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that theoretical approaches that heavily depend on reliable field data for calibration and validation simply should not be published.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.

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).

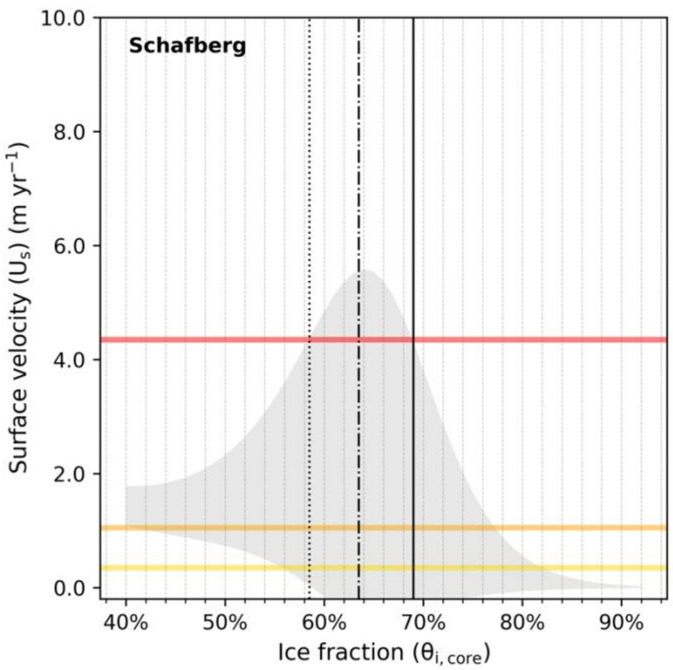

Figure R1: Revised based on Fig. 8 in the discussion paper. The grey shaded area is the simulated relationship between ice fraction and surface velocity. The yellow band shows surface velocity derived from in-situ measurement, and the solid vertical line indicates the inferred ice content (69%). The 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 status 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.

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.

As regards 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.

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 (8.4%) 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 8.4%.

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 will further clarify the relevant concepts in this manuscript to avoid confusion.

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 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 sentence (also see the response to comment #12).

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, we have added necessary clarifications in Sect. 3.2.1 (model design and assumptions, at line 123): "…and deformation at the shear horizon at depth, where most deformation takes place, and the ground ice content is high."

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 environment 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.

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: The five landforms in the Khumbu valley are glacier-derived rock glaciers. We have also added clarifications in Sect. 5.2.4, when discussing the uncertainty introduced by the thickness derivation (at line. 430): "In addition, talus-derived rock glaciers tend to develop very variable thickness, and the uniform thickness based on the empirical relationship may not be applicable."

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.

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. 5.2.4. 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 counted in our estimates.

38. Line 367: importance relative to what?

Re: The importance is relative to a larger region: the 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.

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, as also presented in Sect. 5.2.4: "However, another rock glacier, namely Ritigraben, situated in the same region, does not follow this empirical relationship and has a bias as large as ten meters compared with the field estimates."

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. For example, Ritigraben has a bias as large as 10 m, but this single value cannot represent the average bias of this empirical method. 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)."

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 area–thickness relationship we adopted. Instead, we hope to point out that the reliability of thickness derivation remains to be an issue at the current stage.

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 on 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.

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