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Interactive comment on "Inventory, motion and acceleration of rock glaciers in Ile Alatau and Kungöy Ala-Too, northern Tien Shan, since the 1950s" *by* Andreas Kääb et al.

Alessandro Cicoira (Referee)

alessandro.cicoira@geo.uzh.ch

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Interactive comment on "Inventory, motion and acceleration of rock glaciers in Ile Alatau and Kungöy Ala-Too, northern Tien Shan, since the 1950s" by Andreas Kääb et al. Cicoira Alessandro (Referee) alessandro.cicoira@unifr.ch

General comments:

This manuscript investigates the distribution and the motion of a large sample of active rock glaciers (551 landforms) in norther Tien Shan. The rock glacier inventory is not exhaustive and does not include inactive and relict landforms. In addition, the

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movement of some other landforms, such as debris-covered glaciers and ice-cored moraines, have been classified (900 landforms in total). A combination of satellite based radar interferometry and feature tracking of optical imagery allows the classification of activity classes for all the landforms mapped. For six rock glaciers the authors also perform the calculation of inter-annual variations in surface speed over a period of almost 70 years. By doing so, they provide the first long-term regional investigation of rock glacier motion in the Tien Shan. The methodology is thoroughly described and the uncertainties properly addressed, and the authors discuss the limitations of the manuscript, overall with high scientific rigor. The conclusions are reasonable and coherent with previous research. However, the conclusions are supported by little and at times controversial evidence and should be discussed with more caution. A strong link to Sorg et al. (2015) is evident and well explained in the introduction, but the discussion is only briefly addressing the relation between the two manuscripts. In general, a more detailed discussion would help placing the manuscript in the context of current research and highlight its novelty (extension of rock glacier kinematic observations in the Tien Shan massive). Several minor revisions and a few possible additions to the manuscript are suggested in the specific comments below. Concluding, I consider the manuscript well suited for publication in The Cryosphere after minor revisions.

Specific comments:

Page 1, Line 23: Please delete “very” in “very high resolution data”.

Page 1, Line 23: Most of them reads three out of six. It would be very interesting to include an analysis of the regional scale evolution of surface speed, which would largely improve the confidence in the general conclusions. This might be possible on the basis of the readily available InSar or optical data. The resolution of such an analysis might be lower compared to the one provided for the six study cases investigated in detail. If this analysis is not done, it should be made clear in the text that the conclusions are still rather speculative and based on preliminary results and similitudes to other regions (Alps).

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Page 1, Line 26: The comparison between rock glacier and glacier sediment transfer is poorly constrained in the manuscript. Please provide more information and a context. Please add some relevant references from the literature, and extend the discussion. This might require some additional analysis and a quantitative regional-scale comparison of the two contributions.

Page 1, Line 27: The relation between the stress regime (compressive flow regime) and changes in speed upstream is mentioned, but only superficially discussed, without any reference to rock glacier dynamics and their mass fluxes.

Page 1, Line 28: The conclusion that the Gorodetsky Rock Glacier does not show an acceleration due to the decoupling from its rooting zone contradicts the previous point. It also partly contradicts the hypothesis that rock glacier acceleration is due to warming air and ground temperatures. The two mechanisms are not exclusive, but their interaction is not straight forward and requires a more detailed discussion in the manuscript (and possibly additional analysis).

Page 1, Entire Abstract: I suggest to rewrite the abstract in a more concise and specific fashion, in order to highlight the novelty and the merit of the manuscript.

Page 2, Line 2: Rock glacier move due to the deformation of debris-ice mixtures (as you also mention several times later in the text). Please correct this first sentence, which now suggests that they move due to the deformation of the frozen debris only.

Page 2, Line 3: Please correct the repetition: “rock glaciers, or (rock glaciers).” I can imagine that it is a typo for the one-word version “rockglaciers”.

Page 2, Line 10: Due to the ongoing debate about the genesis of rock glaciers and the origin of their constitutive material, I find the text insufficient for an introduction. I would avoid confusion and keep this topic out of the intro, also because it is not needed nor addressed further in the manuscript. If the authors believe that this is essential for their manuscript, I suggest that they extend the introduction and address the topic in more

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detail.

Page 2, Line 17: The concept of destabilization might be easily misunderstood and confused for structural instabilities. This being not the case, some additional explanation might be needed to present the concept of rock glacier destabilization. However, here again, I suggest to omit this point, because it is not needed in the manuscript. Alternatively, introduce it in more detail.

Page 2, Line 21: please add citations to Arenson and Springman (2005)a-b.

Page 2, Line 25: please be careful when considering the different processes. The response of permafrost creep to increasing temperature is thought to follow a power law. In addition, the role of water can enhance, also in a non-linear fashion, the response of creep to temperatures approaching melting conditions.

Page 2, Line 26: please consider the following references, which are in my opinion amongst the most important publications regarding this topic: Ikeda et al, (2008), Buchli et al, (2018) and Cicoira et al, (2019)b.

Page 2, Line 29: please specify “impacts”.

Page 2, Line 30: inventorying rock glaciers is typically done on the basis of a combination of optical and topographical data. Kinematic information (e.g. InSar) remains an optional and non-sufficient data source. Please be more precise in the text. The authors might refer to the Baseline Concepts for inventorying rock glaciers which are currently being elaborated within the International Permafrost Association IPA.

Page 3, Line 19: please be more specific in the wording. “followed” is not a technical term and might be misunderstood. I suggest to use the key-terms: qualitative – similar patterns, statistical correlation, phase lag, thermal offset, non-linear.

Page 3, Line 20: The influence of temperature forcing through heat conduction on rock glacier dynamics has been quantitatively investigated in detail in e.g. Kääb et al, (2007) and Cicoira et al, (2019)a-b. The authors might want to discriminate between qualitative and quantitative studies that have investigated the processes controlling variations in rock glacier creep and include the state-of-the-art knowledge on the topic.

Page 3, Line 21: The influence of variations in ground temperature through melt water advection has been shown to be negligible for the case of the Furggwanghorn in Buchli et al, (2018) and for the Ritigraben Rock Glacier in Cicoira et al, (2019). I am not aware of any study where the hypothesis (in the submitted manuscript) was tested on the basis of observational data nor modelling studies. If such study exist, please include the reference in the text in an explicit fashion. The study of Ikeda (cited in the text), also concurs to the hypothesis that rock glacier creep is controlled by variations in the effective stresses, rather than variations in ground temperatures (being these close to the melting point).

Page 3, line 21: as a general comment, I see the need of general revisions of the text about the processes controlling rock glacier creep.

Page 3, Line 24: At this point, the reader would expect temperature, precipitation and snow cover data to be analysed along the creep rates. I believe that the study presents enough new insights and does not need this additional step, but I suggest the authors to explain why it has not been done.

Page 4, Line 15: please specify the depth of the ground temperature measurements.

Page 5, Line 7: please consider replacing “rock” with “boulder” in all the appropriate cases.

Page 5, Line 8: add a point at the end of the sentence.

Page 5, Line 24: Please explain the reasons for the choice of the six rock glaciers.

Page 6, Line 1: I am not sure what “frozen snow” means. Please consider replacing this formulation with a more specific terminology.

Page 6, Line 12: The choice of assigning a polygon to the next class when the velocities

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are close to its upper limit seems unjustified to me. Also, what is the reasoning behind the division for the first classes (0-2, 2-10)? The next two classes are (half) an order of magnitude, so I wonder why also the first two are not consistent.

Page 6, Line 13: please specify the nature of the mentioned variations. (spatial variations?)

Page 6, Line 15: is there a reason why the vector of the observed displacements has not been corrected according to topography? Maybe extend on this point.

Page 6, Line 23: is this sentence a list of the criteria used for identifying and locate the rock glaciers? Currently, the sentence is somehow lost in the text. Please consider rephrasing it.

Page 6, Line 24: short-term variations in rock glacier velocity have been observed at many sites worldwide. Seasonal and even weekly oscillations are observed consistently. The statement is therefore unjustified. I suggest to support it with specific evidence for the study area (if available) or to discuss it in more detail. See Haeberli, (1985), Wirz et al, (2016), Strozzi et al, (2020).

Page 6, Line 25: it is now not clear to me weather the analysis has been conducted over multiple time steps. Maybe this has not been explained clearly enough in the text, or I just misunderstood it here.

Page 6, Line 19-30: is this paragraph a list of the criteria used to classify a rock glacier and distinguish it from a rock glacier? Please be more specific and explain in detail the concepts that have been used. I suggest to reference to the ongoing action group on rock glacier inventories and kinematics of the IPA.

Page 8, Line 11: please explain why the measure of accuracy was performed on stable ground only for three of the six field sites.

Page 8, Line 18-24: The results of this very interesting analysis are only briefly described in the manuscript. Also the discussion seem to me not sufficient to address

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this point. I suggest to include more details in the manuscript.

Page 10, Line 10: please specify that only (part of) the labelled rock glaciers are further investigated in the photogrammetric analysis, and not all the visible polygons in the figure.

Page 10, Line 10: please consider indicating the value of the wavelength for the data in the figure.

Page 10, Line 10: it is impossible from the figure to distinguish between the different classes of the polygons. This information is present only in Fig. 1 at a very low resolution. Consider improving the level of detail in this (Fig. 2) or in the following figures (Fig. 3-7).

Page 11, Line 3: consider replacing "photogrammetric velocities" with a more detailed terminology. (such as "Surface velocities calculated by offset tracking").

Page 11, Line 4-12: the determination of the origin of the ice and sediment constituting the rock glacier requires more than the observation of spatial connection, or as in this case, the (legit) supposition of past spatial connection. The state of inactivity of the current glacier forefield (called in the text "zone between glacier and main rock glacier") only shows that the connection is not currently present, but is not sufficient to imply that this (dynamical- and sedimentological) connection was present in the past. Even in this case, it would have been limited to the period when the glacier advanced to its maximum (LIA). No information on the climatic and sedimentological setting of the rock glacier is provided in order to commence such an analysis. Without entering in too much detail, I suggest to limit the discussion to the spatial connection (glacier forefield connected, according to Delaloye and . . . 2018) and avoid speculation regarding the "nourishment" and genesis of the landform.

Page 11, Line 15: please provide quantitative evaluation of the observed trend and its statistical significance.

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Page 11, Line 18: as above, consider removing the concept of “nourishment” from the paragraph.

Page 11, Line 19: consider removing “striking” or replace it with a more technical and specific adjective.

Page 11, Line 25: please describe in more detail the differences between the surface speed for this early period.

Page 11, Line 27: I suggest to improve this paragraph and give a better summary of the results for this analysis. Also, write explicitly what the calculated ice-content would be. It is implicit in the ratio, but the reader might be helped by some repetition here. Consider calculating it for all the available time steps and include an estimation of the uncertainty in the results.

Page 14, Line 11: In figure 4c, the debris-covered glacier and (for what I can see) the glacier forefield are not shown and it is impossible to verify the presence of a material flux from the rock wall to the glacier. As previously, I consider the observational evidence insufficient to support the statement. I suggest to simply avoid the point, which is in my opinion not important for the present manuscript, or to include more data and analysis to support this thesis.

Page 14, Line 24: the deformation profile (on the vertical dimension) also has an important influence on this calculation. Why is it not mentioned here? Please consider spending some words about this point to make the text clearer.

Page 15, Figure 5: it would be interesting to see the displacement on stable terrain.

Page 16, Line 2: what is the mass flux at the boundary between the glacier and the rock glacier? What is the absolute value of the surface speed? It is very hard to see this from the figure provided. In general, a similar comment as for the nourishment above.

Page 18, Line 15: here the authors state that the sediment transfer between glacier and

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rock glacier cannot be determined with the available data. I agree with the conclusion, but still, I would like a more quantitative discussion. Otherwise, I suggest again to completely avoid this point. A possible analysis would investigate the relation between acceleration and max fluxes along flow lines.

Page 18, Line 15: more information about the vegetation could be interesting. What is the size and what are the species growing on the rock glacier? Is this information available from field expeditions?

Page 18, Line 21: the fact that the observed signal from feature tracking is lower than the noise does not allow to conclude that the rock glacier has accelerated. I don't understand why the authors mention "statistical significance" in their argument.

Page 22, Line 1: I agree with the statements, but I would like a more quantitative evaluation of the different error types.

Page 23, Line 2: I agree with the authors. Still, it would be interesting to see the values of the errors on stable ground.

Page 23, Line 7: the divergence of a vector field is a well defined term and as far as I understand this is not what the authors mean. I suggest to replace this substantive with a more pertinent description of the differences observed in the velocity field.

Page 23, Line 10: I have not found any values for the statistical analysis of the velocity time series. I warmly suggest to implement this analysis in a quantitative way. If the authors prefer not to, I would be much more careful talking about statistical significance.

Page 23, Line 25: it would be valuable to have a better quantification of the "strong compressional regime" by means of e.g. strain rates (accompanied by proper interpretation or even with the calculation of the internal stresses).

Page 23, Line 27-29: First, it would be very welcome to see the original results for the upper part. Second, I don't agree with the statement that the lower part responds passively. It is a matter of dynamics, thus of mass and momentum fluxes. I suggest

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to change the formulation, possibly mentioning that the dampened response (I would appreciate an illustration of this) is due to topographical setting and the corresponding dynamic behaviour of the rock glacier. (In detail it could be that the mass flux is mostly compensated by variations in thickness or in mass input rather than variations in velocities, but such a statement should be supported by more evidence.)

Page 23, Line 29: please repeat the advance rate and the surface velocity, and consider discussing the result in more detail – also with a possible range of quantitative values of the ice content.

Page 23, Line 32: given the strong similarity between the two publications, this point might require some more discussion. I would suggest also one or two figures in the appendix or some additional comparisons.

Page 24, Line 10: If I understand this correctly, it means that it is not possible to conclude which one of the two studies is more accurate. If this is the case, please state it more clearly in the text.

Page 24, Line 24: consider citing Cicoira et al., 2019b, where it has been shown that the seasonal and inter-annual variations in rock glacier flow are mostly controlled by variations in snow melt rates and liquid precipitation, rather than in air temperature. Other very relevant citations are Buchli et al., 2018 and Ikeda et al., 2008.

Page 25: in general, in the "5.3 Speed time series" paragraph, more discussion relative to the results and their validity would enhance the validity of the manuscript. Most of the discussion is a very precise comparison to Gorbunov et al., (1992), which could be probably summarized in one or maximum two paragraphs. I suggest to highlight more the originality of the manuscript and discuss better its strength and weaknesses.

Page 25, Line 26: I don't see the link the negative glacier mass balance. Please avoid this point or argument in more detail the linking mechanism.

Page 25, Line 29: it would be very interesting to quantify this sediment transfer. I

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suggest using a simple assumption for the rock glacier thickness (e.g. constant value of 20 meters, see Cicoira et al., 2020) and estimate the overall ice/sediment transport rates for the periglacial environment. This would be a major result, and is not very difficult to calculate (although the uncertainty will be large).

Page 25, Line 31: such a statement definitely requires a quantification of both the sediment transfer.

Page 26, Line 4: this point is very interesting but insufficiently discussed. I suggest the authors to implement it both in a qualitative and in a quantitative fashion.

Page 26, Line 5: This statement appears unjustified to me. As far as I know, no quantitative (and conclusive) evidence that a rock glacier derived from a glacier exist. As for this manuscript, there is not sufficient evidence supporting the statement. Often, an interaction (more or less important) has happened during the LIA. I suggest to rewrite this last paragraph with more focus on the novelty of the manuscript (it is not the geomorphological genesis of the rock glaciers).

Page 26, Line 18: it is not so clear to me which assumptions. Please be more explicit in the conclusions.

Page 26, Line 26: the time scale considered in the manuscript is (almost) only decennial. I would rephrase this sentence and highlight the fact that the original observations in speed also show past periods of acceleration at the investigated temporal scale.

Page 27, Line 4: I am not completely convinced by this statement. The quantification of the trends and their significance for each rock glacier might make this point more convincing and increase the confidence in the results and the conclusions.

Page 27, Line 10: I don't agree at all with this statement. This is very speculative and not supported sufficiently by the evidence provided in the study. As above, I suggest to discuss it in more detail or to avoid this point, which is in my opinion not relevant for the manuscript.

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Page 27, Line 10-15: on the contrary I welcome the topic as an outlook for future studies. With this last comment, I thank the authors for an interesting piece of research.

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