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.

General comments:

- 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.
- 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.
- 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?

- 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.
- 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.

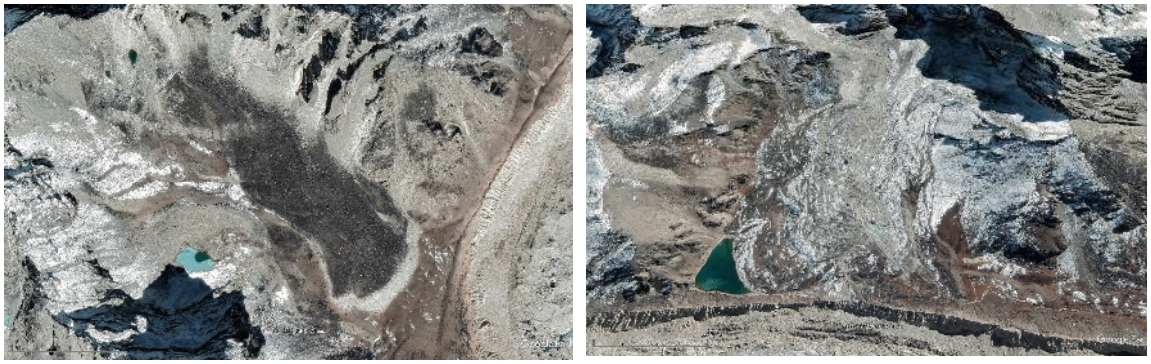

Figure 1: Nuptse and Kala-Patar rock glaciers (Image source: Pleiades from Google Earth)

- 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.
- 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.

- 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.

Specific comments

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.

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.

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.

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.

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.

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.

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.

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

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.

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

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

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

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

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.

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

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.

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.

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.

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

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

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

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

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.

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

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.

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

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

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

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

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.

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)

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

L410: Suggest to wright “reader” instead of “user”

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

Overall, quire 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.

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.