

The Cryosphere Discuss., referee comment RC1
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Comment on tc-2021-110

Anonymous Referee #1

Referee comment on "Modelling rock glacier ice content based on InSAR-derived velocity, Khumbu and Lhotse valleys, Nepal" by Yan Hu et al., The Cryosphere Discuss., <https://doi.org/10.5194/tc-2021-110-RC1>, 2021

The study from Hu et al. entitled 'Modelling rock glacier velocity and ice content, Khumbu and Lhotse Valleys, Nepal' proposes a model to infer rock glacier ice content based on InSAR velocity measurements. The model is calibrated based on the observational data of the Chilean Las Libres rock glaciers and validated using data from four rock glaciers in the Alps, before to be applied in NE Nepal. The objective is to estimate the water storage of the rock glaciers at the regional scale.

The research is very comprehensive, the approach is novel and valuable for future studies in similar mountain permafrost environments. The study's scope is well suitable for publication in The Cryosphere. I have no major concern regarding the main methodology and results, but the paper could definitively be improved by modifying the structure, clarifying some steps of the procedure and extending the discussion. These main points are further explained thereafter. Detailed comments are listed at the end of the review.

Workflow and structure:

Due to the extensive work of the authors, the complex articulation of the research steps, the multiples datasets and areas used for the model calibration, validation and application, it is sometimes hard to follow the workflow. I believe that some adjustments of structure may easily help the reader to go through the paper and understand the main elements.

In the abstract (l.15-18), at the end of the introduction (l.58-65) and in Fig.2, the workflow follows a logical order, starting with the model design and finishing with the model application. However, the methods and results sections are upside-down, starting with InSAR data and continuing with the model. Consequently, we go back and forth between the rock glacier sites used at the different steps and the reader gets a bit lost.

For example: 3.2.5. is far after 3.2.1, although the application is based on InSAR. And 4.2 is coming just after the InSAR results in Nepal but the rock glacier velocity mentioned at l.292 is in that case simulated on swiss rock glaciers.

In addition, I think that Fig.4 is a result and should be added in part 4. The extrapolation to whole Nepal may also be considered as a result (as you also somewhat acknowledge by listing it as a main conclusion at l.478-480).

One suggestion of structure (both for methods and results): model calibration, model validation, sensitivity analysis, model application based on InSAR, regional extrapolation. And then really focus the discussion on the limitations and prospects.

InSAR coherently moving parts:

Something is missing to fully understand your definition of coherently moving parts and why you decided to do so.

At l.109, I don't understand the point (2). It seems to me that it may tend to exaggerate the rate if artificially discarding low velocity. At l.111-112: partly same question: why only higher than 5 cm/yr in more than half of the periods? I don't think it falls into the definition of what is coherent or not, at least not from an InSAR point of view. And from a process point-of-view, what about areas that are coherently not moving (or slowly)?

Do you assume that under < 5 cm/yr there is no more activity/ice, and consider the previous inventory outdated? If yes, it makes somewhat sense but it is important to clearly explain it in the methods and better discuss it in Section 5. If not, one consequence on the results is that the covered areas are much smaller than the initial inventoried landforms (Fig.6, especially for a and b). Did you then extrapolated the ice/water volume to the whole rock glacier, and if not, which potential underestimation may it cause, also for the regional extrapolation presented in Section 5.1?

Method justification vs discussion:

Section 5.2 proposes a relevant list of elements (l.372-375) that can be seen as limitations and supposed to be used to discuss the validity of the approach. However, the way most points are discussed is a bit frustrating: it sounds more like justifying the choices (which should be part of the methods) than acknowledging the limitations and putting the results into a larger context.

For example: at l.403-407: 'we infer that these rock glaciers develop in a warm permafrost environment for the following reasons: ...'. This is not really a discussion, rather an explanation for a chosen assumption. In general for 5.2.2: I don't think the question of the warm permafrost assumption has not been really introduced before.

At l.437: 'We introduce this concept because it corresponds with the general model setup.': Saying that it follows the design you chose is not really an explanation, neither a discussion. Overall in 5.2.5: Before justifying it, explain what could be the problems.

In 5.2.6: Ways to tackle the issue are presented (l.451), but the issue itself is not really introduced (saying that the rheology of rock glaciers in Nepal are not necessarily similar than Las Libres).

Additional thinks that could be further discussed in Section 5:

- Elements previously mentioned regarding the coherently moving area definition and the update of the inventory using InSAR-kinematics.
- How to be sure that the velocity you are measuring is really related to rock glacier creep? As single SAR geometries are used, the values are initially along LOS and could f.ex correspond to subsidence due to melting.
- In the model, there is no water at all in the active layer. Is it realistic? Would it change the results if adding a water content as well?

Detailed comments:

▪

Title: As you actually used velocity measurements as input to the model in your study area, a title such as 'Modelling rock glacier ice content based on InSAR velocity, Khumbu and Lhotse Valleys, Nepal' would sound more correct to me.

▪

l.14 and 16: Repetition 'model to infer ice content of rock glaciers' could be avoid.

▪

l.21-22: This sentence could be simplified. For ex: 'Due to the accessibility of the model inputs, the approach is easily applicable to permafrost regions where..., and thus valuable to estimate the water storage...'

■

I.29: 'The potential hydrological value of rock glaciers, and thus their importance in terms of hydrological research... Corte (1976); despite this, research...': long sentence, with strange structure and quite some repetitions. Possible to simplify?

■

I.35: 'triggers' instead of 'produces'? / 'rock slope failure and mountainside collapses': what the difference?

■

I.38: Could start the sentence directly with 'Jones et al. (2021)...'

■

I.39-40: 'The relative importance of rock glacier ice content compared to glaciers in the region is 1:25, ...'

■

I.42-43: Maybe a personal preference and definitively a detail: Easier to write without ; and making two sentences.

■

I.45: I don't understand 'the likelihood of glacier-rock glacier transition' part and I believe you are anyway not discussing it in this paper. I would suggest: 'However, there is a lack of modelling studies to test these postulations and assess the hydrological impacts of the glacier-rock glacier transition'. But, if the point of it is to potentially use the results of this study as a baseline, with future updates to see the change of ratio (ice content of RG compared to G), you can also add something about it in the discussion (prospect).

■

I.47-48: Contradicts with the previous paragraph where you refer to Jones et al. (2021), who have provided quantitative information concerning ice content. You may consider inverse the paragraph order, and replace "absence of quantitative information" by something like "we have little quantitative information".

■

I.63-65: You are not modelling the kinematic response, you are measuring it and modelling the ice content. Rephrase to for ex: 'We apply the calibrated model for five rock glaciers... and model their ice contents based on remote sensing...'

■

I.67-68: The Khumbu and Lhotse glaciers draining... to remove the unnecessary parantheses.

■

I.73: Altitudinal limit of permafrost: missing a reference here.

■

I.78: 'For the period of 1994–2013, recorded accumulated annual precipitation was 449 mm yr⁻¹, ...

■

I.83: You give a reference for the delineated RGs, but not for the DCG.

■

I.85-86: See main comment: here the structure is counter-intuitive (opposite of the introduction).

■

I.93: I guess here you mean 'We selected the interferograms...'

■

I.97: Missing an information about the final resolution you achieve.

■

I.100: How do you know it is stable? Based on visual interpretation? Good to say it. And rather say: 'supposed to be stable'.

■

I.101: The water vapour is not delayed, the phase is. The end of sentence is also a bit clumsy I think. Maybe 'atmospheric and ionospheric effects including phase delay due to water vapour can be effectively removed because they lead to long-wavelength spatial artefacts and...'

■

I.102: 'because these lead to long-wavelength artefacts across the region'.

■

l.105: 'projected ... onto the downslope direction'.

■

l.107-108: The start of the sentence is about the criteria to select valid pixels, while point (1) describes which pixels were discarded. Phrasing in (1) could be inversed (> 0.3 are kept).

■

l.109: I don't understand point (2). It seems to me that it may tend to exaggerate the rate if artificially discarding low velocity. See main comment about InSAR.

■

l.111-112: Partly same question as my point 24: Why that? See main comment.

■

Table 1, caption: List of ... interferograms used in the study.

■

Figure 2: As I understood, you just used the coherent part as input to the model, so it may be enough to write 'InSAR-derived kinematics on coherently moving parts'.

■

l.123: Since you are not assuming shear horizon at depth in the model, it sounds weird to have it mentioned at the second line of the section, without then acknowledging in a way or another the limitation before the discussion.

■

l.134-137: I am struggling to understand the point of this part. Too detailed or not enough. What is happening when the critical volumetric debris content is reached? What is the implication for this study? If it is important, one would like to know the actual relation between the ice-debris mixture strength parameter, and the debris content.

■

l.186 and 190: Little detail: not sure it is necessary to have 'collected by'/'detailed in' before references.

■

l.195: '... by Arenson and Springman (2005a) who evidenced a parabolic relationship...'

■

l.201-204: Instead of using 4 lines, you could just entitle the equation lines: Scheme 1: $u_s =$ / Scheme 2: $u_s =$ / Scheme 3: $u_s =$

■

Figure 4: It could be moved to Results. Also, since you numbered the Schemes 1-2-3, it would be good to label the subplots a)-c) accordingly, by adding subtitles to make it easy to understand.

■

l.213-216: Long sentence, hard to understand since it is a double-validation of both the velocity and the ice content. May find a way to rephrase / divide the sentence.

■

l.234: Air density: provide the actual values.

■

l.245: Currently not really understandable: what is the usual value range in reality?

■

Table 3: Necessary information? Could be moved to Supplementary, to shorten a bit the really heavy method section.

■

l.255: 'Active layer thickness was determined as the mean value over the extent of each rock glacier, based on the 2006–2017 estimate from the...'

■

l.259: Is the estimate of water based on the whole inventoried rock glacier or the coherently moving part? See main comments.

■

l.265: In a way, this table is already a results, as it is based on the coherently moving

parts of the rock glaciers, presented later in the paper.

■

I.267-268: It cut the workflow to separate InSAR to the model application. See main comment about structure.

■

I.274: `...approximately similar values`

■

I.276: `...during the observational periods`. You may also emphasize somewhere what the timseries vary from site to site.

■

I.279: `since 2010`: The evidenced acceleration is based on one value also, i.e. the difference between the two last acquisitiond dates, right? Maybe writing `between 2010 and 2015 acquisitions` would be more correct.

■

In 4.1: More references to the Fig.5 subplots would help the reader to make the link.

■

Figure 6: Missing scales.

■

I.293-296: Partly repetition with Methods.

■

I.301: Reference and inference ice content: why not simply saying `observed` vs `modelled`?

■

I.301-303: Missing references to Scheme 1/3 graphs (Fig. 7, 9). If you think there are unnecessary, you may consider moving them to Supplementary.

■

I.303-304: 'However, the above bias is not statistically useful for correcting the modelling results due to the limited amount of validation data.' Not clear, could be more discussed, here or in Section 5?

■

Figure 7: Is it correct to say that the intersect of the yellow & blue lines correspond here to the 'truth' (observed values / references)? If yes, it would be useful to highlight it better (encircle it for ex).

■

Figure 8/9, captions: Add full captions instead of referring to 7.

■

I.325: 'The model has higher sensitivity to the surface slope angle...

■

I.327: '...the model is mostly sensitive to...'

■

I.334: Interred ice content based on Scheme 2, right?

■

I.336-339: Separate the information related to % and total volume, and add a reference to geometrical information from Table 4 would help making sense of it.

■

Section 5.1: I would say that it is a result. See main comment.

■

I.364-365: Based on which study? Jones or yours? As you refer to previous research just before, it is not fully clear.

■

I.367: '... across the entire Himalayas'

■

I.373: '(3) absence of shear horizon' (also at I.408).

■

I.382: '... to evaluate the stability...'

■

I.385: You could probably cut 'This is not surprising'.

■

I.391: 'creep parameter' is only mentioned once before and referring to n , not A (I.192). A is described in more general terms at I.128.

■

I.413: 'This short-term feature of rock glacier kinematics is assumed to be insignificant...'. And it would be more logical to move this statement at the end of 5.2.3.

■

I.421: Add reference to Fig.10.

■

I.428-429: 'Thus, the uncertainty introduced... is unavoidable.' I don't see the causal link with the previous sentence here. It is not because Cicoira et al. (2020) also had accuracies at the same level that it is unavoidable.

■

I.437-438: 'We introduce this concept because it corresponds with the general model setup.' That is no explanation... Just saying 'we did it because we designed it that way'...

■

I.443-445: Without more explanations, this is not understandable.

■

I.451: Which issue? You have not mentioned an issue yet.

■

I.465 and I.481: 'surface-velocity-constraints': surface velocity would'nt be enough? To avoid a long word in 3 parts.

■

I.469: 'emerging': What does it mean in that case?