We would like to thank all reviewers for seeing the potential and relevance of our study and for supporting us with their objective, helpful and constructive comments. These have certainly helped to improve our manuscript.

We have made the following major changes to the analysis and the manuscript:

- Adoption of a classical digital elevation model of difference (DoD) analysis instead of the previously used "3D displacement" analysis to calculate the volume and surface changes
- Application of the LoD to calculate mean flow velocities
- profound uncertainty analysis
- Comprehensive restructuring, reformulation and shortening of the results and discussion section
- Separation of the methods section into materials and methods
- General improvements, rewording and shortening of the manuscript
- Comprehensive revision of the illustrations
- Preparation of supplementary materials

In the following, we respond point-by-point to the comments of the individual reviewers. Please find our answers in blue in the text.

Referee 1

General comments

This is a very interesting study investigating the long-term evolution of a relatively large sample of individual rock glaciers. Despite the increasing number of studies on rock glacier dynamics and evolution, there is still a lack of knowledge on the past velocities of rock glacier. This study aims at filling this gap and gives very interesting results. The analyses are thorough, very detailed and original. The errors are systematically considered and their analyses are carried out in depth.

I have however a major concern regarding the length and the structure of the manuscript. First, the text is very long and it should be reduced by about 20%. Second, and most important, the manuscript is not well structured. The results and discussion are merged into a single very long chapter, which does not allow the reader to have a clear view on the most important results of the study. The results must definitely be separated from the discussion, which is the classical way for a research paper. The references to the literature must be systematically moved to the Discussion chapter, allowing the keep the Result section more strictly factual (typical examples P16L424-428). There is also a countless number of subtitles. As a consequence of all of this, we get progressively lost. In the end we lose the main information, which is a pity because the quality of the analyses is very good and the results very interesting. Therefore, a strong effort must be made to improve the structure of the manuscript and to make it clearer.

Examples of modifications to the structure to be made :

- Move chapter 4.1 after 4.2
- Chap 4.4 (and 4.3.3., which should be merged with 4.4) should be moved in the Discussion and condensed.

The difficulty for such a study relies on its intrinsic interest : whereas similar studies generally consider one or two landforms, here a large amount of data is available for 9 rock glaciers. Thus, the authors must find a way between presenting sufficient data without losing the reader in two many details. A way to do it would be to focus more on the general trends and to reduce a bit the analyses of the exceptions and of the special cases.

We agree with the comments on the length and structure of the manuscript. As we have generated a lot of data on the different rock glaciers, it was very difficult to highlight the really important results and discussion points. However, your comments have led to more clarity about what the important statements and results are. We will more clearly provide the important points and elaborate them better. In addition to the proposed changes to the introduction, we separated, restructured and condensed the results and discussion sections for better structure and clarity. In the results section, we present the strictly factual results of the rock glacier inventory, the changes in the meterological forcing, the flow velocity analysis and the surface elevation change analysis, particularly the last two points in a more general way. In the discussion section, we discuss the flow velocities and volumetric changes in the regional context and work out the similarities and differences in the reaction. As a further point, we relate the flow velocities and volumetric changes to the changes in the forcing parameters (temperature, snow cover and precipitation) and discuss them more systematically on the basis of the existing literature.

The introduction is a bit lengthy and not well structured. Up to line 55 it's a long summary on the general characteristics of a rock glacier. Not everything is useful, thus I suggest to shorten this part and to keep only what is necessary. Another issue is that we must wait the end of the introduction to know the goal of the study. Ok, the precise objective must be presented after the state of the art, but the general objective, or at least the topic of the paper must be stated much earlier. Thus, I recommend to reorganize the introduction and to better structure it (see specific comments).

We revised and shortened the passage on general rock glacier characteristics. In addition, we state the aim of the study in a sentence at the beginning of the introduction.

The state of the art is generally good, but additional references on the current state/velocities of rock glaciers, including destabilizing ones, could be added. For example Kummert et al. 2018, Vivero & Lambiel 2019, Marcer et al. 2021,…

We have added a reference to Vivero & Lambiel 2019 and Marcer et al. 2021 to the section on rock glacier destabilisation.

P2L58-58

In such a study it would really help to have a Google Earth link to visualize the rock glaciers, or/and pictures of each rock glacier.

We agree. In complement to figure 1, which showes two pictures of the studied rock glaciers, we included a .kmz file with the locations of the rock glaciers in the suppementary part of the paper.

The results of the rock glacier inventory are presented in the Study area section, whereas the method for achieving it is presented after, in the Material and Methods section. This is not coherent. Since this rock glacier inventory is part of this study, the results must be moved in the corresponding section and removed from the Study area section.

We agree. We mention the previous studies on rock glaciers in the Kaunertal in the study area section and present the results of the inventory in the results section.

The calculated 3D displacements are changes normal to the surface. As explained by the authors, they are an alternative to the traditional DoD, and even a better quantification of the thinning/thickening processes on an ice-sursaturated permafrost body (see Vivero & Lambiel 2019 for a similar study). But this is not 3D displacement. The latter is rather a displacement that considers the 3 components x, y and z. As such this defines the displacement parallel to the slope angle, and thus the real displacement, contrary to the horizontal 2D displacement. The titles and text related to this must then be reformulated.

We agree that 3D displacement is a misleading term. As Referee 2 has pointed out, our methodology for calculating volumes by gridded '3D displacements' is invalid as this leads to small but systematic errors in

volumes. Therefore, for the calculation of the volumes we appied the classical 2.5 D method via DoDs and determined the uncertainties according to Anderson (2019). Since a test has shown that there are hardly any differences in the representation of surface changes between the previous approach and the DoD method, we present these as DoDs as well in order to avoid confusion.

Although there are some studies that directly compare point clouds, as mentioned in your comment, we have come to the conclusion that using DoDs throughout the paper increases comparability to other studies and enhances comprehensibility.

If I understand well the chart on Snow cover onset, snow arrived roughly early September around the years 2010. This means that what you consider as the snow cover onset in fact corresponds to the first snow, meaning that snow can then melt completely until new snow falls. Hence, this parameter cannot have any influence on the rock glacier kinematics. Much more important is the date when a substantial snow cover is established (~50 cm), allowing ground insolation. In addition, I suggest to add as a parameter the date of complete snow melt in spring. This has a strong influence on the MAGST and thus on rock glacier kinematics. See PERMOS 2019. Permafrost in Switzerland 2014/2015 to 2017/2018. Noetzli, J., Pellet, C. and Staub, B. (eds.), Glaciological Report Permafrost No. 16–19 of the Cryospheric Commission of the Swiss Academy of Sciences, 104 pp.

For the analysis of snow cover onset and duration, we followed the criteria described in Peng et al., 2013. They describe the snow cover onset as the first day of the first five consecutive days with snow in fall (considered September to January) and the end of the snow cover as the last day of the last 5 consecutive days with snow in the melt season (February to July). The duration is calculated by counting the number of days between the snow onset and snow end.

We agree that this is probably not the decisive factor for the morphodynamic development of rock glaciers with regard to snow. We made a further analysis, for the onset of a substantial snow cover (50cm) and the date of complete snow melt (P11L277-280). We present (Fig. 5 and Fig.12.), describe (P15L382-383) and discuss (P29L635-649) the results.

The interpretations of the velocities and surface changes regarding the external parameters are sometimes rather hypothetical and should more systematically rely on existing literature. This would be much easily achieved by moving these interpretations in the Discussion chapter.

We agree with this.

We have moved the interpretation of the velocity and surface height changes to the discussion. They have also been rewritten, shortened and more literature has been added.

P26-29L573-654

Specific comments

P1L12. Two times “change” in the same sentence.

We will replaced change with shift.

P1L20. In the rest of the manuscript you don’t talk about vertical 3D, but only 3D. Be consistent. But take also in consideration my comment above about 3D.

For a detailed answer, see comment above. We changed the methodology completely to a DoD analysis. The terms vertical 3D and 3D displacement have been replaced with the expression surface elevation change.

P2L31. **are** responsible

We corrected this.

P2L32. **generally** coarse debris layer (the coarseness depends on the lithology).

We added generally.

P2L33. landforms

We corrected this.

P2L38. Remove “also”. If the origin is periglacial, then the ice forms by freezing of water.

We removed also.

P3L77-80. Here you present the results of a specific study on velocity variations for selected rock glaciers. But it must be moved around L60, where you talk about rock glacier velocities. In addition, it appears weird to give details for a specific region only for one study. Thus, either you stay more general, or you keep these details but, in the meantime, you must give similar details for the other referenced studies.

We have decided to describe the details of the study by Groh & Blöthe (2019) in more detail, as they cover the same research area but with a different temporal and thematic focus.

We moved the more detailed description of the study to chapter 2, study area, as it fits better here.

P4L101-104

P3L85. **of** rock glaciers

We corrected this.

Figure 1: add the location of the study area in Austria; add the location of the highest summit.

We added the national borders to the overview map for better orientation and included the location of the highest peak.

P4L103. Why “pseudo” ? It sounds weird.

The authors of the permafrost map (https://doi.org/10.1594/PANGAEA.917719) refer to it as a pseudo probability of permafrost being present but do not specify the term. In the corresponding publication (Schrott et al. 2012), pseudo probability does not appear either. However, as this is not relevant for our study and the permafrost map is only shown for illustration purposes, therefore, we removed peudo.

P4L106-108. Obviously the road was built for the ski activities. You could make it clearer and say a bit more on the anthropogenic influence.

We have not described anthropogenic influences in detail, as these have little influence on the development of rock glaciers. An exception is RG03, which is intersected by the road. We have clarified this in the chapter study area.

P4L93-96

P6L127. To avoid repetition replace the second “Berger et al. (2004) by “The latter”.

We replaced this.

P6L150-154. This refers to the state of knowledge on factors controlling rock glacier kinematics. Therefore, it should be moved into the introduction.

As this information is already included in the introduction, we deleted it from the methods and material section.

Table 1: Mi**n**istry

We corrcted this.

P7L170. This is an open reproach towards the company that can be critical. I suggest to moderate your sentence.

This was not intended as a reproach to the company, but we understand that it can be understood as such and changed the sentence.

Table 2: Uniformize the font

We corrected this.

P8L200-201. How many GCPs did you use ?

We used 101 GCPs, which we picked very carefully directly from the point cloud in stable areas and as evenly as possible over the entire study area.

We specified the numer of GCPs (P8L190)

P10L240. ha**d**. In general, check the tenses. Sometimes the present is used, sometimes the past (L245: better were than are).

We will checked and corrected tenses used throughout the text.

P11L274. Figure number ?

It should read figure 4. We will add the figure number.

P12, chap. 3.6. See my general comment on the 3D displacements.

See your answer in the general comments.

P12L300. **a LoD**

We corrected this.

P13L320-322. Syntax problem with this sentence.

We rephrased the sentence.

P13L320-322

P15L366-367. The end of the sentence is strange.

We wanted to express the reduced positive temperature trend of winter and autumn temperatures compared to summer and spring temperatures. This sentence was removed during the reformulation.

P15L367. El**e**vation.

We corrcted this.

P15L375. You could complete with additional references.

In an earlier version of the manuscript, we had included additional references at this point, such as Gruber et al. (2004). Unfortunately, The Cryosphere limits the number of references for research papers to 80, so we had to remove some references.

P15L389-390. I don't understand this sentence. You mean that P increased from 931 mm/yr to 957 mm/yr at Weißsee ? Please reformulate. And in the following lines it is not clear of which station you are talking about. And why not showing the data for Weißsee station ?

We wanted to express that in the period under investigation (1953-2017) the mean annual precipitation of the station Plangeroß was 931 mm/yr and in the case of the station Weißsee in the period since the recording (2007-2017) the mean annual precipitation was 957 mm/yr. In the following passage we describe the data of the station Plangeroß.

We completely rewrote this passage.

We plotted the data of the Weißsee station in Fig. 5.

P16L408. Honestly the tiny decrease in the snow duration cannot be considered as a trend. it only takes one year with a positive anomaly for the trend to reverse. And how do you calculate the snow cover onset ? From which snow depth do you consider that the snow cover is permanent ?

We agree that this cannot be seen as a trend. For the analysis of snow cover onset and duration, we followed the criteria described in Peng et al., 2013. They describe the snow cover onset as the first day of the first five consecutive days with snow in fall (considered September to January) and the end of the snow cover as the last day of the last 5 consecutive days with snow in the melt season (February to July). The duration is calculated by counting the number of days between the snow onset and snow end. We agree that this is probably not the decisive factor for the morphodynamic development of rock glaciers with regard to snow.

We made a further analysis, as suggested in the general comments, for the onset of a substantial snow cover (>50cm) and the date of complete snow melt and integrat this into the study.

Furthermore, we have rephrased it so that it is not dicribed as a significant trend. (P15L382-383)

P16L422. How much were the velocities for this period ? According to Fig. 8 they should not have been much higher than 0,5 m/yr. Such displacements should not have provoked decorrelation.

In the period 1953 - 1971, the maximum flow velocity for RG 8 occurred in the area of the front and could only be determined by manual mapping of a few individual blocks. The maximum flow rate was 1.66 m/yr, which corresponds to a total movement of 29.85 m during this period. Therefore, the rate of movement could no longer be determined by image correlation. One should not confuse the maximum flow velocity with the average flow velocity. The maximum flow velocity for RG 08 is still over 1 m/yr in the following epoch (this is somewhat difficult to see in Figure 4, as the maximum flow velocities in the boxplot are outliers and are therefore only shown as dots). Based on the topography, movement pattern, and elevation of the rock glacier front, we suspect the "end" of a rock glacier destabilisation, as is often observed today, as a reaction to the positive temperature anomalies in the 1940s. However, this is only a speculation, as no data are available before 1953.

We rephrased this passage. (P16L402-404)

P17L433. You could also reference to the PERMOS reports.

We have now cited the PERMOS report in several places in the manuscript.

Figure 4: What do the red dots and bars indicate ?

The red dots indicate the mean value, the red bars indicate the insignificant measurements.

We added the description to the legend of figure 4.

P18L454. Space before "Roer"

We corrcted this.

P18L458. Could it be differently ? At the scale of the study area the changes in external forcing are the same for all the rock glaciers.

This sentence was deleted in the process of the rewrite.

P19L482. Fig **5**. To compare the size of the different rock glaciers the scale should be the same, and obviously it is not (in any case it is too small to verify it).

We have completely revised the illustration and made the scale the same.

P19L483-484. … which is so normal ! I don't know any rock glacier showing uniform velocities on its entire surface.

We are aware of that. We just wanted to describe the patterns we see in the flow velocity maps. Reviewer 3 places great emphasis on describing the herterogeneity of rock glacier movement, so we include this in the results section.

P19L491-492. This is highly speculative. With such a low sample it is not possible to conclude anything about the link between rock glacier acceleration and altitude. And there is no objective explanation why higher rock glaciers would react more than lower ones.

The reasoning behind this was hat the higher-elevated rock glacier RG05 only reacted so strongly in the last epoch, as a delayed reaction to the temperature increase due to the higher elevation.

We have deleted this sentence and given another possible explanation for the deviating behaviour (P26L566-572).

P20-21, Figure 5. Figure a bit complicated. Everything is too small and thus difficult to read. I suggest to make 2 figures with 1) the charts and 2) the maps.

We have completely revised the illustration. The diagrams have been removed, as the information they contain can also be seen on the maps. Furthermore, we have enlarged the maps and added violin plots to allow a better representation of the development of the different kinematic zones, which was requested by reviewer 3.

Fig. 7

P22L523. What do you mean by "system state" ?

By system state we mean the change of the rock glacier or permafrost body to increased temperatures, for example the formation of drainage systems.

P22L225. "summer" instead of "autumn".

We corrected this.

P22L530. But the velocities are not only controlled by air temperature but also, and in a large portion, by the historic development of the snow cover, including the date of complete snow melt.

We are aware of this, and will addressed it in the reformulated chapter of the discussion.

P29L635-649

Figure 6: Indicate the period of comparison regarding the anomalies in T and P. I guess 1961-1990 ?

You are correct, the reference period for anomalies is 1961-1990. We indicated this in the caption of the figure and in the method section.

Fig. 13; P11L275

P25L570. But generally a long duration of the snow cover is related to a thick snow cover, and thus leads to increasing liquid water, considering also that the latter is available all along the snow melt period.

As described earlier, we have extended the analysis of the snow cover and reformulated it in the text.

P25L582. Looking at Fig 7 the value for RG 05 seems to be lower than 0.031

Since figure 7 is a box plot, the mean value is not shown.

The mean values of the modified analysis (DoD-analysis) are shown in the revised version in Figure 9.

P25L591. What is this other rock glacier pushed forward ?

This is to describe the changes observed in the surface elevation change maps and associated boxplots. These are made up of positive and negative changes. We wanted to use the term "pushing forward" to describe the frequently observed advance of rock glacier front due to flow.

We have rephrased this to rock glacier advance in the cases where we have used pushing forward.

P26L598-606. Please refer to the corresponding Figure. This is an example of too long paragraph regarding the data that have to be presented. The same could be said in 3 lines. Not necessary to give all these details for RG 02.

We have taken this comment into account in revising, restructuring and reformulating the results and discussion section.

P26L615. …different sizes. We already know this.

We deleted this sentence.

P27L618-621. Despite the fact that the maps are tiny (please increase the size, for instance by making 2 figures), I rather see patterns of positive or negative changes instead of scattering. Or you mean scattering at a larger scale ? But anyway the figures are too small to be analyzed by the reader.

We have revised the illustration and enlarged the maps significantly. Wherever we have referred to scattering in the text, we have replaced it with patterns of positive and negative change.

P27L621-622. I don't understand the sentence. And avoid references in the middle of a sentence.

We reformulated the sentense.

P30L676-679

P29, chap. 4.6.1. I don't see any particular evolution for this rock glacier, since most of the landforms studied show an increase in velocities from 1997. This section is highly speculative and I suggest to delete it.

The particular development is not indicated by a different acceleration, but by the change in the pattern of high and low velocities.

We have deleted the passage and describe the development of the velocity pattern in short P19L432-4433.

P29L671. RG04 is obviously a push moraine (i.e. frozen sediments – probably a rock glacier – deformed by the LIA glacier advance). This is highlighted by the back-creeping movement towards the former glacier position and the strong subsidence, indicating high ice content. This must be considered in the analysis.

We have deleted the chapter on special cases and taken this into account in the discussion of the results. P25L552-555

P29L681-687. Ok for the possible reactivation, but it would be interesting to propose some hypothesis to explain such a reactivation process.

Although we have deleted the chapter on special cases, the special case is discussed in the subsection atypical development of flow velocities. Here we also present possible explanations resulting from the different behaviour of RG04 and RG07.

P25-26L555-565

P30L702. Permafrost is a thermal phenomenon. It can thus not melt.

We deleted this sentence.

P30L708. "… in the area of shear surfaces…" : what do you mean exactly ?

Here we wanted to express that the horizontal movement of the rock glacier (either in the area of the shear zone or by internal plastic deformation) can result in positive and negative values in the surface elevation changes, but a net volume change can only result from material input, output, compaction or melting of the ice body.

We clarify this in the discussion of surface elevation changes. P30L670-676

P30L709. "change" without s

We corrected this.

P31L720. "similar magnitude". Do you mean similar values ? Because it is evident that horizontal velocities are expected to be much higher than "3D" changes.

As suggested by reviewer 3, we have carried out the analysis of flow velocities and surface elevation changes on sub-surfaces. Therefore, the description of the results of the analysis has been completely rewritten. We took your comment into account by doing so.

Referee 2

General comments

I highly agree with the opinion of the anonymous referee #1. Because of the high potential of the present paper I would like to add some personal comments helping the authors to improve their paper.

Permafrost studies are currently a hot topic in view of climate change. The authors focus on mountain permafrost, i.e., they want to understand the spatio-temporal change of rock glacier kinematics not only locally (single rock glacier) but on a more regional scale (several rock glacier, e.g., located in a valley or catchment area). The authors want to find out how nearby rock glaciers react (geometrically) to changing environmental conditions, i.e., MAAT, precipitation, snow cover, etc.

Change detection analysis is based on archival aerial photographs and ALS data. The proper processing of these data is not easy and requires a lot of knowledge and experience. I am confident that the data has been processed accordingly.

My mayor concern is on data analysis which has already been addressed by the anonymous referee #1. I'm referring to page 12 where the concept of 3D displacements on rock glaciers is outlined. The authors should clarify the term 3D displacement. To my understanding 3D displacement is a 3D vector describing the dislocation/movement of a point or distinct feature of an object/surface in space (and time). However, the authors of the paper interpret 3D displacement as a distance into a normal direction following the idea of Lague et. al. (2013). Commonly, this algorithm is called M3C2. This algorithm has same advantages, especially in interpreting surface change and its significance. The authors' quantitative analysis of the rock glacier kinematics is based on 2D/horizontal displacements and on volume change. The latter, however, has not been carried out in a fully correct way. Since volumetric change, as implemented in the paper, is based on gridded '3D displacements' (cp. P12L303-304) the obtained volumetric changes are inherently wrong. The authors would have derived a correct result if they had taken (0,0,1) = vertical axis as a reference direction. Due to the specific kinematics (e.g., extending creep internal mass transport) and the geometry (e.g., steep frontal slope) of rock glaciers the obtained volumetric changes are preferably/systematically negative (see Figue 8). I advise the authors to re-evaluate volumetric change. The authors may use M3C2 (properly modified) or a simple difference of digital elevation models (DoD).

First of all, we agree with you that the term 3D displacement is misleading. After reading your comment we understand that the calculation of the volumes by gridding the point cloud based suface elvation changes is subject to a small but systematic error. This has a slight effect on the absolute volumes, but hardly on the relative change of the volumes over time. Therefore, the conclusions derived from the calculations will not change. Nevertheless, we see the need to improve the calculation of the volumes!

Therefor, we determined the volume via a classic DoD analysis and calculated the uncertainty according to Anderson (2019) - https://doi.org/10.1002/esp.4551 *. In order to avoid confusion and to facilitate comparability with other studies that determine the surface changes of rock glaciers by means of DoD analysis, we changed the other analysis of surface elevation changes on the basis of DoDs.*

In any case, the authors should include profound error analysis, i.e., significance analysis, for their velocity data (2D, horizontal) and their volumetric change results (1D, vertical, integral value obtained for an area).

We have systematically analysed the errors of flow velocity analysis and surface elevation change analysis on stable areas. The approach to determaine the significance of flow velocities of rock glaciers is published in a peer-reviewed journal (Fey&Krainer 2020).

We included this value in Figure 6 and show the areas below the LoD values in the maps of Figure 7.

In the case of uncertainty analysis of volumes, we followed Anderson (2019) and display the uncertainty in Figure 9.

The paper will benefit from a more formal structure, such as

We have completely restructured the paper according to your suggestions.

Introduction (please clearly specify the research questions),

We revised the introduction as suggested by Reviewer 1 and clarified the research questions in the beginning of the introduction.

Study area,

Material (First: aerial photographs and ALS data; Question: What is the reason for not using recent aerial photographs? There is lots of data available at BEV! A comparative analysis would have boosted all results obtained. Second: Supplementary material, such as meteorological data, etc.

We agree that a separation of materials and methods would provide a clearer structure and overview, especially since many different data were used.

We will have revised the materials and methods section of our manuscript according to your suggestions.

We know the availible BEV data as we have obtained many of the historical aerial photographs here. We decided against using the available more recent BEV aerial photographs because we have collected and/or postprocessed ALS data ourselves that cover more recent study period (2006-2017) and provide the desired information we need to answer our research question.

Methods (photogrammetric mapping, georeferencing, SfM; processing/georeferencing of ALS data; 2D-displacement measurements (orthophoto-orthophoto, orthophoto-hillshade, hillshade-hillshade; software used; precision/accuracy assessment). Question: What is the reason to use hillshades instead of original elevation data?; computation of volumetric change (method, precision/accuracy assessment); Supplementary material (explain data aggregation, etc.)

Since the elevation model of the 1997 dataset has a relatively poor resolution, a calculation of the flow velocity by image correlation with the 2006 ALS dataset in the elevation data was not successful. We state this in the manuscript (P9L221-223).

In order to avoid using an additional data type for the determination of the flow velocities, we have also used hillshades for the epochs 2006-2012 and 2012-2017. The calculation of the flow velocity with the help of hillshades is a common procedure that is used in many studies (e.g. Dusik et al. 2015; Bollmann et al. 2015)

We restructured the methods section according to your suggestions.

Results (present the results obtained. Maybe, you can find a good way to also aggregate the results)

We have separated the results and discussion sections and now describe the general trends and the deviations from these trends in the results section.

Discussion (discuss the kinematics (movement, volumetric change) of the rock glaciers in a regional context. Is there a correlation in space and time? Interrelate the kinematic information with the supplementary data.)

In the revised discussion section, we now discuss both stream velocities and surface elevation changes in a regional context and interrelate these to the meteorological data.

Summary (optionally)

Since the publication is very long anyway, we decided against a summary.

Specific comments

The title should reflect the content of the paper. Do you really want to address morphodynamics? Did you mean kinematics? Morphodynamics would imply process understanding.

We changed the title accordingly.

The paper is too long and could benefit from shortening. Maybe, it is not important to discuss each rock glacier in detail. Is there a common response? If not, why?

The restructuring and revision of the introduction, the methods and materials section and the results and disscusion sections has led to the publication being significantly shortened, despite additional illustrations.

Some of the figures are too small and too overloaded and thus they are not readable.

We assume that you are referring mainly to figures 5 and 9.

We have significantly revised these illustrations so that the maps are now much larger and easier to read. In the process, we have deleted the boxplots contained therein, as the information contained therein can also be read from the maps.

See Fig. 7 and Fig. 10

Figure 8: Mean annual volume change (m2) per 100m2 = mean surface height change (cm) !!!

We agree and have changed the unit to m or m/yr.

Referee 3

First of all, my apologies for this late review.

The paper is one of the first publication comparing the evolution of rock glacier kinematics for a set of landforms located in a single catchment area over a period of more than 60 years. The analysis is mostly based on historical aerial photographs and more recent airborne laser scanning data made available by the authors team. It permits to capture the evolution of rock glacier kinematics at roughly at a decadal time step.

This is a very interesting paper suffering however from several weaknesses, which I strongly recommend to improve in order to consider it for publication. The paper is relatively long and needs to be significantly shorten, either via the text content or the concision of some sentences or paragraphs. Any repetition must be avoided. I agree that this is a difficult exercise. The content of the illustration is mostly excellent, but usually much too small, what is deserving the paper. Some very important results are lost in large figures (e.g. evolution of the velocity flow field) and must be highlighted. Maybe some additional figures are needed.

This was noted in the same way by all other referees. We restructure, rewritten and shorten the study. In the process, we have separated the methods section into materials and methods, as suggested by Referee 2, and separated the results and discussion sections, avoiding repetitions and deleting unnecessary parts. All illustrations have been completely revised to improve readability and information transfer.

The structure of the paper must be revised. The description of all rock glaciers, including their spatial flow pattern and connection to upslope unit must come in entrance. It helps the eventual splitting of some rock glaciers in distinct sub-areas to be envisaged. Then the results are presented. Finally a distinct discussion section must come. At present results and discussion are mixed. The discussion must avoid to be too hypothetical.

As noted by referee 1, we now present the results of the rock glacier inventory and the more detailed characterisation of the rock glaciers studied at the beginning of the results section. In the following, we present the results of the flow velocity, surface elevation change (former 3D Distance) and the volume change analyses. Only then will we discuss the results.

We have not subdivided the rock glaciers into sub-areas, but we emphasise the heterogeneity more prominently in the text and have added violin plots in Fig. 7 to enable the reader to better assess the development of different velocity zones from the maps.

The methodology to calculate the rock glacier flow rate (single value) is unclear. It looks to be a mean of all parts of the rock glacier where any data is available, whatever the kinematic behavior. What is the sense of doing so ? Marginal areas, not moving homogeneously with the main rock glacier body, should not be taken in consideration. In addition, for some rock glaciers, it looks that calculating a mean velocity for the entire landform has no sense regarding the heterogeneity of the kinematic behavior over both space and time. Separating some rock glaciers in two or several kinematic sub-aeras could provide results (and conclusions) differing from the current ones.

We have now applied the maximum LoD of all time slices of the respective rock glacier when calculating the mean values. In this way, areas that do not move homogeneously with the rock glacier/have values close to zero are excluded and at the same time the comparability of the mean values is preserved. In addition, we added violin plots of the flow velocities in Fig. 7. These illustrate the heterogeneity and the change of the different

kinematic zones on the individual rock glaciers over time. Also, areas below the LoD are now shown in this figure. In the supplementary PDF there is a figure showing the area proportions of the rock glaciers above LoD and maximum LoD.

The "3D displacement" is not one, meaning it is not a displacement in xyz coordinates, but an inadequate terminology to define somehow a vertical movement only, but not exactly. What is the interest of applying such an approach (movement normal to the surface)?

We agree that 3D displacement is a misleading term, this was replaced by surface elevation change. Although this approach can be advantageous in complex trains (c.f. Lague et al. 2013) *, we replaced the analysis with a DoD analysis in the revised version, as Referee 2 noted that the calculation of the volume by gridding of the point cloud based surface elevation changes is not correct. Although this changed the results just slightly, this approach may be easier to understand and provide better comparability with other studies that largely follow the classic 2.5 D (DoD) approach.*

Both abstract and conclusions must be revised accordingly. They have not been reviewed, because they may change after having adapted the analysis procedure.

Since about L500, I have not performed an in-depth review.

The additional references indicated in my review are suggestions only.

The location of all rock glaciers must be provided.

In addition to figure 1, which shows the locations of the rock glaciers in the study area, we will add a kmz file with the locations of the rock glaciers studied in the supplementary merterial part of the study.

Detailed comments :

Title : I guess it is more the multi-decadal kinematics of the rock glacier which is analyzed and not the morphodynamics

We Agree. We changed the title accordingly.

L14 : nine or eight ? Weird statement.

In the case of the point cloud based surface elevation change there are nine and for the flow velocity analysis there are eight. The ninth could not be considered for the flow velocity analysis due to shadowing and lack of surface structure which made image correlation impossible.

We specified this in more detail at the end of the introduction and in the methods section.

L33: "or pure ice". To be avoided. This would be a debris-covered glacier.

We removed "or pure ice"

L38: "in part" can be omitted. Ice build-up within the ground might be the dominant process and the embedding of external (e.g. glacier, snowpatch) ice might be inexistent.

We deleted this passage due to the requested restructuring of the introduction of Referee 1.

L42: Active layer is consisting of unconsolidated debris (not only "boulders")

We corrected this.

L34

L43: I don’t see the causal relationship between the thermal regime driven by freeze-thaw cycles and the air-filled porosity of the active layer. There is also air and water advection. Are there some references to propose ?

We removed this sentence from the manuscript as reviewer 1 requested that we shorten the very general introduction to rock glaciers up to line 55.

L43: “These”, but which ones ?

By these we mean the previously mentioned freeze-thaw cycles and the air convection. This sentence will also be deleted in the revised version, due to the requested shortening of the general introduction by Referee 1.

L46: No, the debris size is not smaller, but the proportion of coarser debris per volume is less.

We corrected this.

L35

L51: What does mean "long-term"?

Since the previous sentence talks about seasonal changes, by long-term we mean the change in thermal forcing over several years.

L51-54: See also Cicoira et al. 2021 - A general theory of rock glacier creep…

We added Cicoira et al. 2021.

L52

L.54 : The shear zone is maximally a few meters thick.

We specified this.

L42

L57ff: This is only valid in the European Alps.

We will have limited the validity of the statement to the European Alps.

L44-45

L59. Velocity decrease since the 1990s. Which of the mentioned studies are reporting this ? I agree that some rock glaciers are decelerating, but the general trend is a significant continuation of the acceleration (e.g. PERMOS 2019 in the Swiss Alps… must not be very different in the Austrian Alps, a couple of tens kilometers eastward)

We did not want to talk about a general deceleration of rock glaciers following the acceleration in the 1990s, but to show that there were phases (years or multiple years), in which constant or decreasing velocities were measured. This is also partly reflected in our study, in RG 01, 03, 06, 08. In these examples, lower flow velocities were measured in the period 2006 - 20012 than between 1997 - 2006 and 2012 - 2017. At least in two of the metioned studies this is also the case (e.g. Kellerer-Pirklbauer and Kaufmann, 2012 - for three rock glacier in the Hohen Tauern Range, Austria or Kenner et al. 2020 - for Schafberg rock glacier in the Swiss Alps).

We refer to the PERMOS report in many places in the revised discussion section.

L70. No one of both mentioned references is showing this, but Delaloye et al. 2013 – Rapidly moving rock glaciers… - and Eriksen et al. 2018 - Recent Acceleration of a Rock Glacier Complex, Ádjet ...- are doing so. About destabilization, see also Marcer et al. 2019 - Evaluating the destabilization susceptibility of ...

We agree that in the studies mentioned a maximum of 4m/yr is given. In an earlier version we had Vivero & Labiel 2019, which give 60-75 m in about a year, in the citation, this had to be deleted due to the limit of 80 references in the cryosphere.

We added "…up to sveral tens of meters…" and included Vivero and Lambiel (2019) and Marcer et al. (2019) into the citation.

L58-59

L83 I would suggest "e.g." because there are other studies, sometimes difficult of being accessible. Maybe also Kummert et al., (under final review in ESPL) - Pluri-decadal evolution of rock glaciers surface velocity and its impact on sediment export rates towards high alpine torrents. See also Kääb et al. 2020 - Inventory, motion and acceleration of rock glaciers... for an example outside of the Alps

We have added e.g. to the citation. Otherwise, we have left the citation as it is, as it supports the statement sufficiently well.

L85. There is something wrong in this sentence

We rephrased this sentence.

L89. Are the rock glaciers the same, so 8 of 9 ?

Yes, we will have specified this.

e.g. L71-72

L94. Never begin a chapter with a figure. But besides, it would good to precise what are the used coordinates, what is the unit (m ?) and to add (or replace them by) lat/long coordinates.

The coordinate system used is ETRS89 / UTM zone 32N EPSG:25832 and the unit is meter.

We have described this in the caption.

L97. m. a.s.l.

We corrected this.

L108. Anthropic influence on the rock glacier as well ? Which ones precisely ?

Yes, there is also anthropogenic influence on a rock glacier due to the construction of the glacier road between 1979 and 1982, which intersects rock glacier RG03.

We have mentioned this here.

L95

L110. Inactive rock glaciers. How was this classification done ? On which parameters ? Does it fit with the IPA Action Group Rock glacier inventories and kinematics definition ?

Here we follow the classical classification into active - motion (from image correlation) inactive - ice contained; no measurable motion (melting, visible on DoDs) and fossil (e.g. Krainer & Ribis 2012).

The methodology is described in chapter 4.1 Rock glacier inventory (L165-180). We have now described the creation of the inventory in more detail.

L113. Replace by something like : Finally, eight active rock glaciers representing different characteristics and conditions were investigated in detail regarding flow velocities and one more regarding vertical displacements

We have adopted the formulation.

L289-290

L121. The rock glacier moving downwards, it does not make sense to write that it reaches the “highest elevation”. Would it not be that it is located at the highest elevation range among the nine selected rock glaciers ?

We will put the description in the results section, as Referee 1 asked us to put the results of the inventory in the results section.

We have reformulated this sentence.

L292-293

L129. Is the layer below the ice rich permafrost body really ice free ? Because it is very difficult to conceive an active rock glacier which is only frozen in its upper part. Where is the shearing zone developing on the long term ?

Here we describe the results of the geophysical investigations of Hausmann et al. 2012 at the Ölgruben rock glacier (RG 01). Since we are not experts in the interpretation of geophysical surveys, we can only report the results of the study as published.

151-153. There was a paper in 2008 (Delaloye et al. - Recent interannual variations of rock glacier creep in the European Alps) showing that there was an almost good similarity of interannual variations of rock glacier velocity over the entire European Alps, confirmed a decade later by Kellerer-Pirklbauer et al. (2018) at EUCOP - Interannual variability of rock glacier flow velocities in the European Alps. There was also a short communication at ICOP 2016 by Staub et al. - Rock glacier creep as a thermallyâ‹-driven phenomenon: A decade of inter-annual observations from the Swiss Alps - showing that the interannual variations are basically driven by shifts in mean ground surface temperature for a period of about 2.5 years. For sure this is also influencing the liquid water content within the permafrost.

This is why we write liquid water availability and not precipitation, the increased liquid water availability can have several causes, which according to Kenner et al. (2020) is mainly controlled by the ground temperature and the onset and duration of the snow cover. This is described in the introduction (L52-54).

We removed the explanation in the methods section, as Referee 1 noted that this should be explained in the introduction.

153-155. The effects of liquid water availability and snow cover on rock glacier morphodynamics must be precised. Does it mean on-set and melt-out of the snow cover influencing the ground surface temperature or water equivalent of the snow pack which will melt out in spring/suemmer and directly influencing the rock glacier hydrology. Or both ?

The influence of various drivers of rock glacier flow velocity is nicely displayed in Kenner et al. (2020), Figure 5. Although they just derive this from the study of one rock glacier, we think this is a very good overview and we are following it to some extent in our discussion of climate factors.

Both. As the influence of GST indiectly influences water availibility as well. The onset of winter snow cover determines the winter cooling intensity, which in turn influences the timespan with liquid water in the active layer. The strongest influence, however, is shown by the timing of snow melt, but less by the snow water equivalent (Kenner et al. 2020).

L630-654

Finally, the paper is focusing on decadal velocity changes. What relation to short-term (less than annual) changes ?

We do not know exactly to which passage this comment refers, as we are now in the method section in cronological order.

We refer to findings about short-term change and their relation to decadal velocity changes in the discussion.

e.g. L635-640

L276ff. This is not a 3D displacement, but something else (the surface change normal to the surface). But what is the interest of doing so and not calculating simply the vertical displacement? What are the advantages on a rock glacier ?

We have now replaced the analysis with a classical DoD analysis. See previous answer.

L323 “showed good agreement”. Please, provide values, figure or table.

Although a quantitative comparison is difficult because the time periods of the epochs differ. The results are very consistent if we consider that the epoch of the paper by Groh and Blöthe (2019) (2003-2015) includes three epochs of our study (1997 - 2006, 2006 - 2012 and 2012 - 2017).

The comparison is now included in the supplementary PDF in Table S 1.

L.326 Is the stable area so stable? In principle, bedrock is more suited to be stable than a debris slope.

We agree that bedrock can generally be considered more stable than a debris solpe. However, on bedrock, which usually has lower roughness, the errors are underestimated, especially in the image correlation analysis. Since we know the change from our surface elevation change analysis in these areas, we know that they are stable.

L361-363. The introductive part of the sentence could be avoided.

We deleted the introductive part.

L.367. A significant trend cannot be calculated over 11 years only. What is this data meaning ? Is it a difference between the mean of the two periods or a trend in 11 years as expressed in the two previous sentences.

The two previous sentences describe the trend over 65 years at the Obergurgl-Vent and Nauders stations. The one meant compares the increase in the mean value in the last two epochs of the study, including the Weißsee station.

This sentence has been deleted due to the shortening of the description of climate factors.

L370. Precise what are these seasons, e.g. spring is MAM, summer JJA ?

We have specified this in the description of the methodology (L276-277) and in the caption (Fig.12).

L371-372 Conditions causing heat waves in future is not the purpose of this paper looking back into the past. The sentences could be removed.

We removed this sentence.

L388. +152 mm in 65y. Is it a lot or not ? What is the annual value ?

We give the mean annual precipitation value for the study period in the next sentence (931 mm/yr). In the previous sentence we give the range of trends for all the stations studied (53 mm - 241 mm). A positive trend of 152 mm in 65 years is already quite a lot in our view.

L393-395. This sentence about precipitation predictions could be removed (not of interest for this paper)

We removed this sentence.

L408. Snow melt trend: How much ? What are the starting dates and durations ?

Referee 1 has noted that the small decreases in the snow parameters are not a trend. We will therefore delete the sentence. In addition, we will reanalyse the snow parameters and add the onset of a significant snow cover (>50) and the complete snow melt as parameters.

L380-389

L420. Provided max flow values are valid for a single period or as a mean for 1953 to 2017 ?

These values are for the whole period of inverstigation (1953 – 2017).

We stated this in the beginning of the sentence: "For the whole period of investigation…"

L391

L421. How is the mean value spatially calculated ? How is delimitated the area taken into consideration for the calculation ? It looks from the figures that they comprise marginal areas (with velocity close to 0) to sectors moving much faster. Why not to split into sub-areas and perform a comparative temporal analysis ? See also the definition of moving areas within the IPA Action Group Rock glacier inventories and kinematics - https://www.unifr.ch/geo/geomorphology/en/research/ipa-action-group-rock-glacier/ - documents (kinematics as an optional attribute in rock glacier inventories)

We have now applied the maximum LoD of all time slices of the respective rock glacier when calculating the mean values. In this way, areas that do not move homogeneously with the rock glacier/have values close to zero are excluded and at the same time the comparability of the mean values is preserved. In addition, we added violin plots of the flow velocities in Fig. 7. These illustrate the heterogeneity and the change of the different kinematic zones on the individual rock glaciers over time. Also, areas below the LoD are now shown in this figure. In the supplementary PDF there is a figure showing the area proportions of the rock glaciers above LoD and maximum LoD. (see general comments)

L.421. A mean velocity of 3.5 cm/year is rising some questions about the accuracy and reliability of the results (in particular changes over time). Is such a low value significant ?

The value mentioned represents the mean value for RG 03 in the time slice 1970-1982. In this time period, maximum movements of 1.51 m (0.089 m/yr) were measured. The revised mean value, taking into account the LoD and uncertainty, is now 0.14 m/yr ± 0.05 m/yr for this epoch and rock glacier.

This has been described and changed in the text, and a table with all mean values can be found in the supplementary PDF of the study. (Table S 1)

L424. One should note that the period 1997-2006 is marked by the peak of 2000-01 (described for instance by Ikeda et al. 2003 - Rapidly moving small rockglacier at the lower limit of the mountain permafrost belt… - and 2008 - Fast deformation of perennially frozen debris in a warm rock glacier…) and the famous 2003-04 peak (e.g. Delaloye et al. 2008), The period 2012-2017 is embedding the extreme peak of 2015 (e.g. Kellerer-Pirklbauer et al. 2018, PERMOS 2019), whereas the period 2006-2012 contains no peak of activity.

L431. 2006 is often a low, the period with the lowerst velocity recorded since 2000 (e.g. PERMOS). More generally the sentence is difficult to understand unambiguously.
L431-433. You could refer to Delaloye et al. 2008 for the low in 2006 in the European Alps and to PERMOS 2019 for the description of the entire period.

We describe the annual acceleration and deceleration peaks measured in the section "Possible implications of changes in external forcing for rock glacier flow velocities."

e.g. L601-603; L639-644

L434. RG 04 : A detailed spatial analysis (of the morphodynamics) is necessary, with the help of (time-lapsed) maps. It should be the same for the other rock glaciers.

Referees 1 and 2 have requested that the detailed analyses of the individual rock glaciers be greatly shortened and that we rather focus on general similarities and differences in the revised version. But we will describe the general trends, simialrities and differernces of the spatial development. The development of the individual rock glaciers can then be examined by the reader in the flow velocity maps and newly added violin plots.

L435-7. "Many studies mentioned periods of slight decrease or constant flow velocities following the strong acceleration in the 1990s". Not really. This is mostly related to the deceleration drop in 2005-06. Read the related papers (already mentioned earlier), and in particular the PERMOS reports.

We have deleted this sentance and describe the annual acceleration and deceleration peaks as previously described.

L437-9. There are various examples of recent deceleration (or absence of acceleration), particularly in the Swiss Alps (e.g. Aget – see PERMOS 2019 – or Dirru – see Delaloye et al. 2013 – Rapidly moving rock glaciers…- Cicoira et al. 2019 - Water controls the seasonal rhythm of rock glacier flow – and Kummert et al. (under final review in ESPL) Pluri-decadal evolution of rock glaciers surface velocity and its impact on sediment export rates towards high alpine torrents). Probably Val Sassa and Val dal'Acqua rock glaciers in the Swiss national park have done so, but on a longer term since the end of the LIA (e.g. when comparing Chaix 1923 - https://www.persee.fr/docAsPDF/globe_0398-3412_1923_num_62_1_5609.pdf - p.11 and more recent measurements ba the National parc - https://www.parcs.ch/snp/pdf_public/2016/33398_20160921_121930_Sassa_Aqua_Bericht_2012.pdf, the movement rate appears to have been divided by 20 along the last century) . There are also some examples in Roer's PhD (2005). See Roer et al. 2005 - Rockglacier acceleration in the Turtmann valley (Swiss Alps): Probable controls

Since RG04 shows rather constant velocities, considering the uncertainties, we have deleted this statement. However, the development of the flow velocities of the above-mentioned studies are mentioned in the revised discussion.

e.g. L536-539; L553-555

What is the mean velocity of RG04 ? This must be given. What is the uncertainty of the values.

The revised mean values range from 0.12 m/yr ± 0.03 (2012-2017) to 0.14 m/yr ± 0.07m/yr (1997 -2006).

All mean values are included in Table S 1 of the supplementary PDF.

L452 (and others around). Why to be so precise in the values, taking into account their uncertainty?

We reduced the value by a maximum of two decimal places and also indicate the uncertainty when giving the values.

L.451-453. Increase of the 2012-2017 velocity in comparison to which period ? Note that a pluri-decadal acceleration by a factor 2 to 10 has been observed in the Swiss Alps as well (PERMOS 2019 or other related documentation), e.g. Gemmi/Furggentälti, Grosses Gufer, Tsarmine.

Compared to the epoch 1953/54-1970/71. We write this in the introductory sentence to this chapter.

L419

We included the PERMOS report and relativise the statement to: "…one of the highest relative changes in flow velocity compared to other studies."

L559-561

L.453-454. About rock glacier destabilization, see also Delaloye et al. 2013, Eriksen et al. 2018, Marcer et al. 2019 (already mentioned earlier in this review) and Marcer et al. 2020 - Investigating the slope failures at the Lou rock glacier front…

We added Marcer et al. 2021 when referring to rock glacier destabilisation.

e.g. L540-541

L.456-458. Agreed, but this must come in the discussion part and must be explain in details (provide maps/topographical profiles, etc.)

As this is not a case study of a single rock glacier, we will only give hypothetical explanations in the discussion, as a precise analysis of the causal factors of all rock glaciers would go far beyond the scope of this paper.

L.459. "The relative changes regarding the remaining rock glaciers ranges between 23.45% and 271.87%" is a huge difference ! 23% means about constant velocity and 271% an acceleration by a factor close to 4 ! This is not the same behavior.

We do not state that the behaviour is the same, but only present the range of values that the other rock glaciers show.

We emphasised the big difference and the resulting diversity of behaviour.

L424-425

L.459-460. I don't understand the sense of the sentence… If you remove RG04, because it has a very low slope, then one could say that higher elevated rock glaciers (in the set) are steeper, but not that they change their relative flow velocity to a greater extent. If they do so, this is then because of their elevation (and eventually thermal state/structure) or steepness ?

We removed this sentence as referee 1 has noted that this statement is not valid due to the small sample size.

L.463. What are these "topographic factors" ?

Here we mean the topographical factors listed in Table 4 (exposition, slope, elevation).

We have deleted this sentence because of the shortening and restructuring of the manuscript.

L.464. "On rock glaciers RG 01 and RG 08, higher flow velocities have been measured between 1953/54 and 1970/71 compared to the subsequent periods". Be more precise. What are the subsequent periods ?

Agree... but there was then an increase since the 1990s.

By the following epochs we mean the epochs 1970/71 - 1982 and 1982 – 1997 for RG 01. Regarding RG 08, the highest maximum flow velocities (by manual mapping) of the entire study period were measured between 1953 and 1970.

We precised this.

L426-430

L474-476. "These peaks might not be found on the other rock glaciers due to superimposing effects over the long time steps and indicate a slightly different sensitivity, response or response time of individual rock glaciers to intra-annual, inter-annual or multi-annual fluctuations in external forcing parameters." Obscure sentence, which must be either precised, or removed.

We have deleted the sentence in this form.

L476. "the three investigated rock glaciers". Which ones ? There were only two mentioned at the beginning of the paragraph

It must say two.

Due to the rewriting of the manuscript, this sentence was deleted.

L474. “differing substantially in the other characteristics measured”. I do not understand.

Here we wanted to express that the rock glaciers are the two with the lowest elevation, but differ in size, slope, exposure....

Due to the rewriting of the manuscript, this sentence was deleted.

L479. « higher error ». To be precised.

The mean error for the 1997-2006 epoch is 0.08 m/yr. In comparison, it is 0.02 m/yr for the 2012-2017 epoch.

We present this in chapter 5.2 and specified it here.

L430

L481-2. “analyzed for altitudinal zones of 20 meter ». Why to do so ? And not comparing central to marginal zones, or else ?

We have omitted the analysis of altitudinal zones in the revised version.

L482-4. “rock glaciers do not move uniformly, but have zones with higher and lower flow velocities”. It must come at the beginning... and frame the velocity analysis of the previous sections.

We have drawn attention to the heterogeneity in the text (L431-441). This is also evident from the maps of flow velocities and the violin plots (Fig.7).

L484-5. in the terminal section of the rock glacier ? In the front would mean in the frontal (talus) slope.

We have replaced front with terminal section throughout the text when referring to the terminal section.

L489. Relative instead of « percentage »

We corrected this.

L491-2. “This could point to the fact that from 2006 to 2017 higher elevated rock glaciers enter an unstable state as a reaction of changes in the external forcing”. This is a very tricky interpretation, which in any case must be moved to the discussion section.

We have omitted this interpretation in the revised discussion.

L493. There is no lag to permafrost temperature. What temperature is talked about ?
L.494. “temperature limits or similar”. I do not understand.
L.494. Time lack or time lag ?

We have omitted this interpretation in the revised discussion.

L500. Figure 5. Great figure… if made larger. This is obvious here that most rock glaciers are not moving uniformly both in space and time. The multi-decadal kinematic analysis must imperatively be conducted on rock glacier sub-areas separately.

In our study, we do not claim that they move uniformly. In the newly calculated mean values, we exclude areas with flow velocities close to zero. We argue that the heterogeneity is clearly shown in the velocity maps and the violin plots (Fig 7).

Unit of the color scale ?

The unit of the color scale is m/yr and is given in the figure caption.

Fig. 7

L.501. Section 4.4 is mostly an hypothetical discussion, not results.

In the revised version, we have clearly separated results and discussion and supported them more systematically with literature.

L573-654

L579. Figure 7. Never start a chapter with a figure. Moreover, I don't understand what is this 3D displacement. Is it the vertical shift at fixed locations ?

Admittedly, 3D displacment is not the correct term. The method is outlined in Figure A1. It is an established method to calculate the changes between two point clouds. It is commonly known as M3C2 (c.f. Lague et al. 2013). Since we did not use the M3C2 algorithm implemented in CloudCompare, but rather followed Fey and Wichmann (2017) and calculated this in SAGA LIS, we do not call the procedure M3C2, although it is very similar.

As described above, we worked with DoDs in the revised version. Tests have shown that this will hardly change the results, but we think it will make the results more accessible and easier to compare with other studies.

L582. "0.031". Unit ? How is such a value calculated ? What is then its meaning for the rock glacier geometry change ?

This is the mean surface elevation change of the rock glacier. The unit is m/yr. Since the analysis was recalculated with DoDs, the value has also changed slightly. The overview for all rock glaciers is now shown in an additional figure (Fig. 9).

L592. This is not a mass balance, as the value looks not to be computed over the entire landform, which is also changing in geometry.
L608. Figure 8. How is this calculated ? How to take into account the geometry change ? The quality of the figure is poor (labels are much too large for instance).

The values were calculated over the entire rock glacier, by gridding the point cloud based surfaces elevation changes, only snow patches were excluded. What is true, however, is that the calculation of the volume from the point cloud is not quite trivial.

We replaced the volume calculation with a classic DoD volume calculation and improved the associated figure (Fig. 8, 9 and 10). In addition, we have noted that due to the necessary masking of the snow, in many cases it is not possible to speak of a mass balance in the stricter sense.

L670-672

L614-5. This is a setting, not a result. This should come before any kinematic (or morphodynamic) analysis. In addition, the aspect is for sure very different as well. What about the connection to the upslope unit ?

We have now described the rock glacier characteristics at the beginning of the results section (L289-306). Furthermore, in the revised version, we have included the connection with the upslope unit both in the description of the results (Sect.5.5) and in the discussion (Sect.6.5).

L616. “These changes are mostly spatially clustered, but in some cases they also show a clear temporal clustering”. I guess, this is what is explained in the next paragraphs? If not, please do so.

We have reformulated and restructured the entire paragraph on surface elevation changes.

L.618. “Overall, the picture already described for the general trends is confirmed.” That is.. ? What is this picture ? What are the general trends ?

We have moved and reformulated this part.

L499-504

L620 (and elsewhere). Avoid all "clear" and "when looking"... You have to show/express for the reader what is so “clear” “when looking”, but not let him figure out.

We have taken this comment into account in the revision and have now largely avoided these formulations.

L621. “Therefore, the characteristic topography for rock glaciers is formed”. I do not understand.

Dieser teil ist jetzt in der diskussion der surface elevation changes zu finden und wurde deultich umformoliert und prezesiert.

L676-679

L622. “or the rock glacier advances”. It does. It has been shown before. But how is this scattering looking like ?

We reformulated this.

L676-679

L622. « changes in activity ». Activity in what ? A vertical displacement is in particular the sum of the downslope movement of the rock glacier, the strain pattern (compression/extension), aggradation/melt of excess ice. If the flow rate of the rock glacier is increasing, the related component of the vertical movement is increasing proportionally.

Activity is the wrong term in this context. Here we wanted to express that the patterns and magnitude of negative and positive changes are changing spatially and temporally. We will rephrase this.

We now describe this in the discussion of surface elevation changes.

L673-682

L625. “inactive”. The activity must be related to the flow rate, not the vertical movement.

In the revised version, when we use active and inactive, we only refer to the flow rates.

L627. “show hardly any 3D displacements » Is it not a question of scale ? More generally, what about the uncertainty ?

We address this in the description of the results (L454-463) and present it in Fig. 9. The uncertainties of the analysis were determined according to Anderson (2019) and are also included in Fig. 9 and in the text. In the supplementary PDF, all numerical values are listed in Table S 2.

L628. Where to look at on the figure ?

This is clearly shown in Figure 9 by the surface elevation changes and is further explained and detailed in the revised text (Sect. 5.5)

L628. “Here”. Where ?

The description of the results and the discussion has been completely restructured and reformulated so that it should now be made clearer.

L629. What are these active and inactive areas ? I guess the terminology is inappropriate.

Yes, the therminology is not correct in this case. We mean areas with higher and lower changes in the surace elevation change.

We have taken this into account in the restructuring and reformulation of the results and discussion section.

L629. “eleveation dependency”. Absolute or relative to the rock glacier extent?

We have omitted the anlysis of the altitudinal zones in the revised version, as this should be shortened.

"Looking" at the figures, it becomes obvious that aggradation has frequently occurred at the front, whereas subsidence is systematic in the rooting zone, no ?

If one considers all time periods and rock glaciers, we believe that this requires a more differentiated interpretation.

We give a detailed interpretation of the surface elevation changes in the discussion in the revised version (Sect 6.5).

L632. “show very clear activity in subsidence”. How much, please ?

This can now be seen in Fig. 9. Furthermore, we give values in the text wherever necessary. An overview of the values of all rock glaciers and epochs, including their uncertainty, is included in the supplimantary PDF.

L.635ff. Figure ? Where to see that ? Could it be else (than what has been observed) ? How is a null or a positive balance possible ? There should be a feeding of the rock glacier, which is equaling or exceeding the melt of excess ice at the front. For most rock glaciers, it cannot be reached because the motion rate is too fast (should be only a couple of cm/year maximally for most landforms) or there is no connection with any active feeding mechanism (for all glacier forefield-connected rock glaciers or when a small glacier occupied the rock glacier rooting zone during the Little Ice Age, what should be the case for most rock glaciers of concern by this study).

The methodology to calculate the volume balance must be explained, as well as its limitations and uncertainties.

In the revised analysis, only in the case of RG05, epoch 1953-1970 is the value above zero when uncertainty is taken into account. This could be explained by the fact that the glacier is surrounded on both sides by steep slopes that deposit avalanches and avalanche debris.

The methodology is explained in Sect. 4.4, uncertainties and errors are explained in Sect. 5.2 and limitations due to snow cover are mentions and discussed in Sect. 6.5

A volume balance cannot be calculated for a rock glacier unit, which is not entirely covered by the data.

The data cover the entire area of the rock glacier, although snow lies on the surface of the rock glacier in some small areas, as this would lead to bias in the results, the areas in which snow lies in one epoch were excluded for all epochs.

We state this in the description of the methodology (Sect. 4.4) and also note it in the discussion of the results (Sect. 6.5).

L637-8. Provide figure.

We refer to figure 9. In the revised version, we present the changes in more detail and in a more differentiated way (Sect. 5.5).

L650. Figure 9. The figure is very interesting, but too complicated, too small, and almost impossible to read

We have completely revised the illustration, omitted the diagrams and thus improved the readability.

Fig. 10

L657. Provide illustration, map, figure.

As all referees are in favour of shortening the whole paper and referees 1 & 2 suggested that we delet the chapter on special cases.

Therefore, we discuss the rock glaciers that show a distinctive development only briefly in Sect. 6.2 "Atypical development of flow velocities".

L660. What about local loading (by displaced debris) ?

We have taken the comment into account in Sect. 6.5 "Interpretation and implications of surface elevation changes".

L664. Provide values !

The values can be extracted from Figure 9 in the publication. As described above, the values of all rock glaciers are now included in the supplementary PDF of the paper.

L665. But most rock glaciers in the European Alps accelerated since the 1990s ! Why to state here specifically that “a strong increase in flow velocity was measured since 1997, which makes a delayed reaction of the rock glacier to the road construction 17 years before very likely”. This is tricky and even false (i.e. no specific reaction).

We have omitted this interpretation in the revised version and only briefly refer to the unusual change in the pattern of flow velocities in the description of the results.

L432-433

L.667. “It is known”. Add reference(s)

This section was deleted during the revision of the results and discussion section.

L667. “both factors”. Which factors ?

This section was deleted during the revision of the results and discussion section.

L669. Slope, altitude and ice occurrence are not an internal forcing. They are almost not changing over time. The ice/water content ratio does it.

This section was deleted during the revision of the results and discussion section.

L669. “It is evident”. ???

This section was deleted during the revision of the results and discussion section. We address this in the revised discussion.

L687-700 and L550-551

L690. Thermokarst lakes “become a more common feature on rock glaciers due to warming and degradation of permafrost ». But there are not so frequent ! And only where massive (glacier) ice is embedded in the rock glacier.

We agree with his comment. Since we have omitted the part about the special cases, no statement is made about the possible development of the thermokarst lakes.

L.693. “shifted its location”. Or evolved in size and location consecutively to rapid ice melt at its margins.

Since we have omitted the chapter on special cases, the development of the thermokarst lake on RG06 is not discussed. A map showing the development of the lake is included in the supplementary PDF.

L705ff. This section must be heavily synthetized. See also comments on vertical movement in 4.x.x. Has not been reviewed, because the 3D displacement is somehow an obscure concept to me in this paper.

As we have restructured, rewritten and shortened the results section, this section has also been shortened, restructured and rewritten.

Figure 10 looks very interesting, whereas it should be adapted to rock glacier sub-areas. The insert in the upper right is not fully necessary.

We have removed the insert and carried out the analysis on sub-areas. (Fig. 11 and Sect. 5.6)

Table A1:

Elevation: I've tried to identify the rock glaciers on Google Earth. I don't know how these elevations have been determined, what do they represent. In particular, the max elevation appears to be often exaggerated.

The supplementary section now contains a .kmz file with the locations of the investigated rock glaciers.

The elevations were calculated from our ALS dataset from 2017. This is available in the coordinate system ETRS89 / UTM zone 32N (EPSG:25832). In this coordinate system the ellipsoid GRS 1980 is used, so the elevations are m above GRS 1980. Google Earth uses the WGS 84 / Pseudo-Mercator (EPSG:3857) coordinate system. This uses the ellipsoid WGS 84, which may be the reason for inconsistencies.

Connection to the upslope unit : Reference and abbreviations ?

The classification was done according to the IPA Action Group: Rock glacier inventories and kinematics - Baseline Concepts Inventorying Rock Glaciers as described in the text.

L168-170

We have defined the abbreviations in the text.

L304-305

RG 01 : I would say GFC for the main unit.

We agree. However, one lobe is also TC, so kept this but change the order.

RG 03: GFC. There is no glacier in connection with the rock glacier at present.

We agree. We changed this.

RG 04: GFC. There is no glacier in connection with the rock glacier at present.

We agree. We changed this.

RG 05: Not sure about the site. The rock glacier I guess is RG-05 is TC, but maybe it is another one.

No you are right. We changed this.

RG 06: But for sure with a glacier in the rooting zone during LIA, as attested by the thermokarst lake development in probably glacier ice embedded into the rock glacier.

We know that there is clear evidence that a glacier must have been in the root zone during the LIA. However, the glacier inventory (Fischer et al. 2015) does not show a glacier here.

We added a note that there must have been a small glacier evidenced by the thermokarst lake.

RG 09: GFC. Why TC ?

Since in our understanding it is a poly-connected rock glacier.

Connection to the upslope unit and area covered by 1850 glacier extent : These two characteristics show that the rock glaciers cannot be treated all in the same way. This is extremely important.

We have taken this comment into account and address it in particular in the discussion of surface elevation changes.

Sect. 6.5