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Comment on **essd-2021-28**

Glacier Ice

Community comment on "Evaluation of the global and regional glacier inventories and assessment of glacier elevation changes over the north-western Himalaya" by Shakil Ahmad Romshoo et al., Earth Syst. Sci. Data Discuss.,
<https://doi.org/10.5194/essd-2021-28-CC1>, 2021

The paper by Romshoo et al. is interesting research on evaluating the existing glacier inventories over the north-western Himalayan region of Jammu and Kashmir. However, there are certain loopholes, some of them very gross, which have been pointed out below:

The title mentions "global glacier inventories", however, the very first sentence of the Abstract section mentions ICIMOD (for Himalaya only) and GAMDAM (for Asia only) inventories which are regional. As such I would suggest the authors modify the text wherever required. Or maybe mention "Evaluation of the existing glacier inventories..."

What do the authors mean by limited field surveys? How many glaciers were actually field surveyed? Specifically, what type of data was collected from the field, and would it qualify as a representative sample for Quality Control?

The uncertainty of mapping is more in Jhelum (~13%) as compared to Suru and Chenab (~3.3%). Since there are a lot of debris-covered glaciers in Chenab and Suru, uncertainty should be more in these basins as compared to Jhelum (where once predominantly finds clean-ice glaciers with few exceptions). Please explain.

When the authors say "most of the glaciers in the study area are <1 km² in size><1km²", they should mention the number and percentage of these glaciers. Also for the 1-5 km² category.

"Majority of the glaciers....". Please quantify. Similarly "... Jhelum where the glaciers are

mostly...". Again MOSTLY is subjective.

Rather than saying "mainly harbor slopes ranging from 10-30°", it would be better to mention the average slopes of the glaciers in all three basins.

What is RBA? Mention at first occurrence. If KUGI is "consistent" with RGI and GAMDAM, I wonder then as to what is the purpose of KUGI?

I would suggest removing the portion about geodetic mass changes since the authors have already published geodetic mass changes in Scientific Reports very recently (See the reference below). This would mean plagiarism/redundancy.

Abdullah, T., Romshoo, S. A., & Rashid, I. (2020). The satellite observed glacier mass changes over the Upper Indus Basin during 2000–2012. Scientific Reports, 10(1), 1-9.

"Evaluation of the glacier inventories and assessment of glacier elevation change in the data-scarce Himalaya, reported in this article, would constitute a reliable database for research particularly in hydrology, glaciology, and climate change". This is not convincing. How will this effort help, given the fact that authors mention that KUGI is "consistent" with RGI and GAMDAM? How is the KUGI more reliable than GAMDAM and RGI?

Line 70: Azam et al., 212 should be Azam et al. 2012

Line 73: "Indicated" should be "indicates".

I do not find the introduction section very convincing. Especially Line 60-105 appears more of discontinuous sentences where authors jump between various glaciological assessments (area changes, frontal retreat, geodetic and glaciological mass changes). This becomes irrelevant since the focus of the paper is the "evaluation" of glacier inventories over 3 river basins. Besides, I find certain sentences over-referenced and others poorly referenced. In many places, the authors have not even bothered to cite the recent literature (See details after the comment. Although I do not know whether they will be relevant if the MS is revised and contextualized for comparison of glacier inventories). For example, the authors say that using the freely available glacier datasets for glacier change assessment and future projections is not recommended as the glacier inventories have inconsistencies in terms of different glacier variables. Does it mean the regional glacier-related assessments (cited by authors in first paragraphs) are not imprecise and not reliable? The authors further go on to say that "*the glacier inventory database by Shukla et al., (2020), restricted to the Suru basin, is primarily based on the automatic approach (normalized-difference snow index) unlike the present study where the glaciers are mapped manually using on-screen digitization.*" Do they mean the inventory by Shukla et al is not credible? Since on-screen digitization is highly subjective and dependent on the cognition/skill of analysts, the approach could be contested especially when it comes to inventory mapping over large areas. How can/have the uncertainties about cognition been

addressed by authors?

Merely saying Google Earth was used for validation will not have many takers among the remote sensing glaciology community. I tried to dig into Google Earth data of the 2000s for the three basins but found massively snow/cloud-covered data for the assessment period. The authors need to come up clean on this and say precisely where Google Earth data was used for correcting the glacier outlines. And also since Google Earth and Landsat data do not have an exact overlap, how was coregistration achieved.

RECENT LITERATURE:

Nie, Y., Pritchard, H. D., Liu, Q., Hennig, T., Wang, W., Wang, X., ... & Chen, X. (2021). Glacial change and hydrological implications in the Himalaya and Karakoram. *Nature Reviews Earth & Environment*, 1-16.

Farinotti, D., Immerzeel, W. W., de Kok, R. J., Quincey, D. J., & Dehecq, A. (2020). Manifestations and mechanisms of the Karakoram glacier Anomaly. *Nature geoscience*, 13(1), 8-16.

Shean, D. E., Bhushan, S., Montesano, P., Rounce, D. R., Arendt, A., & Osmanoglu, B. (2020). A systematic, regional assessment of high mountain Asia glacier mass balance. *Frontiers in Earth Science*, 7, 363.

Soheb, M., Ramanathan, A., Angchuk, T., Mandal, A., Kumar, N., & Lotus, S. (2020). Mass-balance observation, reconstruction and sensitivity of Stok glacier, Ladakh region, India, between 1978 and 2019. *Journal of Glaciology*, 66(258), 627-642.

Mehta, M., Kumar, V., Garg, S., & Shukla, A. (2021). Little Ice Age glacier extent and temporal changes in annual mass balance (2016–2019) of Pensilungpa Glacier, Zaskar Himalaya. *Regional Environmental Change*, 21(2), 1-18.

Farinotti, D., Immerzeel, W. W., de Kok, R. J., Quincey, D. J., & Dehecq, A. (2020). Manifestations and mechanisms of the Karakoram glacier Anomaly. *Nature geoscience*, 13(1), 8-16.

Nie, Y., Pritchard, H. D., Liu, Q., Hennig, T., Wang, W., Wang, X., ... & Chen, X. (2021). Glacial change and hydrological implications in the Himalaya and Karakoram. *Nature Reviews Earth & Environment*, 1-16.

Pritchard, H. D. (2019). Asia's shrinking glaciers protect large populations from drought stress. *Nature*, 569(7758), 649-654.

Immerzeel, W. W., Lutz, A. F., Andrade, M., Bahl, A., Biemans, H., Bolch, T., ... & Baillie, J. E. M. (2020). Importance and vulnerability of the world's water towers. *Nature*, 577(7790), 364-369.

Hugonnet, R., McNabb, R., Berthier, E. et al. Accelerated global glacier mass loss in the early twenty-first century. *Nature* 592, 726–731 (2021).
<https://doi.org/10.1038/s41586-021-03436-z>

The authors suggest having used satellite data of 2000±3 years to delineate inventories whereas ICIMOD glacier inventory has used satellite data of 2005±5 years. Wouldn't it be comparing apples with oranges? This becomes important especially in the case of small glaciers and needs to be factored for.

"It is hoped that the KU glacier inventory and elevation change databases presented in

this paper shall further help in promoting research in fields like climate change, hydrology, and other allied fields." This is common for any inventory. The rationale should be how KUGI will help to further it. This needs to be mentioned.

Line 132: Need to place "and" between latitude and longitude values.

Line 135-36: "*The area above 3600 m asl in general remains covered with perennial snow and glaciers*". Not true mostly. This has to be ~4000 m asl for the J&K region.

Line 136-37: Why have authors quoted numbers from RGI inventory and not GAMDAM?

Line 138: "thus making the study area the most glaciated terrain". Reframe.

Line 139-40: The authors quote Kamp et al. (2011) and suggest that glaciers are cirque-type in Ladakh which is not true with all the glaciers in the Suru basin and also neighboring Zaskar region. Glaciers in North Ladakh (Siachin area, Rimo group) are not cirque-type either. Please reframe.

Line 140-41: "All the major tributaries" instead of "Most of the major tributaries"

Fig. 1: The caption should mention the following: What does the inset map represent? What is the background image (a DEM or what)? Have a legend for elevation if it is so. Mention may be "GA" for glacier area instead of "A" since "A" also represents Jhelum Basin. The text could be made bold and a little larger for histograms in the study area map. What is the source of number of glaciers, glacier area, debris cover, and glacier volume? Maybe plot glacier volume and debris cover on the secondary axis since the associated values are small.

Line 150-180: Could be better represented as a table. For climatology of Jhelum Basin use: Zaz et al 2019 (ACP). For Suru met data are improper. Authors used Schmidt and Nüsser, 2012 (which mentions a different area of Ladakh, Kang Yatze massif, and not Suru) while Chevuturi et al. (2018) report climate of Leh and not Suru. Need to correct it. Similarly for Chenab, the authors quote Azam et al. (for Chotta Shigri area). Why not cite Bhutiyani et al. 2007 (Climatic change) and Bhutiyani et al 2010 (Int J of Climatology)?

Line 192: "The of moderate resolution". Please correct.

Line 193-96: Some of the references have been quoted above and do not necessarily need to be mentioned here.

Table 1:

The authors mentioned wrong dates for imagery used for ICIMOD inventory. It is 2005±3 years (Weblink: <https://lib.icimod.org/record/9419>. See page 7>Section Satellite images> the second paragraph). I would again repeat my above question: Can 2000 data (in RGI, GAMDAM, and KUGI) be compared with 2005 data (as used in ICIMOD inventory)? Definitely not. Please justify. Also modify the respective entry in the Table. Details of all satellite scenes should be provided as supplementary data.

Google Earth: Mention the date of Google Earth images, if at all they were used for correcting/validating glacier outlines. Maybe have a supplementary file for mentioning which glaciers were validated using Google Earth imagery.

Line 213-14: Delete " hereafter named Kashmir University Glacial Inventory" as it has already been defined as KUGI in the abstract.

Line 214: Delete "global".

Line 129: "acquired during 2002 to 2008". See my earlier comments.

Line 225-28: The authors mention "The RGI glacier outlines have been extracted semi-automatically from the Landsat satellite images between 1998 and 2009. However, most of the glaciers (~98%) in the inventory over the study area have been extracted from the images acquired during 1998-2002". How many glaciers were delineated from 1998-2002 data in RGI inventory? See attribute of RGI shapefile. Again comparison seems a problem here; not only due to dates but also the technique used.

Line 244: What do authors mean by "limited field surveys"? How many glaciers (%) were ground surveyed? What kind of data was collected? Need to reflect all that in the MS.

Line 256-57, 262, 267-270: "were verified from the Google Earth™". The GE data for 2000 is almost not usable for the region. Please explain. Is it a deliberate attempt of misinformation or what?

Line 262-63: "The thin debris layer on the glacier surface, often bearing lower surface

temperature". Do the authors mean differential wrt ice or neighboring landscape?

Line 200, 280-282: The authors mention ASTER DEM here having been used to derive glacier-specific topographic parameters. But there is no mention of ASTER DEM in Table 1. Why was ASTER DEM used when CARTO DEM with a similar resolution is available over the region? Seems repetition of (line 201-202) here (280-82).

Line 285-295: When the techniques used for mapping the glacier outlines are different, it is but obvious that there won't be a high overlap. Would it be so? Add a justification.

Remove section 4.3 as explained in the beginning and also uncertainty related to geodetic mass balance.

Line 330: R_p/A_p is a constant. What does it represent?

Line 368: "Jhelum" instead of "Jehlum". Be consistent with spelling.

Line 368: "The glaciers range in size from 0.01 km²". This means ~11 pixels. I wonder if such small-sized glaciers (1 ha as mentioned by authors) could be mapped from 30 m Landsat data? This would be highly uncertain. Could it be that some of them were snowpacks and not glaciers especially when ascertaining from 2000-02 data?

Table 2: The authors mention glacier volume but have not provided any information as to how ice volumes were derived? Did the authors use VA scaling and why if it is known that VA scaling estimates are highly uncertain, even for entire mountain ranges.

Line 383-84: "mean glacier slope in the basin varies between 9° and 50°". The glaciers with a mean slope of 50 degrees are highly unlikely since by the definition such areas (> 30-degree slope) could be avalanche feeding zones. Please speak otherwise it raises concerns about the inventory itself.

Table 3: Why do the authors need to mention glacier area categories from >20->50 in the Jhelum basin. >50 should not be the category. Let it be >50-the highest glacier area in the respective basin. Similarly, if there is a category 1-2, it should be followed with >2-5, >5-10, so on and so forth.

Line 395-397: Delete as it is already mentioned in 275-76.

Table 4: Elevation categories from 5500-7000 are not relevant for Jhelun and as such could be deleted. If the first category starts from ≤ 4000 , the next category should be $>4000-4500$, so on and so forth.

Mention uncertainties about each A (glacier areas) in table 3, 4, 5 and 6.

The uncertainty in TGA for the Jhelum basin is $\sim 13.3\%$ compared to Suru and Chenab (both 3.3%). Why is this so? This should have been the other way around since there are more clean-ice glaciers in Jhelum. Explain.

How different are the estimates of Table 7 different from Scherler et al 2018 (<https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2018GL080158>)

Line 474: Delete "regional and global".

Table 8: Should be " ΔN " instead of "N". Percentage difference could be mentioned in brackets.

Line 490-500: Use ICIMOD and GAMDAM rather than ICIMODGI and GAMDAMGI.

Table 9: Above in methods authors mention having used median glacier elevation rather than mean glacier elevation.

Section 5.3 and associated tables/figures should be removed.

Line 675-680: The authors mention large glaciers at high altitudes and then low glacier cover at high altitudes which is a kind of contradiction. Large glaciers as authors suggest in results contribute to glacier cover. Please explain.

Table 14: Remove

The authors do not discuss much about the aspect (neither in results nor discussion) except very qualitatively.

Section 6.2 should be "Inconsistencies in existing glacier inventories"

Figure 5: Mention background image and band combination.

Line 720-724: The overlap ratio could be misleading since the inventories were computed using different methods. Please justify.

Figure 6: SG. On checking from Google Earth, the existing inventories have rightly followed GLIMS definition of glacier delineation and divided the ice into two polygons since the ice fluxes move in different ablation zones. However, the authors have erred here (and maybe in many such cases) by considering it as a single glacier. The authors should know that although the ice masses are connected (in the accumulation zone) the movement of ice in different directions owing to the ridge-topography divides the ice into two polygons and hence two glaciers (refer to GLIMS definition of glaciers). This appears to be a huge flaw with the interpretation by authors as the number of glaciers is massively underestimated in KUGI. This needs to be corrected in the data as well as explanations to the data.

Figure 7: Mention background image and band combination.

Remove section 6.3

Conclusions:

While the authors mention "limited field survey" at many places in the text, they have failed to showcase the data collected, the photographs depicting the glacier environments in these three catchments. They should show field data and photographs from all three catchments and demonstrate its usefulness in inventorying the glaciers in these three respective areas.

Delete sentences about geodetic mass changes.

The authors fail to convince the robustness of KUGI compared to at least RGI and GAMDAM. This needs to come up very in the results, discussion as well as the conclusion.