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Interactive comment on "Rock glaciers of the contiguous United States: GIS inventory and spatial distribution patterns" *by* Gunnar Johnson et al.

Anonymous Referee #1

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General Comments: This contribution presents a nation-wide inventory of "intact" rock glaciers (sensu Barsch, 1996) and fully mantled debris-covered glaciers for the contiguous USA. The topic is suitable to ESSD. The authors justify their work with the need for continental-scale inventories, which are currently not available. This is clearly an impressive mapping effort. On the down side, I find the mapping rules adopted, the inherent mapping uncertainty and the metadata specifics to be insufficiently illustrated (i.e., with figures and photos) and documented. I also note a number of drawbacks in the inventorying approach that need to be considered carefully, before this database maybe considered for further analysis. In particular, the typology of landforms (i.e., intact rock glaciers and fully mantled debris-covered glaciers) blended in and the dy-

namic classification scheme adopted, makes the present inventory not comparable with other existing inventories around the world. For these reasons, making statistical inference from this database in its present form may lead to misleading conclusions.

Major points to be addressed are summarized under the following headings:

1. Rock glaciers and debris-covered glaciers: Although I agree that making a clearcut distinction between rock glaciers and debris-covered glaciers in some cases is subject to large uncertainties, which can only be resolved with direct geophysical investigation, a number of morphological features are known to be distinctive of debris-covered glaciers. These include, but are not limited to, the presence of crevasses with exposed ice, ice cliffs, abundant thermokarst and supraglacial lakes, supraglacial streams, outflow breaches. In this regard, adding some sample images showing which kind of debris-covered glaciers were excluded from the inventory would help the reader a lot. I suggest that the authors add a field in the PSURGI attribute table indicating whether a given polygon is a rock glacier, a debris-covered glacier, or uncertain i.e., when they are unable to distinguish between the two.

2. Degree of activity classification scheme: The dynamic classification scheme adopted in PSURGI subdivides intact rock glaciers into three classes: highly, intermediately, and minimally active. This approach makes PSURGI not immediately comparable with most of existing inventories around the world, which discriminate intact rock glaciers into inactive (i.e., no front movement) and active landforms (Barsch, 1996). Recent mapping tests in Northern Tyrol have shown that distinction between active and inactive rock glaciers is subject to high uncertainty, and that inactive rock glaciers (those that supposedly should move more slowly) displayed large disagreement among a pool of international, experienced mappers (Brardinoni et al., 2019). In this context, subdividing intact rock glaciers into three categories (as opposed to the classical two) appears unreliable. Along these lines, PSURGI approach to dynamic classification seems contradictory: on one hand it is stated that visual interpretation of imagery does not afford distinction between rock glaciers and debris-covered glaciers, on the other

hand, this same procedure would allow to discriminate three subtypes of intact rock glaciers. I believe that this type of fine distinction could be achieved reliably only with the aid of InSAR technology. For the reasons outlined above, I suggest that the authors revert their dynamic classification scheme for intact rock glaciers to the classical one (i.e., active and inactive).

3. Completeness: The question of inventory completeness is only brushed upon. A similar large-scale inventory should be coupled by a systematic testing on the variability and uncertainty among mappers involved in the inventory. For example, Google Earth Imagery, when not complemented by LiDAR-derived hillshades and high-resolution orthophoto mosaics has been shown to yield incomplete rock glacier detection, especially due to poor distinction between adjacent coalescing lobes (Brardinoni et al., 2019). In this context, the question of complex multi-lobe (or polymorphic) rock glaciers and the way in which these morphologies were mapped is not addressed. No example was provided. Any geomorphologist familiar with rock glacier mapping is aware of the inherent uncertainties associated with an inventory, yet the authors depict PSURGI as greatly accurate. Please consider tuning down some sentences in that section.

4. Rock glacier delineation (mapping rules): No specific description of the mapping rules applied in PSURGI is provided, and only vague wordy descriptions are given. For example, one of the most problematic issues when delineating a rock glacier polygon is typically represented by the extent of the rooting zone, which borders the upper end of a rock glacier. In the manuscript, I could not find which mapping rule has been applied to delineate the upper end of rock glaciers and exclude the rooting zone (assuming this was excluded from the mapped polygons). In Figure 1, class 2 example, the upper end of the polygon cuts across flow lines, following no apparent discontinuity in curvature or roughness. Was the mapping confidence consistent across the entire perimeter of this polygon? Overall, the three examples provided in Figure 1 do not struck me for being indicative of accurate mapping. Please add more examples and/or refine the outlines of the current ones.

5. Metedata: A database submitted for publication should come with well-documented metadata, including; i) A list of attributes in table format (i.e., the attribute table in the shapefile includes the dynamic classification only). ii) a list of complementary imagery used other than Google Earth Pro (i.e., currently the authors state the following in lines 83-85: "... supplementing with other plan-view imagery imported into ArcMap 10.4 when Google Earth Pro imagery was unsuitable due to cloud cover or other issues."

Specific Comments:

Title: considering the nature of the inventory, the title of the paper should acknowledge the inclusion of (intact) rock glaciers and fully mantled debris-covered glaciers.

Lines 23-25: "Two lesser known components of the montane cryosphere are rock glaciers and debris-covered glaciers, though presently there are no widely accepted formal definitions of either feature type that can be used to universally and unambiguously discriminate the two for all purposes"

In my opinion, this approach leads to confusion. The statement is not supported by any reference and discounts decades of research focused respectively on debris-covered glaciers and rock glaciers. It is more fair to say that there are widely accepted definitions of rock glaciers and debris-covered glaciers, and that a minority disagrees.

Lines 37-38: "Fully debris-covered glaciers are indistinguishable from the more traditionally defined rock glaciers through surface analysis alone".

Please try to support similar clearcut statements with references and illustrative examples (i.e., figures).

Lines 41-42: "The semantics of classifying these two cryospheric feature types is occasionally debated, but is not something we seek to resolve with this inventory (Clark et al. 1998, Potter 1972, Haeberli et al. 2006, Berthling 2011)."

I think the question is way beyond semantics. I understand that you do not want to enter into this dispute, but there are a number of morphological attributes that in many

instances should aid guiding distinction between rock glaciers and debris-covered glaciers. Merging rock glaciers and debris-covered glaciers in one database without any sort of morphological distinction represents a major limitation of this inventory.

Lines 131-134: “To partially address this ambiguity all features identified as rock glaciers were subsequently assigned to a three-tier classification system based on surface characteristics known to correlate with downslope movement motivated by deformation of the internal ice-rock matrix (Figure 1).”

Which would be the surface characteristics known to correlate with downslope movement? Please provide empirical data or reference to empirical publications showing such correlations. My impression is that by increasing the number of activity classes (three in this case), one is going to increase the degree of uncertainty. Please see my general comment #2.

Line 155: “after removing 146 small (< 0.01 km2) Class 3 rock glaciers following glaciological convention of area thresholds“.

Why using this threshold size? Rock glaciers are not glaciers, neither are included in the World Glacier Inventory.

Conclusions (lines 252-260): most of the conclusions paragraph reads more like introduction. Please consider rewriting and connecting the conclusions to the main results outlined in the manuscript.

List of existing inventories:

I suggest adding the following references to the list of existing inventories, the first includes 5769 rock glaciers across Austria:

Wagner et al 2020. The first consistent inventory of rock glaciers and their hydrological catchments of the austrian alps. Austrian Journal of Earth Sciences, 113: 1-23.

Brigitte Magori, Petru Urdea, Alexandru Onaca & Florina Ardelean (2020) Distribution

and characteristics of rock glaciers in the Balkan Peninsula, Geografiska Annaler: Series A, Physical Geography, DOI: 10.1080/04353676.2020.1809905

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