

The Cryosphere Discuss., referee comment RC2  
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## **Comment on tc-2021-110**

Lukas U. Arenson (Referee)

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Referee comment on "Modelling rock glacier ice content based on InSAR-derived velocity, Khumbu and Lhotse valleys, Nepal" by Yan Hu et al., The Cryosphere Discuss., <https://doi.org/10.5194/tc-2021-110-RC2>, 2021

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that theoretical approaches that heavily depend on reliable field data for calibration and validation simply should not be published.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

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 proving it.

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 part 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 a) 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.
- 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?

- 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.
- 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 10<sup>th</sup> of a percent, when the error is in the 10<sup>th</sup> of percent.
- 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 relict 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.
- 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.
- Line 29: What exactly is a "hydrological value"?
- 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.
- 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.
- Line 43: Please provide reference and definition of an ice-cored rock glacier.
- 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.
- Line 54: what exactly is "extremely"? Such qualifying words must not be used in a scientific publication unless clearly quantifiable.
- 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 shear 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).
- 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.
- Figure 1: What is the year of the image? What was the scale used for mapping?
- Line 91: Are you using Ascending and/or descending imagery?
- Line 93: What exactly is high? Can you be quantitative as this is another relative term.
- 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.
- Line 100: relative term, what do you mean by "near"?
- Line 101: what was the landform coverage? How much topographic shade did you experience for the landforms?
- 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?
- 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.

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?

- 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.
- Table 1: How come you only have one interferogram? What is your level of confidence to use just that one interferogram in your assessment?
- 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.
- Line 145: Talus derived rock glacier show very variable thickness. Potential generalization that may lead to misleading results / conclusions.
- 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.
- Line 184: I am very surprised that the single most important parameter, the temperature, is simply ignored.
- Line 187: What is the error range? Geophysics w/o calibration may have significant errors.
- 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.
- Line 224: SRTM resolution is not 30 m, but varies geographically as it is in arc degrees.
- 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 a active layer thickness of several meters, is low.
- 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.
- Line 367: importance relative to what?
- 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.
- 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 several times already.
- 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.
- Line 424: I suggest that you read Arenson and Jakob (2010) and revisit your statement.
- 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.

- Table 7: What is Tref?
- Table 8: 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?
- 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.
- 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.
- 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.
- Line 472: Level of accuracy implied is unrealistic.

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 exist because of the assumptions used.