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 (3.5.1 deriving surface velocity constraints with Differential InSAR, 3.5.2 deriving geometric and structural parameters from remote sensing products); 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 (4.4.1 InSAR-derived surface kinematics as model constraints, 4.4.2 modelled ice content); 4.5 potential water storage in rock glaciers in the Nepalese Himalaya.

We have rewritten the discussion section focusing on the limitation and prospects of our work: 5.1 Incapability of predicting ground ice evolution; 5.2 Limited amount of field data for model calibration; 5.3 Uncertainty in deriving rock glacier thickness; 5.4 Limited application to rock glaciers in quasi-steady-state motion; 5.5 Uncertainty in estimating regional water storage; and 5.6 Potential improvements and prospect of the approach.

(Kindly remind that unless clearly stated otherwise, 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.

In Sect. 5.2.6, we have now rewritten the discussion part.

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: Detailed explanations regarding the coherently moving part have been elaborated in the previous response and presented in the methodology part in the revised manuscript (Sect. 3.5.1): "By defining the coherently moving parts, we aim to identify the portion of the landform that approximately corresponds with our designed model (Sect. 3.1, 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."

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. We discussed this aspect in the new discussion section (Sect. 5.4 and 5.5): "…the motion of rock glaciers undergoing significant subsidence cannot be measured accurately, due to the limitation of 1-D InSAR method: we convert the LOS measurements to surface velocities by assuming the rock glacier moves towards downslope direction without additional subsidence component. Rock glaciers showing strong subsidence indicators from optical images, such as surface depressions or cracks, are not suitable for the current method… an accurate 3-D surface velocity can be obtained by using multi-track InSAR method, allowing us to apply the model to rock glaciers with a complex velocity field."

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, north-eastern 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, yet 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: To make it clear, we have also taken the suggestion from another reviewer and changed the sentence: "The ratio between rock glacier ice content and that in glaciers in the region was 1:25…"

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: "In the future, we expect rock glaciers with high ice content 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: The reviewer is right. We are not going to discuss the transition in this study. In the revised manuscript, we have rewritten this part and removed this sentence.

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 quantitative information" by something like "we have little quantitative information".

Re: We have changed the sentence: "To date, we have little quantitative information concerning the ice content of rock glaciers, which hinders our understanding of the potential 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 rock 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 unnecessary parentheses.

Re: We have changed the sentence: “Among the highest in the world, the Khumbu and Lhotse glaciers draining Everest 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.

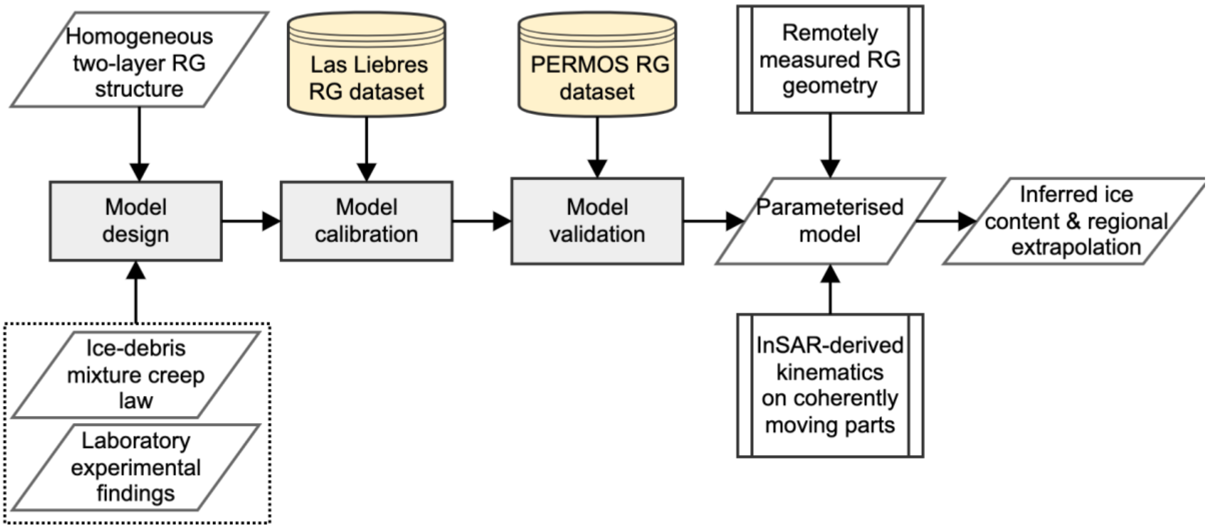
Re: Please refer to the response to the main comment.

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 replaced the sentence with: "Active rock glaciers are viscous flow features distributed in ice-rich alpine permafrost (Ballantyne, 2018; Berthling, 2011; Haeberli, 2000)."

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…'

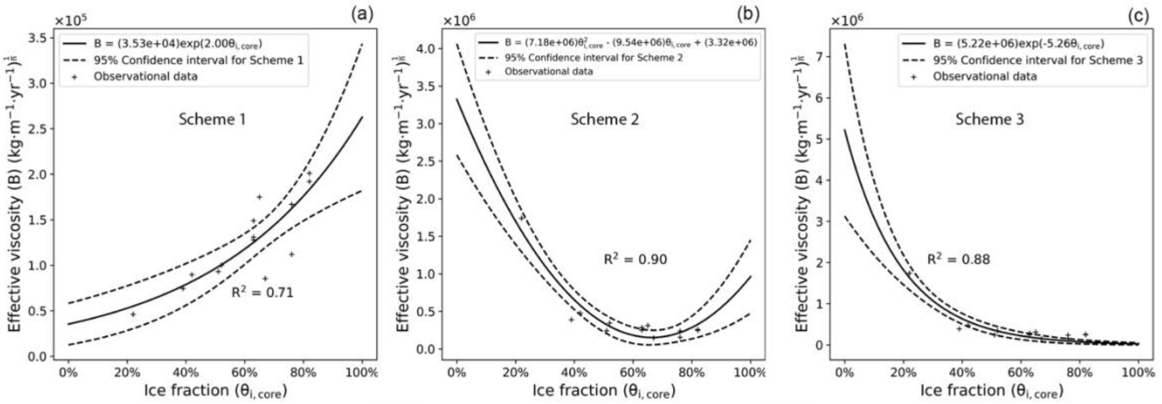
Re: We have changed the sentence: "This trend was also depicted by Arenson and Springman (2005a) who evidenced a parabolic relationship between the minimum axial creep strain rate and the volumetric ice content."

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 have added the realistic range in the sentence: "…we changed the value range to be consistent with the usual value range in reality (ρ_d: 1450–3450 kg/m^3; $\theta_{d,al}$: 13–93%)"

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.

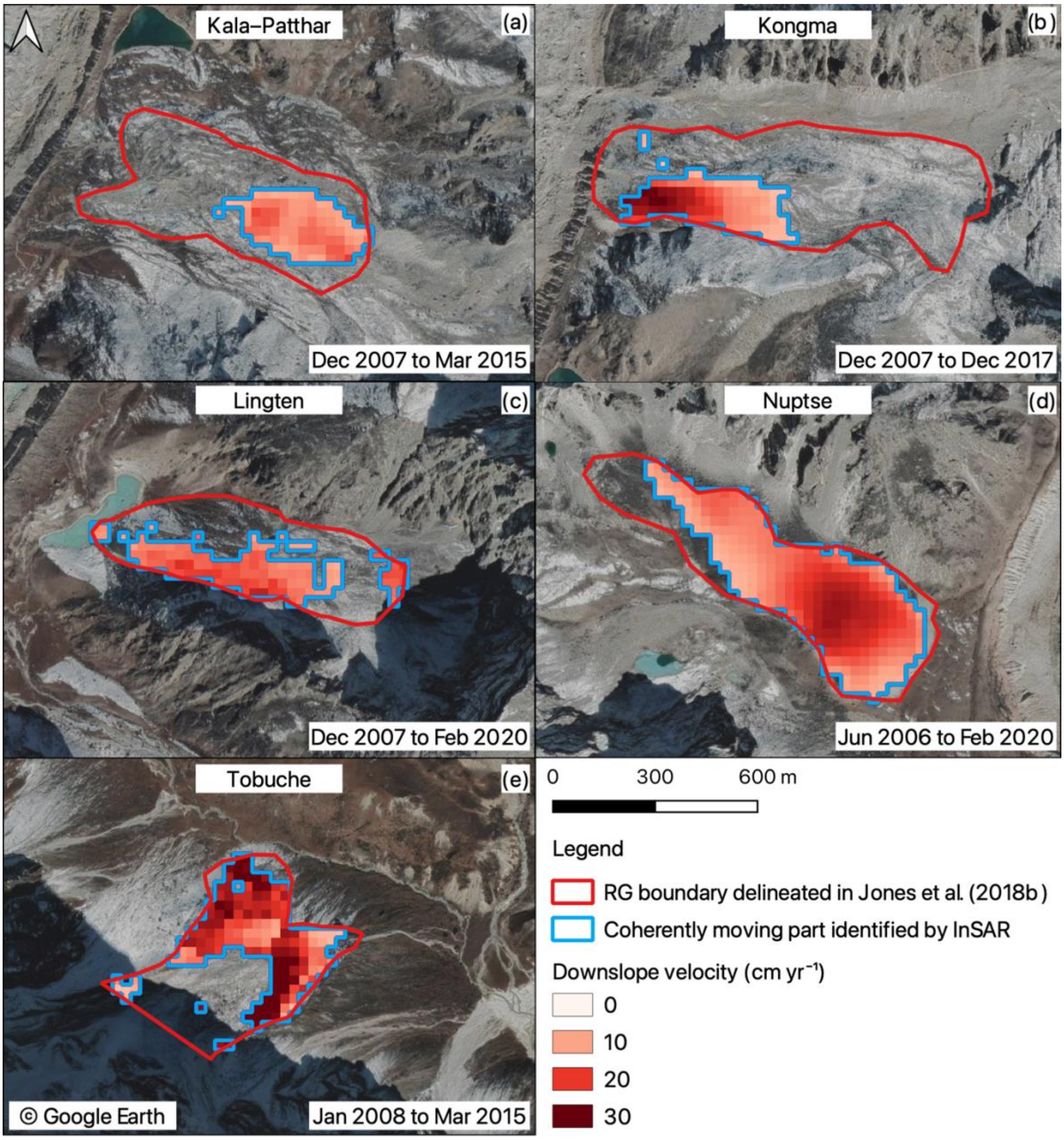
Re: We have changed the wording: "Tobuche displayed similar stable behaviour before 2010 but had accelerated by more than four times from 14.9±0.2 cm yr^{-1} to 81.4±2.4 cm yr^{-1} between 2010 and 2015 acquisitions."

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 the first part of the sentence.

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 added references to Fig. 7 and Fig. 9.

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: In the revised manuscript, we have changed our interpretation of the bias and modified the sentence accordingly: "We used the average bias derived from Scheme 2 to represent the uncertainty of our approach**.**"

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 (or observed) 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.

We also added a description in the caption of Fig. 8: "For each rock glacier, the intersection between the simulated $\theta_{i,core}$- u_s relationship (grey shaded area) and the observed velocity (yellow band) gives the estimated range of ice content, as marked by the dash-dotted black lines. The inference ice content is taken as the average value of the estimated range and indicated by the solid black line."

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

Re: We have added full captions to Fig. 8 and 9.

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 of a first-order prediction made 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: We have restructured the manuscript and paraphrased the relevant sentences: "Here we neglected the presence of shear horizon…"

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 mentioned 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: As the manuscript has been restructured, we have changed the sentence and moved to the Sect. 3.5.1 where we describe the method to derive surface velocity constraints with InSAR : "…the 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: We have rewritten the discussion on rock glacier thickness derivation.

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 rewritten the discussion on thickness derivation and revised the sentence: "The uncertainty introduced by thickness derivation when applied to rock glaciers without known information of structure cannot be eliminated at present."

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: In the restructured manuscript, we have added the method justification in Sect. 3.5.1: "By defining the coherently moving parts, we aim to identify the portion of the landform that approximately corresponds with our designed model (Sect. 3.1, 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 rewritten this discussion section.

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 velocities’ in the first place, and kept the second one to form a short sentence.

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

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

Responses to RC2

1. I read the manuscript with great interest and anticipation, and I do like to congratulate the authors on their effort of presenting a manuscript that is, editorially, well written and properly developed. However, I do have some fundamental concerns with the approach and assumptions used and I therefore do not recommend that the text is published in The Cryosphere as presented. Unfortunately, this manuscript is following a trend that I have observed in recent years, in particular where authors publish their work without having sufficient field data to support it. I do understand that there are only a few data available on rock glaciers, but that is a fact we must accept, which also means that theoretical approaches that heavily depend on reliable field data for calibration and validation simply should not be published. Therefore, I strongly believe that the approach shall be revisited before the authors submit a revised manuscript.

Re: We thank Dr. Arenson for his insightful and constructive 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.

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

2. I will address individual aspects below, but the fundamental problem I have with the proposed approach is that following the author's approach, we would infer that the ground ice contents of the rock glaciers in the Alps, for example, is increasing in response to climate change. Several studies show that the creep velocities of rock glaciers are increasing in the Alps (Note: I specifically do not add many references in this paragraph as I'm sure the authors are well aware of this literature, and I would not be able to pay justice to all the authors that have contributed to some of the elementary statements I use). So, if we were to calculate the ice content using today's surface velocities, and then repeat the calculation again in 10 years it is very likely that an increase in ground ice content would result. This is a fundamental mistake, illustrating that it is impossible to link the parameters used with rock glacier surface velocities in order to estimate ice contents without making huge mistakes. The change in velocity that we are currently monitoring, mainly in the Alps, is related to permafrost degradation in the rock glaciers, specifically the warming of the ice and potential increase in unfrozen water content. Both impacting the creep parameters. While the actual ground ice content may not even change, creep velocities increase in response to warmer conditions, another aspect that was not included in the proposed model where ground temperatures are assumed to be constant. Ground ice melt in rock glaciers in response to climate change is extremely slow because of the latent heat. The higher the ground ice content, which would in turn benefit higher creep velocities, the more latent heat is stored in the ground, requiring more energy (time) in order to melt the ground ice. In other words, there are multiple processes at play that influence the ground ice content, the degradation and the velocity. The simplified approach presented does not consider this complexity, which, as illustrated above, could result in erroneous conclusions.

I do understand that specifically section 5.2 addresses the uncertainties, but if the authors would read those lines carefully, they would probably agree that they are telling the reader that there are so many uncertainties that even the authors are no longer sure if the approach is realistic or not. This is a dangerous approach because on the one hand the manuscript provides a very clear approach on how to calculate ice contents, but at the same time, the paper also says that it may actually all not be correct because of all the simplified assumptions used. For example, I appreciate that the authors indicate the acceleration in one sentence on line 378. However, this does not resolve the major flaw of the paper indicated above. Like many researchers, the authors assume some sort of steady-state behaviour, which is typically an accurate assumption when modelling glacier dynamics. However, rock glacier kinematics responds on different time scales and therefore it is inaccurate to use assumption tailored for quasi steady state conditions on a process (landform) that is constant transition, always lagging behind modern climate conditions.

Re: We would like to clarify that our proposed approach cannot predict an increasing trend of ground ice content. Firstly, the ice content and surface velocity are not positively correlated in the modelled relationship. Take the Schafberg rock glacier presented in Fig. 8 as an example: if the surface velocity increased slightly (orange band in Fig. R1), the inferred ice content would actually decrease (dotted line in Fig. R1); and if the velocity increased significantly (red band in Fig. R1), the inferred ice content would again increase (dash-dotted line in Fig. R1).

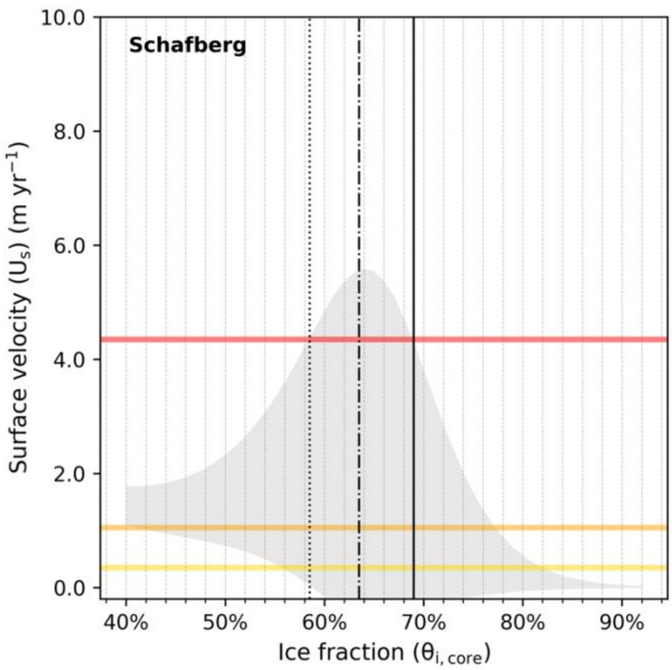


Figure R1: Revised based on Fig. 8 in the discussion paper. The grey shaded area is the simulated relationship between ice fraction and surface velocity. The yellow band shows surface velocity derived from in-situ measurement, and the solid vertical line indicates the inferred ice content (69%). The orange band presents a hypothetical velocity range, which is higher than the real data (yellow band), and the corresponding estimated ice content is shown by the dotted line (58.5%). Similarly, the red band shows a scenario where the surface velocity is very high, and the ice content is indicated by the dash-dotted line (63.5%).

However, we do not indicate that while a rock glacier is accelerating, its ice content would first decrease then increase, which is obviously unrealistic. We should have clearly stated in the manuscript that our proposed approach is unable to model the evolution of ice content from the velocity variations, because if one considers the melting/formation of ground ice, the geometric parameters of the rock glacier would change accordingly, particularly the thickness of the permafrost core and the active layer, respectively. We had tried to deal with the ground ice variations but found it difficult to model the geometric changes due to the complexity of degradation mechanisms. In addition, it would be fundamentally flawed to model ground ice changes without introducing a temperature evolution scheme. In the proposed model, we use the fixed thickness parameter derived from the current rock glacier geometry, and a constant ground temperature distribution. In fact, we quite agree with the reviewer's thought that "ground ice melt in rock glaciers in response to climate change is extremely slow because of the latent heat" and "while the actual ground ice content may not even change, creep velocities increase in response to warmer conditions…", and actually assume that within the time frame concerned in our study (1–2 decades, constrained by available InSAR data), the ice content of rock glaciers remains constant, although their velocities may change. In other words, the proposed approach aims to estimate the current amount of ground ice stored in rock glaciers from their surface velocities in recent years. Predicting ground ice changes from kinematic variations is beyond the scope of our work.

To clarify this point, we firstly added a statement at the end of the introduction section in the revised manuscript: "The proposed approach aims to estimate the current amount of ground ice stored in rock glaciers and to assess the hydrological importance of rock glaciers as freshwater reservoir in the long term." Then we elaborated this point in the introduction section: "Few studies have investigated the hydrological contribution of rock glaciers to surface runoffs at annual or seasonal timescale (e.g., Geiger et al., 2014; Harrington et al., 2018; Krainer and Mostler, 2002; Winkler et al., 2016), and little evidence has shown that rock glacier discharge is a prominent water source at present due to the insulation effect produced by their blocky surfaces (Duguay et al., 2015; Jones et al., 2019; Pruessner et al., 2021). Yet, on the multi-annual to centennial and millennial timescales, we expect rock glaciers of high ice content to serve as water reservoirs long after glaciers have melted." In the new discussion section, we introduced the incapability of predicting ground ice evolution to avoid possible misapplication.

Then we would like to explain how we understand the steady-state creep assumption of rock glaciers adopted by this work. We take advantage of the multi-temporal measurements conducted by InSAR, as detailed in Sect. 4.1, to make sure that the motion of the studied objects is in a quasi-steady state condition. For instance, the sudden acceleration Tobuche rock glacier exhibited (Fig. 5e) has been excluded from the velocity range for inferring ice content. Slight kinematic fluctuations exist (5–30 cm yr^{-1}, line 276, Fig. 5) partly due to seasonal variations. In fact, we take the velocity range as a constraint to estimate ice content (line 256–258), which is consistent with our underlying assumption that within the timescale under consideration, the rock glacier is in quasi-steady-state creep and contains constant amount of ice, as elaborated in the previous paragraph.

In the revised manuscript, we have added justifications in the subsection to describe the method of deriving velocity constraints: "…we take the range of the mean velocities over the observational period as the velocity constraint for modelling ice content. By doing so, the 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."

As regards with the other assumptions discussed in Sect. 5.2, we are aware of the fact that discussing the deviation of assumptions from reality may leave the readers with an impression that the model is unreliable. However, certain simplifications are inevitable in modelling work and deserve sufficient justifications, which motivates us to develop the arguments presented in Sect. 5.2. One way to provide these necessary justifications without overstressing the uncertainties is to move them to corresponding places in the methodology section, as also suggested by another reviewer. The new discussion section focuses on the limitations and prospects of our approach.

3. On line 481 the authors conclude that "This study demonstrates the effectiveness of inferring ice content of rock glaciers by using a surface-velocity-constrained." However, that is not really what this

paper is doing. The proposed approach uses such a correlation, assuming it is accurate, not demonstrating. There is a lack of data that can actually be used to demonstrate that the proposed approach is valid. The authors are therefore turning the initial hypothesis into a conclusion without proofing it.

Re: We agree that line 481 is an inappropriate statement, though we tried to validate the approach using field data from four Swiss rock glaciers (Sect. 3.2.3 and 4.2), in-situ measured ice content of rock glaciers in Khumbu Valley is lacking.

We have changed the sentence: "This study develops an approach to inferring ice content of rock glaciers by using a surface-velocity-constrained model."

4. In the following I will provide some more specific comments I have on the manuscript:

The authors must be much more careful with the wording and make sure to avoid blank statements, such as "... is important" without specifying important in what respect, and providing a reference or demonstrate the importance as par of the contribution.

Line 10: Unfortunately, the authors copy misleading statements others have made regarding using rock glaciers as freshwater resources. It is important to understand that a rock glacier is not a special type of a glacier. There is no exchange in ice, and there is no annual runoff from a glacier as we know it exists from a glacier. The hydrological behaviour of a rock glacier is completely different, and therefore it cannot be compared with a glacier when it comes to how runoff from a rock glacier should be seen as a source of freshwater. In fact, when one does calculate how much ground ice from a rock glacier is melting during a summer, even under an extremely hot summer, the authors would realise that the amount is extremely low, and in fact, often much lower than the potential evaporation. Specifically, in arid areas. In other words, water that is released from a ground ice melt is most likely not available as freshwater. The current wording is therefore creating potential anticipation that simply does not exist.

Re: We agree that rock glaciers, at the present time, do not supply freshwater through surface runoff as glaciers do. This is consistent with our assumption that ground ice stored in rock glaciers remain constant at decadal timescale (See the response to the main comment).

To avoid any misunderstanding, we have changed the sentence (line 10): "Rock glaciers contain significant amounts of ground ice and serve as potential freshwater reservoirs as mountain glaciers melt in response to climate warming in the long term."

5. Line 19: The thickness of a rock glacier is a fundamental parameter. Can the authors please clearly define what they mean by the thickness of a rock glacier? As a first step, it would be helpful to define the bottom of a rock glacier, is it defined by the base of the permafrost, the depth to bedrock, or the interface between the original terrain and the material of the rock glacier that had been transported there?

Re: In this study, we define the bottom of a rock glacier as the depth where no deformation occurs beneath. In practice, we calculate the thickness based on the empirical relationship proposed by Brenning (2005b), who defines the thickness as the depth between the surface and the base of ice-rich permafrost.

6. Line 21: Please provide clear definitions for terms such as reservoir and resource, and explain the differences in how they are used in the manuscript.

Re: We use the two words in an interchangeable way: both terms mean a place where ice is stored and potentially available for use in the future. To clear up confusion, we have replaced ‘resource’ with ‘reservoir’ throughout the text.

7. Line 21 ff. When presenting results, it is a) critical that the error range is provided, and b) that the number of significant digits reflects the accuracy. It is not appropriate to present a result to the 10th of a percent, when the error is in the 10th of percent.

Re: Concur. We have changed the flow and structure of the result presentation by firstly reporting the average bias (8%) as a reference level of uncertainty for the inferred volumetric ice fraction. For point b), we have checked the values throughout the text and made changes according to the error range. For instance, the ice fraction results are reported as 71% and 75%, with an error range of 8%.

8. Line 26: Please provide references for that statement, also, it is worth noting that this is only true for intact rock glaciers. Rock glaciers are geomorphic landforms and you can’t simply ignore relist rock glaciers, for example. As mentioned above, a rock glacier is not a special type of a glacier and as such this periglacial landform must be considered differently when writing about them.

Re: We have re-written the sentence with the reference provided: “Rock glaciers are valley-floor and valley-side landforms occurred in the periglacial realm. Intact rock glaciers contain ground ice and are common in the cold mountain regions (Ballantyne, 2018).”

9. Line 28: With regard to Azócar and Brenning, 2010, I encourage the authors to carefully read the comment by Arenson and Jakob on that paper.

Re: We have carefully read the paper and the response paper followed (Arenson and Jakob, 2010; Brenning, 2010). We found it inspiring to read the in-depth discussion on the “hydrological significance of rock glaciers”.

We have clarified the relevant concepts in the introduction section to avoid confusion: “Few studies have investigated the hydrological contribution of rock glaciers to surface runoffs at annual or seasonal timescale (e.g., Geiger et al., 2014; Harrington et al., 2018; Krainer and Mostler, 2002; Winkler et al., 2016), and little evidence has shown that rock glacier discharge is a prominent water source at present due to the insulation effect produced by their blocky surfaces (Duguay et al., 2015; Jones et al., 2019b; Pruessner et al., 2021). Yet, on the multi-annual to centennial and millennial timescales, we expect rock glaciers of high ice content to serve as water reservoirs long after glaciers have melted.”

10. Line 29: What exactly is a “hydrological value”?

Re: Here the “hydrological value” proposed by Corte (1976) refers to rock glaciers serving as both the water storage and annual runoff sources. In this study, we do not consider the hydrological value of the latter type and assume the ground ice remains constant in the study time scale (as detailed in the response to the main comment).

11. Line 39: I'm not clear what the “ratio of importance” is. Do you simply mean the ratio? If so, then the word "important" doesn't have a meaning.

Re: We have modified the sentence: “The ratio between rock glacier ice content and that in glaciers in the region was 1:25…”

12. Line 42: See my earlier comment regarding water supply. In order to demonstrate that this statement is accurate, please provide a thermal analysis that shows how much melt you will get and then compare it with potential evaporation and infiltration.

Re: We have rewritten the paragraph and removed this inaccurate statement regarding water supplies, as this manuscript does not focus on the runoff contribution from rock glaciers.

13. Line 43: Please provide reference and definition of an ice-cored rock glacier.

Re: We realized that the terminology, ice-cored rock glacier, may cause confusion as to the classification of rock glaciers. We have removed this phrase.

14. Line 44: You write " However, there lacks modelling studies to test these postulations and to assess the likelihood of glacier- rock glacier transition and the hydrological implications of this process." I agree with this statement, but I feel that you do not keep this in mind while wording some of your text. Many of the wording is written as if it was a fact, but in essence it isn't, such as freshwater from rock glaciers.

Re: Concur. We shall be more cautious about the wording and clarify that the hydrological role of rock glaciers as a contributor to surface runoff is beyond the scope of this work.

15. Line 54: what exactly is "extremely"? Such qualifying words must not be used in a scientific publication unless clearly quantifiable.

Re: We have deleted the inappropriate modifier in this sentence.

16. Line 57: Please clarify that Arenson and Springman (you can find details in Arenson 2002), emphasize that the deformation is not related to an "average" ground ice content, because such an average does not really exist, but rock glaciers do show quite complex internal structures. The deformations are often limited to a shear horizon (Arenson et al., 2002), where the ground ice content is high. Concluding from the ground ice content in the share zone to the ground ice content of the whole rock glacier is something that has not yet been confirmed and is associated with significant errors (orders of magnitude).

Re: At line 57, we do not introduce the "average" ground ice assumption or indicate that Arenson and Springman relate rock glacier deformation to "average" ice content. Furthermore, inserting an additional clarification might break the logic flow.

To avoid possible misunderstanding, in the revised manuscript we have added necessary clarifications in Sect. 3.1 (model design and assumptions): "Here we neglected the presence of shear horizon where deformation is enhanced and ground ice content is high, as discovered from borehole investigations."

17. Line 74: Discontinuous permafrost has no altitudinal boundary. The whole concept of continuous and discontinuous permafrost, which has been developed for polar regions, should not be used in mountainous environments. That's why the term Mountain permafrost had originally been coined.

Re: We followed the convention developed in recent years that researchers started to use the concept of continuous and discontinuous to classify permafrost occurring all over the Northern Hemisphere, e.g., Obu et al. (2019). In the context of permafrost study focusing on the Tibetan Plateau, this classification scheme is widely adopted (e.g., Zhao et al., 2021; Zou et al., 2017), and altitude is commonly used to describe the distribution of the continuous and discontinuous permafrost, because it is the primary factor controlling the environmental conditions on the Tibetan Plateau. We have changed the wording (altitudinal boundary) to "the lower limit of permafrost" to be consistent with the literature.

18. Figure 1: What is the year of the image? What was the scale used for mapping?

Re: The background image was taken in the year of 2019. We have added the information in the caption. The scalebar is plotted at the top left corner of Fig. 1.

19. Line 91: Are you using Ascending and/or descending imagery?

Re: Most are ascending imagery. Information of the satellite data is shown in Table 1 (at Line 115).

20. Line 93: What exactly is high? Can you be quantitative as this is another relative term.

Re: We set coherence = 0.3 as the threshold, as detailed at line 108.

21. Line 99: Please provide more details on the analysis methodology used for the InSAR assessment. E.g. did you use PS or any other method? There are many aspects unclear on the InSAR assessment.

Re: We used the Differential InSAR (DInSAR) method in this study.

22. Line 100: relative term, what do you mean by "near"?

Re: In practice, the selected reference points are located within 300 m from the landform.

23. Line 101: what was the landform coverage? How much topographic shade did you experience for the landforms?

Re: The areal extent of the five rock glaciers ranges from ~0.07 km^2 to ~0.2 km^2, as detailed in Table 4. We do not experience the shade issue for our study objectives, because the slopes of the landforms are gentle (< 20°), smaller than the incidence angles of the SAR images we used (38.7° and 36.3°).

24. Line 105: I assume that this was done using SRTM topography and not by combining ascending and descending stacks. This can result in significant errors in deformation due to the significant differences in the resolution between SRTM and InSAR imagery. How much are the errors in your evaluation?

Re: Yes, we reprojected the results using the SRTM DEM. Considering the uncertainties introduced both from the DEM and interferometric quality (assessed by coherence), for one pixel, the velocity error mostly ranges from 1 cm/yr to 10 cm/yr, and the relative uncertainty is between 5% and 25%. The absolute and relative errors are reduced to <1 cm/yr and <5% when considering the spatial mean velocity of all pixels covering one rock glacier. We have reported the errors in the revised manuscript.

25. Line 108: you mean less than half? Is this still representative? Can you quantify that using only 40% of the area is representative for the assessment presented? Also, you probably are biased towards the flatter sections of a rock glacier where there is less topography, but where you likely would have more compressive flow.

Re: Here the value of 40% serves only as a threshold for selecting interferograms of good measurement quality from many InSAR observations. The interferograms adopted after applying the criteria presented here have a mean spatial coverage of 61%. From the velocity distribution maps (Fig. 6), we do not observe obvious biases towards flatter regions, because surface topography does not necessarily cause decorrelation in the interferometric processing, though it contributes to the uncertainty of the results (as detailed in the former response to comment #24).

26. How did you address differences between compressive and extensive flow sections in rock glaciers? Are you implying that the ice content in the compressive areas are representative?

Re: The comment seems to be follow-up questions to the previous one (comment #25). At line 108, we did not separate sections of a rock glacier or differentiate compressive and extensive flow patterns.

27. Line 109: The values presented are averaged over the Dec. 2007 to Feb 2020 stack, is that correct? And I assume it is based on the 40% area coverage.

Re: No, the value is not a temporal average, but the spatial average of all pixels with valid measurements on each rock glacier. The 40% is the threshold value.

28. Table 1: How come you only have one interferogram? What is your level of confidence to use just that one interferogram in your assessment?

Re: By using the conventional DInSAR methodology (unlike the SBAS method for instance), only one interferogram can be used to obtain the displacement occurred during the period between the two SAR acquisitions.

29. Line 144: 1. you assume that the base of the rock glacier equals the base of the permafrost, correct? 2. You assume homogeneous conditions, which I haven't seen in any rock glacier, i.e. this is a huge simplification. I'm not saying there is no value in doing this, but you must be aware of what the consequences of such a simplification are when you draw your conclusions.

Re: 1. No, we define the base of a rock glacier as the depth where no deformation occurs beneath. We divided the rock glacier body into the active layer and the permafrost core. Beneath the bottom of the landform, permafrost may still be present but beyond our research scope.

2. Concur, rock glaciers do not develop a homogeneous internal structure in the nature, and that assuming a homogeneous structure is a huge simplification made in our model setting. The rationale behind this simplification (and many others made in this work) is that we are aware of the reality that knowledge of rock glaciers obtained from direct observations is valuable yet limited, so that at the current stage, we aim to take advantage of the existing data to explore some empirical relationships. Such practice requires us to be rigorous and careful about the level of uncertainties in our results and not oversell our findings. The importance of gathering data based on direct observations should be emphasized. In the future, the accuracy of the modelling results will be improved with more ground truth data obtained and used for method validation.

30. Line 145: Talus derived rock glacier show very variable thickness. Potential generalization that may lead to misleading results / conclusions.

Re: We have added discussion in Sect. 5.3 of the revised manuscript, when discussing the uncertainty introduced by the thickness derivation (at line. 430): "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."

31. Line 146: You also assume constant temperature conditions within the permafrost body, which is often not the case. Again, a rock glacier is not a special type of a glacier and can't be compared to a temperate glacier.

Re: We agree that constant temperature conditions are rare in reality (one example is reported in Monnier and Kinnard (2013)), but a simplified scenario is assumed in this work.

32. Line 184: I am very surprised that the single most important parameter, the temperature, is simply ignored.

Re: We discussed this aspect in Sect. 5.2.2, where we first reviewed the effect of ground temperature in controlling the creep parameter, then we justified our method of assuming a homogeneous warm profile and using the effective viscosity to represent the effects of many factors, finally we explained why this ground temperature condition is likely to be realistic in our study area.

In the revised manuscript, we moved these justifications to the methodology part (Sect. 3.1).

33. Line 187: What is the error range? Geophysics w/o calibration may have significant errors.

Re: According to Monnier and Kinnard (2015), the mean ice fraction is 0.66±0.101. The magnitude of error is ~10%.

34. Line 220: This correlation should simply not be used (See Arenson and Jakob, 2010). Using such a simplified correlation does not account for the complex geomorphic background of why a rock glacier exists. Hence, utilizing a glaciological, mass balance inspired approach to describe a periglacial, topo-geologically driven process will not provide accurate results.

Re: We acknowledge it is another simplification made to the complex structure of rock glaciers. We discussed this aspect in Sect. 5.3 of the revised manuscript. Justifications of the simplified assumptions made in this work are summarized in our response to comment #29-2.

35. Line 224: SRTM resolution is not 30 m, but varies geographically as it is in arc degrees.

Re: We described the SRTM DEM resolution at Line 95: "We estimated and removed the topographic phase with the 1-arcsec digital elevation models (DEM) produced by the Shuttle Radar Topography Mission (SRTM) (spatial resolution ~30 m)." We have modified the wording at Line 224: "…a spatial resolution of ~30 m."

36. Table 4: These active layer thicknesses are extremely thin. Please look at some of the rock glacier active layer thicknesses in the Alps (e.g. PERMOS reports) where you will find that rock glacier active layer thicknesses are often several meters thick. Hence the major thermal protection and the lack of contribution to any runoff, even as the permafrost degrades. The energy available for ground ice thaw below an active layer thickness of several meters, is low.

Re: We derived the active layer thickness from the ESA CCI Product (at line 253), and yes, the values seem to be very small for a real rock glacier. Another rock glacier on the Tibetan Plateau, where we have conducted in-situ measurements, develops an active layer of ~2 m thick. We agree that underestimating the active layer thickness would lead to significant errors when considering the thermal regime of a rock glacier.

However, in this work, we do not take the thermal evolution into account or estimate future runoff contribution as ground ice thaws (detailed in our response to the main comment). In addition, the sensitivity test shows that the active layer thickness exerts the least effect on the model result (Fig. 10), which justifies our use of the ESA CCI data provided that the direct measurements are lacking.

37. Line 358: Rock glaciers do not show a uniform creep. Most rock glaciers have an area that is faster and another that is slower. For example, large rock glacier may no longer advance because the lower part lost too much ice to allow creep. However, the upper part is still creeping, Your approach will completely overestimate the ice content as it does not take the actual rock glacier kinematics into consideration.

Re: We agree that rock glaciers do not creep uniformly at the landform scale, so that instead of considering the landform as a whole, "we defined and outlined the coherently moving part of the landform by considering the time series of downslope velocity of each pixel acquired during all the observational periods. If the InSAR-measured velocity is higher than 5 cm yr^{-1} in more than half of the periods at a given pixel, it was included into the coherently moving part of the landform." (At line 110–112.) In the given example, the lower part of a rock glacier which no longer moves is not counted in our estimates.

38. Line 367: importance relative to what?

Re: The importance is relative to a larger region: the entire Himalayas. Here we compared the ratio (ice storage in rock glaciers vs. in glaciers, 1:17) over the Nepalese Himalaya with the ratio reported in previous research focusing on the entire Himalayas, where the Nepalese Himalaya is a sub-region.

We have changed the wording: "…than across the entire Himalayas (1:24)."

39. Line 378: You state "This premise indicates that our method is applicable to rock glaciers currently moving at a relatively stable rate." For one, based on data from the Alps, we know that this is likely not the case, and more importantly, you use data from rock glaciers that do not show stable deformation to develop your model, which should then be only valid for stable deformation? This does not sound logical to me.

Re: By stating the premise of "moving at a relatively stable rate", we aim to exclude the destabilized rock glaciers from the scope of method application. In the strict sense, an object in the steady-state creep exhibits constant strain rate, which is not the case for any rock glaciers. Many rock glaciers are reported to experience motion fluctuations at the inter-annual scale, including the ones we used for developing the model. To simulate deformation behavior of these non-destabilized rock glaciers, Glen's flow law is still widely accepted. However, in the revised manuscript, we have excluded the data of Gruben rock glacier from the validation set, because it is losing internal ice and changing its morphology rapidly (Gärtner-Roer et al., 2021; R. Delaloye, personal communication, July 21, 2021), which does not align with our model design.

40. Line 385: Call the "clean" (what ever that actually means) as an uncovered or covered glacier. Or simply call it glacier because rock glaciers are not special glaciers, as I've been mentioning severel times already.

Re: Concur, rock glaciers are not glaciers. We have removed the misleading word "clean" from the sentence.

41. Line 419: You are citing Cicoira et al. (2020) to support your statement. Are you sure, since the publication of Cicoira et al. (2020) had a completely different objective and it seems to me that your referencing is taking out of the appropriate context.

Re: The work presented by Cicoira et al. (2020) indeed has a different research objective, yet shares the same technical issue of expressing the rock glacier creep without known internal structure data, and in their work, the shear horizon cannot be considered either. We cited this paper to show this common issue when studying rock glacier kinematics over a regional extent (in contrast to the case studies with sufficient data). To avoid inappropriate comparison between the two pieces of work, we have removed this citation.

42. Line 424: I suggest that you read Arenson and Jakob (2010) and revisit your statement.

Re: We have read through the discussion and reply articles by Arenson and Jakob (2010) and Brenning (2010), respectively. Regarding the empirical derivation of rock glacier thickness presented by Brenning (2005a), we think it is statistically valid but likely to have limited level of accuracy. In the revised manuscript, we have rewritten the discussion on the uncertainty in deriving rock glacier thickness (Sect. 5.3).

43. Line 426: I am not at all surprised by the large bias that you found, however, I do not see this bias be further developed, for example using error propagation theories, to illustrate what that means for your end result.

Re: The uncertainty introduced by the error of thickness can be represented by the model sensitivity to the area variable, as we derived the thickness based on an area–thickness relationship. Fig. 10 shows that the error range caused by the varying area parameter is within the 5% range, given that the area-thickness relationship is applicable to the landform.

When this empirical derivation is invalid, the propagated error can be significant but difficult to quantify due to the limited available data. Therefore, in Sect. 5.2.4, we only acknowledge that the uncertainty introduced by thickness derivation when applied to rock glaciers without known information of structure cannot be eliminated with the existing empirical methods.

44. Table 7: What is Tref?

Re: We missed the symbol in the table caption and have added at line 432: "…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)." We have modified and moved the table to the supplement materials.

45. Table &: How confident are you that these 5 (!) rock glaciers, which all have very specific features, are representative so that a correlation, such as the one you present, can be developed and reasonably be applied for hundreds of rock glaciers in very different settings?

Re: Sect. 5.2.4 does not aim to illustrate the wide applicability of the thickness–area relationship we adopted. Instead, we hope to point out that the reliability of thickness derivation remains to be an issue at the current stage.

We have rewritten the discussion on the rock glacier thickness derivation method (Sect. 5.3).

46. Line 459: Based on my review I do not support this statement and it is my very strong impression that this approach is not yet ready and specifically I would not call the uncertainties "well-quantified". In fact, the uncertainties are unknown.

Re: We agree that this is an inappropriate statement. We have removed this sentence. We've also assigned an uncertainty level according to the average bias derived from the model validation (see also in the response to comment #7).

47. Line 460: I completely agree with the final statement and encourage the authors to put their effort in getting more field data so that can provide a better estimate for rock glacier thicknesses.

Re: Concur. Field data are fundamental for deriving generalized empirical rules, we would like to contribute to the data gathering and improve the approach presented in this work accordingly.

48. Line 468: The authors indicate that they are measuring active layer from remote sensing. First, they have not discussed this aspect in the paper, which means that this should not just pop-up in the conclusion, and secondly, I am not aware of a method on how to measure rock glacier active layer thicknesses from space. Or maybe the authors mean geophysics, which has its own challenges for block rock glaciers.

Re: We introduced the ALT data source in the methodology section at line 253: "Active layer thickness was determined as the mean value over the extent of each rock glacier during 2006–2017 from the European Space Agency Permafrost Climate Change Initiative Product (ESA CCI)." The ALT dataset is produced based on remotely sensed datasets of Land Surface Temperature (LST), Snow Water Equivalent (SWE) and landcover, so that we refer to it as a remote sensing product in the manuscript.

49. Line 472: Level of accuracy implied is unrealistic.

Re: We have modified the structure of the result presentation (see also the response to comment #7).

50. In summary, this manuscript is not ready for publication, and I strongly encourage the authors to re-evaluate their scientific basis and if there is even any merit in the approach presented considering the significant uncertainties that exists because of the assumptions used.

Re: Again, we thank Dr. Arenson for his informative and inspiring comments, especially the clarification of rock glacier hydrological value, the reliability of rock glacier thickness derivation, and the appropriate format of result presentation. We have addressed these aspects in the response letter and made changes accordingly to the manuscript.

Moreover, we agree with Dr. Arenson's emphasis on the field data of rock glaciers, which is essential for improving the accuracy of modelling approaches. In the current manuscript, we aim to take advantage of the existing observational data and build a framework for inferring ice content with remote sensing-based input. In the response letter, we attempt to provide more detailed justifications to the necessary assumptions made in this work and avoid possible exaggerating statements. To further improve the performance of the approach, as stated in the final paragraph of the discussion section, "…more data obtained from field and geophysical investigations, especially detailed data of rock glacier composition, can be integrated in the future to calibrate and validate the empirical rheological model. More reliable methods for estimating rock glacier thickness will also improve the accuracy of the modelling results." And it is a research path we are following at present.

In the revised manuscript, we have summarized the limitations and merits in the discussion section and concluded: "In summary, the lack of ground truth data essentially hinders our approach from achieving high-level accuracy in quantifying ice content of rock glaciers. Nonetheless, the proposed model makes a first attempt to build a framework for inferring ice content with remote sensing-based input by taking advantage of the existing observational data. With the likely emergence of more data to be integrated for model calibration and validation, it forms a promising approach to improve the accuracy of modelling results and application to mountain permafrost regions where rock glaciers are widespread for preliminary water storage evaluation.”

References

Arenson, L. U., and Jakob, M. (2010). The significance of rock glaciers in the dry Andes – A discussion of Azócar and Brenning (2010) and Brenning and Azócar (2010). *Permafrost and Periglacial Processes, 21*(3), 282-285. https://doi.org/10.1002/ppp.693

Ballantyne, C. K. (2018). *Periglacial geomorphology*. John Wiley & Sons.

Brenning, A. (2005a). *Climatic and geomorphological controls of rock glaciers in the Andes of Central Chile* Humboldt-Universität zu Berlin, Mathematisch-Naturwissenschaftliche Fakultät II].

Brenning, A. (2005b). Geomorphological, hydrological and climatic significance of rock glaciers in the Andes of Central Chile (33-35 degrees S). *Permafrost and Periglacial Processes*, *16*(3), 231-240. https://doi.org/10.1002/ppp.528

Brenning, A. (2010). The significance of rock glaciers in the dry Andes – reply to L. Arenson and M. Jakob. *Permafrost and Periglacial Processes*, *21*(3), 286-288.

Cicoira, A., Marcer, M., Gärtner‐Roer, I., Bodin, X., Arenson, L. U., and Vieli, A. (2020). A general theory of rock glacier creep based on in‐situ and remote sensing observations. *Permafrost and Periglacial Processes*. https://doi.org/10.1002/ppp.2090

Corte, A. (1976). The Hydrological Significance of Rock Glaciers. *Journal of Glaciology*, *17*(75), 157-158. https://doi.org/10.3189/s0022143000030859

Duguay, M. A., Edmunds, A., Arenson, L. U., and Wainstein, P. A. (2015). Quantifying the significance of the hydrological contribution of a rock glacier–A review. *GeoQuébec 2015*.

Gärtner-Roer, I., Brunner, N., Delaloye, R., Haeberli, W., Kääb, A., and Thee, P. (2021). Glacier-permafrost relations in a high-mountain environment: 5 decades of kinematic monitoring at the Gruben site, Swiss Alps. *The Cryosphere Discuss.*, *2021*, 1-30. https://doi.org/10.5194/tc-2021-208

Geiger, S. T., Daniels, J. M., Miller, S. N., and Nicholas, J. W. (2014). Influence of Rock Glaciers on Stream Hydrology in the La Sal Mountains, Utah. *Arctic, Antarctic, and Alpine Research*, *46*(3), 645-658. https://doi.org/10.1657/1938-4246-46.3.645

Harrington, J. S., Mozil, A., Hayashi, M., and Bentley, L. R. (2018). Groundwater flow and storage processes in an inactive rock glacier. *Hydrological Processes*, *32*(20), 3070-3088. https://doi.org/10.1002/hyp.13248

Jones, D. B., Harrison, S., Anderson, K., and Whalley, W. B. (2019). Rock glaciers and mountain hydrology: A review. *Earth-Science Reviews*, *193*, 66-90. https://doi.org/10.1016/j.earscirev.2019.04.001

Krainer, K., and Mostler, W. (2002). Hydrology of Active Rock Glaciers: Examples from the Austrian Alps. *Arctic, Antarctic, and Alpine Research*, *34*(2), 142-149. https://doi.org/10.1080/15230430.2002.12003478

Monnier, S., and Kinnard, C. (2013). Internal structure and composition of a rock glacier in the Andes (upper Choapa valley, Chile) using borehole information and ground-penetrating radar. *Annals of Glaciology*, *54*(64), 61-72. https://doi.org/10.3189/2013AoG64A107

Monnier, S., and Kinnard, C. (2015). Internal structure and composition of a rock glacier in the Dry Andes, Inferred from ground-penetrating radar data and its artefacts. *Permafrost and Periglacial Processes, 26*(4), 335-346. https://doi.org/10.1002/ppp.1846

Obu, J., Westermann, S., Bartsch, A., Berdnikov, N., Christiansen, H. H., Dashtseren, A., Delaloye, R., Elberling, B., Etzelmüller, B., Kholodov, A., Khomutov, A., Kääb, A., Leibman, M. O., Lewkowicz, A. G., Panda, S. K., Romanovsky, V., Way, R. G., Westergaard-Nielsen, A., Wu, T., Yamkhin, J., and Zou, D. (2019). Northern Hemisphere permafrost map based on TTOP modelling for 2000–2016 at 1 km2 scale. *Earth-Science Reviews*, *193*, 299-316. https://doi.org/10.1016/j.earscirev.2019.04.023

Pruessner, L., Huss, M., Phillips, M., and Farinotti, D. (2021). A Framework for Modeling Rock Glaciers and Permafrost at the Basin-Scale in High Alpine Catchments. *Journal of Advances in Modeling Earth Systems*, *13*(4), e2020MS002361.

Winkler, G., Wagner, T., Pauritsch, M., Birk, S., Kellerer-Pirklbauer, A., Benischke, R., Leis, A., Morawetz, R., Schreilechner, M. G., and Hergarten, S. (2016). Identification and assessment of groundwater flow and storage components of the relict Schöneben Rock Glacier, Niedere Tauern Range, Eastern Alps (Austria). *Hydrogeology Journal*, *24*(4), 937-953. https://doi.org/10.1007/s10040-015-1348-9

Zhao, L., Zou, D., Hu, G., Wu, T., Du, E., Liu, G., Xiao, Y., Li, R., Pang, Q., Qiao, Y., Wu, X., Sun, Z., Xing, Z., Sheng, Y., Zhao, Y., Shi, J., Xie, C., Wang, L., Wang, C., and Cheng, G. (2021). A synthesis dataset of permafrost thermal state for the Qinghai–Tibet (Xizang) Plateau, China. *Earth Syst. Sci. Data*, *13*(8), 4207-4218. https://doi.org/10.5194/essd-13-4207-2021

Zou, D., Zhao, L., Sheng, Y., Chen, J., Hu, G., Wu, T., Wu, J., Xie, C., Wu, X., Pang, Q., Wang, W., Du, E., Li, W., Liu, G., Li, J., Qin, Y., Qiao, Y., Wang, Z., Shi, J., and Cheng, G. (2017). A new map of permafrost distribution on the Tibetan Plateau. *The Cryosphere*, *11*(6), 2527-2542. https://doi.org/10.5194/tc-11-2527-2017