Report #1

Second review to the article from Hu et al. 'Modelling rock glacier ice content based on InSAR velocity, Khumbu and Lhotse Valleys, Nepal':

I would like to acknowledge the extensive work performed by the authors to address my comments from the first review. The structure is much better and it is therefore easier to follow the workflow and understand the main findings. Assuming the second reviewer will cover the modelling part, this review mostly focuses on 1) suggestions to clarify InSAR elements and acknowledge limitations that were partly explained in the response to my review but not necessarily well included in the manuscript; 2) a list of detailed comments.

Note that the line numbers refer to the version without track changes (except for 4.1 section that is missing in the final version – cut by mistake I guess – but I found it in the version with track changes).

Re: We thank the reviewer for the constructive suggestions regarding a more effective presentation of the InSAR methodology, and the appreciable effort devoted to improving the manuscript in various ways. We consider these comments carefully and provide our point-by-point responses given below. The line numbers refer to the ones in the revised manuscript with track changes, aiming to help the reviewer and editors locate the revisions made correspondingly.

---InSAR method/results/discussion---

1. I understand from your explanation in the response to my previous review that the threshold of 5 cm/yr is both based on an uncertainty estimate and an assumption that everything < 5 can be assumed as not significantly moving and therefore not relevant for the study of active landforms. Sounds still weird to me cause it neglects areas that are inactive (or transitional) but may still contain ice, e.g. some of the areas that were inventoried based on morphological criteria but discarded in your InSAR analysis. But anyway, as long it is well explained, it is surely ok. I still find it hard to follow at l.244-262, so please just try to be very structured in this part (step by step / criterion by criterion). A real InSAR section in the discussion would also be nice (now you are mostly focusing on modelling).

   Re: We agree with the reviewer that we might underestimate the ice storage by discarding the inactive parts of rock glaciers. We have highlighted that our model is applied to the "coherently moving part" and discussed this issue in *Sect. 5.4 Limited application to the coherently moving parts of rock glaciers in quasi-steady-state motion* (L730):

   "Second, our model is suitable to be applied to the coherently moving part. However, the parts of rock glaciers are in a transitional kinematic status (practically defined as velocities < 5 cm yr-1) or move as an individual portion from the coherently moving parts. Moreover, the 1-D InSAR method may fail to detect some moving areas of the landforms creeping nearly along the satellite's flight direction due to the lack of sensitivities of the LOS geometry. These parts may also contain ice but are excluded from our estimation, causing possible underestimation of ground ice as well."

   Regarding the analyzing procedure of InSAR results, we have enriched the InSAR section and divided into three steps: *Step1: Interferometric processing*, *Step 2: Calculating downslope velocities from high-quality interferograms*, and *Step 3: Determining the velocities of the coherently moving parts as the model constraint*. Specific modifications are presented in the following replies to the example questions.

Examples from 3.5.1:

• l.245-246: 'Uncertainties were quantified…': Without mentioning the values you calculated, this sentence is a bit useless. Is it the 10 cm/yr error you are mentioning later (l.259)? And if yes, why to choose 5 cm/yr as threshold based on another reference (Wang et al., 2017)?

Re: This sentence was written to introduce the uncertainty sources (both interferometric and DEM-relevant) considered in estimating uncertainties. We changed the sentence a bit to highlight the main information we would like to convey at L292:

"The projection was conducted considering the satellite's flight direction, the local incidence angle, and the landform topographic parameters including the aspect and slope angles (Massonnet and Feigl, 1998; Bechor and Zebker, 2006). We considered the propagation of errors introduced by the InSAR measurements and DEM data which were used to determine the associated topographic parameters (Hu et al., 2021)."

For each interferogram, we quantified the uncertainty at the pixel-level; among all the high-coherence pixels, the largest uncertainty is 9.8 cm/yr, so we claimed that "for one pixel, the velocity error is < 10 cm/yr". Here 10 cm/yr is the maximum uncertainty instead of the minimum so we do not regard it as the measuring capability of our method.

In fact, the velocity threshold (5 cm/yr) is introduced because we aim to reduce the influence of atmospheric delays, which is supposedly removed by subtracting the phase values of nearby inactive pixels from the landform pixels (Step 1 in Sect. 3.5.1). This operation performs well provided that the detected displacement is significant compared with the reference area. Otherwise, the correction may not be effective due to poor signal-to-noise ratios. The specific value was determined by considering the phase distribution in the interferogram and the morphologic parameters of the landform used in calculating the downslope velocity.

We have provided the reason at L299:

"…the mean velocity of the landform is larger than 5 cm yr^{-1} (Wang et al., 2017). We set this empirical threshold considering the typical noise level (5 cm yr^{-1}) as we observed in most interferograms."

• l.248-249: Is the sentence 'the remaining pixels cover more than 40% of the surface' an additional criterion, i.e. did you discard landforms than have < 40% coverage? Not clear. If it is simply a fact/information, it should not be listed here.

Re: Yes, it is. We have listed as a separate criterion at L298:

"(2) the remaining pixels cover more than 40% of the landform surface;"

• l.250-252: 'Next, we defined and outlined… If the InSAR measured velocity is higher than 5 cm/yr in more than half of the periods…'. This sounds to me like an additional criterion and in that case why not listing it at l.249?

Re: The criteria listed at L.249 are used for selecting interferograms that cannot reliably represent the overall kinematics of a landform. In other words, we excluded some interferograms from further velocity analyses. Here at L.250–252, we listed another criterion to find the pixels in the coherently moving part from the series of observations constituted by the remaining interferograms. The two sets of criteria were put forward out of different purposes: one for selecting interferograms and the other for selecting pixels.

We have separated the two selections into Steps 1 and 2 in Sect. 3.5.1.

• l.255-256: 'an area actively in motion with the landform as a whole': that is a poor phrasing, and one could argue that by discarding areas within the morphological delineation, you don't consider the landform as a whole.

Re: We have re-written this part at L302:

"Field observations have revealed that multiple areas moving differentially can occur on rock glaciers and exhibit complex kinematic patterns (e.g., Buchli et al., 2018), which violates the assumption of a continuously moving body (Sect. 3.1, Fig. 3). Therefore, we aim to identify the coherently moving part of the landform that corresponds with our assumption and is thus suitable for model application.

After the data-refining procedure in Sect. 3.5.1.2, for each landform, the remaining interferograms constituted a series of observations spanning multiple years. Then we defined and outlined the "coherently moving part" of each landform by considering the time series of downslope velocity of each pixel acquired during the observational periods. If the InSAR-measured velocity is higher than 5 cm yr^{-1} in more than half of the interferograms at a given pixel, it was included in the coherently moving part of the landform. Otherwise, the pixel cannot be regarded as actively in motion with the coherently moving area but in an inactive or transitional kinematic status."

• l.258-259: 'For one pixel, the velocity error is < 10 cm/yr, and the error of the mean velocity is limited to < 1 cm/yr.' What is the difference between the mean velocity and the velocity error? If 10 cm/yr of error, why using 5 as criterion (l.249)?

Re: The first value (< 10 cm/yr) is associated with the velocity measured at each pixel, which has two sources of error, i.e., interferometric processing and DEM. The second error (< 1 cm/yr) is derived by error propagation when we calculated the mean velocity, which is the arithmetic mean of all pixel values covering a landform. In this case, the mean velocity error is the square root of the quadratic sum of all the velocity errors. In addition, the equations we used to derive the uncertainties can be found in our open-access codes (l.351–365 and l.401): https://github.com/cryoyan/DeeplabforRS/blob/master/read_raster_for_shapefile.py

We have defined the first error at L294:

"We considered the propagation of errors introduced by the InSAR measurements and DEM data which were used to determine the associated topographic parameters (Hu et al., 2021). For each pixel, we found the velocity error is < 10 cm yr^{-1}."

And second error at L329:

"The error of the mean velocity can be derived by error propagation of all the pixels taken into account, which is limited to < 1cm yr^{-1}"

We have explained the choice of threshold in the response to the first example question.

2. Figure 10: The color scale is confusing (0-30 cm/yr with discrete classes). If you discarded everything < 5, the scale should start at 5. I would also suggest showing differently areas with low

coherence and those that are moving under the threshold. It is not at all the same criterion, it can in some cases mean the complete opposite: e.g. loss of coherence due to too fast movement. It looks for example likely for the upper part of Fig.10e (and generally likely when using 70-92 days interferograms). An alternative is to add an explanation in the caption mentioning that the transparent pixels (areas without InSAR coverage) are either due to low coherent or low velocity. And maybe add a discussion about this in Section 5?

Re: We did not show the low-coherence or low-velocity area because this figure was primarily plotted to present the average velocities of the coherently moving parts which were determined from a series of interferograms spanning different observational periods, making it practically difficult to show the status of the masked-out areas. There can be various situations. For example, one pixel could show a velocity larger than 5 cm/yr in one interferogram out of five interferograms, and in the other four interferograms, this pixel lost coherence due to its fast motion. In the latter case, this pixel can be regarded as an individual moving part from the coherently moving part. It is common that one rock glacier has more than one moving areas. More importantly, decorrelation is not necessarily caused by fast movement. It can occur due to changes of surface conditions, such as the soil moisture, the shift between freeze and thaw status. Therefore, we tend not to over-interpret the kinematics of the decorrelated areas.

Therefore, we have added an explanation in the caption (L598) and mentioned this issue in the discussion (L715, presented in our response to the 1# comment), according to the reviewer's suggestion.

We have also changed the color bar to start from 5 cm/yr. In addition, the discrete classes are only one way to present the magnitude of velocity, as a commonly used format in the open-source software QGIS that we adopted for plotting this figure, The coloring scheme is continuous so that the velocities values presented are not discrete.

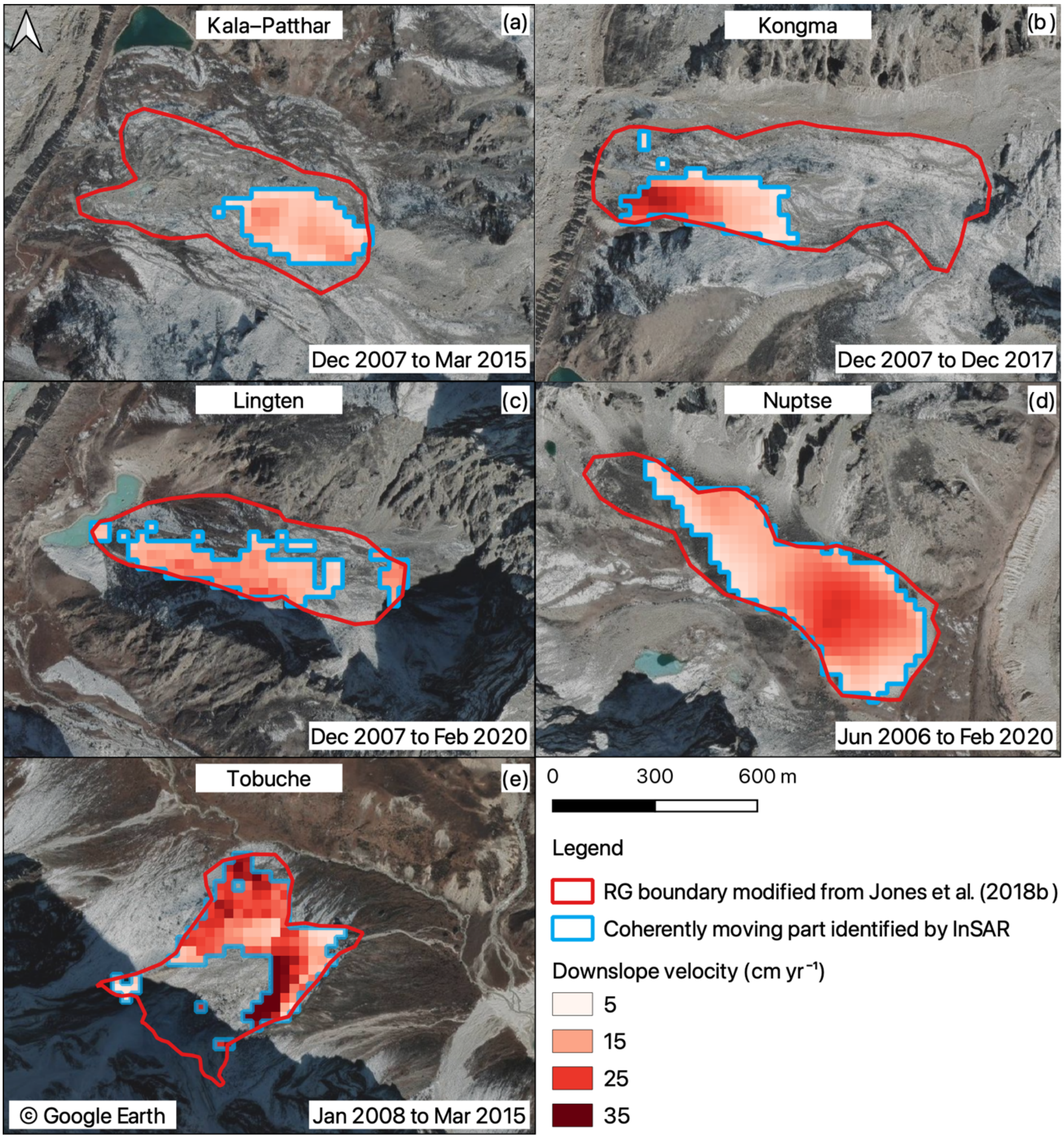


Figure 8: Velocity field maps show the average velocities of the coherently moving parts of the five rock glaciers (blue outlines) in the study area. The boundaries of the landforms delineated in Jones et al. (2018b) are in red. The transparent areas between the red and blue boundaries are due to low coherence or low velocity during the observational periods.

3. Another issue is the LOS measurements and the downslope projection (quickly mentioned at l.245 and partly discussed in 5.4 and 5.6). You now cover the question of potential subsidence, but I think it is also important to mention that LOS measurements can lead to significant underestimation on slope facing the radar or on N-S slopes (with creep direction orthogonal to the LOS), as in the cases shown in Figure 10a-c (a: because you mainly used ascending data; b-c: N-S facing). I believe it is no coincidence that the coverage is better for both cases facing away from the radar (d-e). You may have areas that fall under your 5 cm/yr threshold although that are in reality moving at cm-dm level. I am aware there is not much to do to solve this issue (except discarding these areas or applying multi-geometry InSAR methods as mentioned in 5.6), but the limitation must at least be clearly acknowledged.

Re: We would like to provide additional explanations to the issue the reviewer pointed out here. We acknowledged that our method may fail to perform effectively on rock glaciers creeping nearly along the satellite's flight direction and lead to weak signals in the interferograms recording the LOS phase shifts. Fundamentally, LOS measurements from InSAR are insensitive to any movements perpendicular to the LOS direction, resulting in completely omission of that particular component and an underestimation of the magnitude of the 3D velocity vector. If the true 3D velocity is exactly along the slope direction, the LOS velocity can be restored without an underestimation issue. In other words, the velocities presented in Fig 10a–c are not underestimated under the downslope motion assumption. In reality, the reprojected velocity might be underestimated or even overestimated depending on the geometry (1D downslope vs. 3D real downslope vs. LOS).

However, as the reviewer observed, the phenomena that data coverage is smaller for landforms moving nearly perpendicular to LOS direction, is likely to happen. This is because the very small phase shift (the phase difference between the subject pixel and the reference pixel, L286) may not be successfully recognized from the interferogram due to the poor signal-to-noise ratio (source of the noise is mainly atmospheric errors). Theoretically, the omitted part might move slower than the detected part, though may still lie above the threshold value.

We have extended the discussion to include this aspect at L732:

"Moreover, the 1-D InSAR method may fail to detect some moving parts of the landforms creeping nearly along the satellite's flight direction due to the lack of sensitivities of the LOS geometry. These parts may contain ice but are excluded from our estimation, causing possible underestimation of ground ice as well."

4. In general, your answer to my comments contains several elements/references to other studies that can be added to the article. I believe some of my questions may also be raised by other readers. Due to your choice of introduction structure and limited discussion regarding InSAR you have almost no reference to other studies using InSAR for mountain permafrost applications (except Wang et al., 2017). I think it will strengthen the article if you show you have read what other have done (either in the intro – as background/state of the art; in the method – to justify your choices; or in the discussion – to relate with the way others deal with similar challenges).

Re: We did not present a very detailed introduction of InSAR to avoid a very lengthy methodology section, but a more comprehensive list of relevant bibliography, as the reviewer suggested, would improve the quality of discussion. We have added the citations to place our InSAR work in context in method at L264:

"InSAR has been widely applied to quantifying surface velocities of rock glaciers (e.g., Bertone et al. 2022; Reinosch et al. 2021; Rouyet et al. 2019; Zhang et al. 2021). In this study, we adopted the conventional Differential InSAR method to derive the surface velocities by assuming rock glaciers creep along the slope direction (Brencher et al., 2021; Hu et al., 2021; Liu et al., 2013; Wang et al., 2017)."

And in the discussion at L755:

"In addition, a more accurate 2-D surface velocity can be obtained by using multi-track InSAR data (e.g., Bertone et al., 2022; Zhang et al., 2021), allowing us to apply the model to rock glaciers with a complex velocity field."

Reference

Bechor, N. B. D. & Zebker, H. A. (2006). Measuring two-dimensional movements using a single InSAR pair. *Geophysical Research Letters*, 33(16). https://doi.org/10.1029/2006gl026883.

Bertone, A., Barboux, C., Bodin, X., Bolch, T., Brardinoni, F., Caduff, R., Christiansen, H. H., Darrow, M. M., Delaloye, R., Etzelmüller, B., Humlum, O., Lambiel, C., Lilleøren, K. S., Mair, V., Pellegrinon, G., Rouyet, L., Ruiz, L. & Strozzi, T. (2022). Incorporating InSAR kinematics into rock glacier inventories: insights from 11 regions worldwide. *The Cryosphere*, 16(7), 2769–2792. https://doi.org/10.5194/tc-16-2769-2022.

Liu, L., Millar, C. I., Westfall, R. D. & Zebker, H. A. (2013). Surface motion of active rock glaciers in the Sierra Nevada, California, USA: inventory and a case study using InSAR. *The Cryosphere*, 7(4), 1109–1119. .

Massonnet, D. & Feigl, K. L. (1998). Radar interferometry and its application to changes in the Earth's surface. *Reviews of Geophysics*, 36(4), 441–500. https://doi.org/10.1029/97rg03139.

Reinosch, E., Gerke, M., Riedel, B., Schwalb, A., Ye, Q. & Buckel, J. (2021). Rock glacier inventory of the western Nyainqêntanglha Range, Tibetan Plateau, supported by InSAR time series and automated classification. *Permafrost and Periglacial Processes*, 32(4), 657–672. https://doi.org/10.1002/ppp.2117.

Rouyet, L., Lauknes, T. R., Christiansen, H. H., Strand, S. M. & Larsen, Y. (2019). Seasonal dynamics of a permafrost landscape, Adventdalen, Svalbard, investigated by InSAR. *Remote Sensing of Environment*, 231, 111236. https://doi.org/10.1016/j.rse.2019.111236.

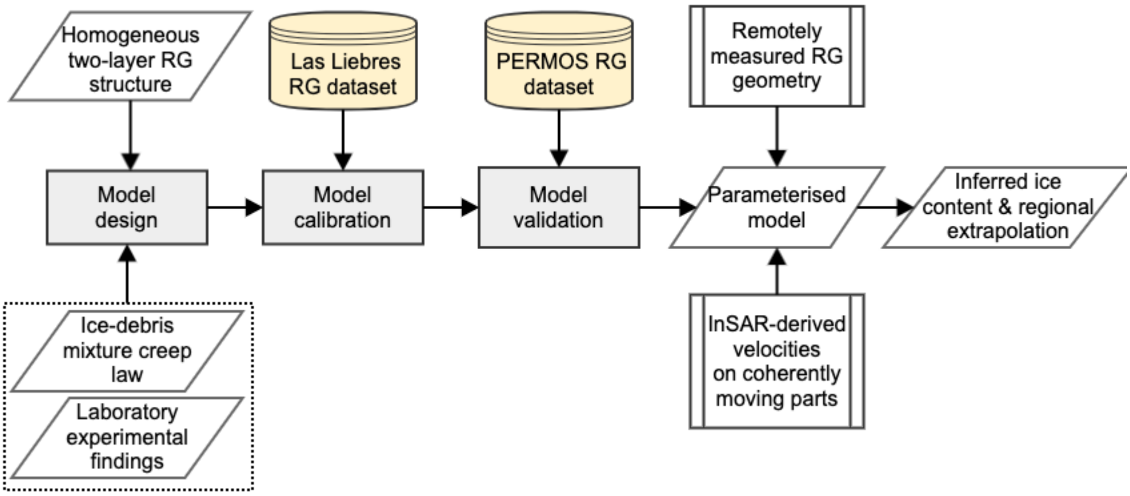
Zhang, X., Feng, M., Zhang, H., Wang, C., Tang, Y., Xu, J., Yan, D. & Wang, C. (2021). Detecting Rock Glacier Displacement in the Central Himalayas Using Multi-Temporal InSAR. *Remote Sensing*, 13(23), 4738. https://doi.org/10.3390/rs13234738.

---Detailed comments---

1. - l.15 and l.17: you use a mix of terminologies 'creep rate' / 'velocity' / 'kinematics' all along the manuscript. It would be easier to follow if you are consistent. Creep rate is ok when referring the mechanisms of the landforms, but InSAR only gives an information about the surface velocity that we infer being representative of the creep rate. Kinematics is a more generic terminology often referring to all spatio-temporal patterns of rock glacier creep rate. So when referring to InSAR products, I would suggest using 'velocity' only (see also Fig.2). Creep rate/kinematics can still be used when more generally referring to the processes.

Re: Thanks for the clarification. We have changed the terminology throughout the manuscript according to the reviewer's suggestion. Many of these modifications are mentioned in the response to the following detailed comments.

We have also changed the label in Fig.2:



2. l.15-16: …the Chilean Andes and the Swiss Alps.

   Re: Modified.

3. l.19-20: Based on previous inventories and extrapolating our findings to the entire…

   Re: Modified.

4. l.34: I have commented it in my previous review: what is the different between rock slope failure and mountainside collapse? Sounds very unclear to me. If you mean 'well-delineated rockslides' compared to 'large deep-seated gravitational slope deformation', maybe just say it so.

   Re: We have changed it to "rock slope failures such as rockslides and rock avalanches" and added a reference paper focusing on rock slope failures.

   Jarman, D. & Harrison, S. (2019). Rock slope failure in the British mountains. *Geomorphology*, 340, 202–233. https://doi.org/10.1016/j.geomorph.2019.03.002.

5. l.35: Could add here a reference to paraglacial studies, e.g. Ballantyne, or more recent references.

   Re: Please refer to the response above.

6. l.40: …and/or undergo transitions to rock glaciers (not all degrading glaciers transition to rock glaciers).

   Re: The reviewer is right. Now modified.

7. l.53: …approaches to quantify (or estimate) the likely ice content…

   Re: Added.

8. l.57-58: twice rheological in that sentence, you can probably remove the first.

   Re: Removed.

9. l.64: …in Nepalese rock glaciers….

   Re: Added.

10. l.65: …freshwater reservoirs…

Re: Changed.

11. l.72: …region. Previous seismic…

Re: Changed.

12. l.82: I think 'critical condition' is a bit vague. Just as a suggestion: 'critical limit' instead?

Re: We have changed the phrase as suggested.

13. l.84: Same here about 'warm and unstable condition'. What about 'warm and unstable state' instead?

Re: Replaced.

14. l.94: …the method to extrapolate the results at the regional scale (Sect.3.6).

Re: Changed.

15. l.99: 'feature distributed in ice-rich alpine permafrost': sounds a bit upside-down to me. It is ice-rich permafrost at these locations because of favorable conditions to accumulate debris and water that accumulates as ice due to permafrost conditions. Anyway, this sentence should probably come at the very beginning of the introduction and could be simplified as: 'Active rock glaciers are ice-rich permafrost landforms creeping downslope in alpine environments' (for example).

Re: We have changed the sentence to "Active rock glaciers are viscous flow features embodying ice-rich permafrost." We do not move it to the beginning of the introduction because the manuscript starts with introducing the ice storage in intact rock glaciers. Active rock glaciers are not the focus.

16. l.104: (mainly temperature and pressure)

Re: Modified.

17. Eq (2) and l.112-115: Tau / Tau-threshold is not clear to me. Probably because Tau is not defined, independently of the threshold stress. Good if it can be slightly clarified.

Re: We defined Tau at an earlier place when introducing Eq (1). We have added a definition here as well at L126:

"τ is the driving stress…"

18. l.120-122: long/heavy phrasing: to develop… to describe… in a… based on… Two sentences?

Re: We have re-written the sentence at L141:

"…we used a constant effective viscosity (B) to develop an empirical formula to describe the deformation behaviour of rock glaciers in a warm permafrost environment (> -3°C). The empirical formula was developed based on existing observational data and laboratory findings."

19. l.123: One could argue that it is wrong applying a glaciological model to rock glacier. The phrasing of that sentence is at least a bit unfortunate if not better explained. Consider rephrasing or explain better the choice of applying a glaciological setup.

Re: We have rephrased the sentence to mention the rationale of doing so at the beginning (L144):

"We assume a homogeneous structure and consider each rock glacier as a slab with uniform width and thickness and a semi-elliptical cross-section, resting on a bed of constant slope, which is a common setup in glaciology (Cuffey and Paterson, 2010)."

20. l.132-133: …the short-term rock glacier kinematic patterns are irrelevant to this study focusing on modelling the relationship between…

    Re: Changed.

21. l.138: Here Tau is defined. It could come before (and potentially repeated here as well). See comment regarding l.112-115.

    Re: Added. See the previous response.

22. l.166. 'glaciological studies': see my previous comment. To be clear, I am aware it is usual to adapt glaciological ice flow models to rock glacier flow calculation, but there is maybe a more elegant way to phrase it?

    Re: We have rephrased it at L195:

    "… we first used an empirical average value as assumed in modelling pure ice creep:"

23. l.167-171: You are performing two tests with different calculations of n, right? It is not well highlighted, and I think the conclusion of this comparison is not mentioned further in the results. Which one did you finally choose and why? If not so relevant to the paper, I would suggest to just mention the selected n calculation you finally used for the presented results.

    Re: Yes, we used both relationships and mentioned this in model calibration (Sect. 3.2). The results and our selection are presented in Sect. 4.1 and 4.2. (We realized that Sect. 4.1 were missing in the clean version, now added back.)

24. l.180: Eq. 12 or 13: see previous comment: missing info on where/when you use each and why.

    Re: We have added back the missing Sect. 4.1 where we show the results. The reasons were mentioned at L217:

    "First, we adopted the exponential $B–\theta_{i,core}$ relationship estimated by Monnier & Kinnard (2016) with the same dataset and a constant creep parameter n (Eq. 12). Then by integrating the relationship between n and ice content (Eq. 13) …"

25. l.201: …can be extract based on optical imagery (xxx). Nice to specify which images and which acquisition dates are used for this step.

    Re: We have added the imagery information at L229:

    "…we first outlined the boundaries of the three rock glaciers from Google Earth images (September of 2018), from which their shapes and areal extents can be extracted using Geographic Information System tools."

26. l.204 and Table 1: A (Area) is the actual parameter used in Eq. 14. You explain the way W is measured (and list the values in the table) but you don't specify the length. Looks weird, considering

that A is probably a simple product of both. Also based on the envelop rectangle? If yes, you can just write 'the length and width of each rock glacier' (l.204) and add the length in Tab.1.

Re: We don't use length as an input parameter. Area (A) can be directly obtained based on the boundary polygon using QGIS. Only width (W) is derived based on the envelop rectangle. And the W parameter is used for calculating the shape factor (S_f) to consider the frictional effect between the rock glacier and surrounding bedrock.

27. l.217: …with variable ice fractions…

    Re: Changed.

28. l.219-222: Heavy/unclear. Suggestion: …naming each scenario after a multiplication factor which indicate the ratio between the applied parameter and the reference scenario. For two parameters, we applied a value range according to the known natural variability based on observations (…).

    Re: We have re-written the sentence at L252 based on the suggestion.

29. l.227: 'surface velocities' instead of 'surface kinematics'? See my comment at l.15/17.

    Re: Yes, we have checked and changed the terminology throughout the manuscript.

30. l.266: See comment at l.201: specify which data are used to measure the geometry.

    Re: We have added the data and tools used in Sect. 3.3 as the reviewer suggested.

31. l.230: See my main comment

    Re: We have extended the InSAR section as presented in the response to the main comment.

32. l.234-235: why different baselines for ALOS and ALOS-2? Regarding the temporal baseline, if applying a simple calculation (24 cm (wavelength) divided by 4, divided 92 days, multiplied by 365 days), you get 24 cm as a theoretical limit of phase ambiguity. This could be documented somewhere, and used to discuss the related limitation. You may induce that you miss metric velocities due to decorrelation (for ex. in the upper part of Fig.10e?).

    Re: The different maximum temporal baselines for ALOS and ALOS-2 we adopted were empirical based on the coherence level of interferograms generated. And the temporal baselines we actually used are 46 days and 14 days, respectively, as listed in Table 2.

    The theoretical limit of phase ambiguity as derived and suggested by the reviewer is not very meaningful in our practical case for two reasons. First, we used interferograms with 46-day and 14-day time spans, thus were able to resolve faster movements than a 92-day-span interferogram. Second, we projected LOS velocities to downslope directions and thus scaled up the upper limit. For example, when a pixel moves faster than 24 cm/yr along LOS, in our real data, it is 47 cm/yr for 46-day pairs and 156 cm/yr for 14-day pairs, allowing us to resolve relatively fast movement. Therefore, we have chosen not to add a quantitative assessment of detection limit.

33. l.236 and 239: …were applied to the interferograms, which… In general: applying an 8x8 multi-looking on ALOS data, I doubt you get a 30m ground resolution, so does it mean you oversampled the final product and simply georeferenced it according to the DEM? If yes, good to say it.

    Re: Yes. We have added this information at L285:

"The final georeferenced interferogram has a ground resolution of ~30 m according to the DEM."

34. l.260: I guess you simply mean 'Finally, we averaged the velocities over the entire observational period and used the results as constraint for modelling ice content'.

    Re: Not exactly. We meant to say that we consider the range of the spatial mean velocity of each observational period and use the range as the constraint. For example, we measured the velocity of Kala-Patthar rock glacier during seven periods, which produced seven spatial mean velocities (the average of all pixels covering the landform), namely 10.3cm/yr, 11.4 cm/yr, 12.0 cm/yr, 10.5 cm/yr, 11.7 cm/yr, 13.4 cm/yr, and 13.6 cm/yr. Instead of taking the average of the seven values, we take the range of the values as the constraint, which is 10.3–13.6 cm/yr (as shown by the yellow band in Fig. 11).

    We introduced how to get the mean velocity at L328:

    "Then, we analysed the velocity values of all pixels within the coherently moving part of the landform and selected the mean, median, and maximum values for each observation to characterise the surface kinematics of the landforms."

    We have specified that it is the spatial mean at L331:

    "Finally, we take the range of the spatial mean velocities…"

35. l.261: partly copied-pasted from l.132-133. See my previous comment.

    Re: We have noticed this repetition and changed the sentence at L332:

    "By doing so, the short-term feature of rock glacier kinematics is neglected in our study."

36. l.262: …averaged velocity in our study.

    Re: We have changed the sentence. See the previous response.

37. l.268-269: …The empirical relation for calculating rock glacier thickness used in the validation processed (ref Section?) was again applied here to…

    Re: We have changed the sentence as suggested at L339:

    "The empirical relation for calculating rock glacier thickness used in the validation procedure (Sect. 3.3) was applied here to obtain the thickness parameter."

38. l.270. …the averaged InSAR-derived downslope mean annual velocity based on the entire observation period, except…

    Re: We keep the phrasing here. Please refer to our response to comment #34.

39. l.273-276: …the modelled ice contents and the volumetric extent of the… Then, we used average water equivalents to represent the water storage in a typical rock glacier in this region. … that reported 4226 intact rock glaciers over the Nepalese Himalayas.

    Re: Re-phrased as suggested.

40. Section 4.1 (l.550 according to version with track changes – missing in the version without track changes): By applying the different regression models…

Re: We have restored the missing section and corrected the typo.

41. l.286: Uncertainties from the statistical analysis (dashed lines in Fig.4) have been considered in the simulation.

    Re: Changed.

42. l.288: …mean annual surface velocities…

    Re: Modified.

43. l.291-292: …the reference ice content, i.e. the average value of the…

    Re: Modified.

44. l.294: …we see that Scheme 2 is the optimal model for the…

    Re: Modified.

45. Figures 5-7: It would be easier to have similar caption texts for all figures. You could also consider moving Figures 5 and 7 in Supplementary, considering that you conclude that 2 is the best, without detailing much of the results from the other Schemes. Table 3 anyway summarizes the main points. That would reduce the length of the paper while still providing necessary info.

    Re: We have moved Fig. 5 & 7 to the supplementary document as suggested.

46. l.322: The results of sensitivity experiments are normalised … (Fig.8).

    Re: Changed.

47. l.325: …slope angle. In the extreme…

    Re: Changed.

48. l.336-337: Surface velocities of the nearby… were also measured...

    Re: Modified.

49. l.343: 'downslope rate'

    Re: Modified.

50. l.347-348: 'The acceleration of Tobuche cannot be confidently revealed by our data and 2015 acquisition was therefore discarded to calculate the average of the entire period.' If I understood correctly, you did not use that value in the following, correct?

    Re: Correct. The value was not used, but we did not calculate the average velocity (detailed in response to comment #34). We have re-written the sentence at L579:

    "The acceleration of Tobuche cannot be confidently revealed by our data and 2015 acquisition was therefore discarded from the velocity series used as the modelling constraint."

51. l.349: 'the observation period'

    Re: Modified.

52. Fig.10: See main comment. Caption: …are shown in red (or …are the red polygons).

Re: We have modified the caption as suggested here and in main comment.

53. l.361-362: …ice fraction of the landforms / …for individual landforms. Is it not more correct to write here: …of the coherently moving part of the landforms? In general: good to add a discussion about this limitation (discrepancy between morphological delineation and InSAR-based polygons).

Re: We have specified the coherently moving part in the sentence, and in the abstract (L19) and conclusions (L787) as well. The discussion has been added in Sect. 5.4 as detailed in the response to main comment.

54. l.369-370: The ranges of the InSAR-derived velocities (yellow bands) are used as velocity constraints…

Re: Modified.

55. l.377: Based on the estimated water… the extrapolated amount of water…. The smaller estimate compared to Jones is most likely due to the smaller considered extent of the landforms. That is maybe something to add in the discussion.

Re: The sentence has been modified.

We do not add the suggested comparison because Jones et al. (2018b and 2021) used a completely different method to estimate ice content of individual landform, making it difficult to assess whether the gap is primarily caused by the conservative areal extent used in this work. The uncertainty of extrapolation result is discussed in Sect. 5.5.

56. l.383: See main comment: missing an InSAR section in the discussion.

Re: We have extended the discussion in Sect. 5.4, as detailed in the response to main comment.

57. l.397-398: …within the timescale of our study…

Re: Modified.

58. l.399: '…which is consistent with the fact that rock glaciers are currently not a major contribution to surface runoff.' This is just a phrasing comment but in addition I don't really see the link with the start of the sentence.

Re: We have re-phrased the sentence as suggested.

The link between this part and the start of the sentence is as such: the fact that rock glaciers are not major sources of surface runoff indicates that the ice stored in rock glaciers are not melting rapidly, so that we can assume the amount of ice remains constant within the timescale of our study.

59. l.403: Upside-down sentence: it is not the lack of knowledge that limits the field data but the opposite. 'amount of data' instead of 'size'

Re: We have re-written the sentence at L666:

"Currently, the amount of field data is limited for deriving a statistical relationship with a low degree of uncertainty since detailed knowledge of rock glacier composition is largely lacking."

60. l.405: we relied on geophysical data (n = 14): quite cryptic explanation for a discussion. No need to use the parameter letter and value, but what it actually means.

    Re: We have removed the mathematical expression and added the information at L684:

    "However, due to the limited amount of calibration data (14 measurements in total)…"

61. l.406: …hypothesized that the empirical…

    Re: Modified.

62. l.408: 'amount of data' instead of 'size'

    Re: Modified.

63. l.418: …has generated discussions…

    Re: Modified.

64. l.428: …introduced by thickness derivation cannot be eliminated when applied to rock glaciers without known information of structure.

    Re: Modified.

65. l.435: …we measured surface velocities…

    Re: Modified.

66. l.438: 1-D InSAR method. We converted the LOS measurements… by assuming the rock moves downslope without… (or along the slope direction). See also main comment, I believe the LOS measurements are not only an issue when dealing with subsidence. Something about N-S facing slopes (and slope facing towards radar) could be added somewhere.

    Re: We have modified the sentence and extended the discussion. See the response to the main comment.

67. l.446: "Errors may arise" sounds like an understatement to me. Why not clearly saying: 'The simple extrapolation method was not designed for an accurate quantification of… but for an order of magnitude estimation of the potential water storage…' Here it could also be said more clearly that the dimensions of the rock glaciers are the main constraints to the ice content according to your results (due to relatively similar velocities), so as a prospect: the extent of the landforms could be used to extrapolate the results.

    Re: We have re-written the first sentence to state the limitation more clearly as suggested.

68. l.450-451: Upside-down phrasing, I think. 'The average velocity of five rock glaciers is not able to represent the ice content of all rock glaciers in the entire mountain range' is what you want to say, I guess?

    Re: We meant to say that not all rock glaciers have similar velocities as the five landforms, and the other rock glaciers could have ice contents different from our study objects as well. We have re-phrased the sentence:

"In reality, rock glaciers typically creep at rates ranging from decimetre to several metres per year (RGIK, 2020), thus the average ice content of the five rock glaciers with similar velocities may not be able to represent that of all rock glaciers with various velocities in the entire mountain range."

69. l.456: No, due to polar orbits, combining asc+desc does not provide an accurate 3-D info in most cases. Rather 2-2.5D. In general, it is good to mention it of course but a bit weird considering that you have not fully introduced the problem. Add references?

    Re: We have rephrased the sentence and added citations. We have also better introduced the limitation of 1-D InSAR according to the major comment.

70. l.460-463: make two sentences. The end does not work.

    Re: We have re-written the sentence into two at L782:

    "With the likely emergence of more data to be integrated for model calibration and validation, it is promising to improve the accuracy of the approach. We expect the improved model to be applied to mountain permafrost regions where rock glaciers are widespread for preliminary water storage evaluation."

71. l.465: past tense in this sentence?

    Re: Modified.

72. l.477: …from our inferred results…

    Re: Modified.

73. l.479: the ratio between… is 1:17.

    Re: Modified.

74. l.483: final point is missing ;)

    Re: Added.

75. Reference list: Wang et al., 2017 is missing.

    Re: Added.

Again, we sincerely thank the reviewer for the great effort to improve the quality of the manuscript considerably.

Report #2

Dear authors and Editor,

Thank you very much for allowing me to act as a referee for the present manuscript. I read the manuscript with great interest and I want to congratulate the authors for their efforts in presenting this manuscript, which is well written and developed.

The study from Hu et al. entitled "Modelling rock glacier velocity and ice content, Khumbu and Lhotse Valleys, Nepal" intends to propose a model to infer rock glacier ice content based on InSAR velocities. The model parameters were calibrated based on observational data on "Las Liebres rock glaciers, in Chilean Andes" and validated using data from four-rock glaciers in the Swiss Alps, because no field observations are available on the current study site. Then, they applied it to the five-rock glacier in Nepal intending to estimate ice/water storage on the studied rock glaciers and at regional scale.

Re: We thank the reviewer for the valuable comments regarding the velocity measurement, the uncertainty analysis, and the extrapolation method. We consider these comments carefully and provide our point-by-point responses given below. The line numbers refer to the ones in the revised manuscript with track changes, aiming to help the reviewer and editors locate the revisions made correspondingly.

From a general point of view, the manuscript is well written, this approach is quite novel. However, I think that there are some major drawbacks in this manuscript that from my point of view could question their acceptance. I would not recommend that the manuscript be published on The Cryosphere as presented for the following reasons:

1. A better assessment of surface velocities must be presented:

i. Even with the new changes on the "Track changes version", there is still unclear (at least for me) how the authors have obtained surface velocities. For example, why do they use 5 cm yr-1 as a threshold instead of that inner intrinsic value for each interferogram? It is not clear to the reader what does mean "coherent moving parts" and how they have been selected. More details are needed to clearly understand why the authors selected these values.

Re: We have re-written the InSAR section (Sect. 3.5.1) according to the comments from Reviewers #1 and #2.

The velocity threshold is introduced considering the typical level of noise in the interferograms based on our experience. We used this threshold for two purposes: first to select high-quality interferograms; second to find the percentage of reliable measurements at each pixel across all interferograms. It is a practically effective way for us to outline the "coherently moving part" of each rock glacier. For example, assuming that one pixel shows reliable velocities (>5 cm/yr) in four out of ten interferograms, meanwhile it is decorrelated and less active in four and two interferograms, respectively, it is difficult to decide whether this pixel is moving along with the main part of the rock glacier, or moving as a separate part, or in transitional status. In other word, we regard the typical noise level (5 cm/yr) as a more reliable and conservative threshold than the "inner intrinsic value" for obtaining velocity data.

We have added the reason at L299:

"…the mean velocity of the landform is larger than 5 cm yr^{-1} (Wang et al., 2017). We set this empirical threshold considering the typical noise level (5 cm yr^{-1}) as we observed in most interferograms."

Regarding the coherently moving part, we have provided more details at L301:

"Step 3: Determining the velocities of the coherently moving parts as the model constraint

Field observations have revealed that multiple areas moving differentially can occur on rock glaciers and exhibit complex kinematic patterns (e.g., Buchli et al., 2018), which violates the assumption of a continuous moving body (Sect. 3.1, Fig. 3). Therefore, we aim to identify the coherently moving part of the landform that corresponds with our assumption and thus suitable for model application.

After the data-refining procedure in Sect. 3.5.1.1, for each landform, the remaining interferograms constituted a series of observations spanning multiple years. Then we defined and outlined the "coherently moving part" of each landform by considering the time series of downslope velocity of each pixel acquired during the observational periods. If the InSAR-measured velocity is higher than 5 cm yr^{-1} in more than half of the interferograms at a given pixel, it was included in the coherently moving part of the landform. Otherwise, the pixel cannot be regarded as actively in motion with the coherently moving area but in an inactive or transitional kinematic status."

ii. InSAR uncertainties need revision. The authors mentioned that they took "three random pixels" within a buffer of 300 m around each rock glacier. This selection is a bit tricky because, with Sentinel-1, you have a large scene from which it is possible to identify stable areas, which are not susceptible to change. More details are needed to understand why only three pixels? Are these three pixels statistically representative of the area? Then, the authors argued that they used the methodology of Hu et al., 2020, but even in this publication, is very hard to understand how they obtain an averaged uncertainty value. For example, later on, in section 3.5.1 (around L485), the authors mentioned that for one pixel, the velocity error is < 10 cm yr-1, why do not mention how much is the uncertainty precisely? Have been the uncertainties quantified?

Re: First we would like to clarify that the objective of using the average phase of the "three random pixels" is to represent the background signal detected in the near surroundings of a rock glacier, which should contain the phase shift caused by atmospheric delay. Then we correct this error by subtracting the reference phase value from each landform pixel. The reference pixels are not required to show large-scale representativeness.

Second, we have precisely quantified the uncertainty of all the rock glacier pixels in all the interferograms. We did not mention the exact value (the maximum error is 9.8 cm/yr) because here we aim to show the level of uncertainties in general.

Then, regarding how we obtain an uncertainty value based on our previous publication, we followed the rules of error propagation. The equations are not listed in the paper but can be found at l.351–365 and l.401 in our published code:
https://github.com/cryoyan/DeeplabforRS/blob/master/read_raster_for_shapefile.py

iii. I wonder if the SRTM DEM is the adequate DEM to correct the topographic effect on InSAR interferograms. It is partially well known that better quality of DEM used for this purpose, better results could be obtained on the InSAR interferograms and later on in the unwrapped products. However, I wonder, why the authors do not consider the highest resolution DEM like "8m High Mountain Asia DEM" (free available at https://doi.org/10.5067/0MCWJJH5ABYO)? I mention that because, in the European Alps, many surprises have been found when SRTM DEM is used to correct the topography effect.

Re: We conducted comparison experiments between the SRTM DEM and HMA DEM for our previous study focusing on periglacial landforms on the Tibetan Plateau and didn't find noticeable differences between the interferograms. Then we returned to SRTM DEM due to a preference of radar imagery-generated DEM.

We thank the reviewer for the sharing research experience in the European Alps. We should systematically compare different DEMs for topography correction in the future work.

iv. The delimitation of moving/coherent parts. There is no clear how the authors define "coherent moving parts" and how they obtain velocity fields. For example, for "Kalaa-Patthar and kongma rock glacier", there is only a small path of movement (i.e. it is very surprising because the velocities plotted in Figure 10 are an average of several unwrapped velocity products). However, on the rock glacier inventory made by jones et al., 2018b, a bigger polygon has been delineated. How do the authors assess the average velocity of the rock glacier by considering a single small patch? Is this patch statistically representative of the entire landform? If the other sectors of the rock glaciers do not move (still within the Jones et al 2018b delineation), does it means that these parts can be considered relict rock glaciers? Another example, for the case of "Tobuche rock glacier", the authors did not identify coherent velocities in the upper sector of the rock glacier (this could be probably due to relatively rapid movements of this sector or simply because there are no good enough results in the interferograms given the complex topography) which supposed to have more important ice concentration than the lower sector, how the authors deal with this problem? No explanation has been done yet in the manuscript. For this specific rock glacier, is the lower sector surface velocity representative of the behaviour of the upper sector? From my experience, I would say no, but it should be demonstrated statistically for your case.

Re: We have extended our introduction to the "coherently moving part" to better explain our motivation for the defining this.

In Sect 3.5.1 at L301:

Step 3: Determining the velocities of the coherently moving parts as the model constraint

"Field observations have revealed that multiple areas moving differentially can occur on rock glaciers and exhibit complex kinematic patterns (e.g., Buchli et al., 2018), which violates the assumption of a continuous moving body (Sect. 3.1, Fig. 3). Therefore, we aim to identify the coherently moving part of the landform that corresponds with our assumption and thus suitable for model application.

After the data-refining procedure in Sect. 3.5.1.1, for each landform, the remaining interferograms constituted a series of observations spanning multiple years. Then we defined and outlined the "coherently moving part" of each landform by considering the time series of downslope velocity of each pixel acquired during the observational periods. If the InSAR-measured velocity is higher than 5 cm yr^{-1} in more than half of the interferograms at a given pixel, it was included in the coherently moving part of the landform. Otherwise, the pixel cannot be regarded as actively in motion with the coherently moving area but in an inactive or transitional kinematic status."

In the modelling work, we only focus on the coherently moving part recognized by InSAR data, because it is in line with the model assumptions. We did not intend to assign the velocity of the coherently moving part to the entire rock glacier, nor to assume it is able to represent the velocity of other parts of the landform. The other parts can be either inactive or in very rapid motion, but we cannot draw conclusions since both situations result in poor InSAR data quality. Besides, this identification is not directly relevant to ground ice estimation.

We agree with the reviewer's (and reviewer #1's) insight that the rapidly moving or inactive sections could also store ice, which could lead to underestimation. We have highlighted that our model is applied to the "coherently moving part" and discussed this issue in *Sect. 5.4 Limited application to the coherently moving parts of rock glaciers in quasi-steady-state motion* (L730):

"Second, our model is suitable to be applied to the coherently moving part. However, the parts of rock glaciers that are in a transitional kinematic status (practically defined as velocities < 5 cm yr^{-1}) or move as an individual portion from the coherently moving parts, may also contain ground ice but are not taken into account following our homogeneous moving assumption. Moreover, the 1-D InSAR method may fail to detect some slow-moving parts of the landforms creeping nearly in parallel to the flight of satellite due to the poor signal-to-noise ratio, causing possible underestimation of ground ice as well."

Finally, we made modifications to the boundaries (the terminus part in particular) according to those delineated by Jones et al. (2018). We realized that our delineation of the rooting zone was not precise and have modified the rooting part in the revised Fig. 8. We have also changed the legend of Fig. 8 to specify this operation.

2. Uncertainty analysis is probably too optimistic: I partially agree with the second reviewer who said that field data must support this study. However, I will not criticize this fact because there is not always possible to collect field data. So, in this case, the authors must turn on a reliable and compressive uncertainties computation. Is in this part where I have my biggest concern because even if there is a section on how the authors have assessed uncertainties (i.e., Section 5.3 and 5.5) this section remains too vague and qualitative, instead of quantitative. By applying Azocar and Brenning, 2010 methodology, very high uncertainties are obtained from this empirical relationship and those, are not fully understood/analyzed in the manuscript. Later on, in the new version of the manuscript, you mention that "as suggested by Wagner et al., 2021, you subtract 10 meters on the initial computed thickness", but it seems very delicate to me to subtract "10 meters" (i.e. to avoid overestimation) knowing previously that average rock glacier thickness is "30 ± 3 meters" (Cicoira et al., 2020). By doing quick calculations using those mentioned values, you have an uncertainty of 1/3 of the rock glacier thickness (±30%) without considering the uncertainties on the physical assumptions (i.e. simplified model), and surface velocities (partially well known) and ice/water content (poorly known).

The ice and water storage will depend on rock glacier thickness, thus, this is a critical factor in the equation, which is assessed with ambiguity. The authors do not explain how they deal with this source of uncertainty and assume an error of ±2m as coherent, but observations in the European Alps have shown that sometimes is even more (i.e., ±5-7m), so the question is, how much will impact when water storage is computed? As I said before, as no field data is available, a clear, replicable and coherent uncertainty analysis must be present to strengthen the analysis and support the results.

Re: We understand the concern the reviewer raised about the uncertainty of thickness derivation. First, we would like to clarify the method we adopted for thickness derivation. In Sect. 5.3 (L698), we stated that "Wagner et al. (2021) suggested an adapted relationship by subtracting 10 m from the derived thickness to remove the likely overestimation effect" only for reviewing previous discussion on this uncertainty, yet we did not use their adapted relationship. We specified that we used the "classical thickness-area relationship" at L701.

We followed the classical relationship because we did not observe significant overestimation in thickness of the validation rock glaciers. Table S2 presents the comparison results and explains why we estimated an error of ~2 m. The suggested uncertainty level (~30%) based on Wagner et al. (2021) is not applicable to this work.

Table S2. Estimated rock glacier thickness (T_{area}) derived from the thickness–area relationship used in this study, and the corresponding bias relative to in situ measured thickness (T_{ref}) (Barsch et al., 1979; Cicoira et al., 2019a; Arenson et al., 2002; Hoelzle et al., 1998). The rock glacier thickness (T_{slp}) derived from thickness–slope angle relationship proposed by Cicoira et al. (2020), and the associated bias. The last row gives the mean absolute error (MAE) derived from the two methods.

Rock glacier	T_{area} (m)	T_{slp} (m)	T_{ref} (m)
Murtèl-Corvatsch	29	26.2	27
Muragl	24	19	20
Schafberg	24	20.8	25
MAE	2.3	2	–

As the thickness is derived from surface area, the different bias obtained, i.e., 10 m (Wagner et al., 2021), 5–7 m (suggested by the reviewer), 3 m (Cicoira et al., 2020), and 2 m (validation result in this work), could be attributed to the different area delineation, which is an issue raised in the next comment and also by the #3 reviewer. In this revision, we have reported more details about rock glacier delineation in Methodology at L229:

"To derive the input parameters, we first outlined the boundaries of the three rock glaciers from Google Earth images (September of 2018), from which their shapes and areal extents can be extracted using Geographic Information System tools. As Muragl and Schafberg rock glaciers consist of multiple or overlapping lobes, we focus on a single active lobe of each rock glacier where the borehole is present and composition data are available."

To facilitate a "clear, replicable and coherent uncertainty analysis" suggested by the reviewer, we have incorporated the uncertainty of the area parameter into the error propagation, as detailed in the response to the next piece of comment.

Here we have also analysed the uncertainty of ice estimation given that the absolute error of thickness is 6 m (as the average of 5–7 m) or 10 m (Fig S7 and S8; Table S3). The uncertainties associated with the two scenarios are 12% and 13%, respectively. The codes will be open-access at GitHub upon publication of the manuscript. We have described the uncertainty experiment in Sect. 5.3 at L703:

"In the validation part, we estimated the thickness-related error by considering the uncertainty involved in delineating the rock glacier area based on Google Earth images, which derives from the occurrence of different image quality and the contrasting interpretations by different operators due to the complex morphology of rock glaciers (Brardinoni et al., 2019; RGIK, 2020; Schmid et al., 2015; Way et al., 2021). We assumed a 40% uncertainty in the area parameter, leading to a ~10% error (or an absolute error of 2–4 m) in thickness. In addition, we conducted analysis assuming a more significant thickness error according to previous studies (Cicoira et al., 2020; Wagner et al., 2021), i.e., 6 m and 10 m, and obtained errors in ice content of 12% and 13%, respectively, which are greater than the 8% uncertainty in our results (Fig. S7 and S8; Table S3)."

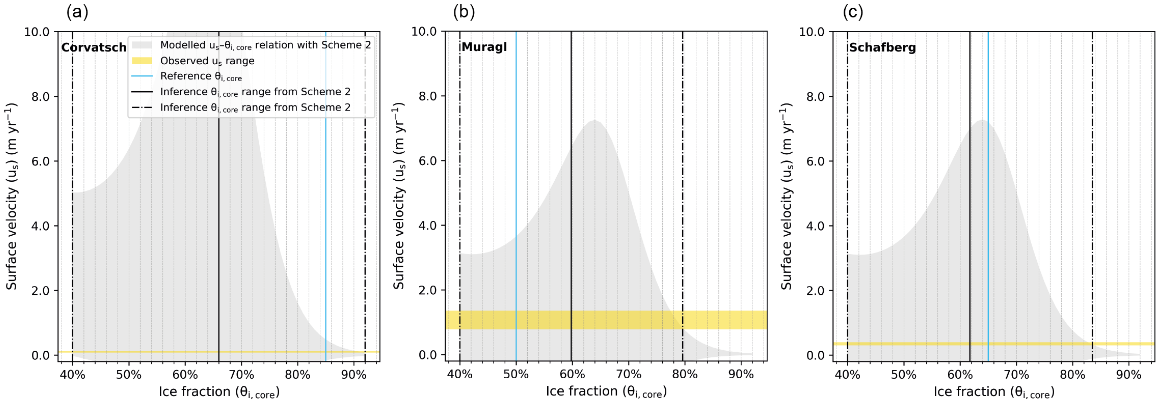


Figure S7: Modelled relationships (grey shaded areas) between the ice fraction ($\theta_{i,core}$) and the surface velocity (u_s) of 95% confidence intervals for the three RGs monitored in the PERMOS network assuming a thickness error of 6 m. The ranges of the observed velocities (yellow bands) are used as velocity constraints for inferring ice content from the modelled relationships. Also shown are the reference ice content obtained from previous field-based surveys (blue lines). The inference ice contents are the mean values (solid black lines) with the estimated ranges (dash-dotted black lines).

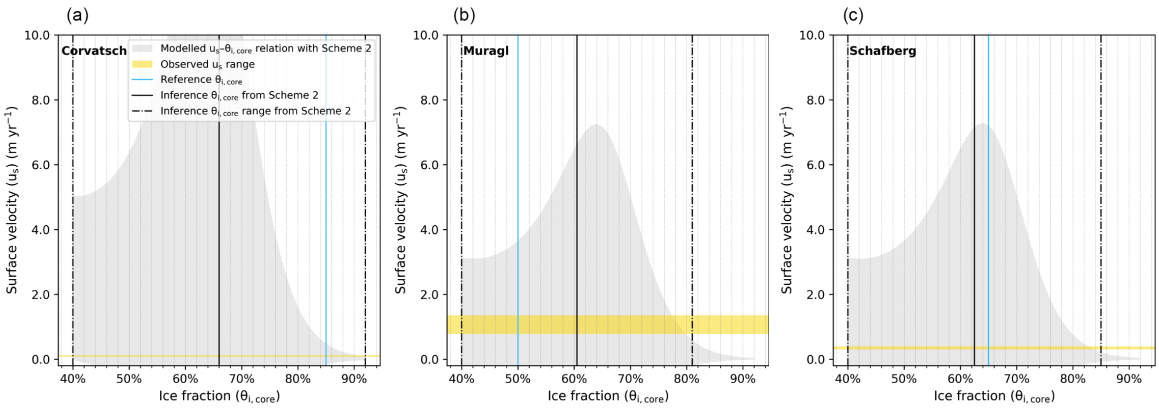


Figure S8: Similar to Fig. S7, but showing results with a thickness error of 10 m.

Table S3. Summary of the reference and inference ice contents derived from two scenarios assuming different thickness errors, namely 6 m and 10 m. The values in brackets following the inference ice contents give the corresponding bias from the reference ice contents. The last row presents the root mean square error (RMSE) of the two scenarios.

Rock glacier	Reference (%)	Inference and bias (%)	
		6-m thickness error	10-m thickness error
Murtèl-Corvatsch	85	66 (-19)	66 (-19)
Muragl	50	60 (10)	61 (11)
Schafberg	65	62 (-3)	63 (-2)
RMSE	–	12	13

Another problem that I have seen is the fact that Azocar and Brenning, 2010 methodology is based on the rock glacier area delineation. However, a lot of ambiguity is present when delineating rock glacier borders. This is visible in your results, for example, the Kala-Patthar, kongma rock glacier appears to be better delineated (i.e. clear distinction between headwall and rock glacier) than the Tobuche rock glacier (with a straight line in the rooting zone). This is a recurrent problem when rock glacier area is estimated even in the European Alps (please refer to the new IPA guidelines; https://www.unifr.ch/geo/geomorphology/en/research/ipa-action-group-rock-glacier/). The authors have not analyzed how much will impact if a different area is taken into account.

Re: In fact, we have investigated the impact of different areas on the ice estimation by analysing the model sensitivity, and found that the varying parameter of surface area does not significantly affect the prediction result (Sect. 4.3).

Considering the importance of thickness in controlling the rock glacier movement, we have calculated the thickness error introduced by the area parameter. The codes will be open-access at GitHub upon publication of the manuscript.

In Sect. 3.3 at L236:

"We assigned a relative uncertainty of 40% to the area parameter and considered the propagated error to the final modelling result."

We have updated the validation and prediction results (Fig. 5, S2, S3, 9; Table 3 and 5):

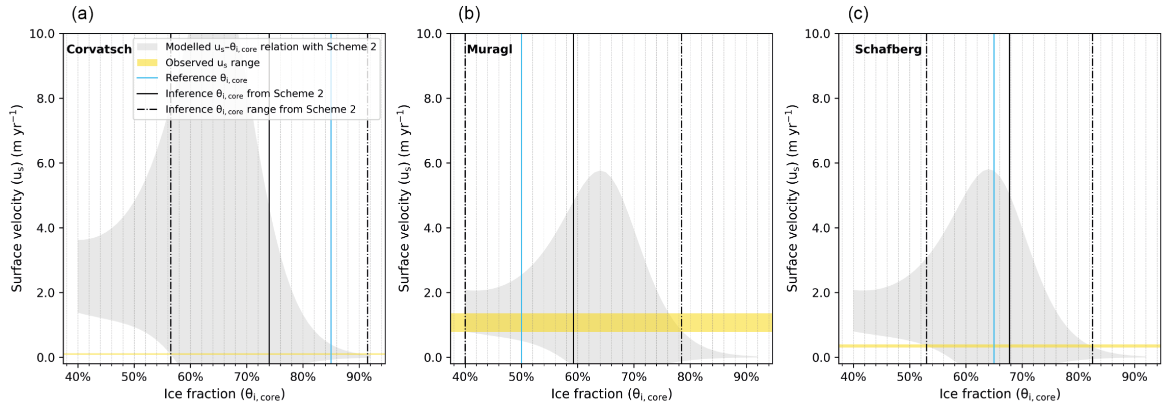


Figure 5: Modelled relationships (grey shaded areas) between the ice fraction ($\theta_{i,core}$) and the surface velocity (u_s) of 95% confidence intervals for the three RGs monitored in the PERMOS network with model parameterisation Scheme 2. The yellow bands show the observed surface velocities, and the blue lines denote the reference ice contents. For each rock glacier, the intersection between the simulated $\theta_{i,core}$- u_s relationship (grey shaded area) and the observed velocity (yellow band) gives the estimated range of ice content, as marked by the dash-dotted black lines. We take the estimated average as the inferred ice content and show the value by the solid black line.

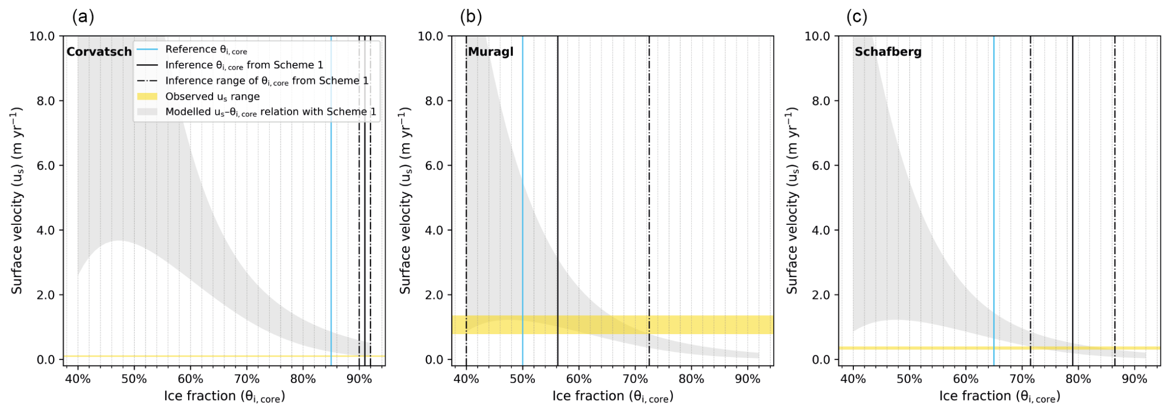


Figure S2: Modelled relationships (grey shaded areas) between the ice fraction ($\theta_{i,core}$) and the surface velocity (u_s) of 95% confidence intervals for the three RGs monitored in the PERMOS network with model parameterisation Scheme 1. The ranges of the observed velocities (yellow bands) are used as velocity constraints for inferring ice content from the modelled relationships. Also shown are the reference ice content obtained from previous field-based surveys (blue lines). The inference ice contents are the mean values (solid black lines) with the estimated ranges (dash-dotted black lines).

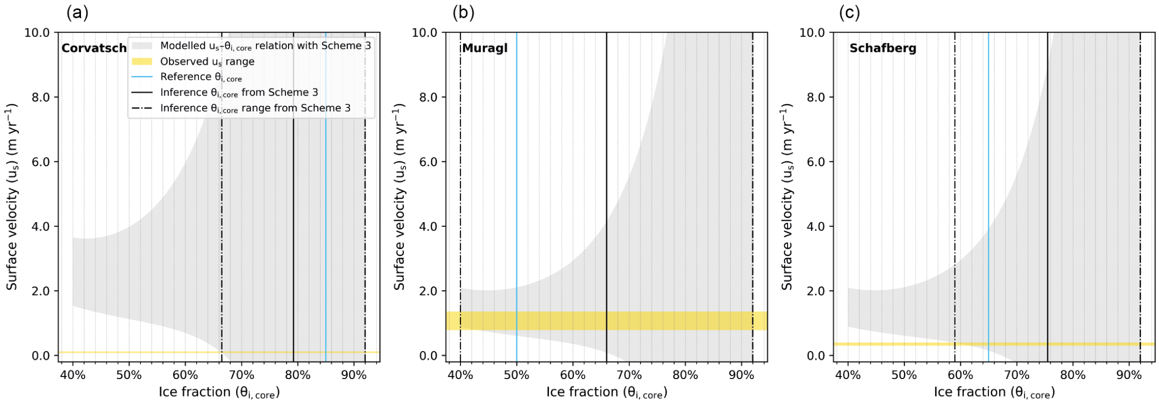


Figure S3: Similar to Fig. S4, but showing results derived from model parameterisation Scheme 3. The grey shaded areas outline the modelled relationships between the ice fraction ($\theta_{i,core}$) and the surface velocity (u_s) with 95% confidence intervals. The yellow bands show the observed surface velocities, and the blue lines denote the reference ice contents. For each rock glacier, the intersection between the simulated $\theta_{i,core}$-u_s relationship (grey shaded area) and the observed velocity (yellow band) gives the estimated range of ice content, as marked by the dash-dotted black lines. The inferred ice content is taken as the average value of the estimated range and indicated by the solid black line.

Table 3. Summary of the reference and inference ice contents derived from the three model parameterisation schemes. The values in brackets following the inference ice contents give the corresponding bias from the reference ice contents. The last row presents the root mean square error (RMSE) of the schemes.

Rock glacier	Reference (%)	Inference and bias		
		Scheme 1 (%)	Scheme 2 (%)	Scheme 3 (%)
Murtèl-Corvatsch	85	91 (6)	74 (–11)	79 (–6)
Muragl	50	56 (6)	59 (9)	66 (16)
Schafberg	65	79 (14)	68 (3)	76 (11)
RMSE	–	9	8	12

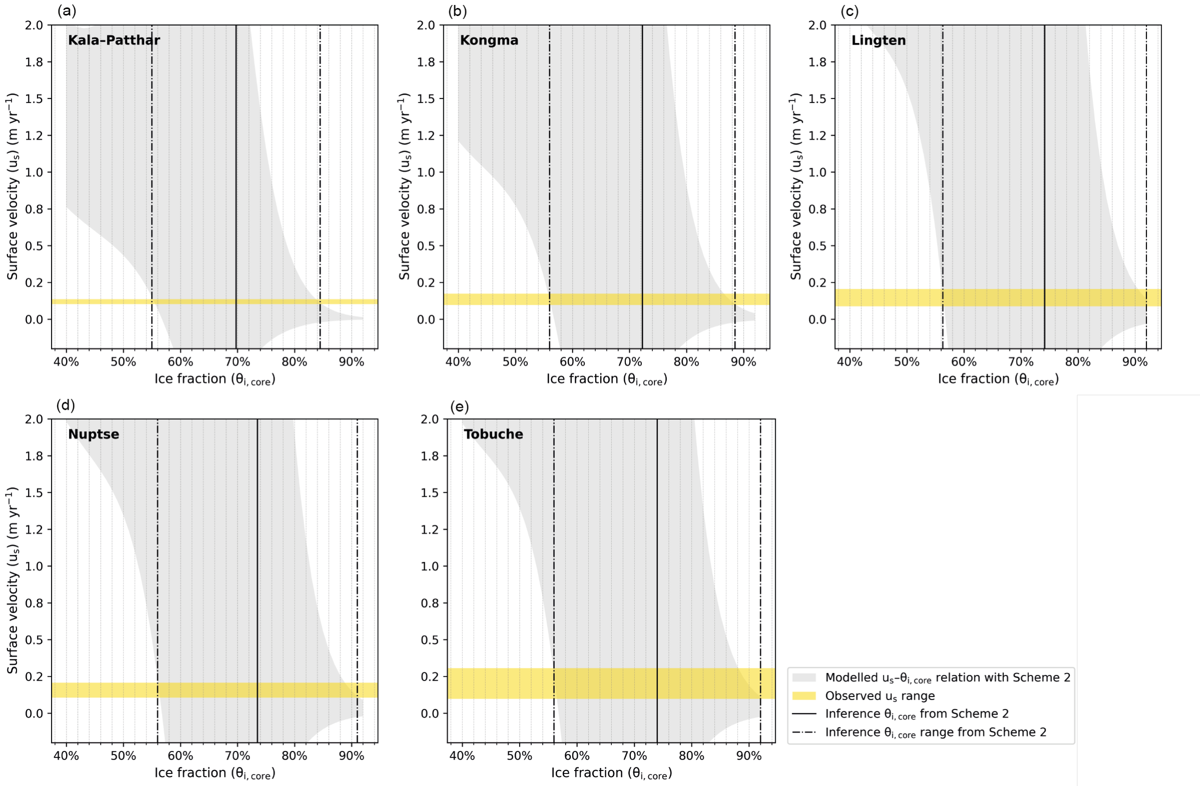


Figure 9: Modelled relationships between the ice fraction ($\theta_{i,core}$) and the surface velocity (u_s) of 95% confidence intervals for the five RGs in Khumbu Valley with model parameterisation Scheme 2 (grey shaded areas). The ranges of the InSAR-derived velocities (yellow bands) are used as the velocity constraints for inferring ice contents from the modelled relationships. The upper and lower bounds of the estimated ice contents are within the range outlined by the dash-dotted black lines and the solid black lines show the mean values representing the inference ice contents.

Table 5. Modelled average ice contents, as well as the minimum and maximum estimates (in brackets) of rock glaciers in Khumbu and Lhotse Valleys and the corresponding water volume equivalents.

Rock glacier	Inference ice content (%)	Water volume equivalent (million m^3)
Kala-Patthar	70±8	1.4±0.2
Kongma	72±8	1.5±0.2
Nuptse	74±8	5.9±0.6
Lingten	74±8	2.0±0.2
Tobuche	74±8	2.7±0.3

3. The extrapolation of key parameters. For me, it is very delicate to extrapolate ice-content values from Andean mountain ranges to Asia mountain ranges on one hand, because no comparison between precipitation/temperature ranges has been done. Temperature and precipitation will play major roles in the ice content and water availability. These conditions are completely different in an arid region (i.e. the Andes) and high mountain Asia.

Re: In fact, we considered the temperature factor, i.e., the thermal status of permafrost in the Las Liebres rock glacier and the rock glaciers in our study area. We drew the comparison because both are in the warm permafrost status. We introduced this precondition at L141:

"In this study, we used a constant effective viscosity (B) to develop an empirical formula to describe the deformation behaviour of rock glaciers in a warm permafrost environment (> -3°C) … This warm ground condition is likely to be realistic in our study area (Sect. 2)."

The analysis of ground conditions in our study area was given at L88:

"…we infer that these rock glaciers develop in a warm permafrost environment for the following reasons: (1) the landforms are located near or below the altitudinal limit of permafrost distribution in Nepal (Fujii and Higuchi, 1976; Jakob, 1992), indicating that the local environment is at the critical limit of permafrost occurrence; (2) based on empirical relationships between mean annual ground temperature (MAGT), mean annual air temperature, latitude, and altitude, the estimated MAGT is >0.5°C, which suggests that permafrost in this area is in a warm and unstable state (Nan et al., 2002; Zhao and Sheng, 2015)."

Second, we agree that precipitation plays an important role in controlling the ice content and water availability of rock glaciers. The water input mainly contributes to the seasonal variations of rock glacier velocities (Kenner et al., 2017; Cicoira et al., 2019), yet our method focuses on the multi-annual kinematic status and neglected the short-term behavior. We claimed our focus in the Methodology section at L153:

"However, the short-term rock glacier kinematic patterns are irrelevant to this study focusing on modelling the relationship between ice content and multi-annual average movement velocity in our study."

Regarding the ice content, the Andes rock glacier indeed has different ice content (42%-82%, according to Monnier and Kinnard (2016)) from the rock glaciers in our study area (70%–74%), which might be attributed to the different precipitation condition.

Reference

Cicoira, A., Beutel, J., Faillettaz, J. & Vieli, A. (2019). Water controls the seasonal rhythm of rock glacier flow. *Earth and Planetary Science Letters*, *528*, 115844. https://doi.org/10.1016/j.epsl.2019.115844

Kenner, R., Phillips, M., Beutel, J., Hiller, M., Limpach, P., Pointner, E. & Volken, M. (2017). Factors controlling velocity variations at short-term, seasonal and multiyear time scales, Ritigraben rock glacier, Western Swiss Alps. *Permafrost and Periglacial Processes*, *28*(4), 675–684. https://doi.org/10.1002/ppp.1953

Monnier, S. & Kinnard, C. (2016). Interrogating the time and processes of development of the Las Liebres rock glacier, central Chilean Andes, using a numerical flow model. *Earth Surface Processes and Landforms*, *41*(13), 1884–1893. https://doi.org/10.1002/esp.3956

4. From my experience, I do not think that the surface velocity is the best parameter to determine ice/water content. Following your stated hypothesis, the velocity increase has a direct relationship with the ice content. However, a generalized increase in creep rates has been observed recently in the European Alps, but it does not imply an increase in ice content.

Re: The same concern has been raised by Dr. Lukas Arenson in the previous round of review. In brief, the phenomena (increased velocity indicates increased ice content) described here cannot be deduced from our work, because the relationship between velocity and ice content in our model in non-linear. Moreover, our approach is designed for estimating current ice content by assuming the amount of ground ice remain constant within the past 1–2 decades.

We presented a discussion in Sect. 5.1 with an example for better illustration in Fig. S6:

"**5.1 Incapability of predicting ground ice evolution**

Our results were presented in the form of a modelled relationship between the ice content and surface velocity (as shown by the grey shading in Fig. 5, S2, S3 and 9), which might mislead the users to interpret the ground ice evolution from rock glacier kinematic variations. For instance, assuming the surface velocity of Kala-Patthar rock glacier reaches 1 m yr^{-1}, the corresponding ice fraction would be approximately 60% (detailed in Fig. S6 in the supplement material). However, we cannot draw the conclusion that ground ice stored in Kala-Patthar rock glacier would decrease by 10% if it accelerated to 1 m yr^{-1}, because the geometric parameters of the landform would change accordingly, particularly the thickness of the permafrost core and the active layer, making the current modelled relationship no longer valid.

In the proposed approach, we assume that the amount of ice stored in rock glaciers remain constant within the timescale of our study (1–2 decades, constrained by InSAR data), which is consistent with the fact that rock glaciers are currently not a major contribution to surface runoff (Duguay et al., 2015; Jones et al., 2019b). Predicting ground ice changes from kinematic variations is beyond the applicability of our model."

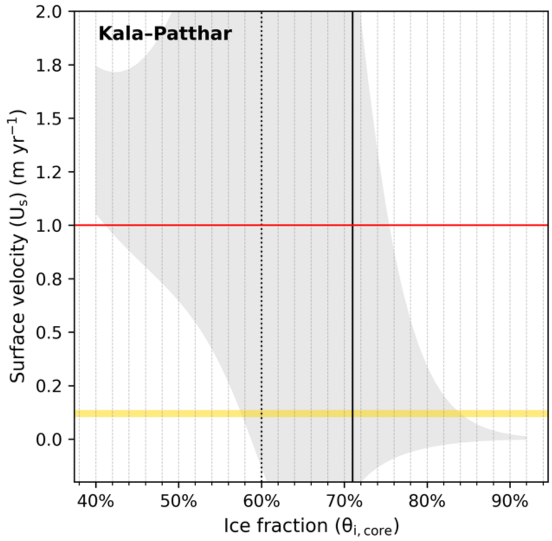


Figure S6: Modified after Fig. 11a in the manuscript. The yellow shading shows the observed surface velocity (~0.1 m yr^{-1}) and the vertical solid black line denotes the modelled ice content (71%). The red shading marks an assumed surface velocity (1 m yr^{-1}) and the estimated ice fraction is shown by the vertical dotted line (60%).

Finally, I strongly encourage the authors to re-evaluate this approach. Estimate ice/water content in a rock glacier is a very difficult task, which uncertainties should be estimated properly and supported with field observations if it is possible.

Best regards

Re: We thank the reviewer again for taking time to review this manuscript and providing valuable comments, especially regarding an enriched uncertainty presentation, which greatly helps improve our work.

Report #3

The manuscript deals with the modelling of rock glacier ice content based on InSAR-derived surface velocity in 5 active rock glaciers of Khumbu and Lhotse Valleys, northeastern Nepal. These estimates rely on empirical ice content and kinematic data drawn from three rock glaciers in the Swiss Alps. Modelled ice content in the five rock glaciers of interest are then applied to an existing inventory of active rock glaciers in Nepalese Himalaya.

I have read with interest the reviewers' comments, the authors' responses and the revised manuscript. The authors have done an excellent job in their point-by-point replies. The revised manuscript shows an extensive effort made to address all of the reviewers' concerns. Although some of the main objections raised by Dr. Arenson remain unsolved, the revised/rewritten discussion acknowledges most of the limitations adequately. In this regard, the upscaling procedure to estimate water storage from the five study rock glaciers to the entire Nepalese Himalaya represents quite a leap, and therefore inherent uncertainties could be described in a more explicit and systematic way.

Re: We thank the reviewer for the constructive suggestions, especially regarding an extended description of the delineation-related uncertainties, which we believe considerably help improve the quality of the manuscript. We consider these comments carefully and provide our point-by-point responses given below. The line numbers refer to the ones in the revised manuscript with track changes, aiming to help the reviewer and editors locate the revisions made correspondingly.

1. In particular, the authors could enrich their state-of-the-art by adding reference to recent work on the uncertainties involved in the compilation of rock glacier inventories on optical imagery, and on Google Earth (GE) in particular: (e.g., Schmid et al., 2015; Jones et al., 2018b; Brardinoni et al., 2019; Way et al., 2021). Uncertainty derives from: (i) the spatial resolution of optical imagery and cloud cover, which in GE vary greatly across a given region; (ii) the mapper (experience, training and personal interpretation); (iii) rock glacier typology (e.g., lobate, tongue-shaped, and multilobe polymorphic).

   Uncertainty applies to: (1) identification of rock glaciers; (2) delineation of rock glacier outline, whose inter-operator variability will affect the rock glacier area, hence the estimated ice/water content; and (3) dynamic classification of the rock glacier (active, inactive and relict), which will affect the number of rock glaciers for which ice/water content is estimated (Brardinoni et al., 2019; Way et al., 2021). Variability in point 2 between mappers has been shown to vary greatly depending on rock glacier type. Uncertainty in point 3, including inter-operator variability, can be reduced greatly by incorporation of InSAR-based kinematic attribute, following a protocol tested on 11 regions across the globe (Bertone et al., 2021).

   Bertone, A, Barboux, C, Bodin, X, Bolch, T, Brardinoni, F, Caduff, R, Christiansen, H H, Darrow, M, Delaloye, R, Etzelmüller, B, Humlum, O, Lambiel, C, Lilleøren, K S, Mair, V, Pellegrinon, G, Rouyet, L, Ruiz, L, and Strozzi, T. Incorporating kinematic attributes into rock glacier inventories exploiting InSAR data: preliminary results in eleven regions worldwide. The Cryosphere Discuss. [preprint], https://doi.org/10.5194/tc-2021-342

   Brardinoni F, Scotti R, Sailer R, and Mair V. 2019. Sources of uncertainty and variability in rock glacier inventories. Earth Surface Processes and Landforms, 44, 2450-2466.

   Jones et al 2018b (already in reference list)

Schmid MO, Baral P, Gruber S, Shahi S, Shrestha T, Stumm D,Wester P. 2015. Assessment of permafrost distribution maps in the Hindu Kush Himalayan region using rock glaciers mapped in Google Earth. The Cryosphere 9(6): 2089–2099.

Way RG et al., 2021 Consensus-Based Rock Glacier Inventorying in the Torngat Mountains, Northern Labrador. American Society of Civil Engineers Proceedings. Regional Conference on Permafrost and the 19th International Conference on Cold Regions Engineering. https://doi.org/10.31223/X5C60W

Re: We have introduced the uncertainties associated with area delineation in the Methodology and Discussion. We have also considered the impact of the area uncertainty on the modelling result. More details are summarized in the response to comments from the #2 reviewer.

At L229:

"To derive the input parameters, we first outlined the boundaries of the three rock glaciers from Google Earth images (September of 2018), from which their shapes and areal extents can be extracted using Geographic Information System tools. As Muragl and Schafberg rock glaciers consist of multiple or overlapping lobes, we focus on a single active lobe of each rock glacier where the borehole is present and composition data are available. The three rock glaciers for validation have a tongue-shaped typology."

At L700:

"In the validation part, we estimated the thickness-related error by considering the uncertainty involved in delineating the rock glacier area based on Google Earth images, which derives from the occurrence of different image quality and the contrasting interpretations by different operators due to the complex morphology of rock glaciers (Brardinoni et al., 2019; Schmid et al., 2015; Way et al., 2021). We assumed a 40% uncertainty in the area parameter, leading to a ~10% error (or an absolute error of 2–4 m) in thickness. In addition, we conducted analysis assuming a more significant thickness error according to previous studies (Cicoira et al., 2020; Wagner et al., 2021), i.e., 6 m and 10 m, and obtained errors in ice content of 12% and 13%, respectively, which are greater than the 8% uncertainty in our results (Fig. S7 and S8; Table S3)."

2. With reference to the five rock glaciers in Khumbu and Lhotse Valleys, and the three rock glaciers from Switzerland, please consider adding an attribute in Tables 1 and 4 to characterize rock glacier typology (e.g., talus lobate, debris tongue-shaped, or others) so that the reader can compare area, width, slope, but also typology. Perhaps you could acknowledge briefly that the three rock glaciers in Switzerland (lines 60-61) are substantially smaller than the five selected in Nepal.

Re: We have included the typology information in the description, as all the features concerned are tongue-shaped. At L232:

"The three rock glaciers for validation have a tongue-shaped typology."

We have also mentioned the extent contrast between the two groups of rock glaciers at L601:

"The geometric and structural data used as input parameters are detailed in Table 4. The five rock glaciers are tongue-shaped features and their areal extents are substantially larger than the three validation rock glaciers (Table 1 and 4)."

3. Since you are extrapolating your modelling results to Nepalese Himalaya, please consider: (i) justifying briefly the selection of those valleys and the five rock glaciers in particular; (ii) describing where the average size of your five rock glaciers plots (percentile) within the size distributions of rock glaciers across Nepalese Himalaya. The latter would allow the reader to understand where the five sample rock glaciers stand compared to the regional population.

   Re: We selected the Khumbu and Lhotse valleys as the study region mainly because the rock glaciers and frozen ground status in this area are among the best studied in the Nepalese Himalaya. We have also conducted field investigations in the Khumbu valley (see Knight et al., 2019), making it possible for in-situ investigations for validating our results in the future.

   Knight, J., Harrison, S. & Jones, D. B. (2019). Rock glaciers and the geomorphological evolution of deglacierizing mountains. *Geomorphology*, *324*, 14–24. https://doi.org/10.1016/j.geomorph.2018.09.020

   We have analyzed the size distribution of the rock glaciers in the Nepalese Himalaya (Fig. S7), and added descriptions at L746:

   "Second, the dimensional extent of rock glaciers varies across the Nepalese Himalaya (Fig. S9). Considering the surface areas of rock glaciers and the thickness–area relationship, the volumes of the landforms lie between 0.08 million m^3 and 228 million m^3. The dimensions of Kala-Patthar, Kongma, Nuptse, Tobuche, and Lingten rock glaciers are at the 26th, 27th, 35th, 50th, and 72nd percentiles of the regional population (Jones et al., 2018), respectively, and cannot represent the sizes of all rock glaciers across the mountain range."

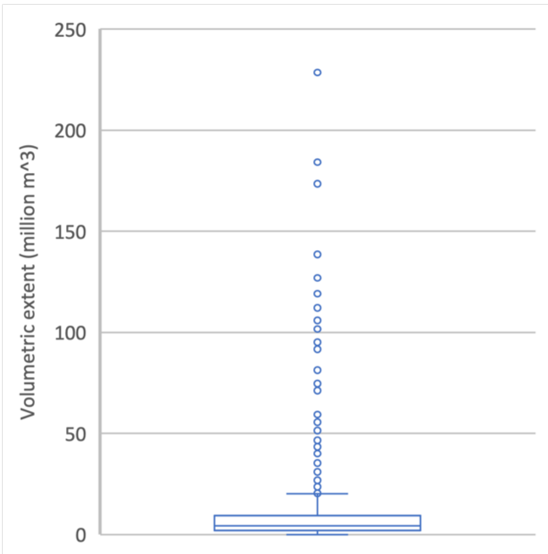


   Figure S9: Box and whisker plot showing the statistical distribution of rock glacier volumetric dimensions in the Nepalese Himalaya (Jones et al., 2018b).

   Jones, D. B., Harrison, S., Anderson, K., Selley, H. L., Wood, J. L. & Betts, R. A. (2018). The distribution and hydrological significance of rock glaciers in the Nepalese Himalaya. *Global and Planetary Change*, *160*, 123–142. https://doi.org/10.1016/j.gloplacha.2017.11.005

4. InSAR methodology: please describe how movement along LOS was projected to the line of maximum slope, adding relevant reference (e.g., Bechor, NB and Zebker, HA. 2006. Measuring two-dimensional movements using a single InSAR pair. Geophysical Research Letters, 33(16)). Please specify whether the projection was conducted systematically or was limited to pixels with

slope below a given threshold. When the angle between LOS and line of maximum slope is high (>60°), projecting may amplify InSAR related errors.

Re: We have added citations and described the factors considered in the reprojection procedure at L292:

"The projection was conducted considering the flight direction of satellite, the local incidence angle, and the landform morphologic parameters including the aspect and slope angles (Massonnet and Feigl, 1998; Bechor and Zebker, 2006)."

The reprojection equation is given below:

$$V_{slp} = \frac{V_{LOS}}{\sin(\theta_{asp} - \alpha)\, sin\theta_{inc} cos\theta_{slp} + cos\theta_{inc} sin\theta_{slp}}$$

where α is the flight direction of the SAR satellite; θ_{inc} is the local incidence angle; θ_{asp} and θ_{slp} are the aspect angle and slope angle of the lobe, respectively.

We applied the projection to all pixels given that no steep slope occurring on the five rock glaciers in our study area.

Other minor comments:

1. Line 434: please consider adding the following reference:

   Scotti R, Crosta G B, and Villa A. 2017. Destabilisation of Creeping Permafrost: The Plator Rock Glacier Case Study (Central Italian Alps): The Destabilised Plator Rock Glacier. Permafrost and Periglacial Processes, 28(1), 224–236.

   Re: Added.

2. Lines 439-440: please consider removing the following sentence: "Rock glaciers showing strong subsidence indicators from optical images, such as surface depressions or cracks, are not suitable for the current method". It defeats the purpose of using InSAR data. Interpretation of vertical surface deformation (e.g., subsidence) based on morphologic features observed on optical images is unreliable and potentially misleading.

   Re: We have removed this sentence as suggested.

3. Line 450: please consider modifying the citation of the IPA report, currently referred to as "Delaloye & Echelard, 2020", with the "How to cite" indication contained in the updated version of the document: "RGIK, 2021" and in the reference list as: "RGIK (2021). Towards standard guidelines for inventorying rock glaciers: baseline concepts (version 4.2.1). IPA Action Group Rock glacier inventories and kinematics (Ed.), 13 pp.". This effort involved a broad international working group.

   Re: We have updated the citation to the correct format.

I enjoyed reading the thread of revisions and look forward to seeing the paper published.

Re: We thank the reviewer again for the encouragement and the effort for making the manuscript better.