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The Cryosphere Discussions

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.

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Kääb et al. inventoried active rock glaciers and other periglacial landforms in Tien Shan using InSAR and further analyzed the spatial-temporal patterns of six fast-moving glacial-derived rock glaciers using optical offset tracking. They present a nice piece of work that shows the value of radar and optical remote sensing for mapping and monitoring creeping permafrost landforms over a large and hard-to-access areas. The study focuses on a region that had not been intensively documented before. The contribution is well suitable for a publication in The Cryosphere and likely to become a reference in remote sensing of mountain permafrost.

I have no major concern regarding the way the study has been designed but I think the manuscript could benefit from some clarifications about the inventory procedure, a better visualization of the results and an extended discussion on the findings. Here I develop three main elements. I also listed some complementary suggestions of correction at the end of the review.

–Main comment 1. Inventory and movement classification–

For the movement types, I think it should rather be named 'landform types' as e.g. a rock glacier or a moraine is not a process but a landform that has been shaped by a periglacial or glacial process. Consider also renaming 'solifluction / debris movement' (not sure what debris movement means; a rock glacier is also composed of debris. Scree deposit?). 'Dead ice / subsidence' could just be 'thermokarst'?

About the velocity classes in the inventory: The choice of the velocity classes needs at least to be explained. 0-2, 2-10, 10-50, 50-100, >100 does not look so intuitive to me. Why not equal intervals: e.g. 0-25, 25-30, 50-75, 75-100. Or 0-3, 3-10, 10-30, 30-100, >100? Or just an order-of-magnitude (cm/yr, cm-dm/yr, dm/yr, dm/yr-m/yr, m/yr), as now recommended by the IPA action group 'rock glacier inventories and kinematics'? The criteria for defining the class are also not fully clear: when writing that 'if two or more classes were present during the observation time-span, the higher displacement rate was used to determine the velocity class' (l.14-15, p.6), does it mean that a little fast-moving part of the rock glacier could lead to classify the whole landform as for ex. >1m? If so, that sounds a bit bold to me. Maybe it should just be acknowledged in the discussion that it could lead to a rate overestimation. Similarly, at l.3-4 (p.7), it is acknowledged that the max. measurable rate for a C-band with 12d repeat-pass is 85 cm/a. To my understanding, a decorrelated part in a 12d-interferogram means >85 cm/a, but so not necessarily >100, right? In these cases, were the 1d ERS interferograms used to ensure a correct classification? If yes, it could just be mentioned.

–Main comment 2. Figures–

Most of the figures could benefit from slightly more job to clearly disseminate the findings. Considering the volume of work that the study represents, it is a bit a pity if the presentation does not fully help to maximize the understanding of the results.

Figure 1: This map is hard to read. I understand the wish to be comprehensive, but it is most likely too much information in once and the color contrast does not allow for differentiating the velocity classes for specific landforms. I would suggest making two maps: one with only the different movement types, without any velocity classes. And another focusing on rock glaciers (the main point of the article) with a different color for each velocity class (for ex. green to red, blue to red). A similar map for the other landform types can potentially be placed in Supplementary.

Figure 2: Add the equivalent in cm of the phase color scale. Missing information about SAR geometry (LOS arrow or mention of the ascending/descending geometry in legend).

Figures 3-7: It would be more comfortable for the reader if the optical images always had the same extent and scale than the velocity field maps. The error bars (especially the red) are hard to see due to the transparency. The arrows of the photogrammetry results are sometimes impossible to see and interpret (Fig. 5-7). Less importantly: it could be nice to have altitude references at some locations on the optical images.

Figure 8: Add the equivalent in cm of the phase color scale. Missing information about SAR geometry. Here, as the background map from the interferogram is not directly related to the size of the arrows (contrary to Fig. 3-7), it would be good to add a legend of the arrow length. It is a really nice figure to show the agreement of both methods, as well as the value of combining them. What about adding the same for the five other rock glaciers (potentially in Supplementary)?

–Main comment 3. Time series and acceleration–

There is something that does not completely add up in the interpretation of the tem-

poral variations: contradictory information that gets the reader a bit lost at the end of the paper. The title includes ‘acceleration of rock glaciers’ and it is presented in the abstract that five of the six temporally investigated rock glaciers exhibit acceleration (l.23-24, p.1). However, at several locations, the authors acknowledge that the statistical significance is not always good enough to confirm the trends (several cases with std dev of speed differences greater than the actual measured difference, e.g. Fig. 7c). As explained at l.28-29 (p.24), ‘Gorodetsky, Morenny and Archaly rock glaciers [. . .] are not showing a clear increase of speed over the study period’. Gorodetsky, Moreeny and Archaly are three out of six rock glaciers, which does not fit anymore with the findings’ summary in the abstract (l.28, p.1) or the conclusion (l.4, p.27). Maybe I misunderstood something, but in that case, it is probably possible to be clearer in the text.

The statement ‘the behaviour observed in the study area confirms findings [. . .] that rock glacier creep speed increase overall under atmospheric warming’ (l.23-24, p.25) sounds a bit too bold to me considering that no comparison with temperature is provided in the study. Other elements could be discussed in greater details. In 5.4, the authors remind that ‘all rock glaciers studies in detail in this study are derived from contemporary or former glaciers’ (l.5, p.26). Some fuzzy thoughts here: When did the glacial direct connection expected to have stopped? During the time span of the study (50th-70th)? If yes, are some of the variations related to the glacial dynamics instead of the permafrost creep, or due to the transition between the two flow types? What if you had also analyzed talus-derived and slower landforms? Not to say that it should be done here, but it could be discussed as a prospect. Some of the conclusions here are maybe only valid for glacial and extreme (>1m) cases.

I don’t think this comment requires critical changes. The results are well described in Section 4, but the discussion could be improved (especially 5.4) and the abstract/conclusion (and even the title?) better matched the actual findings.

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–Complementary comments–

Page 1:

- Title: ‘acceleration’. Maybe consider ‘Inventory and spatial-temporal trends of. . .’ See main comment 3.

- l.23 and l.28: ‘most of them’. . . ‘The only rock glacier’. . . Maybe just write the actual number in both cases: five of them – the only, if this is really the case. See also main comment 3

- l.30: maybe missing a general sentence in the abstract about what does it tell, what is the relevance of the study in a broad sense?

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Page 2:

- l.3: rock glaciers (or rock-glaciers) I guess

- l.9: ‘In a similar way, the ice content [. . .] can be directly incorporated. . .’

- l.19: As rock glacier distribution is also a valuable environmental indicator, I would suggest removing the four first words of the sentence.

- l.21: ‘. . .although it is also influenced by. . .’

- l.22: ‘. . .temporal and spatial variations of ice content. . .’ I guess lateral variations are as important as vertical?

- l.25: ‘. . .variable around thawing conditions’. Maybe ‘sensitive’ instead?

- l.25: ‘. . .i.e. in cases the ice content starts to degrade. . .’

- l.29: ‘impacts from spatio-temporal variations’. Impacts on what in this context?

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Page 3:

- l.3-5: Start of the sentence (‘in addition to . . .,’) does not sound necessary to me. Instead, maybe: ‘The categorization of landforms into classified movement rates or orders of magnitude (e.g. cm/day, ...)’

- l.8: (more details below) is not really useful.

- l.17: ‘constant speeds’ instead of ‘stable speeds’?

- l.19-20: repetition in these two sentences (creep velocity <-> temperature)

- l.24: ‘for the first time’ sounds a bit weird considering the extensive literature presented in review 2 and reference to Sorg et al. article. What about ‘The aim of the present study is to systematically inventory contemporary rock glacier crepp velocities. . .’

- l.27-30: last two sentences are maybe not necessary.

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Page 4:

- l.5-6: potential to rephrase here ‘with the largest of magnitude8.0 and more [. . .] and stronger earthquakes. . .’ Stronger than 8 and more? . . .

- l.9-10: Sth unclear with that sentence: ‘. . .and for elevations of about 3000 m asl.’ Does it mean ‘For elevations of about 3000 m asl, mean annual precipitation exceeds 1000 mm on the northern slopes and is less than 800 mm on the southern ridges.’?

- l.18: larger than what? ‘. . .permafrost is very likely although the fronts of the large rock glaciers are located. . .’

- l.25: ‘inactive ones’: maybe wise to define here the activity categories. What about relict landforms? Are they considered in the study? In the activity morphologically determined or using InSAR/photogrammetry?

- l.33: ‘Blöthe et al. (2019) mapped. . .’

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Page 5:

- l.7: ‘Displacement rate of individual blocks vary greatly. . .’

- l.9: ‘blocks’? Replace ‘down-valley’ by ‘slope direction’?

- l.17-18: New sentence at ‘Rock glacier activity/tree growth anomalies. . .’? Respectively? To what? Not clear.

- l.21: ‘different statuses of the rock glaciers on their path towards. . .’ -> ‘different stages’?

- l.25: remove ‘for the offset tracking work realized here.’

- l.30: ‘short spatial baseline and time intervals between 1 day and three years. . .’ According to your table A1, the max. interval is over one year.

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Page 6:

- l.5: I guess $\frac{1}{4}$ of a phase cycle’ will be abstract for most of the readers. Maybe add what it means in terms of displacement?

- l.9: I think that 1-2 sentences could added here to quickly explain the principle. Count the fringes on coherent parts and relate it to velocity considering the wavelength interval between pairs + use decorrelation information. In the paper, you show only 12d Sentinel examples, but it is important to be clear that you used a large stack with different wavelengths/intervals/periods to get different detection capability and avoid misinterpretation of short-term velocity variations. Someone who does not know the technique could misunderstand if not quickly explained here.

- l.11: Why these classes? See main comment 1.

- l.14: ‘the highest displacement rate. . .’ See also main comment 1.

- l.15-16: ‘We empirically considered the line of sight in the assessment’: It could be explained what it practically means. If the slopes significantly deviating from LOS and between two classes, the highest cause it is considered as underestimated?

- l.16-18: last sentence of paragraph could be moved after first sentence of the next one (l.20).

- l.25: Check this sentence. I doubt you get coherent information on supraglacial ponds.

- l.27-28: Similar appearance to what? Check this sentence. I don’t see why solifluction would give spot-like fringe patterns. Also: debris movement does sounds like right terminology. See main comment 1.

- l.30: Redundant. Remove ‘also optical images were not of help due to’.

- l.32: Consider using everywhere 100 cm/yr instead of 1 m/yr, as in Fig.1

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Page 7:

- l.2-3: >85 could also mean 90, which is in 50-100 class. See main comment 1.

- l.30 and 32: ‘the above’: not necessary.

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Page 8:

- l.4: thresholds on correlation coefficients -> the actual values of the thresholds could be documented.

- l.12: the point (ii) sounds odd to me: looking at the chosen areas on the figures, they are significant velocity variations that are likely to be natural (for ex. C on Fig.6). Not possible to select areas with more similar patterns? What about the correlation

coefficients, the average correlation cannot be used as an accuracy estimate?

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Page 10:

- Legend Figure 2: ‘Wrapped orthorectified interferogram’? Raw sounds weird, and not especially right considering the corrections described in Section 3.1.

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Page 11:

- l.5: ‘agree well with the interferograms’. Plural? To emphasize that several interferograms have been investigated.

- l.20: ‘they show coherent fringes’

- l.24: ‘the central part of Morenny rock glacier speeds are in the order of 1 m/a: It does not look like that in Fig.4, most of the rock glacier is blue and the averaged time series show values clearly under 1, even for the highest parts of the series. . .

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Page 14:

- l.16: see main comment 1: in theory, noise could also mean 90 cm/yr, so can we really say consistent?

- l.24: ‘This points to block movement of the entire rock glacier column. . .’ Sounds odd, potential for rephrasing.

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Page 16:

- l.6: ref. to Fig. 6e

- l.12: ‘because of the many different frontal parts’ -> ‘because of heterogeneities’?

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Page 18:

- l.19-20: I don’t see the point of adding zone B if the results are not shown. Either the tseries are shown somewhere (potentially in Supplementary) or you maybe don’t mention this part at all? About ‘changes documented for zone B [..] are within their error bars’: looks like there is the similar problem for A since the 90th (Fig.7 c).

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Page 19:

Figure 7: Remove area B) if not used? See also main comment 2.

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Page 20:

- l.4: ‘reasonably very’ -> just reasonably?

- l.6-7: ‘all rock glaciers [. . .] were classified correctly from the interferograms’: does not sound right for Morenny (see comment page 11, l.24)

- l.22-23: see main comment 1: so 85 cm/yr is approximate as 1m/yr?

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Page 22:

- l. 2: missing space ‘very-highresolution’

- l.4: ‘more difficult to quantify precisely’: Sounds weird cause I don’t think biases a) have been quantify in the manuscript.

- l.6: ‘could translate 1:1 to a lateral offset’: without being an offset-tracking expert, I

must say that I don't get this.

- l.24-25: if median values are safer, why not have used these for the point clusters?

- l.33-34: here comes quickly what I commented for p.8, l.12: maybe it could come before in the methods?

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Page 23:

- l.23: 'acceleration between 1964-2017'

- l.26-29: make two sentences. For ex: the lowest rock glacier part deforms under a regime... It responds rather passively to...

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Page 24:

- l.5: 'the latter part, under a strong compressive creep regime, reaches and diverts the river...'

- l.28: 'are the only systems'. 3 out of 6... sounds weird to say 'the only'. See main comment 3.

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Page 25:

- l. 24: maybe 'is aligned with findings' instead of 'confirms'?

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Page 26:

- l.11: 'an inventory of active rock glaciers and other periglacial landforms'? or 'a slope movement inventory including rock glaciers and other periglacial landforms'?

- l.16: well, maybe it is me being picky, but 85 cm is not a meter. And again, what about the use of ERS 1-day? See main comment 1.

- l.28: 'A possible explanation [...] is...'

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Page 27:

- l.2: '...more passively to...'

- l.4: Not in line with l.28-29 p.24. See main comment 3.

- l.10-12: this could be more discussed in 5.4. See main comment 3.

- l.12-15: The last sentence is too long and hard to understand. I would suggest as prospect to include not only glacial-derived and landforms with lower averaged velocity, to confirm the accelerating trend.

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Page 29:

- Table A1: Why no 2017 Sentinel-1 image?

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