

## ***Interactive comment on “InSAR-based characterization of rock glacier movement in the Uinta Mountains, Utah, USA” by George Brencher et al.***

### **Anonymous Referee #1**

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#### **Overall Comments**

The authors used InSAR technique to map and characterize rock glacier movement in a region where previous knowledge of rock glaciers is limited. It produces a new dataset that sheds light on the kinematic behavior of those permafrost landforms and provides interesting insights as to how the rock glaciers respond to the climatic conditions and their potential local hydrological importance in the future. Hopefully, this paper will be published and help generate more interest in studying rock glaciers in North America.

It is a well-written paper in general. However, I would raise a few issues mostly regarding the necessary details of the InSAR method adopted in this work. Accuracy in

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terminology and clarity in the argument can be further improved in a few places. Please see my comments below.

#### **Detailed comments**

1. Line 21, the definition of rock glaciers here is inaccurate because they are not entirely “perennially frozen bodies”, the upper part of which is seasonally frozen ground or the so-called active layer.
2. Line 32–34, it might be inappropriate to draw an analogy between rock glaciers and ice glaciers here, because some of the enumerated drivers (e.g., liquid water, pore water pressure) influence the motion of the two types of landforms in ways that can hardly be regarded as similar.
3. Line 46, shear horizon is NOT “at the base of the rock glacier”. Borehole investigations have revealed that sediments exist below the shear horizon, though the motion of which is negligible. The authors may refer to the two papers cited in the caption of Figure 1(i.e., Arenson et al., 2002; Kenner et al., 2017) and modify Figure 1b and 1d accordingly.
4. Line 69–70, what are the “significant patterns”?
5. Line 124–125, why do the authors use the 10-m resolution DEM for selected one-year pairs only, instead of applying it to all data?
6. Line 138–139, the description “LOS velocity signal consistent with the downslope direction” is confusing, because a LOS signal is obviously always in the LOS direction, which is from the ground to the satellite, and thus cannot be consistent with downslope direction.
7. Line 147, which one-year interferograms do the authors use for calculating annual velocities? Here the authors mention both ascending and descending stacks of interferograms, however, Figure 2 only shows results derived from one descending track.

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8. Line 149, why do the authors remove negative LOS values? The motion towards the satellite is possible and Figure 2a does include negative values.
9. In Figure 2a, Line 715–716, the authors should specify the time span they used to calculate the average velocity, instead of just providing satellite orbital information. Also, the legend shows the unit of the velocity map in distance unit (cm) which may confuse the readers. Is Figure 2a a displacement map or a velocity map?
10. In Figure 2a, Line 715, the legend shows the unit of the velocity map is in centimeters which may confuse the readers. And the period of the observation should be specified.
11. Line 163–172, this part is not under the topic of “InSAR analysis”. The authors may consider reorganizing the structure of this section. Please also refer to the first technical correction below.
12. Line 236–239 and Figure 2, Line 716, the previous inventory (Munroe, 2018) didn’t classify the mapped rock glaciers based on their activities. How do the authors identify the inactive rock glaciers from the previously published dataset? If the inactive rock glaciers are landforms that do not show displacement in the interferograms, is it possible that some of those landforms are actually active, but their activity is not detected by InSAR, due to limitations of the technique, such as decorrelation, shadow, overlay, or the flow direction of landform is insensitive to the LOS direction?
13. Line 279–280, the references here do not fully fit. Delaloye et al. (2010) focus on the Swiss Alps which is a regional study and cannot represent rock glaciers “around the world”.
14. Line 283–285, Janke et al. (2005) reported average velocities of 7.3, 6.3, and 9.5 cm/yr for three rock glaciers in the Front Range, which are not notably faster than the LOS rates between 0.88 and 5.26 cm/yr presented in this paper in my opinion, especially when accounting for the underestimation in LOS values, as the authors dis-

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cussed in the last paragraph of Section 4.2. Besides, the three rock glaciers in Janke et al. (2005) cannot represent “most other North American rock glaciers”. The authors may consider changing their conclusions or drawing different comparisons.

15. Line 295–296, are there any references supporting this alternative explanation proposed here? Some studies suggest a contrasting point of view that the rock glacier accelerates when ice content decreases (Arenson et al., 2002), or a non-linear relationship between ice content and surface velocity (Cicoira et al., 2019).

Arenson, L., Hoelzle, M., & Springman, S. (2002, Apr-Jun). Borehole deformation measurements and internal structure of some rock glaciers in Switzerland. *Permafrost and Periglacial Processes*, 13(2), 117-135. <https://doi.org/10.1002/ppp.414>

Cicoira, A., Beutel, J., Faillettaz, J., Gartner-Roer, I., & Vieli, A. (2019, Mar). Resolving the influence of temperature forcing through heat conduction on rock glacier dynamics: a numerical modeling approach. *Cryosphere*, 13(3), 927-942. <https://doi.org/10.5194/tc-13-927-2019>

16. Line 308–316, this part is not discussing rock glacier velocity. Please consider restructuring this section.

17. Figure 8, Line 758–759, this sentence is unclear to me. Please explain how to scale the ascending and descending LOS and the purpose of that.

18. Line 418–421, I would suggest the authors specify those rock glacier velocities are InSAR-derived LOS velocities, otherwise the readers may misinterpret them as 3D creep velocities.

### Technical corrections

1. Line 81 and 106, these two parts are better to be numbered as 2 and 3, as there is no Section 2 in this manuscript, and I don’t see clear relations between the two subsections “Study area” and “InSAR analysis”.

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