Responses to RC1

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.

Re: We thank the reviewer for his/her insightful, constructive, and detailed comments. We take the suggestions carefully and address all the comments with our point-by-point replies given below. The line numbers refer to the previously submitted discussion paper, aiming to point out where the revisions are made to the discussion paper accordingly.

1. 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.

Re: We agree with this more consistent and easy-to-follow structure proposed by the reviewer. We have adopted the suggested paper structure and reconstructed the sub-sections of methods and results into the following sequence: 3.1 model design and assumptions, 3.2 model calibration, 3.3 model validation, 3.4 sensitivity test, 3.5 model application based on InSAR, 3.6 regional extrapolation; 4.1 Calibrated

parameterization schemes, 4.2 model validation, 4.3 model sensitivity, 4.4 modelled ice content in Khumbu and Lhotse valleys, 4.5 potential water storage in rock glaciers in the Nepalese Himalaya.

(Kindly remind that the section/sub-section numbers in this response letter still refer to that in the previously submitted discussion paper)

2. 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?

Re: We set 5 cm yr^{-1} as a threshold for selecting valid InSAR observations (l. 109) in consideration of the conservative estimate of uncertainty in ALOS-1 PALSAR interferometry (Wang et al., 2017).

Then at l. 111–112, we define the coherently moving part mainly for simplifying the non-uniform spatial distribution of surface velocities of rock glaciers in nature, which deviates from the assumed homogeneous model (as illustrated in Fig. 3), where the surface velocities should be constant all over the landform given a homogeneous composition and geometry (as mathematically expressed in Equation 9). To deal with this deviation, we intentionally reduce the spatial and temporal resolution of the InSAR-derived kinematic data by taking the range of spatially averaged velocities of the rock glacier during the observational periods to represent its overall movement. By defining the coherently moving parts, we aim to identify the portion of the landform that approximately corresponds with our designed model (Sect. 3.2.1, Fig. 3) and thus to ensure it is suitable for applying the homogeneous model and inferring an average ice fraction accordingly. We set 5 cm yr^{-1} as a threshold considering that a pixel with a velocity above it is an area actively in motion with the landform as a whole.

For the areas that do not meet the criteria, they may be active (> 5 cm yr^{-1}) during certain periods and contain ice, but we doubt whether they should be regarded as part of the assumed homogeneous landform — or from the process point-of-view — whether permafrost in these active areas move along the same plane at depth, as the internal structure of rock glaciers in reality are not homogeneous either.

As regards to the covered areas for estimating ice content in our model application (Sect. 4.4), they are indeed smaller than the inventoried landforms. In this regard, the inference we made is a conservative one. However, the mountain range scale extrapolation (Sect. 5.1) is not drawn from the areal extent of the previously inventoried rock glaciers. We made a simple extrapolation based on the average ice

content of the rock glacier estimated from our study area and the number of the landforms across the Nepalese Himalaya.

3. 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).

Re: We appreciate the sound judgement made by the reviewer. Sect. 5.2 lacks the clear acknowledgement of the limitations and discussion in a larger context. Much of the content mentioned here, such as the Sect. 5.2.2, should be re-structured to the methodology section as necessary justifications.

For Sect. 5.2.5, a more detailed explanation has been provided in the previous response. One problem of our definition is the likely underestimation of the ice content in the rock glacier.

In Sect. 5.2.6, we have now clearly stated that the rheology of rock glaciers in Nepal are not necessarily similar to 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?

Re: The issues regarding the coherently moving part have been elaborated in the previous response and constructed a discussion section in the revised manuscript.

We acknowledge the limitations of using 1-D InSAR detection for measuring downslope velocities of rock glaciers, though it is a commonly adopted method in recent studies (Brencher et al., 2021; Hu et al., 2021; Liu et al., 2013; Wang et al., 2017). The assumption that the rock glacier moves towards the downslope direction is no longer valid provided that significant subsidence is ongoing. However, the

landforms in our study area are not undergoing melting-induced subsidence, as we do not observe any surface depressions or cracks from optical images.

It is unrealistic that the active layer does not contain any water at all. However, we ignore this variable for two reasons: (1) it is difficult to quantitatively determine or assume the water content stored in the active layer of a rock glacier. (2) in our model setup, the active layer only affects the landform motion by altering the driving force. Therefore, if the new variable, i.e., water fraction in the active layer, is integrated, it would play a similar role as the existing variables including the active layer thickness, the debris density, and the debris fraction in the active layer, all of which are insensitive factors of our model (Sect. 4.3, Fig. 10).

4. 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.

Re: We agree with the suggested title.

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

Re: Revised. At l. 16, "We apply the model to five rock glaciers in Khumbu and Lhotse Valleys, northern Nepal."

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…'

Re: We have simplified the sentence: "Due to the accessibility of the model inputs, the approach is easily applicable to permafrost regions where previous investigation is lacking, and thus valuable to estimate the water storage potential of the remotely located rock glaciers."

l.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?

Re: We have simplified the sentence: "Corte (1976) first proposed the potential hydrological value of rock glaciers. However, research on the role of rock glaciers in maintaining hydrological stores in mountain catchments remains limited."

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

Re: We have re-written the sentence: "The paraglacial response of high mountain slopes would contribute to this process, as glaciers undergo downwasting, which triggers rock slope failures and mountainside collapses and increases the flux of rock debris to glacier surfaces."

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

Re: We have changed the sentence: "Jones et al. (2021) was the first to show that…"

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

Re: We have changed the wording according to the reviewer’s suggestion.

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

Re: We have edited the sentence: “We also expect rock glaciers to provide water supplies long after glaciers have melted. In other high arid mountains, such as the Andes, ice-cored rock glaciers have persisted in valleys long after glacier recession (Azócar and Brenning, 2010; Monnier and Kinnard, 2015a).”

l.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 impactions 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).

Re: We would like to take the suggestion and change the sentence: “However, there is a lack of modelling studies to test these postulations and assess the hydrological impactions of the glacier-rock glacier transition.”

l.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 quantitive information” by something like “we have little quantitative information”.

Re: We have changed the sentence: “We have little quantitative information concerning the ice content of rock glaciers, which hinders our understanding of the likely future hydrological role of rock glaciers.”

l.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 rok glaciers… and model their ice contents based on remote sensing…’

Re: We have revised the sentence: “Finally, we apply the calibrated model for five rock glaciers in the study area of north-eastern Nepal and model their ice contents based on remote sensing-derived downslope velocities as constraints.”

l.67-68: The Khumbu and Lhotse glaciers draining… to remove the unecessary parantheses.

Re: We have changed the sentence: “Among the highest in the world, the Khumbu and Lhotse glaciers draining Everest and Lhotse and have well defined debris-covered snouts.”

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

Re: The references are the following two papers: Jakob, 1992; Fujii and Higuchi, 1976. We have changed the sentence: “The five rock glaciers examined in this study are situated at 4900–5090 m a.s.l., near the altitudinal boundary of discontinuous permafrost in the region: previous seismic refraction surveys conducted on active rock glaciers indicate that the lower limit of permafrost occurrence in this

region to be ~5000–5300 m a.s.l. (Jakob, 1992), which is consistent with an earlier estimate of 4900 m a.s.l. based on ground temperature measurements (Fujii and Higuchi, 1976)."

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

Re: Modified.

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

Re: We have added an introduction in the caption (l. 83): "The RGs are delineated by Jones et al. (2018) and the DCGs by the authors based on Google Earth images."

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

Re: We have changed the structure according to the reviewer's suggestion.

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

Re: Yes, we have modified the wording.

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

Re: We have added a sentence at l. 97: "The final resolution we achieved is ~ 30m."

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

Re: Yes, we made the assumption based on visual interpretation of the Google Earth images, for instance, a reference pixel tends to occur at the surface of flat bedrock. We have changed the sentence: "We randomly selected three pixels at places supposed to be stable near each ice–debris landform and …"

l.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...'

Re: We have revised the sentence to the suggested more accurate one: "By doing so, atmospheric delays can be effectively removed because these lead to long-wavelength artefacts and can be assumed as constant within the range of our study objects."

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

Re: Modified. See the previous response.

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

Re: Revised.

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).

Re: We have changed the sentence: "(1) the pixels showing acceptable coherence (> 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 artifically discarding low velocity. See main comment about InSAR.

Re: This is due to the precision estimate of InSAR measurement using ALOS-PALSAR data. See our previous response to the main comment.

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

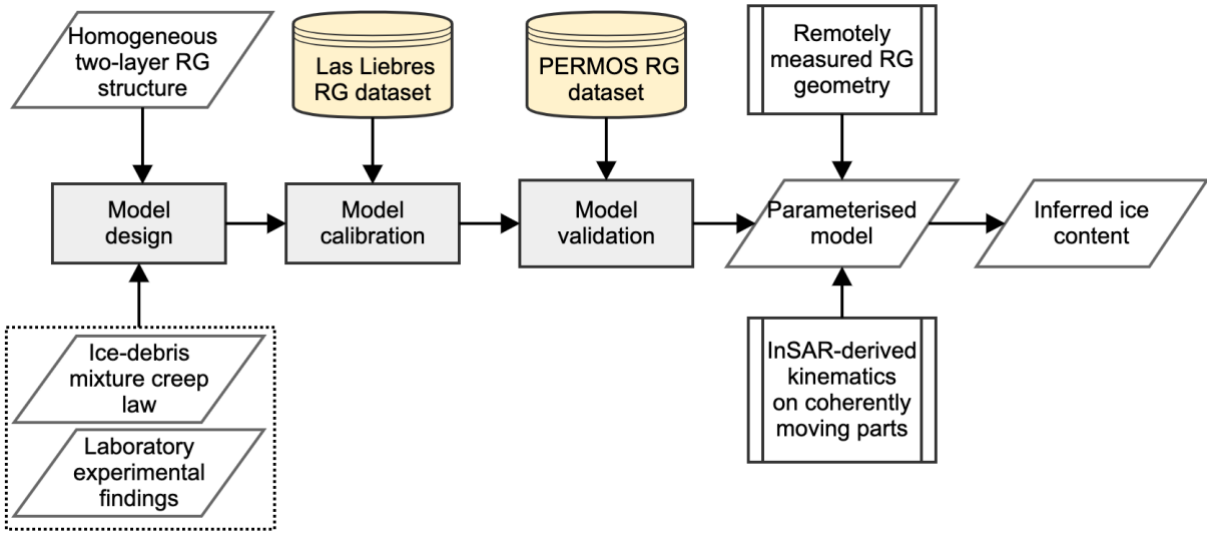
Re: Please refer to the response to the main comment.

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

Re: We have revised the caption: "List of ALOS PALSAR and ALOS-2 PALSAR-2 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'.

Re: We have changed the notes of this figure.



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.

Re: We have added an acknowledgement immediately after this sentence: "In this study, we neglected the existence of shear horizon in the model design."

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.

Re: When the critical volumetric debris content (42%, according to Moore (2014)) is reached, the presence of rich debris would introduce competing effects to the deformation of ice-debris mixture: on the one hand, increased debris fraction causes strengthening of the mixture by introducing interparticle friction (Ting et al., 1983); on the other hand, the addition of debris decreases the shear strength due to

the lubricating and stress-modulating effects exerted by the unfrozen water concentrating at the debris-ice interfaces (Arenson et al., 2007; Ikeda et al., 2008). In this study, we ignored this complicated mechanism produced by increased debris and assumed that the rock glacier has an ice-rich core, i.e., debris fraction is less than 42%.

We have added a statement at l. 134: "We assumed the rock glacier has an ice-rich core."

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

Re: We have removed the phrases.

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

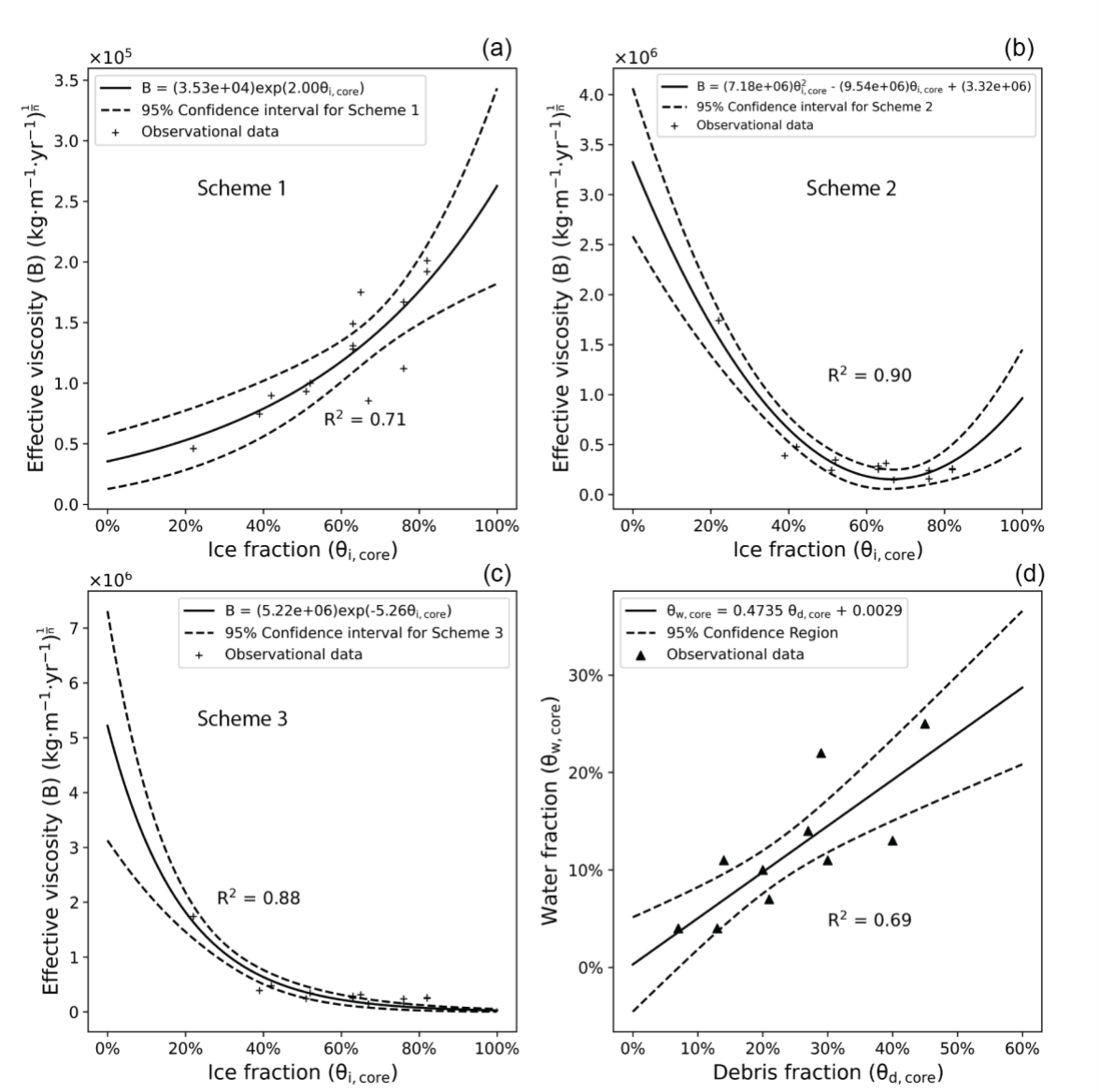
Re: We have changed the sentence: "This trend was also depicted by Arenson and Springman (2005a) who evidenced a parabolic relationship between the minimum axial creep strain rate and the volumetric ice content."

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

Re: We have made the suggested changes and removed the four lines.

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.

Re: We have labeled the subplots according to this comment.



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.

Re: We have re-written the sentence: "We simulated the surface velocity (u_s) of each rock glacier by varying volumetric ice content ($\theta_{i,core}$) of the permafrost core. Then we compared the modelled velocity with the measured velocity from Terrestrial Geodetic Surveys (PERMOS, 2019).

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

Re: In the validation part, ρ_a=1.007 kg/m^3, as the range of elevation for the four rock glaciers is between 2600 m and 2900 m.

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

Re: We take 1450–3450 kg/m^3 as a realistic range, as the lower value…

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

Re: We have moved the table to the Supplementary File.

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…'

Re: We have changed the sentence to: "Active layer thickness was determined as the mean value over the extent of each rock glacier, based on the 2006–2017 estimate from the European Space Agency Permafrost Climate Change Initiative Product (ESA CCI) (Obu et al., 2020)"

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

Re: It is based on the coherently moving part.

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.

Re: Concur. We have moved the table to the result section.

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

Re: We agree with this comment and have changed the manuscript structure accordingly.

l.274: '...approximately similar values'

Re: Modified.

l.276: '...during the observational periods'. You may also emphasize somewhere what the timeseries vary from site to site.

Re: Detailed information is presented in Fig. 5. We did not further analyze the variations among the landforms because here we want to emphasize the common feature that all the rock glaciers move at a nearly stable rate.

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

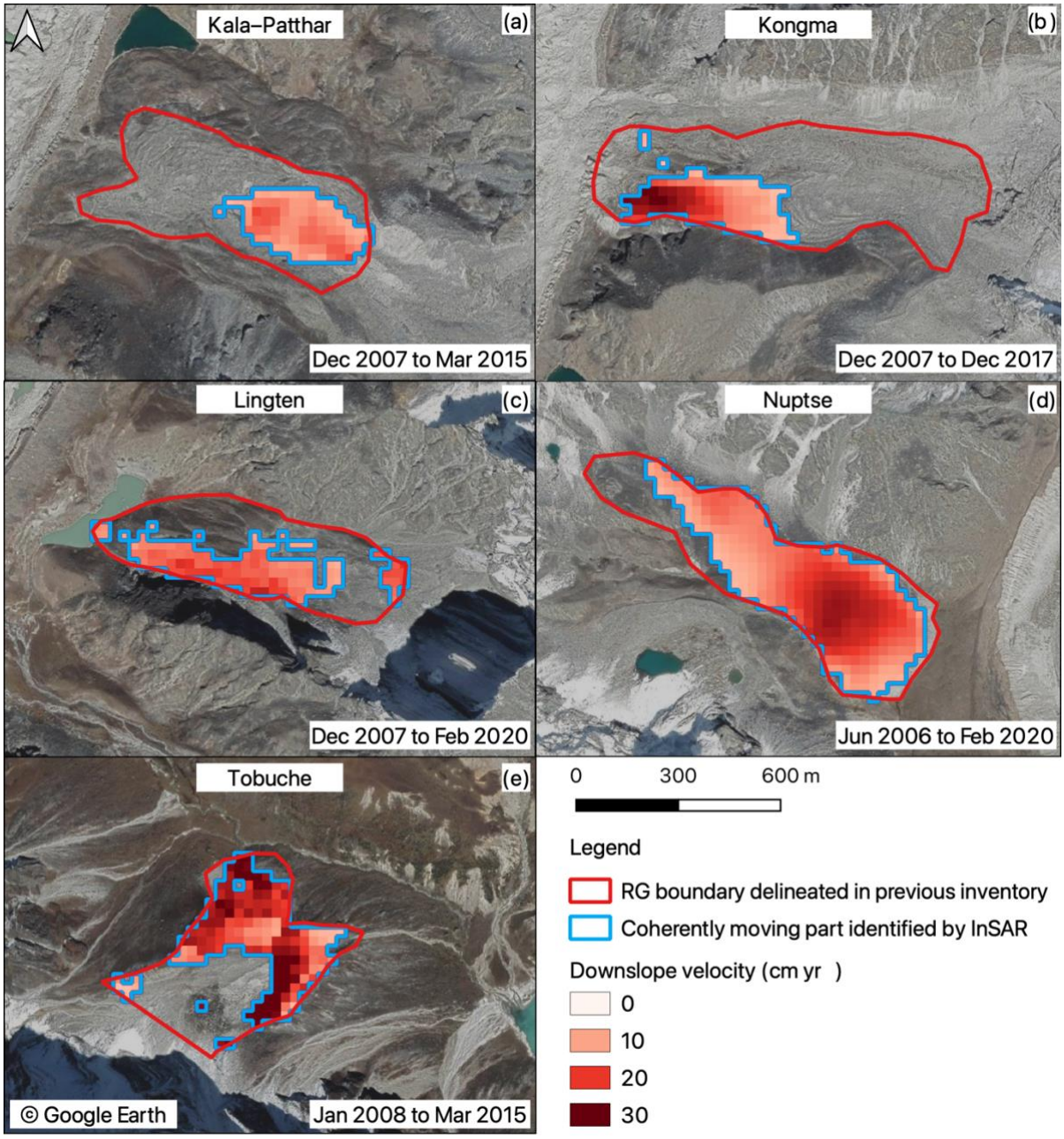
Re: We have changed the wording: "Tobuche displayed similar stable behaviour before 2010 but had accelerated by more than four times from 14.9±0.2 cm yr^{-1} to 81.4±2.4 cm yr^{-1} between 2010 and 2015 acquisitions."

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

Re: We have added more references: "…with the largest standard deviation being 3.4 cm yr^{-1} for Lingten (Fig. 5d). The maximum velocity represents the local extreme of downslope motion and was as high as 112.1±12.4 cm yr^{-1} for Lingten during 2019/07/15–2019/08/26 (Fig. 5d). Tobuche displayed similar stable behaviour before 2010 but had accelerated by more than four times from 14.9±0.2 cm yr^{-1} to 81.4±2.4 cm yr^{-1} between 2010 and 2015 acquisitions (Fig. 5e). The maximum velocity reached 181.0±57.4 cm yr^{-1} during 2015/03/18–2015/03/22 (Fig. 5e)."

Figure 6: Missing scales.

Re: We have added a scale to Fig. 6.



l.293-296: Partly repetition with Methods.

Re: We have removed these three lines.

l.301: Reference and inference ice content: why not simply saying ‘observed’ vs ‘modelled’?

Re: We have changed the wording to ‘observed’ and ‘modelled’.

l.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.

Re: We have moved Fig. 7 and Fig. 9 to Supplementary.

l.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?

Re: Here we want to point out that we did not use the average bias 8.4% to correct the modelled ice content for the rock glaciers in Khumbu Valley. We have changed the sentence: “The above bias (8.4%) is not used for correcting the modelling results due to the limited amount of validation data.”

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).

Re: The yellow lines are the observed velocities, and the blue lines are the reference ice content. They somehow correspond to the ‘truth’. What we want to highlight in Fig. 7 is the intersection between the grey shade and the yellow band, as marked by dash-dotted black lines. Such intersection indicates the range of estimated ice content.

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

Re: We have added full captions in the Supplementary File.

l.325: ‘The model has higher sensitivity to the surface slope angle...

Re: Modified.

l.327: ‘...the model is mostly sensitive to...’

Re: Modified.

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

Re: Yes, we have changed the sentence.

l.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.

Re: We keep the original structure of this paragraph because the two water volume equivalents were calculated corresponding to the two ice content estimates, i.e., the average value and the inferred range. We have added a reference to Table 4: “Nuptse stores the most ice by volume due to its largest dimensions (Table 4).”

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

Re: We have re-organized the paper structure according to the reviewer’s comments.

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

Re: It is Jones’s study we are referring to. We have changed the sentence: “…which is in the same magnitude predicted by Jones et al. (2018).”

l.367: ‘… across the entire Himalayas’

Re: Modified.

l.373: ‘(3) absence of shear horizon’ (also at l.408).

Re: Modified at the two places (l. 373 and l.408).

l.382: '… to evaluate the stability…'

Re: Modified.

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

Re: We have removed the phrase.

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

Re: A and n both can be referred to as creep parameters. We have made edits to l. 128: "…where A and n are creep parameters reflecting variations in environmental conditions…"

l.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.

Re: We have changed the sentence and moved to the end of this subsection: "This short-term feature of rock glacier kinematics is assumed to be insignificant to modelling the relationship between ice content and multi-annual average movement velocity in our study."

l.421: Add reference to Fig.10.

Re: Added.

l.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.

Re: We have revised the sentence: "The uncertainty introduced by thickness derivation when applied to rock glaciers without known information of structure cannot be eliminated with the existing empirical methods."

l.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'…

Re: We have provided more detailed explanations (also see our response to the main comment): "We introduce this concept mainly for simplifying the non-uniform spatial distribution of surface velocities of rock glaciers in nature, which deviates from the assumed homogeneous model (as illustrated in Fig. 3), where the surface velocities should be constant all over the landform given a homogeneous composition and geometry (as mathematically expressed in Equation 9). To deal with this deviation, we intentionally reduce the spatial and temporal resolution of the InSAR-derived kinematic data by taking the range of spatial mean velocities of the rock glacier during the observational periods to represent its overall movement. By defining the coherently moving parts, we aim to identify the portion of the landform that approximately corresponds with our designed model (Sect. 3.2.1, Fig. 3) and thus suitable for applying the homogeneous model and inferring an average ice fraction accordingly."

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

Re: We have removed this unnecessary sentence.

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

Re: We have re-written the sentence: “In addition, we tested this hypothesis in two ways: first…”

l.465 and l.481: ‘surface-velocity-constraints’: surface velocity wouldn’t be enough? To avoid a long word in 3 parts.

Re: We have replaced the long word with ‘surface velocity’.

l.469: ‘emerging’: What does it mean in that case?

Re: We have replaced it with “forthcoming datasets”.

Arenson, L. U., Springman, S. M., and Sego, D. C. (2007). The rheology of frozen soils. *Applied Rheology, 17*(1), 12147-12141-12147-12114.

Brencher, G., Handwerger, A. L., and Munroe, J. S. (2021). InSAR-based characterization of rock glacier movement in the Uinta Mountains, Utah, USA. *The Cryosphere, 15*(10), 4823-4844. https://doi.org/10.5194/tc-15-4823-2021

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