciers (first arrival traveltimes for RST and pseudosections for ERT) in new Figures 4 and 5 where we presents the individual inversion results.

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 colour 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.

We have improved the quality of the figures as suggested (increasing the font size of the labels and dividing Figure 1 in two new figures 1 and 2). Given the new scope of the paper we eliminated Figures 5 and 8.

Major concern indicated by Referee #2:

- **Target audience:** I feel that the two objectives (both of which are really interesting) present a challenge, because the latter (comparison of structurally and petrophysically-coupled joint inversion approaches) requires a lot of prior knowledge on the two inversion approaches (and regularized inversion in general) considering that TC targets a broad (and not necessarily geophysical) audience. In contrast, a reader with an expertise in geophysical (joint) inversion would probably be interested in a more detailed comparison of the two approaches, i.e., a comparison which allows to see under which circumstances one approach outperforms the other for instance. Such a comparison should also come with a discussion on the motivation of two approaches. For example: Are structurally coupled joint inversions (and the underlying assumption of structural similarity) appropriate in a permafrost context, where a transition from ice to air can result in an order of magnitude change in velocity, while electrolytic conduction stays negligibly low?

Indeed, we agree with this review and we decided to change the title and focus of the manuscript to the geophysical signature difference between stagnant and inactive rock glaciers. The new proposed title is now "Geophysical signature of an intact and stagnant Andean rock glacier". In the new manuscript, we focus on the individual inversion results and present the petrophysical joint inversion to aid the interpretation of the differences in the geophysical signature of the two rock glaciers and completely delete the structural joint inversion approach.

- **Scope and objectives:** Somewhat related to the previous point, I question if the 3 inversion approaches and their comparison are actually necessary for the conclusions drawn in this paper. In the key figure 9 for example, which the authors use for drawing several conclusions and recommend as a diagnostic tool for future studies, only 2 of the 3 inversion approaches appear and the corresponding inverted velocity and resistivity distributions and thus also the scattered points look very similar. This makes me wonder, if this case study could be presented with individual inversions only, while a follow-up study could then focus on a detailed comparison of joint inversion approaches in a permafrost context.

Accordingly to this and the previous comments, we re-structured and re-focused the manuscript as briefly presented above. Figure 9 has now been changed with a resistivity-velocity density plot to address the lack of correlation pointed out by reviewers 1 and 3). The new figure is still useful as a diagnostic plot and we have rewritten section 5.4 accordingly:

5.4 Towards a diagnostic model representation for the ice presence in rock glaciers.
The results from the petrophysical joint inversion help quantify the volume content of air, water, ice and rock and identify El Jote as relict and El Ternero as intact rock glaciers. However, in many cases such an interpretation is limited by the lack of proper petrophysical models (or parameters). When petrophysical model coupling is not possible, the comparison of velocity and resistivity model inversion results can still deliver plenty of information about the rock-glacier's internal structure. In Fig.8 we show resistivity-velocity density plots for each rock glacier, built from the individual model inversion results of figures 4(c),(d) and 6(c),(d). The differences between the two rock glaciers are clearly noticeable, with relatively low resistivity and low velocity clusters for the relict rock glacier, while the intact one is associated with higher velocities and resistivities.

The relatively low resistivities and low velocities (Fig. 8a) are in agreement with air filled unconsolidated sediments inferred through the petrophysical joint inversion results (Figs. 5e,f). The lowest resistivities may be associated with liquid water and/or a proglacial aquifer (Fig. 5c; section 5.2).

The gradual increase in resistivity and velocity (Fig. 8b) are evidence of material consolidation such as bedrock or ice-rich layers. Given the very high resistivities (over $10^5 \Omega$ m) our interpretation is that these are ice rich layers (Table 1, resistivity values), which agrees with the petrophysical joint inversion results (Fig. 7d).

The rather different appearance of the two density plots (Fig. 8a and b) can be used as an indicator of the distinct nature of the two rock glaciers: overall, the stagnant rock glacier is characterized by lower resistivities and velocities while the intact rock glacier is indicated by higher resistivity and velocity values, reflecting the ice rich layer. The schematic plot (Fig. 8c) summarizes the findings for our two end-member rock glaciers and could be useful for identifying ice-rich landforms using seismic and electrical resistivity methods.

- **Brevity:** Some figures are discussed too briefly. The first paragraph in subsection 4.1for example ends with the sentence "The model results for El Jote are given in Fig. 2(a) and (b)." (line 275). This should be directly followed by a discussion on what can be seen in Fig. 2. The reader is left alone here until the figure is briefly mentioned again in the next subsection (4.2, line 322). Furthermore, subsection4.1 ends with a single sentence on which quantity is plotted in Fig. 5. A further discussion on this figure and the shown residuals is missing.

We address the brevity issue within the manuscript adding a proper description of each figure within the text. Also, because the focus is now on the individual inversion results as principal tool for the building of a diagnostic model, we presents the collected datasets and inversion results separately for each rock glacier in new Figure 4 (for El Jote) and Figure 6 (for El Ternero) and comment on those within the text. Moreover, we avoid presenting Figures 5 and 8 simply referring to the chi2 values as a measure of the goodness of fit.

- **Structure:** The paper formally follows a standard structure, i.e. introduction, methods, results, discussion, conclusions and outlook. However, the current version of the manuscript deviates from this structure several times, which is confusing for the reader. For example section 3 "Methods" contains a lot of theory and could be renamed more appropriately to "Theory and methods". Furthermore, many details with regard to the processing (e.g., used correlation lengths in the geostatistical regularization, choice of regularization strengths using L-curve analysis, choice of starting model, etc.) appear in the results section (rather than in methods). The manuscript would benefit from a clearer differentiation between theory, methods and results.

We have followed the suggestion and changed the title of section 3 in Theory and Methods and summarize the processing and inversion parameter choices in a new subsection of it:

3.3 Data processing and Inversion

The ERT observation were automatically filtered by the acquisition software which did not take measurements when the contact resistance was too high, while for the seismic refraction traveltime, we manually picked the first arrivals after applying a gain to the seismic traces and therefore the traces were filtered according to our ability of identify the first arrival times.

The inversion algorithms we use in order to interpret the geophysical observations are part of pyGIMLI, an open-source library developed in python for geophysical inversion and modelling (Rücker et al., 2017). On each rock glacier we implement the same discretization mesh for both ERT and RST inversion routines and use a regularization weight of λ= 10 for the inversion of all the dataset, chosen according to the L-curve analysis (Hansen, 2001). A schematic plot of the L-curve analysis for each dataset collected is given in Figure 3, in all cases we present the model solution L2-norm against the residual L2-normobtained for λ=1, 5, 10, 15, 50 and 100. For both rock glaciers we use an homogeneous resistivity starting model, with a value equal to the median of the apparent resistivities (ρ^{median}_a= 4561 Ω m for El Jote and ρ^{median}_a= 36054 Ω m for El Ternero)and a gradient model for the seismic velocity, starting with 300m s^{-1}at the top of the tomogram and gradually increasing to5000m s^{-1}at the bottom of it. In each case, we refer to the error-weighted chi-square fit, where $\chi2$= 1signifies a perfect fit (Günther et al., 2006), to quantify the resulting model parameters' ability to explain the field observations.

Moreover, in order to quantify the volumetric percentage of water, ice, air and rock within each of the two rock glacier, we implement the four phase petrophysical joint inversion scheme presented by Wagner et al. (2019) and tested in Mollaret et al.(2020). For this inversion scheme we kept the same discretization meshes used for the individual inversions. The methodological details regarding this inversion algorithm and its application for this case study are given in Appendix A.

- **Missing information / lack of clarity** I had problems following the data acquisition and processing. For example: Where were the off-line shots located? I feel that an additional figure illustrating the roll-along scheme and source/receiver positions, potentially in combination with Fig. 1, would come a long way here. With regard to the processing, not much information is given. How was the data quality? How did the authors process and filter the data sets? Please provide a plot with raw and filtered seismic and electrical data (e.g., apparent resistivities and apparent velocities) and explain the filtering steps applied.

We have addressed this point adding the new subsection 3.3 Data Processing and Inversion and clarifying in section 3.2 Acquisition strategy the location of the off-line shots on El Ternero:

"The total length of 575 m was then obtained by moving the 24-channels set-up four times and adding off-line shots (Fig. 2f) to link the different acquisitions at distances of 5, 15 and 25m from the last geophone at each end of the cable."

We have modified Figure 2, where we show aerial images of the two rock glaciers, to include the location of the geophysical survey line and its topography (from DGPS measurements) with schematics to clarify the roll along schemes and geophone-inline/offline shots positions.

The filtering is addressed in section 3.3 Data processing and Inversion (above), and we show the filtered data in the new Figure 4 and 6.

Major concern indicated by Referee #3:

- The paper lacks a clear focus as the authors are trying to cram too many ideas and thoughts, some supported by good evidence and others purely speculative.

With the aim of strengthening the focus of the paper we decided to follow the suggestion of Reviewer 2 and change the title and focus of the manuscript to the geophysical signature difference between active and stagnant rock glaciers. The new proposed title is now "Geophysical signature of two contrasting Andean rock glaciers". In the new manuscript, we focus on the individual inversion results and present the petrophysical joint inversion to aid the interpretation of the differences in the geophysical signature of the two rock glaciers and completely delete the structural joint inversion approach.

- 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 joint inversion analysis as presented in this paper is not novel. A comparable structural joint inversion analysis was presented in the paper by Jordi et al., 2019; and the petrophysical joint inversion was first presented in Wagner et al., 2019 and then tested for different parameters and sites in the study by Mollaret et al., 2020. As stated before, and following the suggestion of reviewer 2, we decided to focus on the geophysical signature rather than on the joint inversion methods completely deleting the structural joint inversion from the manuscript and leaving the petrophysical joint inversion as an interpretative aid to the individual inversion 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. 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.

Thank you for this critical review of the paper with regards to the hydrology. We agree that water may originate from groundwater sources as well as from the rock glacier. We have modified the sentence starting on line 310 to clarify our uncertainty in the interpretation and remove the part of the statement implying that the water originates from the rock glacier. We agree that the discussion on periglacial hydrology is tangential to the main results and conclusions of the paper. We have therefore significantly reduced section 5.5, maintaining those portions of the text that are most directly supported by the geophysical data. We do think it is valuable to maintain this section as it synthesizes some of the main geophysical results from El Jote and El Ternero and provides context for the importance of the findings with respect to their hydrological role.

- 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%. (IS 1%) 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).

After careful review of the geophysical results we agree that El Jote should be classified as a relict rock glacier. We have modified the text to refer to this glacier as "stagnant" before the results are presented, and "relict" in the discussion section once the rock glacier has been interpreted as relict. Also, we quantified the maximum average volume content for different scenarios varying the initial porosity and porosity ranges within the petrophysical inversion results. These sensitivity results have been added to the paper and can be used to quantify the model error.

- 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 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.

As specified above, the petrophysical joint inversion presented in this paper is not novel and has been presented in Wagner et al.(2019) and thoroughly tested by Mollaret et al.(2020). It is true that the paper could be significantly improved with in-situ sampling data to validate the choice of the petrophysical parameters, but it was impossible to collect core data for this field study given the remote location and equipment available. For this reason, in the new manuscript the inversion approach has been moved to the appendix and used solely to aid in the interpretation of the individual inversion results.

- 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 cryoforms 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 rockglacier as well as providing a cross calibration point.

Thank you for this comment, we will consider collecting such soil samples in a future field campaign. During the field work for this study there logistical constraints impeded the complete geotechnical characterization of the material at the rock glacier front. Regarding the geophysical line we add the following lines to the manuscript:

"While the geophysical line extended slightly past the edge of the El Jote rock glacier, it was impossible to do so for El Ternero due to the high, steep, unstable and therefore dangerous slopes of the rock glacier front and lateral margins."

- 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.

In the new version of the manuscript we added a new section in the discussion where we address the data quality:

5.1 Data quality and comparison of inversion routines

For both field sites the acquisition of data and their quality were limited by the environment: the presence of large boulders with air-filled voids between them at the surface of both glaciers attenuated both mechanical and electrical energy propagation. The quality of the data was especially affected in the case of El Ternero rock glacier, clearly demonstrated in Figures 4(a)-(b) and 6(a)-(b). It must be stressed that the 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 the ray-coverage within the displayed area is limited….

- 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..

We are really thankful to the reviewer for their comments and have tried to correct and address them within the new manuscript.