Earth Syst. Sci. Data Discuss., author comment AC3
https://doi.org/10.5194/essd-2021-28-AC3, 2021

Reply on CC1

Shakil Ahmad Romshoo et al.

Author 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-AC3, 2021

CC#1

General Comment: 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:

Response: The authors thank the anonymous reviewer/commentator "Glacier Ice" for the useful comments and suggestion on the manuscript. The comments/suggestions have been responded point-by-point in the following sections and the useful suggestions have been incorporated in the revised manuscript which have improved the quality of the revised manuscript.

Comment #1: 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..."

Response: Agreed, that the ICIMOD and GAMDAM are regional inventories and the RGI is a global glacier inventory. Accordingly, the title and text have been modified in the revised manuscript. The "Global glacier inventories" has been replaced with "Global and Regional glacier inventories" in the title of the revised manuscript.

Comment #2: 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?

Response: In the present study, we have done field surveys/validation on 20 glaciers located across the study area. The locations and the field photographs of these glaciers has been provided as Supplementary figure in the revised manuscript. We have collected the snout positional data of the debris-covered tongues of these glaciers to validate the glacier delineation. The field measurements of these glaciers acted as an interpretation tool for delineation of the debris-covered glaciers in the study area. The terminus of the heavily debris covered glaciers like the Hoksar glacier in Jhelum basin was not easily delineated even using the thermal and google earth imagery. We therefore, mapped the glacier terminus on field and further observed that the debris on the glacier is relatively smooth and aligned in the direction of glacier flow when compared to the debris-cover in the surroundings which was found a useful field-based information for mapping of debris-covered termini of other glaciers when viewed on Google earth. Further, eight of these twelve glaciers have been designated as benchmark glaciers and are continuously studied for mass balance, GPR, debris thickness, Surface mapping temperature profiling and other glaciological studies since the last 5-8 years. In addition, glacier outlines of several other glaciers in the vicinity of these 8 benchmark glaciers have been verified during annual glacier field expeditions during the last 5-8 years.

Additionally, all the heavily debris covered glaciers and a majority of the clean glaciers, numbering more than 850 were qualitatively verified on Google Earth image for the rectification of any delineation error. A similar approach of quality check using Google Earth has been previously adopted by Nagai et al. (2016) and several others and is an accepted method for validation of such a large number of glaciers located in inaccessible complex terrain.

Field photographs of the about 20 selected glaciers in the three basins, which have been visited over the last few years for field measurements/validation along with their GLIMS ID are presented in the revised manuscript (The field photographs of some of these glaciers are provided above). A locational map of these glaciers in the form of a KML file would be provided as a supplemental material in the revised manuscript (The field photographs have also been attached as a supplement file in pdf format).

Comment #3: 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.

Response: For the uncertainty assessment of the glacier area, we have referred to the previous detailed uncertainty analysis by Paul et al. (2013) that reported an error of ~3% for the alpine glaciers. Paul et al. (2013), however, reported a perimeter-area ratio of 5.03 km^{-1}, unlike the perimeter-area ratio of 0.96 km^{-1} observed for Chenab in this study, 3.9 km^{-1} for Suru and 6.22 km^{-1} for the Jhelum basin. We therefore applied a scaling after Braun et al, (2019) to determine the uncertainty in glacier outline delineation in the three basins in this study. As indicated by the value of Perimeter-Area ratio of 6.22 km^{-1} for the Jhelum basin, it is quite obvious that the basin is expected to have higher uncertainty as the perimeter-area ratio of the basin is higher amongst the three studied basins. This has been specified in the revised manuscript.

Comment #4: When the authors say "most of the glaciers in the study area are <1 km2 in size><1km^2", they should mention the number and percentage of these glaciers. Also for the 1-5 km^2 category.

Response: The detailed statistics of the glacier number and area in each size category is described in detail in the Results section and already provided in Table 3 of the manuscript. On an average around 91% of the glaciers (glacier number) are in the size category < 1 km^2 in the Jhelum basin. Similarly, around 77% of the glaciers (number) are falling in the < 1 km^2 area class in the Suru and Chenab basins (Table 3). In terms of the glacier coverage (area), the area class 1-5 km^2 harbor ~44% of the glacier area in the Chenab basin, followed by ~55% in the Suru basin and ~90% in the Jhelum basin (Table 3). The statistics are averaged for all the glacier inventories and the numbers vary between the individual inventories (Table 3). This has been specified in the revised manuscript.

Comment #5: "Majority of the glaciers....". Please quantify. Similarly "... Jhelum where the glaciers are mostly...". Again MOSTLY is subjective.

Response: The elevation range 4500-5500 for the Chenab and Suru basins harbor around 85.5% and 91.3% of the glacier number and area respectively. Whereas, the elevation band 4000-5000 harbors 91.8% (number) and 95.8% (area) of glacier number and area respectively in the Jhelum basin (Information already provided in Table 4). However, the information has been specified in the revised manuscript as suggested.

Comment #6: 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.

Response: The slope range (10-30°) harbors 78% of the glaciers in terms of number and 84% of the glacier area (Table 4). The numbers have been specified in the revised manuscript.

Comment #7: 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?

Response: The overlap ratio of individual glaciers is represented by "r_{ov}" whereas, the "R" ($R^B{}_A$) has been used to represent the average overlapping ratio of the base and target glacier inventories (overlap ratio averaged for all the glaciers in a particular inventory combination e.g, KUGI-RGI for each basin). This has been described under the methodology section in the revised manuscript. The inventories are consistent in general pattern and distribution which means that a majority of the glaciers in all the inventories are found in a particular area class, slope category etc. however, large differences occur in individual glaciers and overall statistics in terms of the area/size (Table 3-6). However, the specific purpose of the KUGI is provided is provided in the response to the **Comment #9** below.

Comment #8: 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.

Response: The geodetic mass changes of the entire region of Upper Indus Basin comprising of 12000 glaciers, of which Jhelum, Chenab and Suru are a part, has been published in the *Scientific Reports* which has been referred to in the manuscript and therefore would not in any way amount to plagiarism. However, we wanted to retain the elevation change information in the inventory data base for the ready availability of this valuable database to scientific community as the same provides a valuable additional information about the behavior/dynamics of the glaciers in the study area.

Comment #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?

Response: Please see the response to the comment above where it has been clarified that there is "consistency" in terms of the general pattern/distribution, however there are significant differences in various glacier parameters reported in the three inventories compared to the KUGI. The value addition and novelty of the KUGI over the three inventories evaluated in this study has been prominently mentioned at relevant places in the revised manuscript and the same is consolidated and reproduced here for your perusal is also reproduced as follows:

"Primarily, the motivation for the KUGI is to develop a high-resolution glacier inventory with improved accuracy with visual interpretation and manual delineation of glaciers from Landsat satellite data supported by the limited ground truth and supplementing the glacier outlines with additional data like debris-cover, thickness changes and other glaciological parameters, that are either missing or incorrect in the existing databases so that the database is made available to the large research community for various applications.

Purportedly the global and regional glacier databases that were chosen for comparison in this study have been generated using a semi-automated method (manual for GAMDAMGI) allowing less human error, quick delivery, and high accuracy. However, it was found in this study that there are significant errors in the evaluated databases due to the misinterpretation of seasonal snow cover particularly on the glacier headwalls at high altitudes, shadow-covered glaciers and debris-cover. Against the reports/claims of the overall accuracy of the global/regional glacier databases, ~3% for ICIMOD (Bajracharya er al, 2014), ~5% for RGI (Pfeffer et al. 2014; RGI Consortium, 2017) and 15% for GAMDAM (Nuimura et al. 2015), it was found that, compared to the KUGI, the ICIMOD is overestimating glacier area by 12.2%, RGI underestimates the glacier area by 2.4% and the GAMDAM inventory by 1.5%. However, the three databases overestimate the glacier numbers in the three basins considerably; RGI by ~45%, ICMOD by ~68% and GAMDAM by ~56%. Gardelle et al. (2013) found that in the southeastern Tibet, RGI 2.0 database has glacier extent 88% greater than their estimate. Similarly, Nuimura et al. (2015) while comparing GAMDAM with ICIMOD found a significant discrepancy between the two inventories. Frey et al. (2014) and Mölg et al. (2018), have highlighted the presence of debris-cover, seasonal snow and cloud cover as the main source of uncertainty in the Himalayan region. Mohammad et al. (2019) has also highlighted the differences between the existing glacier inventories in Indus basin.

Keeping in view the worldwide use and applications of global and regional databases, it is important that a rigorous evaluation of these global and regional inventories is undertaken for the continued refinement of the methodology which is a fundamental requirement for any meaningful application of the global or regional database. It is hoped that the future releases of the databases will improve these and other shortcomings identified in this manuscript. Although this cross-checking improved the quality of the data, the mapped glacier outlines are also affected by various other types of obscurities, which are mostly dependent on image resolution which is the also the case with KUGI.

KUGI improved the mis-mapped glacier outlines/boundaries from existing global and regional inventories and any mismatches of the glacier geometry due to the seasonal/temporal snow cover and shadows were manually corrected using additional Landsat images and Google earth images. Further the mapped glaciers with better georeferencing were overlaid with high resolution images in Google Earth environment for validation wherever available. Though, the mis-mapped/mis-located outlines, observed on the global/regional inventories, may have only limited effect on measurements of glacier area, but can introduce serious errors into applications that rely on absolute positioning (e.g. co-registration to other datasets such as DEMs). The only realistic way to correct them is to provide more accurate outlines as done in the KUGI which would serve as source of improved outlines for the scientific community interested in conducting various application studies using the glacier outlines.

The analysis of the debris-cover (>19%)-the criterion we used to classify the debris-covered glaciers (Brun et al., 2019) showed that the RGI, ICIMOD and GAMDAM glacier inventories have underestimated the debris-covered glaciers by ~15%, ~25%, 8% respectively. Debris cover, present on 44% of Earth's glaciers, significantly influences glacier melt. Despite its significant importance, the debris cover has not been mapped with accuracy in the three global/regional glacier databases evaluated in this study. Due to lack of debris-cover map at the global level (Sam Herreid & Francesca Pellicciotti, 2020), debris cover has been omitted from global glacier models and forecasts of their response to a changing climate. Therefore, the KUGI, has fundamentally resolved this omission and provided improved debris-cover outlines of the three basins in the Northwest Himalaya. KUGI has added a separate debris-cover database in the three basins which is missing in the global/regional databases. This is a major improvement and correction to the existing global/regional databases evaluated in this study. Use of the KUGI outlines of the debris-covered parts of glaciers in glacier-melt models will enable improved estimates of melt over the three basins.

Other than the debris-covered glaciers, the discrepancy related to the shadowed glaciers is another major error with the glacier inventories. Though, KUGI has not generated a database of these glaciers, but the same is under preparation for inclusion in the revised version of the KUGI database being submitted after this revision"

Comment #10: Line 70: Azam et al., 212 should be Azam et al. 2012

Response: The typo has been corrected in the revised manuscript.

Comment #11: Line 73: "Indicated" should be "indicates".

Response: The word "indicated" has been replaced by "indicates" in the revised manuscript.

Comment #12: 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?

Response: The introduction section provides a broader overview of the research under consideration and therefore, the authors felt that the reference to geodetic mass balance and other glacial studies is relevant. However, as suggested, a few relevant references suggested by the anonymous commentator have been incorporated in the revised manuscript.

We never said that the existing inventories are unreliable. Instead, we reiterate that a thorough evaluation of the glacier boundaries, as reported in this paper, is required before using them for impact assessment or any other climatic and hydrological application especially when the spatial domain of investigation is small (basins or sub-basins). The discrepancy in the glacier area might result in significant uncertainty in the use of the data for various applications.

The commentator is again insinuating when he emphasizes in italics something that is not meant by the authors-Not our words. We did not discredit the glacier inventory by Shukla et al, in fact, a part of the study area in Suru overlaps with the glaciers studied by Shukla et al. and therefore it is necessary to refer the paper. However, we observed a difference in glacier coverage reported between for a few glaciers in the two inventories and it is therefore important to explain this difference which we have attributed to the different techniques used in the present study.

Regarding minimizing the error due to the onscreen digitization by the analyst, it was made sure that analysts (authors) use the same criteria in terms of pre-processing, mapping scale etc. for the delineation of glacier boundaries. Furthermore, all the glacier boundaries were checked for quality control and corrected by the Lead author before finalization. This minimized the uncertainty due to the skill/interpretation of the analyst. The uncertainty approach used in the present study is also based on detailed analysis of glacier uncertainties mapped by multiple analysts (Paul et 2013) and therefore we believe that this approach is quite valid and addresses the uncertainties well.

Comment #13: 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.

Response: This is very unfortunate that the anonymous commentator is again using very strong words like "need to come clean" in his review comments which sound like personal and unprofessional**.** We do agree that the Google Earth data for 2000s for the study area is cloud and snow covered. However, it is pertinent mention here that the Google Earth image of post-dating 2000s were used for quality check and verification of ambiguous glacier outlines. For example, glacier headwalls of many glaciers covered with thin layer of snow appeared smooth on the Landsat imagery, however, when checked on the high-resolution Google EarthTM imagery, it usually turned out an undulated non glaciated surface or a rock surface covered with thin snow. Besides all the heavily debris covered glaciers were qualitatively verified on Google Earth for rectification of any delineation error. A similar approach of quality check using Google Earth has been previously adopted by Nagai et al. (2016) and several others and is an accepted method for such a large number of glaciers located in inaccessible complex terrain. Further, we did not use the Google Earth data for the quantification of glacier area, and therefore an exact-overlap was not required. All the statistic quantifications and analyses in the present study is based on the Landsat images. This has been specified in the revised manuscript. The Google Earth images were used for the verification and validation of the glacier outlines only. The location of the 850 glaciers verified from the snow- and cloud-free Google Earth images would be provided as supplemental KML file in the revised manuscript.

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, Zanskar 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

Response: The relevant literature has been cited at appropriate places in the revised manuscript.

Comment #14: 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.

Response: The difference in the dates of source images does not explain the significant glacier area over-estimation observed in the Jhelum and Chenab basins. Using 2005±3 data (compared to 2000±2 in case of KUGI), normally an under estimation in glacier area is expected in ICIMOD but the comparison of the ICIMOD database with KUGI shows over-estimation of the glacier area which has been attributed to inclusion snow covered glacier headwalls and at places some season snowpacks also. Further, the area underestimation of 10.97% in case of the Suru basin is not fully explained by the expected area change between 2000±2 and 2005±3. Such comparisons using data gap of 2-3 years have also been carried out in previous studies (Nuimura et al. 2015) and therefore, the comparison is valid. However, the data difference has been explained in the revised manuscript.

Comment #15: "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.

Response: Please see the response to the similar comment above. The novelty, value addition and usefulness of the KUGI over other inventories has been discussed in the revised manuscript and is reproduced in the authors response against the **Comment#9.**

Comment #16: Line 132: Need to place "and" between latitude and longitude values.

Response: "and" placed between the between latitude and longitude in the revised manuscript.

Comment #17: 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.

Response: In general, areas above 3600 masl remain snow covered with snow for the entire year in the study area. However, there might be some exceptions. Therefore, as suggested the sentence has been modified in the revised manuscript to show that areas ~4000 m asl remain covered with snow.

Comment #18: Line 136-37: Why have authors quoted numbers from RGI inventory and not GAMDAM?

Response: Here, we are providing an overall estimate of the glacier cover over the entire Jammu, Kashmir and Ladakh region extracted from RGI and used in the previous study (Abdullah et al. 2020).

Comment #19: Line 138: "thus making the study area the most glaciated terrain". Reframe.

Response: The sentence has been rephrased as suggested in the revised manuscript.

Comment #20: 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 Zanskar region. Glaciers in North Ladakh (Siachin area, Rimo group) aren't cirque-type either. Please reframe.

Response: Agreed, there are a few exceptions. Therefore, the sentence has been modified in the revised manuscript.

Comment #21: Line 140-41: "All the major tributaries" instead of "Most of the major tributaries"

Response: The sentence has been modified as suggested in the revised manuscript.

Comment #22: 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.

Response: The suggested modifications in the figure have been incorporated in the revised manuscript.

Comment #23: 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 Bhutiuyani et al 2010 (Int J of Climatology)?

Response: We agree that the meteorological data in Schmidt and Nüsser, (2012) is for the neighboring area, however, due to lack of meteorological data observations in the study region, we used the data from the referred paper assuming that the climatology of the Ladakh region does not vary much spatially from the Zanaskar region, as both being the part of the cold desert climatic zone. Again, due to non-availability of the meteorological data specific to Chenab basin, we cited Azam et al. which is located in the upper Chenab basin. Like, Chevuturi et al. (2018), Bhutiyani et al. (2007) also reported the climate data for the Ladakh Range based on the data available at the Leh station as there is no climate data available for the Zanaskar region. However, the time series in case of Chevuturi et al. (2018) is longer and as such, we cited the same in the manuscript. However, we have cited the relevant literature cited by the commentator in the revised manuscript.

Comment #24: Line 192: "The of moderate resolution". Please correct.

Response: The sentence have been corrected to "The use of ...." in the revised manuscript.

Comment #25: Line 193-96: Some of the references have been quoted above and do not necessarily need to be mentioned here.

Response: As suggested some of the references has been removed from here in the revised manuscript.

Comment #26: 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.

Response: Sorry for inadvertent typo and the same has been corrected in the table. The rest of the comment has already been responded to (Please see the response to **Comment#14** above.)

Comment #27: 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.

Response: This comment has been already responded to above. All the debris-covered glacier and several clean numbering more than 850 have been validated/corrected using the Google Earth data. Google earth images are dated, 2009-2011 for the Jhelum basin: 2006-2013 for the Suru basin and 2000-2006 for Chenab basin have been used to verify the glacier outlines. The locational location of the glaciers verified on Google Earth is provided as KML file in the supplemental material of the revised manuscript.

Comment #28: Line 213-14: Delete " hereafter named Kashmir University Glacial Inventory" as it has already been defined as KUGI in the abstract.

Response: The sentence has been modified, as suggested, in the revised manuscript.

Comment #29: Line 214: Delete "global".

Response: The modifications, as suggested, have been incorporated throughout the revised manuscript.

Comment #30: Line 129: "acquired during 2002 to 2008". See my earlier comments.

Response: This has been explained in the response to Comment#14 above and specified in the revised manuscript.

Comment #31: 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.

Response: The information in the text is relevant for the entire Jammu, Kashmir and Ladakh region which has been specifically mentioned in the revised manuscript. Out of 15064 glaciers in RGI, 14894 glaciers were delineated from source images acquired between 1998-2002 (1998: 2228; 1999:2065; 2000:4034; 2001: 2117; 20002:3789; 2006:17; 2009:153). For the three basins under consideration, all the glaciers except one in the Suru basin, have been delineated from source images acquired between 2000-2002. This has been specified in the revised manuscript.

Comment #32: 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.

Response: This is again a repeated comment and has been responded to above (Please see response to the **Comment#2** above)

Comment #33: Line 256-57, 262, 267-270: "were verified from the Google EarthTM". The GE data for 2000 is almost not usable for the region. Please explain. Is it a deliberate attempt of misinformation or what?

Response: This is very unfortunate that the commentator is repeating the use of strong language like "Misinformation or what". This is comment is repeated 4th time in the review. However, the comment stands already responded above. We in general appreciate the suggestions of the anonymous reviewer but the repeated use of the personal and unscientific language is unnecessary and therefore very unfortunate.

Comment #34: 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?

Response: Yes, it is with reference to the neighboring landscape. The same has been mentioned in the revised manuscript.

Comment #35: 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).

Response: The information pertaining GDEM2 has been incorporated in the revised manuscript. We preferred ASTER GDEM2 as its use for glacier studies is well established as mentioned in section 3.2. Furthermore, since both the DEMs have same spatial resolution and we did not find any study reporting any specific advantage of using CartoDEM for glacier inventory studies.

The repeating sentence at line 201-202 has been deleted in the revised manuscript.

Comment #36: 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.

Response: Of course, there won't be a high overlap and in fact we have already mentioned this (different techniques used for glacier mapping) as one of the reasons for the discrepancy observed in glacier outlines in terms of the overlap ratio

Comment #37: Remove section 4.3 as explained in the beginning and also uncertainty related to geodetic mass balance.

Response: As mentioned in response to **Comment #8** above, we believe that the elevation change information will be a value addition to the database as such there is merit in retaining this section.

Comment #38: Line 330: Rp/Ap is a constant. What does it represent?

Response: Please refer to Paul et al, (2013).   is the perimeter-area ration reported by Paul et al, (2013) in a detailed analysis aimed at the uncertainty assessment of glacier mapping which is equal to 5.03 km $^{-1}$. In the present study the perimeter-area ratio however, varied from 0.96 – 6.22 km^{-1}, we therefore applied a scaling after Braun et al, (2019) to determine the uncertainty in glacier delineation over the study region. We have incorporated the description in the revised manuscript.

Comment #39: Line 368: "Jhelum" instead of "Jehlum". Be consistent with spelling.

Response: The typo has been corrected in the revised manuscript.

Comment #40: Line 368: "The glaciers range in size from 0.01 km^2". 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?

Response: Glaciers of the size (0.01 km^2) have been previously mapped by Paul et al. (2002); Paul et al. (2009); Pfeffer et al. (2014); Abdullah et al (2020). Shukla et al. (2020) have also mapped glaciers with minimum size of 0.01 km^2 in a recent study comprising a part of the study region using 30 m Landsat data. Also, the glaciers of the same size have been mapped in the GAMDAM inventory using Landsat 2000 data

(Nuimura et al. 2015). So, mapping such glaciers from 30 m data is not a problem, however, to ensure that snow-packs are not misinterpreted for glaciers, we specifically checked satellite images dating before and after the satellite image under consideration. We also used Google earth imagery (post-dating) to verify that snow-packs are not misinterpreted as glaciers as already explained in response to your previous comment.

Comment #41: 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.

Response: The glacier volume was estimated using the slope-dependent volume estimation approach (Haeberli and Hoelzle, 1995), this has been mentioned in the manuscript.

Comment #42: 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.

Response: In the Jhelum basin, there is only one glacier with the mean slope of 49.7° and similarly there are a few more in the other basins. We rechecked these glaciers and found them situated at higher altitudes with very little exposed headwall area, and therefore, there is not enough terrain to form avalanche zone.

Comment #43: 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.

Response: As suggested, the non-relevant categories have been removed from the tables and the tables have been modified in the revised manuscript.

Comment #44: Line 395-397: Delete as it is already mentioned in 275-76.

Response: As suggested, the line is deleted in the revised manuscript.

Comment #45: 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.

Response: The non-relevant categories have been removed from the tables in the revised manuscript.

Comment #46: Mention uncertainties about each A (glacier areas) in table 3, 4, 5 and 6.

Response: Thanks for the suggestion. The uncertainties for each area, elevation, slope and aspect class has been provided in the revised manuscript.

Comment #48: The uncertainty in TGA for the Jhelum basin is ~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.

Response: This is again a repeating comment and has been already responded to (Please see the response to **Comment#3** above)

Comment #49: How different are the estimates of Table 7 different from Scherler et al 2018 (https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2018GL080158)

Response: On comparison of KUGI with Scherler et al. (2018), We found that the DC estimates are in general higher compared to the Scherler et al. (2018) for all the three basins. For example, for the Jhelum basin, the DC estimate of 7.2 km^2 is around 8% higher than Scherler et al. (6.6 km^2).

Comment #50: Line 474: Delete "regional and global".

Response: Deleted in the revised manuscript.

Comment #51: Table 8: Should be "ΔN" instead of "N". Percentage difference could be mentioned in brackets.

Response: The table is modified as suggested in the revised manuscript with percentage (%) difference provided in brackets and the difference in number is represented by "ΔN" instead of "N"

Comment #51: Line 490-500: Use ICIMOD and GAMDAM rather than ICIMODGI and GAMDAMGI.

Response: GI was added to reduce the word count and also it becomes monotonous to append the phrase "glacier inventory" each time an inventory name is mentioned. Therefore, we are retaining GIs after global, and regional inventories.

Comment #53: Table 9: Above in methods authors mention having used median glacier elevation rather than mean glacier elevation.

Response: Sorry, we have used the mean rather than median and the same been corrected in the methods section of the revised manuscript.

Comment #54: Section 5.3 and associated tables/figures should be removed.

Response: As mentioned in response to **Comment#8 and Comment#37**, we find merit in retaining the elevation change information in the revised manuscript.

Comment #55: 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.

Response: The glacier cover observed in the study region is in general concentrated at higher altitudes for example above 4500 m asl in the Suru and Chenab basins which is justified by the elevation and temperature relationship for glacier growth. However, glacier coverage starts to decrease considerably with the further increase in the altitude e.g., above 5500 m asl in the Suru and Chenab which has been attributed to the steeper headwalls above this altitude facilitating snow/ice avalanches, thereby precluding the glacier formation. The sentences have been rephrased in the revised manuscript for more clarity and better understanding.

Comment #56: Table 14: Remove

Response: Please see our response to the suggestion above (**Comment #37 and Comment#54**)

Comment #57: The authors do not discuss much about the aspect (neither in results nor discussion) except very qualitatively.

Response: More details regarding aspect have been added in the revised manuscript.

Comment #58: Section 6.2 should be "Inconsistencies in existing glacier inventories"

Response: The heading has been modified, as suggested, in the revised manuscript.

Comment #59: Figure 5: Mention background image and band combination.

Response: The background images is FCC (7,4,2) of Landsat ETM+ dated 04-09-2000. The information has been added to the figure caption.

Comment #60: Line 720-724: The overlap ratio could be misleading since the inventories were computed using different methods. Please justify.

Response: Nagai et al. (2016) have demonstrated usefulness of the overlap ratio to assess the consistency of glacier outlines including location shifts which would be difficult to assess using the absolute value of delineated areas. Furthermore, the results of the overlap ratio observed in the present study are corroborated by the comparison of the glacier inventories in absolute terms (number and area). For example, the overlap ratio between KUGI-ICIMOD combination is relatively poor and the same is reflected by relatively larger differences in glacier number and area. Therefore, overlap ratio is quite useful indicator to assess the consistency of glacier inventory irrespective of the methodology used to delineate glacier boundaries.

Comment #61: 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.

Response: We have followed the GLIMS criteria, by dividing a glacier into two or more polygons as determined by the underlying ridge topography during the inventory. It is fact that the latest Google Earth imagery shows that the glacier has fragmented, however, when we closely look at the Google Earth data, the glacier has fragmented or the under lying ridge divides the glacier far away from the position it was previously divided in case of GAMDAM and ICIMOD glacier inventories. Neither, GAMDAM nor the ICIMOD has divided the glacier where it appears to have fragmented in the recent years. Also, when we drape the 2000 satellite data over DEM, the ridge-topography is not prominent enough to manifest the flow direction/ridge divide on the satellite images especially in the accumulation zone. Therefore, there is no question of dividing a glacier into multiple polygons.

Comment #62: Figure 7: Mention background image and band combination.

Response: The image source and the band combination has been specified for the figure in the revised manuscript.

Comment #63: Remove section 6.3

Response: Please see our response to the similar comments above.

Comment #64: 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.

Response: This is again repeating comment. Please see our response to **Comment #2; Comment #9.**

Comment #65: Delete sentences about geodetic mass changes.

Response: Again, as explained above, we want to retain this section in the revised manuscript.

Comment #66: 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.

Response: This is a repeating comment. Please see the response to the similar comments above, particularly the author response to **Comment #9**.

Please also note the supplement to this comment:
https://essd.copernicus.org/preprints/essd-2021-28/essd-2021-28-AC3-supplement.pdf