Reply to Anonymous Referee2

The study entitled 'Geophysical measurements of perennial snow patches in Pirin Mountain, Bulgaria, by Kisyov, A., Tzankov, C and Georgieva, G. presents interesting results in a region still poorly investigated so far. The paper brings valuable knowledge from a small glacier in the Pirin Mountains (Bulgaria), but should be highly improved to be published in this journal. The authors should address several important problems before the paper can be accepted. The paper is clearly structured and well-illustrated, but requires some supplementary explanations regarding the study site and the methodological approach. In addition, interpretations should be improved, new figures inserted and confusion regarding the inappropriate usage of some concepts should disappear. The English needs some smoothing in places.

After careful consideration, I recommend that the paper be published only after the authors address the issues listed below.

General comments

In several previous papers, the ice bodies assessed in this paper in the Pirin Mountains are called 'glaciarets' (Gachev et al., 2016) or 'microglaciers' (Grunewald et al., 2006) (even you mention in the paper Snezhnika as being a microglacier!). In addition, they are considered the southernmost glaciers in Europe by Hughes (2008), Grunewald and Scheithauer (2010) and other authors. Gachev (2017) mention typical glacial processes associated with these glaciarets, such as: striations and initiation of small moraines, suggesting that these ice bodies display motion and play a role in the present-day morphodynamics in the proglacial area. Moreover, the drillings performed by Grunewald in 2006 in Snezhnika revealed the presence of ice (Grunewald and Scheithauer, 2010). These glaciarets are probably several hundred years old (at least from the XIXth century). In this context, I think the authors should consider changing 'perennial snow patches' with 'glaciarets'.

Thank you for the suggestion. We started the projects and later the paper with the idea to prove that the observed snow patches in high mountains of Bulgaria at the end of the summer are perennial. In the present version of the manuscript only the results for Snezhnika microglacier are presented. We will consider your suggestion and will replace "perennial snow patches" with "glaciarret" or "microglacier". We will add also some more explanation what is meant as "glacierret" because the word is not widely used in the literature.

According to the title and objectives, the approach deals with geophysical measurements of perennial snow patches. However, the article would have a broader impact if the achieved results are used to gain knowledge regarding e.g., the evolution of these small glaciers, present-day changes/ behaviour of glaciaret, glacial-periglacial processes at this site, hydrological significance etc. In the present form, the article focused on identifying several different layers on GPR radargrams/ERT profiles, but the considerations regarding these layers' geomorphological/ hydrological importance are lacking almost completely. Therefore, I suggest going further with the analysis and interpretations than only identifying the bedrock depth, the permafrost, etc., but trying to explain the relevance of these findings for the mountain cryosphere. Otherwise, I am afraid that the paper seems to make an impact only locally.

Thank you for the comment and suggestions. The manuscript was first submitted with a second part describing the measurements of another perennial snow patch in Banski Suhodol valley (a neighboring valley of Golyam Kazan cirque). The title has remained unchanged after we removed this part. We are going to think about a correction of the title. An extension of discussion part is also intended in which we will add more interpretation and analysis as suggested.

I have some concerns regarding the design of the approach.

First, I didn't understand why the authors performed geophysical measurements on different alignments in different years? In the beginning, I thought that the authors would like to compare the results and quantify the changes, but it seems that was not the case. Because the profiles were not conducted on precisely the same lines, quantifications are not possible.
Second, the distribution of the profiles is not adequate. Most of the profiles were performed in the downslope part of the glacier, where the glaciaret is thin. It would have been good to have at least 1-2 transversal profiles in the upper part of the glaciaret (in this case, you could have calculated the glacier volume and then the water equivalent etc.).
Third, you used a simple handheld GPS, which can have low accuracy in this type of environment. Therefore, the profiles' exact position might be different from what appears on your map.

1. All measurements were made within low budget projects for students with the main idea to demonstrate the capabilities of the available equipment for studying snow and ice patches as there are no big glaciers in Bulgaria. The interesting results and the very few of such studies were motivation for us to prepare a publication. It was not possible to make the measurements in the

exact same date every year due to many reasons (the weather in the mountain for example) but also we don't think that it is so important in this case. The time of measurements was selected to be at the end of the summer and before the first snow in the mountain or the time of the expected lowest size of perennial snow patches. This is actually relative time, because in on year the first snow falls in September and in the other, the weather in October is still warm. Even the profiles are not conducted on the same lines an assesment of change within the years can be made.

2. The distribution of the profiles was meant to be more dense but the steep slope of the microglacier didn't allow us to make more profiles in the upper part in 2018. There is clarification about this in the text. We made more GPR profile later in 2020 in order to supplement the data about the thickness of Snezhnika and underlying structure.

3. Thank you for this comment. The accuracy of the handhels GPS is 2-3 m and this is less than the obtained subsurface structures. You are right that the quantification are not possible although some changes can be estimated. Actually the accuracy of the profiles positions was improved with the LUA images used later to produce DEM. This is not explained deeply in the text while DEM is not used for the calculations. We decided that including this information will encumber the text. Although we will add some more information in the manuscript. We were not able to use geodetic GPS in the previous studies but we will consider using it in the future as we are going to continue the monitoring of this site and also to conduct measurements in new places in the mountain.

You used ERT to investigate the lower part of the glaciaret, but as far as I know, ERT is problematic when the electrodes are fixed in the snow. Please refer to similar studies using ERT on snow patches/small glaciers and highlight the capabilities/limitations of ERT on snow/ice surface. In addition, I didn‘t understand why setting the electrodes distance at only 1.5 m? Because the distance between electrodes was small, the penetration was not enough to estimate ‘frozen areas‘ thickness in some profiles. Generally, the measurement protocol in this environment uses a 5 m electrode distance.

Yes, the ERT is problematic when it is used fully on snow. There are laboratory experiments demonstrating the use of special electrodes in ice but it was not working in real measurements in the mountain. In our case all electrodes were in the gravel and only one electrode from one profile was in the ice. This is mentioned in the text.

The distance between electrodes was selected as the maximum possible length due to the terrain divided by 24 (the number of electrodes). Using a smaller distance between the electrodes we have better resolution. If we made measurements with lower resolution, we would miss the small anomalies like the watered layer near the surface. There is also clarification in the text about the distance between the electrodes.

It would also be helpful to mention the precise date of geophysical measurements each year.

Thank you for this suggestion, we will add this information in the text.

The radargrams have no topography, and because of this is difficult to interpret the reflections.

We have added topography according the similar comment of the Referee 1.

A recent study (Persoiu et al., 2021) showed that significant changes might occur at Snezhnika between different hydrological years (e.g., 2018 vs 2019). Therefore, please consider the interannual changes of this glaciaret when interpreting the results of profiles performed in different years. From the pictures, it seems that in 2018 was much more snow than in the following years. Do your GPR profiles tell you anything about ice-thickness changes between 2018 and 2020? Because according to Persoiu et al., significant variations in the surface may occur at this site.

The periodic changes in size of Snezhnika microglacier are observed since 1994 (Gachev et al.,2016). Between 2018 and 2020 we also observed the decrease of its size. The GPR profiles from 2018 show well the thickness of the "new" snow. The changes in size of the microglacier and how this affects the results from geophysical measurements are described in the text.

Because this site is unknown to most readers, you should give more details about this site. First, please include a map with the localization of the study area. Then, please add a short description of the evolution of glaciers in the Pleistocene and Holocene in this area supported by the morphology of this valley (e.g., moraines). Because karstic rocks occur here, please also refer to the presence of karstic features in this cirque. You did measurements in the proglacial area of the glacier, but you didn‘t describe it: type of surface, vegetation, clasts dimensions, presence of soil, water etc. It is also essential to describe the climate in the Pirin Mts.

Thank you for this suggestion. We are going to prepare a more representative figure of the study site. We will include also information for evolution of glaciers in Pirin. According to the description of the site and the climate we can add a few sentences but mainly we think that the information given in 2.1 in the text is enough for this manuscript.

Please refer to other similar studies regarding the interpretation of the GPR measurements. For example, you interpreted a pattern of reflections in the substrate as a' frozen zone'. Based on what characteristics of the reflections do you make this interpretation? Are there similar findings in other studies? The same observation for the second layer where the 'voids are filed with ice and water'. How do you be so sure that the voids between blocks are filled with ice and water? If the voids are filled with ice in this layer, then this is also permafrost. Then, what is the difference between layer 2 and 3? You should interpret all the other reflections by comparing them with similar findings elsewhere. Have you noticed any hyperbola in the radargams? You can use it to calculate velocities and see whether you have ice/permafrost/rocks or a mixture (most probably). What about internal coarse layers embedded in the ice? Grunewald and Scheithauer (2010) found such layers in the drillings done in 2006. Please also discuss the transition between snow/firn/ice.

Interpretation of geophysical data (from one method) only by pattern is mainly ambiguous. In most papers there is previous information for the structure form boreholes or other methods. Due to the lack of previous information especially for the underlying structure of Shnezhnika we have compared the results from GPR and ERT measurements. The zone estimated as "frozen zone" on GPR profiles has very high resistivity on ERT. According to the small reflections within this zone on GPR profile it should be an ice rich zone. There are very few hyperbolas with well-preserved shapes. The estimated velocities range from 0.12 to 0.16. We used an average values for the different layers we distinguished. We outlined some internal layers within the ice on the radargrams from 2020. Thank you for the comment, we should extend discussion, adding more information and interpretation.

One of the most interesting findings in the geophysical profiles is the so-called 'frozen zone'. Unfortunately, the interpretation based on the presented results is partly vague. For example, it is not clear if there is a lens of massive ice in the substrate or a mixture of ice and rocks (ice-cemented materials). The term 'frozen zone' is problematic and I suggest replacing it with ground ice/permafrost. First, you should clarify if you have periglacial or glacial ice in the substrate and discuss the origin of the ground ice/permafrost. Then you should describe the mechanisms involved in forming the permafrost at this site below the glacier and the non-frozen bedrock and whether it is ice-rich permafrost or massive ice is missing. Finally, try to explain processes that control permafrost occurrence at this site below an unfrozen/frozen ?? (this is not clear) bedrock and what happens with water below the glacier. Since this is a region with karstic rocks, please also refer to the hydrogeology in the Discussions and the presence/absence of caves/dolines etc. in the region.

Thank you for this comment, it is very useful for us and we are going to extend the discussion part including the suggested information. Most probably, there is mixture of ice and rock blocks.

Specific comments

Abstract

Line 7: "in order to evaluate changes in the snow patches size and thickness"... replace with "in order to assess glaciaret thickness and its internal structure". You haven't cuantified changes of size/thickness.

Actually we made each year also measurements of the size of the microglacier but this information is not included in the text. We will consider your suggestion for correction.

Line 8: Maximum thickness of ice can be higher than 8 m in the upper part of the glaciaret where there are no transversal profiles. Please add that maximum thickness of 8 m or even higher occur in the upper part of the glaciaret.

We are going to mention that the thickness can be higher than estimated during our measurements.

Line 10: replace "frozen zone" with permafrost/ice-cemmented sediments.

Replaced

Line 11: the presence of permafrost in the Pirin was also indicated by Onaca et al., (2020, 2022).

Onaca et al. 2020 is cited in the Introduction part. Onaca et al. 2022 was submitted on 29 December 2021 when our manuscript was still available for discussions. It was not possible to refer a future work.

Introduction

Please write a paragraph on the importance of knowing the ice thickness, internal structure for glaciology/hydrology/geomorph

Thank you for this suggestion, we will add this information in the text.

Line 18: add a citation after 'global changes than glacier'.

Reference is added.

Line 19-20: What do you want to say with "permafrost is the last stage of glacial life cycle"? This doesn't seem right. The occurrence of permafrost is not necessarily conditioned by the presence of a glacier. For example, in never-glaciated regions in Canada permafrost exists for several hundred of thousand years. In mid-latitude mountains, in regions without glaciers in the last 10 ka, permafrost still exists due to favourable topo-climatic conditions. In the Pirin Mts., permafrost probably also occurred at sites free of ice in the last 10 ka.

Thank you for this comment. We will rewrite this sentence to be more clear.

Line 21: This is wrong! During LIA the only glaciarets in Bulgaria were very small (see Gachev, 2000, Holocene glaciation in the mountains of Bulgaria, Mediterranean Geoscience Review, 2, 103-117). Large glaciers occurred in Bulgaria only in the Pleistocene. Please refer to this (see Kuhlemann et al., 2013, QI).

Thank you for the comment. We will correct the sentence.

Line 32: please see this recent study (Onaca et al., 2022) in which geophysical measurements on Snezhnika are presented.

Thank you for the reference. It is released after the our manuscript was available for discussion.

Line 36: it is not clear if you are talking about permafrost or air temperature?? Please also add a citation here.

Thank you for the comment, we will correct the sentence.

Line 36: not only "mountain slopes with permafrost are significantly vulnerable to climate change"; flat permafrost terrain is also vulnerable (see, Biskaborn et al., 2019).

Yes, you are right, but in the Introduction we focus on mountain areas as the study is carried out in the mountain.

Line 41: you are right that snow acts as a shield for radiation, but on the other hand it also may hamper the aggradation of permafrost.

Thank you for the comment, we will add it in the text.

Lines 74-75: "The polar ice. . . ." - this is irrelevant here.

Thank you for this comment, we will remove this part of the sentence.

Line 84: "ERT can successfully be applied for studying glacial structures" - What types of structures? Please be explicit and add citations.

We will refer to Kneisel et al. 2008 and wi will correct the sentence.

Line 92: You didn't present any results from Banski Suhodol Valley and since is not the subject of this paper you should avoid referring to this site when presenting the aim of the paper.

A correction of the text is made.

Methods

Line 98: You didn't present any DEM in the paper. Please delete this sentence.

We decided to leave this and to add some information for the DEM and the work with LUA on site.

Line 99: 2.1. Study site description – please give more details on this site. A localization map + a detailed map of the topography of this cirque is also necessary. Please indicate on this map: Dzhamdziev ridge and all the other peaks.

Thank you for this suggestion.

Line 103: replace "snow patch" with "glaciaret".

Done

Line 112: "They were formed during the final phase. . . ". It is not clear who?

The sentence is rewritten.

Line 131: What about mean velocities of snow? And permafrost? You mention that this glaciaret is a snow patch, but using the velocities for ice. In other studies ice is 0.16 m/ns. How can affect the thickness estimation of the glaciaret?

The surface of Snezhnika in 2018 was presented with a wet snow layer, which was also compacted and semi-frozen, up to 2 m thick mainly at the lower part of the glacier, so we used the same average velocity 0.15m/ns as for the ice below. The change from 0.15 m/ns to 0.16 m/ns (+0.01 m/ns) will add 6.7 cm to every meter from the glacier section. This means that about 0.54 will by added to the estimated depth of the glacier in our deepest investigated part.

Line 134: I have serious doubts regarding such a low error of the GPS in this shaded cirque. What about the vertical error? Topography is extremely important for the interpretation of geophysics profiles. When doing geophysics in such a rough terrain the protocol says that differential GPS is mandatory!

The horizontal accuracy of the handhels GPS is 2-3 m and this is less than the obtained subsurface structures. The vertical error is double the horizontal. We were not able to use geodetic GPS in the previous studies but we will consider using it in the future. We are going to continue the monitoring of this site and also to conduct measurements in new places in the mountain.

Lines 135-140: it is not clear if there are GPR profiles repeated exactly on the same line in 2020 compared with 2018. From fig 3 it seems that GPR in 2020 is different than those performed in 2018. It means that you can not actually compare the radargrams, by means of changes.

We don't have such profiles. The effort was to cover more are of Snezhnika with GPR profiles in order to estimate better the thickness of the microglacier and its internal and underlying structure.

Line 140: why didn't use topography when creating the radargrams? Topography is extremely important for interpretation. Without topography how can you interpret if reflections are parallel with the surface etc?

The topography is added to the radargrams according to comments from Referee1.

Line 142: It would have been good to try at least 1 or 2 GPR profiles in the upper part of the glaciaret in 2018 (when the glacieret size was the greatest in the last years) and where the thickness is probably greater.

It was planed so but the slope was very steep and the antenna moved up and down along the slope, the people making the measurements walked faster or slower in different part of the profile which produced side effects on the radargrams. It is mentioned in the text but the problems during the measurements are not described in detail.

Figure 3: Give more details about the picture in the background (when it was taken?). If possible, would be good to overlap the contour of glaciaret (or at least of the front) in 2018, 2019,2020 to see if it was ice in 2019 and 2020 where you did some profiles. Please replace Glacier Snezhnika with glaciaret Snezhnika on the picture. In the caption replace the Golyam Kazan area with Snezhnika glaciaret.

We have new picture according to the comments of Referee 1.

Line 155: Why setting the distance between electrodes at 1,5 m? Following the protocols in permafrost environments a distance of 5 m between electrodes allows you to measure 120 m profile length and probably around 20 m penetration depth. The moraine looks a bit challenging, but it would have been so interesting to make at least a profile on it, to see its internal structure!

For the distance between the electrodes please see the answer above. The inner slopes of the moraine are very steep and this will cause false anomalies due to deformation of the profile geometry. Additionally the inner slope of the moraine is very unstable.

Line 159: It is not clear if some profiles/parts of the profiles cross the glacieret. It seems that ERT 3 and 2 cross the glacieret and in this case, you should interpret the ERT values with extreme caution, since ERT in the snow is extremely tricky. Write a phrase about the contact between the electrodes and the ground?

Thank you for this comment, we should explain it better in the text. Only one electrode from the last profile (the third) was in the ice. This was possible due to the shape of the microglacier and boarder between the ice and the gravel.

Line 162: "real geoelectrical section" - what do you mean (inversion from apparent to true resistivity?)

Yest it is inversion.

Results and Discussion

Line 170: "which are horizontal relative to the slope"... how do you know, since your profile has no topography?

This is corrected according to the comments of Referee 1.

Line 170: replace "snowfield" with "glaciaret".

Done

Line 171: "The uppermost layer represents the microglacier". What do you mean? The ice?

Yes, mainly ice. The first layer is the microglacier and other layers represent the structure beneath the microglacier.

Less 173: you identified some discontinuities in the ice. Vey nice... can you say something about these?

Thank you for this comment. This is mainly the discontinuity between the snow and the ice but also some discontinuities in the ice are notified. We will add a few sentences in the text to explain this.

Lines 175-175: is not very clear here. Please rephrase.

Thank you for the comment, we will correct this.

Line 177: "The second layer lies under the ice"... you are not referring to GPR2018-1, GPR2018-2 and GPR2018-3, right?

We are referring namely to these profiles. We will add also "snow" in the text.

Line 178. You say that the voids are filled with water and ice in layer 2, but is not clear based on what you affirm this? Please, give references to similar findings. If this layer is draining the melted glacial water, why are some voids filled with icer? And how do you explain the presence of water in the so-called "meltwater zones", which are between the glacier front and the LIA moraine? Here is a possible scenario, but it might be wrong: the melting water may infiltrate layer 2, but because layer 3 is permafrost (impermeable), it follows the permafrost table downslope and accumulates in the proglacial area where ERT reveals a high concentration of water. Maybe if you agree with this scenario, you can make a simple model in which to represent the primary circuit of glacial melting water and the role of permafrost for drainage.

We estimated the presence of meltwater zone from very law values of resistivity. We observed also on site near the microglacier water which disappeared very close to it. We think that the ERT profiles are not enough to make adequate model of the drainage system in the area but we will consider it during the future measurement.

Line 186: This is very important. Can you comment on the large differences in the velocity between layers 2 and 3? Ice lenses mean massive ice (pure ice)? It is hard to believe... only if a thick mantle of debris-covered old glacial ice. I think here might be rather an ice-rich permafrost (usually around $105\Omega m$).

The velocity of both layers is equalized later when we reproduced the radargrams according some comments of Referee1. Because of the lack of strong reflections we interpret this layer as very rich on ice permafrost. It is possible ice-rich glaciofluvial sediment. We are going to extend the discussion part with this.

Line 194: replace "snowfield" with "glaciaret".

Done

Lines 199-200: It is not clear which figure is the frontal moraine? If it's 5a it means that the frontal moraine is well below the ice because GPR1 ends somewhere in the middle of the glacieret? Please clarify!

Both profiles along the slope from 2020 end almost at lower border of Snezhnika. The size of the microglacier in 2018-2020 can be seen on fig.1.

Line 210: according to Onaca et al., 2022 the maximum dept was 12 m. You should also refer to this finding.

We will comment the estimated thickness from Onaca et al. 2022 in the Discussion.

Line 222: apparent resistivity? Why not true resistivity?

Thank you for the comment. We have remove the "apparent" from the text.

Line 231-233: please be more precise: ground ice/permafrost (avoid 'frozen zone').

Thank you for the comment, we will clarify the text according your suggestion.

Line 238: Interesting finding! Can you explain why the active layer has thickened so much in only 1 year?

In the first year of the study (2018) a snow layer was covering the most of the glacier bed area. In the summer there have had more melting (measurements were made in the end of August). While in the last year (2020) the snow layer was not presented, the size of the microglacier was smaller and also the measurements were made in the beginning of October. There was less melting.

Line 256: How can you explain the occurrence of "frozen zones" only in the lowest part of the glaciaret?

Thank you for the comment, we will consider explaining the reasons for it when we extend the discussion part. Several reasons can be given like for example the available data and penetration depth of the geophysical equipment. In the lower part of the microglacier (microglacier's bed) we can compare the results from GPR and ERT and based on both methods to estimate the presence of frozen zone (or ice rich permafrost). In the upper part we can compare only GPR but the profiles from 2020 are shallower.

Lines 274-275: Not clear. Rephrase this.

Thank you for the comment we will rephrase this part of the text.

Please write a paragraph on the methodological uncertainties (mainly the limitations of GPR and ERT) and where the interpretations should be treated with cautions.

Thank you for the suggestion, we will add this information.

Conclusions

Line 278: replace "snow" with "ice".

Done.

Line 280: "... the ice body it reaches 8 m", but the maximum thickness may exceed this value in the upper part of the glacieret.

8 m is the thickness according to our study. We explain in the text that we don't cover the whole area.

Line 286 (and within all the manuscript): you are using rock blocks in many cases, but please refer to a classification of clasts based on the size of individuals (e.g.,pebbles, cobbles, boulders etc.).

Thank you for this suggestion.

Line 287: Please check again if ice occurs in layer 2.

Zone 2 is the melted layer. The areas with bigger resistance are probably rocks and not permafrost

Line 299: Indeed, shading is essential, but is not acting alone. You should consider the other controlling factors of permafrost in the Discussions.

Thank you for the suggestion, we will consider it in extension of the discussion part.

Line 300: I suggest to delete the last phrase. It is not a conclusion of this study.

We will delete this sentence.

References:

Biskaborn et al., 2019. https://doi.org/10.1038/s41467-018-08240-4

Gachev et al., 2020. DOI: 10.1007/s42990-020-00028-3

Kuhlemann et al., 2013. 10.1016/j.quaint.2012.06.027

Onaca et al., 2022. https://doi.org/10.1016/j.catena.2022.106143

Persoiu et al., 2021. https://doi.org/10.5194/tc-15-2383-2021 Citation: https://doi.org/10.5194/tc-2021-337-RC2