

### Comments to the author:

Dear Fabian Fleischer and co-authors,

Your manuscript received in the first review round three very detailed and critical reviews that both highlighted the relevance and novelty of the research, in particular the extensive dataset covering 9 rockglaciers over close to 6 decades and they confirm that the manuscripts seems in general to be well within the scope of TC. Besides the generally positive comments, all reviews also raised some important aspects that should be improved before publication and in brief encompass:

- improvement of the methods and its description (issue 3d-displacements/DoD for elevation change)
- improvement of the structure (proper separation of results from discussion)
- shortening of the manuscript (avoid repetitions)
- adjustment and separation of figures to make them easier to read
- minor adjustments in text

As the revisions were rather substantial the revised version went to review again (to one of the previous reviewers), and as the editor, I also read and checked the revised version again.

The re-review clearly stated the substantial improvements (figures, structure, methods) and makes clear that this is a interesting and valuable contribution with regard to longterm dynamical behavior of rock glacier in context of climate forcing. Some of the interpretation remains somewhat speculative but the presented base for interpretation is clear now.

I confirm the referees view and that the comments of the first round of reviews have been well addressed in the revised version (the structure, figures, methodological issues and the interpretation). Thus, this manuscript is now close to publication but there are still quite a few mostly minor editing issues (typos, formulations, captions).

The list of the minor comments below that of the re-review and myself the editor should be addressed by the authors before acceptance of the paper.

I thank the authors for their considerate and detailed response of how to address the points raised by the referees.

Andreas Vieli, editor

30 oct 2021

Dear Andreas Vieli,

We thank you and the reviewer for seeing the relevance of the study and also for acknowledging the improvements we were able to achieve as a result of the review in the revised version. This would not have been possible without the detailed comments and remarks of the reviewers and yourselves. We would also like to thank you and the reviewer for the improvements and comments of the revised mauskript.

Based on the comments, we have made the following minor changes to the mauskript:

- correction of typos
- improvement of formulations and captions
- minor changes in illustrations
- In some places, it has been pointed out that due to the drawbacks of the input data (e.g. snow patches) some results have to be interpreted with caution.

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

List of comments by re-review that should be addressed  
The title is now fine.

#### Abstract

L11: Mention the elements of external forcing. Is it a 'shift' or a 'change'?

Another reviewer noted the repetition of change in this sentence.

We have refrained from mentioning the external forcing parameters, as these are mentioned below in the abstract. We have changed shift back to change.

L13: Please check 'nine or eight', it's not clear to me.

The half-sentence specifies that the study was conducted on eight or nine rock glaciers, depending on the analysis. This is described in more detail in the introduction (L71-74) and throughout the manuscript.

We have changed the sentence to: "...on nine or eight active rock glaciers respectively..."

L120: Table 1, N/A ? The flight altitude is a by-product of georeferencing. Fill in appropriate values.

Thank you for the suggestion.

We have now inserted the values from the respective Agisoft Metashape report files for all processed aerial images. These show the mean flight altitude over the survey area. The old values represented average flight altitudes for the entire flight campaigns which were not always representative for the study area.

#### 4. Methods

L182: Exchange 'orthofoto' by 'orthophoto'. Please stick to the same wording, either orthophoto or orthoimage.

We agree and have now used orthoimage throughout the manuscript.

L215: The resulting raw vector maps can contain erroneous displacement vectors. Vector maps do not contain decorrelation per se. Image decorrelation might be caused by ... Please rephrase.

We agree and have reformulated the sentence accordingly.

L225: What is a 'non-matching' DEM? Do you mean non-contemporary?

Here we wanted to express that the DEM might not be from the same year as the aerial photos. Therefore, you are right non-contemporary fits better.

We have replaced non-matching with non-contemporary.

L235: ... and therefore DEMs (?) were still a source of error.

We wanted to express that the different quality of the DEMs can also have an impact on the accuracy of orthorectification.

We have reworded the sentence to: "... and therefore might influence the accuracy of the orthorectification."

L249: Formula 1 is a rather strange error propagation.

This formula is used by Fey and Krainer (2020) to determine the LoD for flow velocity measurements derived by image correlation. Kummert et al. (2021) use the mean value of three manual point measurements on stable areas between two image pairs as the error value for flow velocity measurements derived by image correlation. Since we have many measurements by using image correlation in stable areas, we do not simply use the mean value, but add two times the standard deviation. In our opinion, this provides a good and relatively conservative estimate of the error.

L266-267: The sentence is semantically correct. However, because of this (drawback) the subsequent analysis is somehow imperfect.

We are aware of this and address this issue in the discussion of surface elevation changes (L681 – 683, L691-693, L709-711). Since permanent snow patches often occur at altitudes where rock glaciers are found, it will hardly be possible to prevent this.

We have included a reference to Figure 10, which shows the snow masks, and we have also included a sentence at this point to draw attention to the problem:

"This implies that in some cases the entire landform cannot be considered for the mass balances, so the results of these must be interpreted with caution."

L268: Omit the word 'strictly'. What would have been the other options?

We omitted the word strictly.

Alternatives to the approach of Anderson (2019) would be, for example, to simply use the standard deviation estimated from stable area for the characterisation of uncertainty as done in a similar study by Kaufmann et al. (2018). Another alternative would be, for example, the approach of Wheaton et al. (2009) which, however, is based on thresholding. Anderson (2019) clearly shows that his approach provides better and more robust results than approaches based on thresholding. As this is a well-established, peer-reviewed approach for determining the uncertainty of topographic changes, we think it is very suitable.

## 5. Results

L326, Figure 4: Which kind of error is shown? Flow velocity error? Please annotate appropriately.

Yes, the flow velocity error is shown here.

We have indicated this accordingly.

Boxplots: The paper includes several boxplots. Maybe, it's good for the understanding to describe in one sentence what is shown.

We agree. We have now added a general description of the boxplots to the caption of the first boxplot in the manuscript (Fig. 4) and we refer here to the following boxplots. (L329-331)

L328: Period is missing: ... RG08.

Done.

L399: Figure 6: What are the black dots? Outliers? Do the 'black dots' influence the mean value? Or asked differently: The computation of a mean value is somehow problematic considering masked areas or incomplete areas.

Yes, the black dots are outliers, spatially they are often found at the terminal part of the rock glacier. These are included in the calculation of the mean value. Since the same areas are excluded for each epoch, we believe that a comparison of the mean values is conclusive. In addition, it is only one means of comparison alongside the flow velocity maps, boxplots and violin plots. From another point of view, GPS point measurements on rock glaciers, such as those carried out by PERMOS, also produce mean values, even though the rock glacier was not measured over the entire area.

We have added a sentence in the methods section stating that due to the snow masks the mean values might not be representative for the entire rock glacier, but are only valid for the areas in which measurements were possible in all time periods. (L267-268)

L410, Figure 7: This Figure is rather complex and small. Please add annotation of the vertical axes of the violin plots.

We know that the illustration is somewhat complex, so we will describe it better in the caption. In order to save space and to be able to display the images in a larger format, we have dispensed with a description of the axis and linked it to the images using colour codes (e.g. 2012 - 2017 yellow).

We have now described this in the caption.

Background image is always the older date of the epochs(?). Remark: Timespan is the difference between two epochs.

We have replaced epoch with timespan.

Paragraph 5.5: The reviewer thinks that the computation of a mean surface elevation change and its equivalent volumetric change is questionable. As indicated in the paper analyses were restricted to certain areas only or to incomplete catchment areas. Thus, results obtained have to be analysed critically.

As described before we are aware of this and address this issue in the discussion of surface elevation changes (L681 – 683, L691-693, L709-711). Since permanent snow patches often occur at altitudes where rock glaciers are found, it will hardly be possible to prevent this.

We have now mentioned this again at the beginning of chapter 5.5 and added a reference to the discussion here. (L453-455)

Remark: The reviewer thinks that in Figure 9 the black error bars of the uncertainty of measurements are too optimistic. It would be good to add at least one critical sentence about the significance level of the volumetric change.

The uncertainties were determined according to Anderson (2019) as this is a well-established, peer-reviewed approach for determining the uncertainty of topographic changes, we think the uncertainties are valid. Anderson (2019) shows that if the systematic error is very small, positive and negative errors balance each other out and thus the total uncertainty, is then largely determined by uncorrelated and correlated random errors. This means that the total error is small if these two error components are small. This is described in chapter 5.2 Errors and uncertainties (L333-341). In the supplement, the different types of error are broken down and specified to three decimal places. Since the uncorrelated and correlated random errors are very small, they are often given here as 0.000.

L504: typo previous

Done.

Paragraph 5.6: If there is surface-parallel flow/creep mean flow velocity and mean surface elevation change are highly (100%) correlated. Because of differential mass movements (straining) proper interpretation of subset areas is rather difficult. (comment of editor: I agree with the referee but I think you already discuss this issue pretty well, so not much to do here).

We agree and thank you for your assessment.

We are aware that the analysis is not perfect due to natural conditions (e.g. snow patches) or missing data (e.g. information on the internal structure of rock glaciers and its change), but we clearly address these drawbacks and nevertheless present valid and meaningful results.

L554: typo multidecadal multi-decadal

Done.

6.(?) Discussion

Analysis is still speculative and open for discussion and further research. (comment of editor: I do not expect you to change too much here, but maybe the tone of the interpretation can be a bit more open).

In our assessment, we do not make a hard statement for any of the considerations and validate our hypotheses with existing literature. Nevertheless, we have chosen a more open tone in some places:

Sentence added: "Although a direct connection cannot be proven by the qualitative analysis."

Rplaced: "...can be seen..." by "...might be seen..."

Rephrased: "This possibly led to..." "This may have potentially led to..."

Figure 12: Figures (c) and (e) are rather small.

We agree that the figures are small. Since they are important and we want to present the seasonal data alongside the annual data, but do not want to create an additional figure, we have decided to present it this way.

We have enlarged the sub-figures c) and e) a little.

7 Conclusion

L704 Use kinematics instead of dynamics.

Done.

List of comments by editor that should be addressed

In general:

- check and better describe the captions again, quite a few figure are not explained that well (box-plot, etc...)

We have revised some of the captions.

- please carefully proofread the manuscript at the end again, preferably by a native english speake, to avoid any issue of editing, language/formulations, etc..

We read the manuscript carefully at the end and corrected some mistakes.

L13: nine or eight rock glaciers respectively

Done.

L16: to variable degrees

Done.

L16/17: Rock glaciers related to glacier forefields showed...

Done.

L26: ...rock glaciers in the same catchment...

Done.

L32: 'common layers' awkward formulation, maybe better say 'They suggest rock glacier to be similarly composed by such layers, although...

Done.

L37: ...therefore the temperature...

Done.

L84: I would be more specific in referring to the Copernicus data (official reference, weblink where/how downloaded etc...)

We have referred to the copernicus data used according to their guidelines.

We have now added a web link.

Line 97: which (not wich)

Done.

Line 102: I think it should be in plural: ...velocities of rock glaciers ...

Done.

Line 130/131 and also in table 2: why saying 'chair of physical geography', better say the name of the responsible person or just mention the institute, Physical geography....

We have used "chair of physical geography" because there is no association with an institute at the university. Chair of physical geography does not refer to a single person but is used as a synonym for the physical geography workgroup.

Line 136: typo, there is a space missing between 'investgated' and 'rock glacier'

Done.

Line 161, caption: Style, repetition of word distance, maybe say: The distance is given to the center of the study area in (km).

Done.

Line 171: delete 'the DFG founded project', this is irrelevant repetition, just say 'of the PROSA project'

Done.

Line 182: '...from the aerial images...'

Done.

Line 366, in caption: there is a full stop missing after 'forcing'. Also the 'mean' at the end of the line should start with a lower case 'm'.

Done.

Line 379/380: this sentence can be deleted or moved to discussion, here you present the results! 'Throughout the Alps, there is only a very small or no clear trend with regard to the annual precipitation development in the 20st century (Beniston, 2006).'

We agree and have removed the sentence.

Line 398ff/caption fig 6: better explain the box plots (what percentiles used, black dots? Medians or means shown,...what are red boxes and dots.... Just be a bit more specific in the caption.

Also what is 'KT 09'? do you mean 'RG09'?

As described earlier, we have included a general description of the boxplot parameters used in the caption of the first boxplot in the manuscript, with reference to the other boxplots (Fig. 4). In Figure 6, we also explain the red boxes, dots and the outliers.

Yes, we mean RG09 and have changed this accordingly.

Line 408: ...characterized by relatively...

Done.

Line 411/caption Fig 7: the 'violin' plot could use some more info and it should be: 'The units of ...'

We agree, although these are a helpful illustration of the development of flow velocities, they can be a little confusing if you don't know what is being depicted.

We have included a short definition of a violin plot in the caption and explained its colour coding (L416-419). Even before the revision, the text briefly explained how to read the violin plots (L445-447).

We have corrected "The units of...".

Line 415: ...has to be put into perspective, ...

Done.

Line 431: style, repetition: replace 'had zones with higher and lower flow velocities' by 'flow velocities varied spatially'

Done.

Line 440: I think it should be 'give an indication'

Done.

Line 448: ...are also shown...

Done.

Line 449, caption: where are these greyed out areas that are stable?????

These are located to the right of the measurements on the respective rock glaciers. For each rock glacier, there is one group of four measurements on the rock glacier and one group of four measurements on the stable areas (greyed out) to the right.



We have added a text to the figure to make this clearer.

Line 459: ... are of TC-type...

Done.

Line 461: style, ...In contrast for RG06....

Done.

Figure 9: I suggest to label the x-axis properly with the years of the epochs rather than numbers.

We agree. Since the long years (e.g. 1953-1970/71) make it very confusing and narrow to label the x-axe directly, we have inserted the years as a legend in the figure.

Line 495: rather confusing figure references, I think you mean: '...we have plotted their mean values for subareas of the individual rock glaciers in Fig. 11, with the subareas shown in Fig. 10. '

Done.

Lines 496/497: add a comma after each numbered point, e.g.: (1) representative of the rock glacier, (2) both

Done.

Line 504: previous (not prvious)

Done.

Line 507 caption: '...and the four epochs...

Done.

Line 624: an overarching summary of the figure would help: 'Flow velocity variations in context of climatic forcing. (a)...

Done.

Line 681: ...on TC-type rock...

Done. We have also corrected this elsewhere in the manuscript.

Line 760: a space is missing between 'the' and 'study' (thestudy)

Done.

## References

Anderson, S. W.: Uncertainty in quantitative analyses of topographic change: error propagation and the role of thresholding, *Earth Surf. Process. Landforms*, 44, 1015–1033, <https://doi.org/10.1002/esp.4551>, 2019.

Fey, C. and Krainer, K.: Analyses of UAV and GNSS based flow velocity variations of the rock glacier Lazaun (Ötztal Alps, South Tyrol, Italy), *Geomorphology*, 365, 107261, <https://doi.org/10.1016/j.geomorph.2020.107261>, 2020.

Kaufmann, V., Seier, G., Sulzer, W., Wecht, M., Liu, Q., Lauk, G., and Maurer, M.: Rock glacier monitoring using aerial photographs: conventional vs. UAV-based Mapping - a comparative study, *Int. Arch. Photogramm. Remote Sens. Spatial Inf. Sci.*, XLII-1, 239–246, <https://doi.org/10.5194/isprs-archives-XLII-1-239-2018>, 2018.

Kummert, M., Bodin, X., Braillard, L., and Delaloye, R.: Pluri-decadal evolution of rock glaciers surface velocity and its impact on sediment export rates towards high alpine torrents, *Earth Surf. Process. Landforms*, 105, 113, <https://doi.org/10.1002/esp.5231>, 2021.

Wheaton, J. M., Brasington, J., Darby, S. E., and Sear, D. A.: Accounting for uncertainty in DEMs from repeat topographic surveys: improved sediment budgets, *Earth Surf. Process. Landforms*, 25, n/a-n/a, <https://doi.org/10.1002/esp.1886>, 2009.