**Report #1**

Dear Editor, dear authors,

I think the manuscript has improved substantially from the last version and I am happy with the changes made at this time. Please consider some minor changes aimed at improving the readability of the following sentences.

Re: We thank the reviewer for the encouraging comment and the substantial help we received to improve the quality of the manuscript through the revision process. Thank you.

In particular, please consider revising as follows:

1. L 209: “As Muragl and Schafberg rock glaciers consist of multiple and/or overlapping lobes, in each of them we focus on a single active lobe for which borehole and composition data are available. The three rock glaciers selected for validation are tongue shaped.”

Re: Changed.

2. L 292: “We applied the projection to all pixels given that no steep slope occurs on the five rock glaciers of interest.

Re: We have added the sentence.

3. L 700: “... which derives from interactions among variable image quality, the operator’s mapping style and interpretation, and the complexity of the rock glacier morphology (Brardinoni et al., 2019; Schmid et al., 2015; Way et al., 2021)."

Re: Changed.

**Report #2**

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

I would like again to acknowledge the extensive work performed by the authors to address my comments from my previous review. Although I do agree with some critical comments from the other reviewers regarding the methodology, I believe the current version is now acceptable for publication after minor adjustments. The research raised several questions about the feasibility/relevance of coupling remote sensing observation and modelling, quantifying the ice content based on this approach and extrapolating the results to an entire region. However, these questions are now well discussed, and the limitations are acknowledged. Independently of if we agree or not with the assumptions, I believe the procedure and resulting outputs are now clearly described and therefore worth publication. Generating debates in the community is also what open access dissemination is about.

Re: We are grateful to the reviewer for the valuable feedbacks we received to improve the manuscript in many aspects through the review process. Thank you.

I have focused the work for this third review on the main parts that I commented in my second review, suggesting some minor corrections to facilitate the understanding of the content. Note that the line numbers refer to the version without track changes.

1. l.232-234: "... we present our method to measure surface velocities of rock glaciers with InSAR for constraining the model (Sect. 3.5.1) and use complementary remote sensing products to derive geometric and structural parameters (Sect 3.5.2)."

Re: Changed.

2. l.248-249: "multi-looking operation and adaptive Goldstein filter (8x8 pixels) were applied using the open-source software..."

Re: Changed.

3. l.250: "The final georeferenced interferograms have a ground resolution..."

Re: Changed.

4. l.252: "Ice-debris landforms"? Is there a reason not using "rock glaciers" here? If it also considers debris-covered glaciers, just say it so.

Re: Yes, we did also apply the same procedure to the Chola debris-covered glacier. Relevant data is presented in the Supplementary Materials.

5. l.261: "For each pixel, we found the velocity error is < 10 cm/yr". Rather used the way you explained it your response to my questions, it is much clearer: "For each interferogram, we quantified the uncertainty at the pixel-level. Among all the high coherent pixels, the largest uncertainty is 9.8 cm/yr. The velocity error is therefore considered as < 10 cm/yr."

Re: Agree. We have changed the phrasing.

6. l.262-265: Still unclear to me if the criteria are applied to discard interferograms, entire or part of the landforms. Based on what you explained in your response, I would suggest writing: "... we selected the interferograms and documented rock glacier parts meeting the following criteria... (1) only pixels showing acceptable coherence (> 0.3) are kept; (2) the coherent pixels must cover more than 40% of the landform surfaces; (3) the mean velocity must be larger than 5 cm/yr (Wang et al., 2017). We set this empirical threshold considering the typical noise level from atmospheric delays (5 cm/yr). The interferograms and landforms that do not meet these criteria were discarded." If I misunderstood: please adjust the content and make it clear.

Re: We have changed the sentence according to the suggestion.

7. l.271: "After the procedure described in Step 2, for each selected landform, ..."

Re: Changed.

8. l.274-276: "... in more than half of the interferograms, the pixel was included in the coherently moving part... Otherwise, the pixel is discarded, i.e. not included in the coherently moving part. The area is considered as inactive or in a transitional kinematic status."

Re: Changed.

9. l.279: Again, it is better explained in the response to my question: "the mean velocity error is the square root of the quadratic sum of all the velocity errors, which is limited to < 1 cm/yr". You could btw consider referring here to your github codes (basically referring to the code and data availability section) to make it simple to look for answers about how it has been calculated.

Re: Changed.

10. l.280: "... the range of the spatially averaged velocities within the coherently moving parts... By doing so, isolated patterns are neglected assuming that they may be related to short-term kinematic fluctuations, not representative of the multi-annual kinematic behaviour of the whole landform."

Re: Changed.

11. l.291: "...the abnormal value in 2015 has been removed from the range"

Re: Changed.

12. l.350: "... (Sect 4.4.1) and present the modelled ice content of the five rock glaciers..."

Re: Changed.

13. l.418: "consistent with the fact that rock glaciers are currently not a major contribution to surface runoff in the study area"

Re: Changed.

14. l.450: "the uncertainty in deriving rock glacier thickness remains ambiguous" could be rephrased. I don't think "ambiguous uncertainty" means anything.

Re: We have changed the sentence: "…the uncertainty of in deriving rock glacier thickness remains challenging to accurately quantify…"

15. l.455: One thing that is missing as limitation in this part: when considering your definition of coherently moving area, you discard quite big parts of the morphologically delineated landforms, which raises the question: how representative is the resulting InSAR average when it is based on data covering less than a quarter the rock glacier surface (for ex. Fig.8b)? As you acknowledge in your answer to my previous questions, it can be both due to low-coherence (potentially due to high-velocity) or low-velocity. It basically means that you have a big uncertainty in both directions: the results may be underestimated or overestimated. Good to mention the problem, I think.

Re: We thank the reviewer for raising this point. It can lead to either underestimation or overestimation issue if we attempt to use the ice content (%) of the coherently moving area for representing the percentage of ice in the whole landform. However, we do not intend to do so. Here we only aim to discuss the bias when estimating the amount of ice stored in rock glaciers, which would always be an underestimation problem. As we stated in L. 574:

"This part may also contain ice but are excluded from our estimation, causing possible underestimation of ground ice storage as well."

16. l.458: Avoid using "stable" / "stability" when speaking about moving landforms. "at a relatively constant rate."

Re: Changed.

17. l.461: Same here "the stability of the motion". Maybe "to consider an average rate and avoid misleading conclusions based on unrepresentative short-term patterns".

Re: Changed.

18. l.463: "... or behave differently from the coherently moving parts"

Re: Changed.

19. l.467: “Third, rock glaciers affected by significant subsidence (instead or in addition to downslope creep) cannot be...”

Re: Changed.

20. l.471: “Finally, ...”

Re: Changed.

21. l.496: “The likely emergence of... will likely allow for improving the accuracy of the approach”. “We expect the improved model can be valuably applied to...”

Re: Changed.

22. l.505: “(2) Mean downslope velocities in the coherently moving part of the rock glaciers in...” I think it is important to specify it here – that is the mean of what you define as coherent, not the entire morphologically-delineated rock glaciers.

Re: Agree. Information added.

23. l.507: “remained constant” – wise to do a “find&replace” function for the entire document cause using “stable” is quite misleading.

Re: We have changed the wording consistently throughout the manuscript.

24. l.516: Maybe “confirms” instead of “highlights”?

Re: Changed.

**Report #3**

Review of the manuscript tc-2021-110

Modelling rock glacier ice content based on InSAR-derived velocity, Khumbu and Lhotse Valleys, Nepal

By Hu Yan et al.

**General remarks**

The authors present a novel method to estimate the ice volume in rock glaciers based on a modified ice flow model and InSAR derived surface velocity. The model is calibrated using literature data from a rock glacier in the Andes, validated at rock glaciers in the Swiss Alps where detailed information is available and then applied to five rock glaciers in Khumbu Himal. Finally the authors upscale their results to the whole Nepalese Himalaya based on an existing inventory using a scaling relationship. The topic is of high importance as the ice contained in rock glacier could potentially be of hydrological importance, measurements can only be done on a very limited number of rock glaciers and therefore a modelling approach could provide valuable information.

This paper has gone through few round of reviews and even though all reviewers are in line that work is in general if high interest there seems still to be some concerns which could not be fully addressed. I won't recall the available reviews but provide an independent opinion based on the current version but acknowledging that the manuscript has clearly improved.

Re: We thank the reviewer for the constructive and insightful comments which would significantly improve the quality of the work. We consider these comments thoroughly and revise the manuscript correspondingly. In this letter, we provide the point-by-point responses with line numbers referring to the ones in the revised manuscript with track changes, aiming to help the reviewer and editors locate the revisions we made correspondingly.

**General comments:**

1. The main focus of the study is to estimate the ice content of few rock glaciers in Khumbu Himal using a novel methodology. I know that the authors cannot change now this setup, but it is in general questionable to develop a new method based on one rock glacier (which is quite long and narrow) calibrate it on others and then apply is to rock glaciers with different characteristics in a region with different climate and topographic settings. Hence, it remains unclear how well this information can be transferred. The authors must more convincingly show this, e.g. by providing more detailed information about topographic, climatic and ground temperature conditions of the different regions. I understand that the authors maim focus is the Himalaya, but why not first develop, calibrate and validate the model on rock glaciers and regions in the Alps or Andes where much more information and also in-situ measurement of rock glaciers are available? If the authors decide to keep this set-up then at least a rational for choosing these rock glaciers need to be given. Las Liebres rock glacier is measured by quite thoroughly by GPR. However, to get better information about the ice content it is in general recommended to combine different geophysical techniques.

   Re: We understand the concern regarding the apparent differences among the rock glaciers in our study, such as their planar shape, the local climate, and the topographic settings. However, these factors are irrelevant in our model setup, but play a role by adjusting the ground temperature. Therefore, we have taken the reviewer's suggestion (in this comment and the next) to further elaborate the rationale behind the selection of rock glaciers by demonstrating their similar ground temperature conditions, i.e., a warm permafrost environment (> -3°C).

   This assumption was introduced in *Sect. 3.1 Model design and assumptions* (L.153):

   "In this study, we used a constant effective viscosity ($B$) 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. This warm ground condition is likely to be realistic in our study area (Sect. 2) and occurs in the rock glaciers in the Andes and Swiss Alps selected for model calibration and validation (Sect. 3.2 and 3.3)."

As quoted above, in *Sect. 2 Study area*, we illustrated that the rock glaciers in the Khumbu and Lhotse valleys are situated in a warm permafrost environment (L.108):

"…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)."

We have then added information of the ground thermal conditions of the rock glaciers in the Andes and Swiss Alps of which in-situ data were used in our study for model calibration and validation in *Sect. 3.2 Model calibration* (L.219) and *Sect. 3.3 Model validation* (L.236), respectively:

"Las Liebres rock glacier was considered to have a near 0 °C permafrost temperature (Monnier and Kinnard, 2016), according to the borehole measurement of a nearby rock glacier (Monnier and Kinnard, 2013)."

"All of the selected rock glaciers have warm cores showing permafrost temperatures between -1 and 0 °C (PERMOS, 2019)."

2. The authors should also more clearly present how they define rock glaciers in their study. When the authors first present a definition of rock glaciers it is very general without mentioning of permafrost. But at line 100ff at the beginning of the model they refer to ice- rich permafrost. The also mention the transition to rock glaciers with glacier melt (L40). Is this also true for glacier flowing into regions where permafrost is unlikely? This is important to consider are the presence of permafrost influences melt and the ice flow. In this sense more information about the ground thermal conditions and possible permafrost presence (e.g. by considering available climate measurements and permafrost modelling results) is required for all considered regions.

   Re: We have stated the definition of rock glacier adopted in our study. In *Sect. 1 Introduction* (L.25):

   "Rock glaciers are valley-floor and valley-side landforms that commonly occur in the periglacial and glacial realm. Intact rock glaciers develop permafrost or glacial ice cores containing varying amounts of ground ice."

   We have also introduced the definition at the beginning of the *Abstract* (L.10) according to the Specific Comment #3:

   "Active rock glaciers are viscous flow features embodying ice-rich permafrost and other ice masses. They contain significant amounts of ground ice and serve as potential freshwater reservoirs as mountain glaciers melt in response to climate warming."

   The reviewer questioned the existence of permafrost considering that glaciers flow into non-permafrost regions. In our opinion, there exists isolated permafrost in the bodies of the transitioning landforms, as long as the ground ice has not been entirely melted out. In that case, permafrost temperature stays at around 0 °C. However, our approach is not applicable to these actively transitioning features because they are probably experiencing rapid changes in ice content (detailed in *Sect. 5.2 Limitations of the model application*). None of the rock glaciers selected in this study are transitioning landforms either.

   To clarify this point, we have omitted the discussion of glacier–rock glacier transition in the revised manuscript in response to this comment and Specific Comment #5. We have also provided information of the ground thermal conditions of the rock glaciers in different regions (detailed in response to General Comment #1).

3. The flow modelling seems to be suitable as applied in a similar way by different other studies. One issue as also mentioned in the text is the seasonal variation occurring mainly at the shear horizon which is not captured in the model. The seasonal is according to the available measurements 60-90% of the surface velocity. The authors assume that they neglect the short term variations by taking

"the range of the spatial mean velocities of the coherently moving parts". This needs to be more convincingly shown. E.g. Can't the authors generate a time series of the velocity and show them?

Re: Fig. 7 plots the time series of the velocities derived by InSAR, from which we can observe fluctuations in velocities in different seasons. Take Nuptse rock glacier as an example (Fig. 7a): in 2007, the mean velocity in Aug (20 cm/yr) was 30% larger than that in Dec (15 cm/yr), showing the occurrence of seasonal variations in surface movements. However, the absolute values of velocities lie within a narrow range during the multi-year observation window, as reported at L.440:

"…most rock glaciers, except for Tobuche, moved at a nearly constant rate, ranging from 5 cm $yr^{-1}$ to 30 cm $yr^{-1}$ during the observational period…"

In this study, we take the range of the spatially mean velocities within the coherently moving parts as model constraints (as shown by the yellow bands in Fig.7), because they represent the multi-annual kinematic behavior of the whole landform. We have further explained this point at L.349:

"By doing so, isolated patterns are neglected assuming that they may be related to short-term fluctuations, not representative of the multi-annual kinematic behaviour of the whole landform."

4. In case I understood correctly they transfer the derived velocity for the part they obtained values to the whole landform (L280ff). This is questionable as variable parts of the rock glaciers might have different ice contents and for some rock glaciers they get results for clearly less than 50% of the rock glacier. Moreover, the part of which the authors obtain suitable results varies strongly, e.g. it is the upper part of Kala-Patar, only one side Kongma and the lower part for Tobuche. The only rock glacier for which the approach is reasonable is Nuptse rock glacier.

Re: We might be unclear about this point. In fact, we do not "transfer the derived velocity…to the whole landform" but focus on the "coherently moving part" of the rock glacier. Some of the landforms indeed develop a small area of the coherently moving part, as the reviewer pointed out. However, we only model the ice content and calculate the corresponding water equivalent of these areas without any extrapolation to the entire rock glacier.

We gave the reasons for doing so at L.322:

"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."

5. This is also true for the scaling. The authors use a formula based on area which was developed for the Andes by Brenning (2005) and apply it without adjustments to the rock glaciers in the study region and the Nepal Himalaya. This is equally questionable as it is well known that scaling parameters vary and should be calibrated for the specific regions. Also the characteristics of the identified rock glaciers varies clearly. While the Nuptse Rock Glacier has a clearly identifiable tongue Kala-Patar not and has a depression with small lakes (see Figure 1). It is hence likely that the different parts pf the rock glaciers have different ice contents. I suggest that the authors analyse the topography of the rock glaciers and adjust the scaling accordingly or use other suggested approaches.

Re: Regarding the thickness derivation, firstly, the same empirical relationship has been adopted in previous publications focusing on study areas other than the Andes, such as the Austrian Alps (Wanger et al., 2021) and the Himalayas (Jones et al., 2018, 2021).

Furthermore, we adopted another approach, i.e., the thickness–slope relationship established based on field data gathered in the Alps (Cicoira et al., 2020), made comparisons between the two methods, and found that the two sets of results display the same level of errors (~2 m, Table S2).

We also agree upon the opinion that different parts of rock glaciers have different ice contents. Therefore, this study focuses on the coherently moving parts without any extrapolating to the entire landforms.

**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 | – |

6. Figure 1: Nuptse and Kala-Patar rock glaciers (Image source: Pleiades from Google Earth)

   1) Another issue is the rock glacier delineation. I know this is quite difficult and also subjective (see e.g. the cited study by Brardionini et al. ) and the delineation is fine for a regional study, but for this localised rock glaciers the authors should make more effort to provide the most precise outlines possible.

   Re: We have consistently updated the boundaries of the landforms in Fig. 1 and Fig. 8.

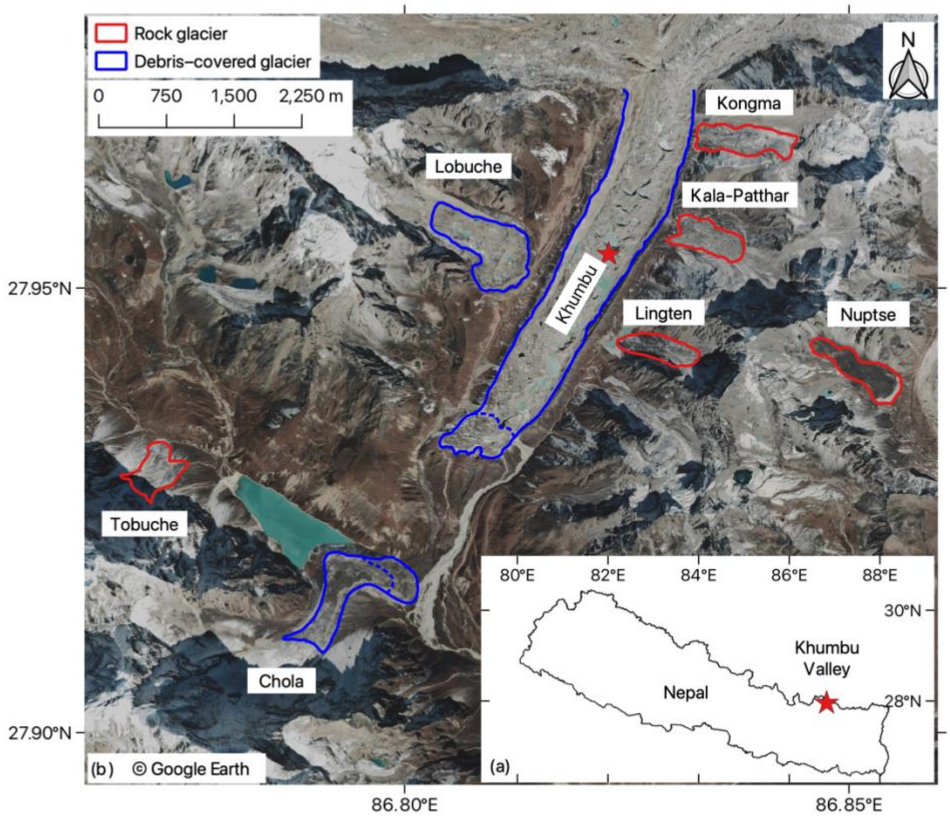



**Figure 1: (a) Location of the study site; (b) Google Earth images (taken in 2019) showing the spatial distribution of the active ice–debris landforms, including rock glaciers (RG) in red outlines and debris-covered glaciers (DCG) in blue boundaries. The RGs are delineated by Jones et al. (2018) and the DCGs by the authors based on Google Earth images. The termini of Khumbu and Chola DCGs (outlined by dotted lines) are transitioning into rock glaciers (Knight et al., 2019).**

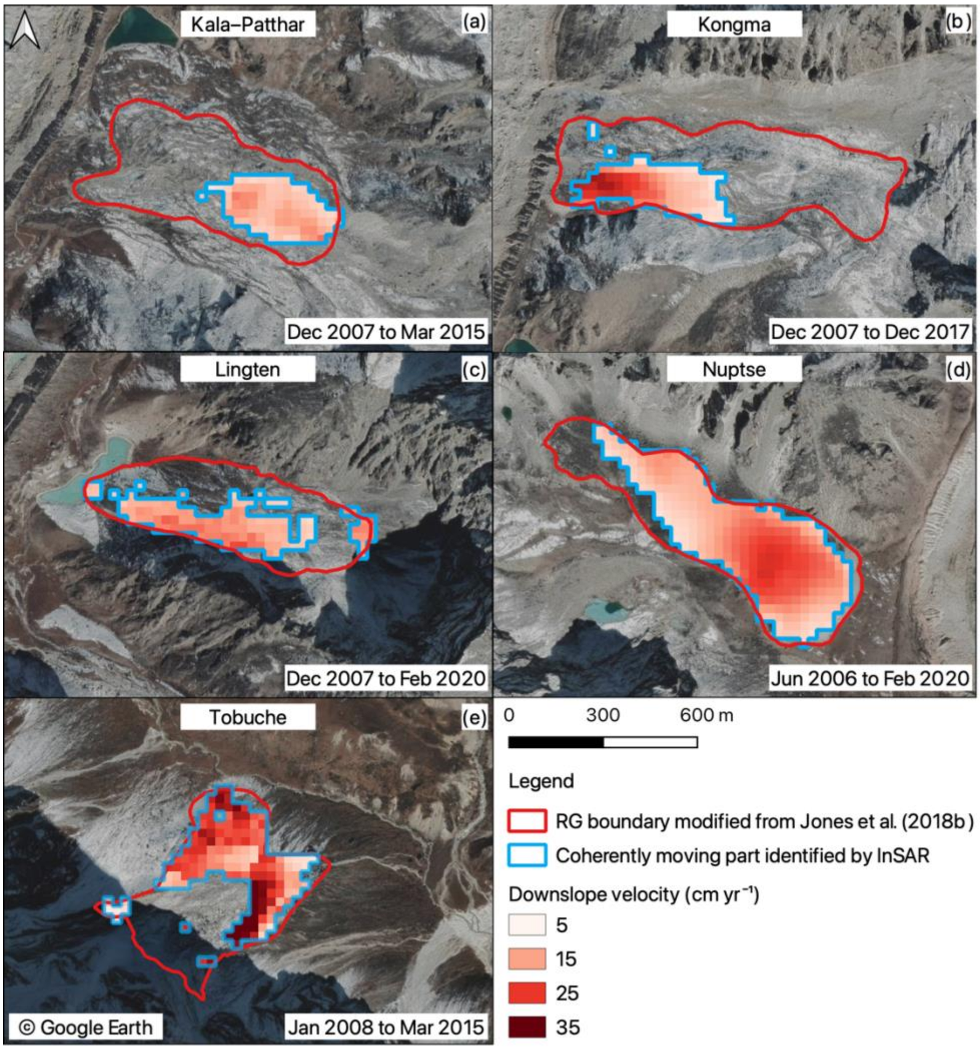
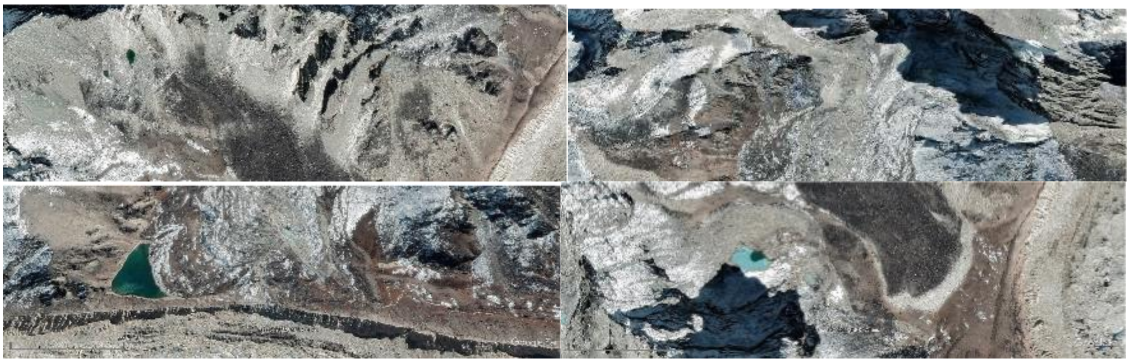



**Figure 8: Velocity field maps show the average velocities of the coherently moving parts of the five rock glaciers (blue outlines) in the study area. The boundaries of the landforms delineated in Jones et al. (2018b) are in red. The transparent areas between the red and blue boundaries are due to low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

2) What about the terminus of Khumbu glacier? Knight, Harisson and Jonas (2019) argue it might be a future rock glacier. Even though I am not fully in line with the argumentation it would be highly valuable to also model the ice content and have some comparison to the rock glaciers (the authors state that they also calculated the velocity of debris-covered glaciers so it can be easily done). The ice core taken close to the more rock glacier part as identified by Knight et al. (2019) might provide some valuable data (Miles et al. (2021). This value could then also nicely compared to the modelled ice content of the identified rock glaciers.



Re: We agree with the reviewer that the terminus of Khumbu glacier is transitioning into a rock glacier. We have also outlined the transitioning part in Fig.1 as suggested in the Specific Comment #10. However, landforms in active transition are unlikely to fulfil one of the requirements of the model, i.e., that the amount of ice remains constant at the decadal timescale, and are therefore beyond the application of our approach.

We explained the reasons in *Sect. 5.2.1 Incapability of predicting ground ice evolution* (L.557):

"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 in the study area (Duguay et al., 2015; Jones et al., 2019b). Landforms undergoing rapid changes in ice content and corresponding morphology, such as transitional features from glaciers to rock glaciers, are beyond the applicability of our model."

7. It is not fully clear to me how the uncertainty of the final result was calculated. The uncertainty ranges are much too low considering all the uncertainties. Provide an own section for clarification.

These issues make the regional extrapolation highly uncertain and there is no advance in knowledge compared to the first rough estimate presented by Jones et al. (2018) as cited in the study. If the authors really want to extrapolate they should do so by applying their velocity-based approach to the large region or a subset (e.g. the whole Khumbu Himal) and then compare to the available data.

Several of these issues are discussed in the discussion section acknowledging the uncertainty. This is well appreciated, but does not really make the results more accurate. At minimum I ask the authors to provide the most important information affecting the accuracy in the methods section to that the reader clearly knows limitation before knowing the results and can then better interpret them.

Re: We estimated the uncertainties by comparing the observed and modelled ice content of the validation rock glaciers, as introduced in *Sect. 4.2 Model validation* (L.411):

"We used the RMSE (8%) derived from Scheme 2 to represent the uncertainty of our approach."

We have rewritten the *Discussion* to include an individual section on the *Uncertainties* (Sect. 5.1) where the two major sources of errors are carefully described.

"**5.1 Major uncertainty sources**

The effects of minor error sources were tested and discussed in Sect. 4.3. Here we introduce the two major uncertainty sources.

**5.1.1 The amount of field data for model calibration**

The empirical relationship between the effective viscosity and ice content is fundamental to model calibration in this study (Sect. 3.2). Currently, the amount of field data is limited for deriving the statistical relationship since detailed knowledge of rock glacier composition is largely lacking, which is the most important factor affecting the accuracy of our approach.

We relied on the geophysical data obtained from Las Liebres rock glacier in the Andes to calibrate the model (Monnier and Kinnard, 2015b), and hypothesized that the empirical expressions can be generalised to rock glaciers developed in a warm permafrost environment. The validation results achieved from samples in a different region, i.e., the Swiss Alps, prove the transferability of the model (Sect. 3.3). However, due to the limited amount of calibration data (14 measurements in total), the uncertainty of the derived effective viscosity–ice fraction relationship (dash lines in Fig. 4b) leads to a wide range of propagated uncertainty when modelling the ice content–surface velocity relationship (grey shadings in Fig. 5). More field data are necessary to refine this empirical relationship.

**5.1.2 Derivation of rock glacier thickness**

We discuss the uncertainty in deriving rock glacier thickness because it influences the surface velocities most significantly. As shown in Eq. 8, the surface velocity is proportional to the thickness to the power of $n + 1$, resulting from the vertical integration of Eq. 7. We use the thickness–area scaling relationship (Eq. 14, Brenning, 2005a) which has also been adopted by previous research on assessing the hydrological importance of rock glaciers (e.g., Azócar and Brenning, 2010; Bodin et al., 2010; Janke et al., 2017; Jones et al., 2018, 2021; Perucca and Esper Angillieri, 2011; Rangercroft et al., 2015; Wagner et al., 2021); yet the reliability of this empirical derivation method has generated discussions (Arenson and Jakob, 2010; Brenning, 2010). Wagner et al. (2021) suggested an adapted relationship by subtracting 10 m from the derived thickness to remove the likely overestimation effect. An alternative empirical method was proposed as a linear relationship between surface slope angle and thickness (Cicoira et al., 2020). We compared the estimated thickness of the validated rock glaciers from the classical thickness–area and the recently established thickness–slope relationships with the field measurements and found that the two sets of results display the same level of errors (~2 m, Table S2).

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 image. The uncertainties were caused by the multiple factors such as the variable image quality, the subjective judgment of operators, and the complexity of the rock glacier morphology (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).

In general, the uncertainty in deriving rock glacier thickness remains challenging to accurately quantify, which is primarily attributed to the insufficiency of ground truth data to build a rigorous relationship between the rock glacier thickness and surface parameters (e.g., area, slope). In addition, rock glaciers, especially the talus-derived ones, tend to develop very variable thicknesses across the landform, the distribution of which cannot be inferred using the existing empirical approaches. Thus, the uncertainty introduced by thickness derivation cannot be eliminated when applied to rock glaciers without known structure information."

As elaborated above, the most important factor affecting the accuracy of the approach is the amount of calibration data for establishing the viscosity–ice content ($B - \theta_{i,core}$) relationship. We have also taken the suggestion and provided this piece of information in the *Methods Section* at L.224 accordingly:

"The limited amount of calibration data plays an important role in the calculation of the uncertainty associated with our approach (detailed in Sect. 5.1.1)."

Finally, following the reviewer's advice, we have omitted the discussion of the regional extrapolation throughout the manuscript.

## Specific comments

1. Abstract, general comment: The abstract is missing the information about how the volume the rock glacier and the of ice of the water equivalent was estimated. The volume of the rock glacier is estimated on an existing scaling approach.

   Re: We have added the information at L.20.

2. L22/23: If the model is easily applicable why didn't the authors do so for upscaling? An important prerequisite is also a rock glacier inventory.

   Re: We did not do so because this paper aims to propose a novel approach and providing exploratory findings, rather than to conduct a comprehensive large-scale investigation. We also agree upon the insight that a rock glacier inventory is a prerequisite. We have removed the adverb "easily" in this sentence and clarified the motivation of the study at L.14:

"This study proposes a novel approach for assessing the hydrological value of rock glaciers in a more quantitative way and presents exploratory results focusing on a small region."

3. L25ff: The authors need to extend their definition of rock glaciers, state the relation to permafrost and also move the information given in L100 to here.

   Re: We added the definition of rock glaciers which also indicates their relationship to permafrost given in L.100 at the beginning of the abstract (L.10).

   "Active rock glaciers are viscous flow features embodying ice-rich permafrost and other ice masses."

4. L28: The authors might want to include one or two more citations about other mountain regions where rock glaciers store a significant amount of water.

   Re: We have named the geographical regions with citations at L.28:

   "Recent research has suggested that they represent important hydrological reservoirs in areas where glaciers are undergoing recession in the face of climate change, such as South America (Azócar and Brenning, 2010; Rangecroft et al., 2014), North America (Munroe, 2018), and South Asia East (Jones et al., 2018a)."

5. L31ff: Can the authors please a bit more specific about debris-covered glacier to rock glacier transition. What about the distal part of Khumbu (Knight et al. 2019)? Please also cite a reference from another research group not only one from the authors.

   Re: We have taken the suggestion in the next comment and removed the paragraph discussing the debris-covered glacier.

6. L35f: This is in theory correct, but it is well known that the debris-covered glaciers in the Himalaya lost at least as much mass as debris-free glaciers (due to manyfold reasons incl. reduced ice flux, supraglacial ponds, ice cliffs etc.). Hence, this argument is not valid. Please revise. My general recommendation is to omit this entire paragraph and really focus on rock glaciers and not debris-covered glaciers.

   Re: We have removed the paragraph as suggested.

7. L41: I agree that the ratio can be higher if the glaciers melt, but disagree with statement with the transition to rock glaciers as this ice already existed and was considered in models.

   Re: We are not sure if we understand this comment correctly. If glaciers transition to rock glaciers, certain amounts of ice transfer from glaciers to rock glaciers. In the ratio $\frac{rock\ glacier\ ice}{glacier\ ice}$, the denominator becomes smaller, and the numerator becomes larger, resulting in a higher ratio.

8. L49ff: These are way to many citations in a row. Please be more specific about the cited papers or remove some.

   Re: We have removed some citations.

9. L56: Would be good to mention all relevant factors controlling the movement and then specific the most important ones and then provide more details about the ice content.

   Re: We have listed the other first-order factors in this sentence:

   "Ice content is one factor controlling the movement of rock glaciers by influencing the driving force and the rheological properties of materials which constitute the permafrost core (Arenson and Springman, 2005a; Cicoira et al., 2020), in addition to other first-order factors including ground temperature, sub-surface structure, debris content, and water pressure (Moore, 2014)…"

10. L70: Please indicate in Fig. 1 the ones which transition to rock glaciers according to the cited references.

    Re: We have outlined the termini and specified them in the caption:

    "The termini of Khumbu and Chola DCGs (outlined by dotted lines) are transitioning into rock glaciers (Knight et al., 2019)."

11. L74f: The authors should also consider Fukui et al. (2007)

    Re: We have added the suggested citation.

12. Fig. 1: The digitisation of the debris-covered glaciers is quite poor. Even though not the focus of the study, this needs improvement.

    Re: We have updated the figure.

13. L126f: Please be more specific about the permafrost core, in particular about the water occurrence.

    Re: We have specified that the water is "unfrozen water", which is a normal constituent of permafrost.

14. L130: Please check the statement about the high ice content. The shear horizon is mentioned in the cited studies, but no information about the ice content (but maybe I have overseen this). The shear horizon is nicely presented by Cicoira et al. (2021) and they also state here that the ice content is lower than in the ice-rich core.

    Re: The reviewer is right. The shear horizon has high debris content instead. We have corrected this.

15. L133: The seasonal variation are first presented by Wirz et al. (2016).

    Re: We have added the suggested citation.

16. Fig. 3: The figure is quite similar to the one by Monnier & Kinnard (2016) apart from the fact that the deformation at the front is not shown. I suggest to show also the deformation and also include the shear horizon. I would then also refer to the reference (but add adjusted or similar) as the idea seems to be taken from it.

    Re: We have updated the figure and added the reference in the caption.

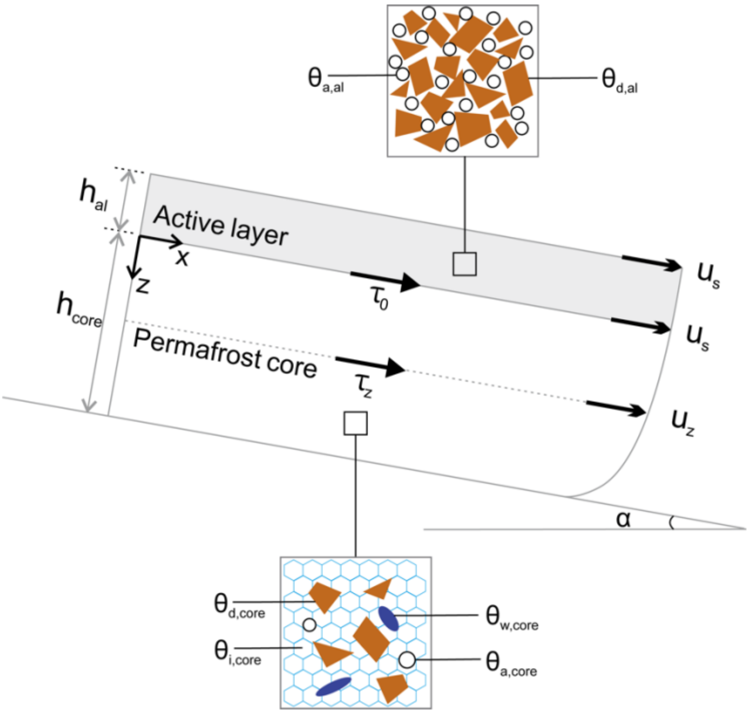



**Figure 3: Schematic geometry, structure, stress status, and composition of rock glaciers (adapted from Monnier and Kinnard (2016)). The rock glacier consists of a permafrost core underlying the active layer. Parameters involved in the model include surface slope ($\alpha$), active layer thickness ($h_{al}$), thickness of permafrost core ($h_{core}$), driving stress at the base of the active layer ($\tau_0$), driving stress at depth z ($\tau_z$), surface velocity ($u_s$), velocity at depth z ($u_z$). $\theta_{d,al}$ and $\theta_{a,al}$ refer to the debris fraction and air fraction of the active layer. $\theta_{d,core}$, $\theta_{i,core}$, $\theta_{w,core}$, and $\theta_{a,core}$ are the fractions of debris, ice, water, and air in the permafrost core, respectively.**

17. L205f: The authors apply the empirical formula established by Brenning (2005). See my general comment about applying the formula to other regions. How do the derived volumes of the Las Liebres rock glacier and the three rock glaciers in the Swiss Alps compare to measured volumes? Provide this information here.

Re: We conducted a comparison between the measured thickness of the three Swiss rock glaciers and the derived thickness from two different empirical methods (the Andes and the Alps, respectively) in the Supplementary material (Table S2). Las Liebres is not included because we only used its measured thickness in this study. We have also presented a thorough discussion on thickness derivation in Sect. 5.1.2.

The volume can be calculated from thickness and area, so we did not directly validate the volume.

**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 | – |

18. Table 1: Where do the values given in the table come from? I recommend to show only three decimals for the area. The delineation is not so precise.

Re: We introduced the way we obtained these values at L.241:

"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."

We have changed the precision of the parameters. In addition, we assigned a relative uncertainty of 40% to the area parameter for considering the error propagation.

19. L236: Consider to cite also more classical papers which introduced the approach, e.g. Strozzi et al. (2004).

Re: We have added the suggested citation.

20. L261: I would rather call it uncertainty as no validation measurements are available.

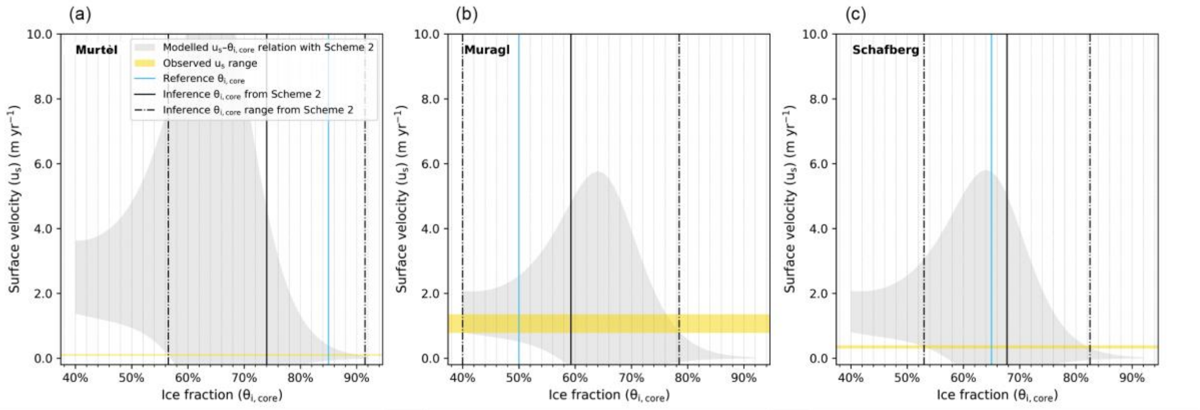
Re: Changed.

21. L298: Provide the information about the source of the ice content of the glaciers.

Re: The amount of glacier ice was estimated based on Randolph Glacier Inventory 4.0. However, we have removed the discussion of regional extrapolation from the revised manuscript after considering the reviewer's suggestion.

22. Fig. 5a: Should be Murtèl-Corvatsch (or only Murtèl)

Re: We have updated the figure.




23. Model-sensitivity: I have no time to think through the model sensitivity in detail. As the model seems to be quite incentive to the different input parameters the authors want to provide more details about the possible reasons.

    Re: We are not sure if we understand this comment correctly. We think the model has relatively low sensitivity to the varying parameters, which proves it suitable to use the model constraint, i.e., surface velocity, for deriving ice content.

24. L349f: This and similar kind of sentences are from my point of view not needed as this is evident from the headings.

    Re: We thank the reviewer for the comment yet we kept these introductory sentences in the hope that the readers can quickly grasp the content and structure of the following section. In the particular example here, these sentences may seem redundant as the current section only consists of two sub-sections. In other long sections (e.g., Sect. 3.5), the short introductions at the beginning may be helpful and worth retaining. Therefore, for the sake of writing consistency, we keep the sentences here as well.

25. L353: The information about the glacier velocity should either be better integrated in the study and compared to the rock glaciers or omitted. I suggest the latter as it distracts from the general topic of this study.

    Re: We have taken the suggestion and omitted this information.

26. Fig. 7: Indicate in this figure which velocity was used to calculate the ice content.

    Re: We have highlighted the velocity range used in the model:

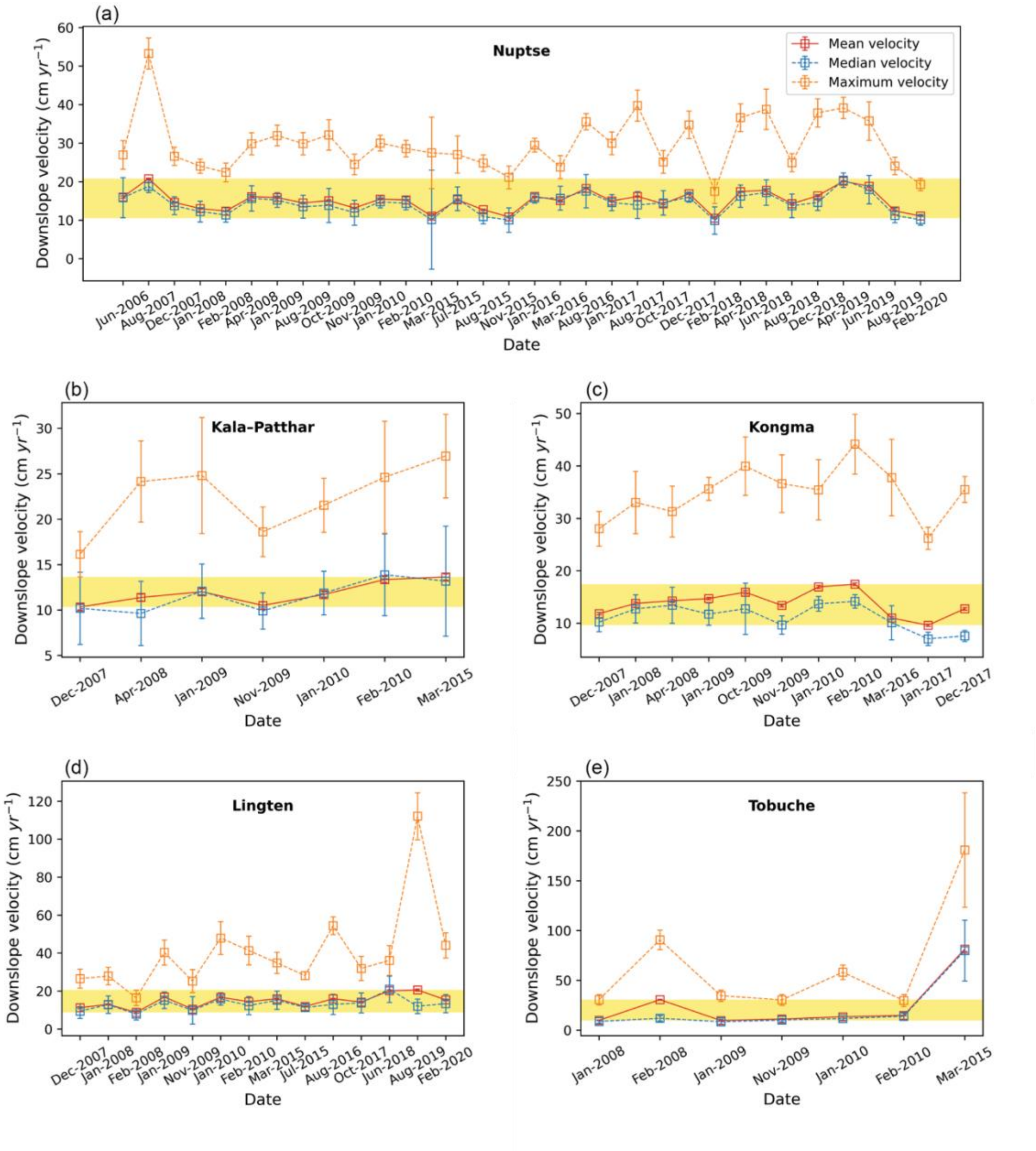



**Figure 7: Time series of the InSAR-derived downslope velocities of the landforms. The spatial mean velocities and uncertainties during each period are shown (red squares and error bars) as well as the median (blue) and maximum (orange) velocities. The yellow bands highlight the range of the mean velocities which were used as model constraints for estimating ice fractions.**

27. L.378: What is the area of the coherently moving parts and what is the water equivalent (w.e.) of the moving part?

Re: We defined the "coherently moving part" in the methodology section (Sect. 3.5.1–Step 3):

"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, the pixel was included in the coherently moving part of the landform. Otherwise, the pixel is discarded, i.e., not included in the coherently moving part. The area is considered as inactive or in a transitional kinematic status."

We have introduced the latter concept at L. 470:

"…the water volume equivalents of the moving parts of individual landforms, which are calculated based on the ice fractions and the volume of the moving parts…"

28. L381: Provide the information how much water is stored in all the rock glaciers was derived.

    Re: We are not sure if we understand the comment correctly. We derived the total amount of water stored by simply adding up the amount of water stored in all the individual rock glaciers.

29. Table 4 and 5: I suggest to combine and also include the area and the w.e. of the coherently moving parts.

    Re: In fact, the *area* and *water equivalent* in the tables correspond to the coherently moving parts. We have provided the area of rock glaciers in the combined table:

**Table 4. Summary of the geometric and structural parameters and the inferred ice content of the coherently moving part of rock glaciers in the study area.**

| Rock glacier | Area ($A_{rg}$) (km$^2$) | Area of the coherently moving part ($A_{cmp}$) (km$^2$) | Width (W) (m) | Active layer thickness ($h_{al}$) (m) | Surface slope ($\alpha$) (°) | Inference ice content (%) | Water volume equivalent of the coherently moving part (million m$^3$) |
|---|---|---|---|---|---|---|---|
| Kala-Patthar | 0.275 | 0.074 | 240 | 0.68 | 9 | 70±8 | 1.4±0.2 |
| Kongma | 0.384 | 0.077 | 300 | 0.83 | 13 | 72±8 | 1.5±0.2 |
| Lingten | 0.228 | 0.094 | 240 | 0.65 | 20 | 74±8 | 5.9±0.6 |
| Nuptse | 0.310 | 0.234 | 400 | 0.30 | 13 | 74±8 | 2.0±0.2 |
| Tobuche | 0.236 | 0.128 | 400 | 1.67 | 16 | 74±8 | 2.7±0.3 |

30. Section 4.5: As written above this is a very rough approximation. Simply extrapolation the values from 5 rock glaciers does not really add to our knowledge considering the uncertainty. If the authors aim to upscale then using their approach and including the velocity information.

    Re: We have removed the discussion of regional extrapolation from the revised manuscript.

31. Section 5 Discussion: As written the discussion about the uncertainties is appreciated, but also highlights the large uncertainties and hence sheds many questions on the approach. I recommend the authors also to highlight the advances of the presented approach in relation to the literature and, hence, better justify their presented approach. A relevant paper to consider here or maybe already when presented the method is Hartl et al. (2016)

    Re: We have taken the suggestion and rewritten the *Discussion* to present both an analysis of the uncertainties (Sect. 5.1) and the advances of the approach in relation to previous research (Sect. 5.3):

    **"5.3 Contribution and prospect of the approach**

    For the first time, we build a model framework to infer ice content with remote sensing-based input by taking advantage of the existing observational data. Previous research either relied on costly and labor-intensive in-situ methods, such as borehole drilling and geophysical surveys, to measure the ice content of individual rock glaciers (e.g., Haeberli et al., 1998; Hauck, 2013), or provided categorized estimates for regional scale studies (e.g., Jones et al., 2018 and 2021). The approach we have developed makes it possible to more conveniently and quantitatively assess the ground ice stored in individual or even region-wide rock glaciers.

    The proposed approach can be further improved. The likely emergence of more data to be integrated for model calibration and validation, will allow for improving the accuracy of the method. 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. We expect the improved model can be valuably applied to mountain permafrost regions where rock glaciers are widespread for preliminary water storage evaluation."

32. L403ff: As written above: This is basically a listing of the headings of the subchapters and can therefore be removed.

    Re: We have rewritten the *Discussion* while keeping a short introduction at the beginning of the section. We have given reasons for this in our response to Comment #24.

33. L410: Suggest to wright "reader" instead of "user"

    Re: Changed.

34. L470ff: Either provide more detailed information about the investigated rock glaciers (the Tien Shan is larger) and an accessible reference or omit this paragraph.

    Re: We have omitted the paragraph from the discussion as suggested.

Overall, quite difficult to judge the overall value of the study. One hand the study is highly interesting and important on other hand contains many shortcomings leading to highly uncertain results. My suggestion would be to split the paper into two: One which focusses on model development (in a region with suitable in-situ measurements) and one which applies an improved method to the larger region.

Re: Again, we would like to thank the reviewer for providing many constructive and insightful feedbacks. We take the suggestion from the reviewer to omit the regional extrapolation from the revised manuscript.

This work is motivated to propose a novel approach for assessing the hydrological value of rock glaciers in a more convenient and quantitative way. Essentially, the methodology consists of both model development based on data from well-studied regions and model application to less-studied areas. Therefore, we maintain the manuscript as one piece for the sake of the integrity of the overall research objective and methodological framework.

Additional references not cited in the manuscript:

Fukui, K., Fujii, Y., Ageta, Y., Asahi, K., 2007. Changes in the lower limit of mountain permafrost between 1973 and 2004 in the Khumbu Himal, the Nepal Himalayas. Global Planet. Change 55, 251–256.

Hartl, L., Fischer, A., Klug, C., Nicholson, L., 2016. Can a simple numerical model help to fine-tune the analysis of ground-penetrating radar data? Hochebenkar rock glacier as acase study. Arct. Antarct. Alp. Res. 48, 377–393. https://doi.org/10.1657/AAAR0014-081.

Miles, K.E., Hubbard, B., Miles, E.S., Quincey, D.J., Rowan, A.V., Kirkbride, M., Hornsey, J., 2021. Continuous borehole optical televiewing reveals variable englacial debris concentrations at Khumbu Glacier, Nepal. Communications Earth & Environment 2, 12. https://doi.org/10.1038/s43247-020-00070-x.

Strozzi, T., Kääb, A., Frauenfelder, R., 2004. Detecting and quantifying mountain permafrost creep from in situ inventory, space-borne radar interferometry and airborne digital photogrammetry. Int. J. Remote Sens. 25, 2919–2931.

Wirz, V., Gruber, S., Purves, R.S., Beutel, J., Gärtner-Roer, I., Gubler, S., Vieli, A., 2016. Short-term velocity variations at three rock glaciers and their relationship with meteorological conditions. Earth Surf. Dynam. 4, 103–123. https://doi.org/10.5194/esurf-4-103-2016.