

Comments on “Assessment of Glacier Area Change in the Tekes River Basin, Central Tien Shan, Kazakhstan Between 1976 and 2013 using Landsat and KH-9 Imagery” by Usmanova *et al.*, submitted to *The Cryosphere*.

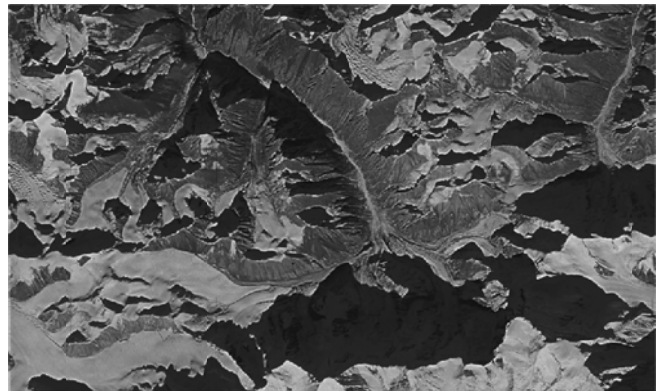
General Comments

The manuscript submitted by Usmanova *et al.* introduced the results of glacier change study in upstream basin of Tekes River, Central Tien Shan. This study used remote sensing and GIS techniques as the primary methods, and analyzed the climate change by observations of nearby meteorological station. The results filled some gaps of glacier change studies over Tien Shan, and also contributed to the knowledge of regional climate change and its impacts on glaciers.

However, from my point of view, this manuscript is currently not suitable to be published on *The Cryosphere*, mainly due to several of its intrinsic defects. Papers about glacier change study that were recently published on *The Cryosphere* are either cover several aspects (e.g. mass balance, volume change, surface velocity, etc.) rather than glacier area change only, or cover larger regions and detailed in-depth glacier change analyses. Central Tien Shan is featured by larger continental glaciers, while this manuscript only focusing on a small partial of Tekes river basin with some small glaciers, and can very limitedly contribute to understandings of regional glacier change study, neither to the hydrological studies of Tekes or Ili River. The glacier change derived in the manuscript are also only relatively complete for the period of 1992-2013, while fairly partial in 1975-1992 (for only 28 glaciers) and uncertain in 1960s-1975 (by using unverified USSR glacier catalogue). The methods and data used in this manuscript are widely used by most researchers thus with limited innovations, while the results were also poorly presented. Therefore, **from the spatial coverage, the innovations, and the studying depth, this manuscript are all not sufficiently qualified to be published on *The Cryosphere*.**

Besides above reasons, many other fatal shortcomings are still existing in the manuscript (see below), which are also significantly reduced the suitability of acceptance.

1. Currently the writing style of this manuscript is fairly awkward. The whole manuscript are glutted with typing and writing errors and/or mistakes, leads to relatively poor readability.
2. The quality of KH-9 image used in this study is also very limited, which was influenced by heavy hill shade and large amounts of seasonal snow (see right). From my view, it cannot adequately supports the accurate delineation of glaciers in 1975.
3. The lack of glacier data on more dates has significantly lower down the effectiveness of the glacier change analyses in this study, which already aware by the authors. This seems blunder away more interesting glacier change patterns and the relationships to climate change.
4. The glaciologists of former Soviet Union have done great works to compile the Catalogue of Glaciers of the USSR, and we must give complete respects to their works. However, due to the source material and methods used during the compilation of the glacier catalogue, larger uncertainties must be introduced. That's why many researchers have performed validation study on the accuracy before using this glacier catalogue which resulted in a large range of uncertainties. This work is absolutely absent in this study.
5. The multi-stage change patterns of local climate can be easily readed from the figures presented in the manuscript just by simple visual analyses. However, the authors only performed a linear trend analysis over



entire observation period, while ignored more meaningful climate change patterns at different stages. This somehow caused wastes of the valuable meteorological observations.

Some suggestions for the authors:

1. Although the manuscript currently is not suitable to be published on *The Cryosphere*, it can be enough qualified to be published on other related international journals. Of course some shortcomings still need to be revised.
2. The national boundary should not become the barriers of glacier change study. If the authors still want to publish the manuscript on *The Cryosphere*, please consider to extend the study area to whole Tekes river basin, and use more efficient remote sensing data as suggested below, to retrieve detailed glacier change pattern.
3. According to my check, there are even higher resolution (1.8m) KH-4B imageries in 1970 on USGS website, with similar quality of current KH-9 image, but can cover all regions of the study area and neighbouring regions. Why not using those images and present a thorough study of glacier changes in whole basin?
4. A verification of the USSR glacier catalogue is strongly suggested to provide reference to its users to surrounding regions, whether the authors want to use it as a primary data source or not to study glacier change.
5. An in-depth check-up by native English speaker with sufficient glaciological knowledge and sufficient writing skills is strongly suggested.

Specific Comments

Line 20: "in the sample", use another word instead, e.g. "basin", and also for "combined" ("total"?).

Line 20-22: This should not be a finding of this study and thus better not to be listed here.

Line 24: "61% of ... 1992" is not needed here, just one latest estimation is enough. The other and the comparison can barely tell anything.

Line 25-28: These two sentences actually tell same things, thus should better to be merged into one.

Line 30: This sentence should be split into two after "all seasons". Currently its structure is awful, and the meaning is also inexplicable.

Line 31-34: The first sentence is suggested to be revised like "The precipitation show insignificant change patterns with strong fluctuations. It decreased in 1952-1977, increased in 1978-2000, and again decreased after 2000 with a number of dry years in 2010s". The second sentence makes no sense and should be removed.

Line 34-36: This conclusion (steady recession rate) is problematic. Also see comments on Line 485-488.

Line 40-41: Remove "at present". Besides, the RGI has become the new standard database of global glacier distribution, so the glacier data of Tien Shan should also cite the latest RGI database, i.e. RGI 5.0.

Line 41-44: "in the region" should be "in this region", and this long sentence should better to be split into two sentence around the "and recent assessments ...". The "overall" should also be removed here, the "glacierized area and mass respectively" should better to be revised as "area and mass of glaciers, respectively, ".

Line 46-47: "potentially leading to ...", this part of the sentence is awful and need to be revised.

Line 47-49: The structure of this sentence is also awful (with very confused subject-verb-object) and hard to follow. Please revise.

Line 49: "high retreat rates", maybe give an approximate value of the average change rate here will be better.

Line 52: What the “close to the accuracy of measurements” want to tell? It’s confusing and also should better to be substituted by more specific approximate value of glacier change rate.

Line 53: Like Line 49 and 52, give a more specific approximate value of the glacier change rate rather than “similarly low recession rates” here. Besides, “similarly” should be “similar”.

Line 54: “for the 1975-2008 period” should better to be revised as “during 1975-2008”.

Line 55-56: “as did ...”, this part should be split into new sentence, and should be revised like “The study of Shangguan *et al.* (2009) gives similar rate during 1960s-2000 for glaciers in drainage basins of Chinese Tien Shan that flow to Tarim Interior River Basin”.

Line 57: Remove “the” before glacier extent.

Line 58: “examining” should be “also examined”, and add “which” before “highlight”.

Line 59: Add “the studies of” before “Aizen *et al.*”

Line 60: Revise “Having analysed ...” as “By using ...” or “Depending on ...”.

Line 65-66: This sentence should better to be revised like “... showed that glacier recession rates in northern Tien Shan were comparable between the periods of 1955/56-1975 and 1990-2008, but 2-3 times higher during 1970s”.

Line 67-69: Revise this sentence like “However, the value of glacier area for earlier stage in this study were simply read from Catalogue of Glaciers of the USSR that was compiled based on unarchived historical aerial photography and topographic maps, made their uncertainty assessments problematic”.

Line 70-73: Too much information contained in this long sentence, which makes it awkward. Please revise this long sentence, e.g. split it into multiple sentences or make the meaning and structure more clear.

Line 76-78: Same to Line 70-73, it’s a too long sentence that contains too much information. Please revise it.

Line 80: Revise the last part of this sentence like “... to assess the changes of glacier extents of the basin”.

Line 81: Revise as “... can dates back a century ago ...”.

Line 83: Add “(see Fig.1)” after “Saryjaz”.

Line 87: Split the sentence into two at “and this was followed”.

Line 88-89: Remove “More recently” because the contents after it (glacier changes) are not parallel or sequential to glacier distributions or inventories before. The following words should also be revised to more transitional words, e.g. “The glacier area in this basin has decreased 15.8% (0.45% a-1) according to Vilesov (2006).”

Line 91-92: “There are no ...”, it’s incorrect because the study of Xu *et al.* (2015) extends to 2009 and can be a good reference for this basin due to its spatial neighborhood. Maybe what you want to say here is no glacier change study between 1990 and recent years for this basin.

Line 93-96: This part also talks about glacier inventories, thus it’s better to be placed after USSR inventory (Line 88). Besides, in contrast to some former sentences, these two sentences should be merged into one. One suggested form is “The RGI 5.0 (Randolph Glacier Inventory,) contains updated glacier inventories for this region, which came from GAMDAM (.....) glacier inventory compiled based on Landsat imageries acquired during 1999-2003”.

Line 96-99: This sentence makes no sense and should better to be removed.

Line 101: Why didn’t mention the glacier inventory of 1976 here? It should better than the inventories from Landsat imageries because of the higher resolution. Besides, remove repeated “)”.

Line 103-104: Content of this point (glacier change) is repeated with previous one (ii).

Line 107-109: The longitude and latitude ranges are not necessary here which can be read from Figure 1 (or using more accurate ranges if you insist). “slope” is more common than “macroslope” in similar

literatures, and using “Ranges” rather than “Ridges” as well. The mountain names should be clearly marked on Figure 1 (also see comments on Figure 1).

Line 116&118: As commented on Figure 1, the glacier names mentioned should better to be marked on scale-enlarged map, to make clear which glaciers are the authors referring to.

Line 119-122: Using “total” rather than “combined” to represent the aggregated glacier area.

Line 123: What is “flat-summit glacier”? Does it mean ice cap? Use ice cap instead if it is. “flat-summit glacier” is not a widely accepted definition (see GLIMS Glacier Classification Manual).

Line 172-174: I cannot imagine how the ice divides delineated from present ASTER GDEM can match with the old USSR glacier catalogue. Please give a figure to illustrate it if possible.

Line 184: Use equivalent ground resolution instead of scan resolution here.

Line 188: Give the information of image that actually be referred (it should be one image rather than two or four images if using ERDAS Imagine 9.0).

Line 191-192: Were the accumulation zone margins for KH-9 image also mapped from Landsat imagery? Why didn't map it on KH-9 image? How to deal with different margins in accumulation zone like gradually emerged rocks?

Line 195: Comments on section 3.2

- 1) The authors should use more visualized forms, like formula, to describe what terms were involved and how the uncertainties were assessed.
- 2) From my view, the glacier area accuracies are mostly determined by the delineation processes rather than the orthorectification uncertainties. The residual error of image orthorectification mostly only affects the geolocation of glacier rather than their area (except the residual deformations in images on rugged terrain caused by errors in DEM or mis-placed GCPs), thus should better not be included in glacier area uncertainty assessment in case of Landsat imagery because of their well-known fine orthorectification accuracy (can be seen in a lot of literatures). However, it should be considered in case of KH-9 images because no equivalent resolution DEM and orthorectified higher resolution reference image can be found so far to accurately orthorectify them.
- 3) The mis-registration between images to derive glacier changes should be properly taken into account, especially in the case that same ice divides were used to split glaciers in different dates.

Line 198-199: Where did these 1:50000 maps come from? In which kind of coordinate system? How the transformations between different coordinate systems were processed? Especially if the 1:50000 maps using coordinate system defined by former Soviet Union. Please clarify. Besides, I am very certain that the identifiable terrain features (they must be mountain peaks, river confluences, road crosses, etc.) picked from 1:50000 maps can not be always perfectly matching with those picked from Landsat imagery, because of the low spatial resolution of Landsat, and also the change in the features' geolocation with time. Therefore, the orthorectification uncertainty resulted from such method is inconvincible to me.

Line 200-201: How the uncertainties of glacier margin identification were defined for different types of images? Please clarify.

Line 202-205: What is the $RMSE_{x,y}$? The residual error of orthorectification? And how was it determined for different images?

Line 203-204: It is fairly unclear how the glacier area uncertainty was calculated here. According to your description, was it come from dividing the original area by the buffer extended area?

The percentage uncertainty is just one of the forms to present uncertainty. The normal description of glacier area uncertainty should using the form of $x \pm y \text{ km}^2$, while how the value “y” was determined should be the key of uncertainty assessments. It is also the key in calculate percentage uncertainty (by $y/x*100\%$).

Line 206-209: The 3.5% uncertainty of Paul et al. (2013) is a general result for large amount of glaciers. How did it been taken into account of the area uncertainty assessments in this study? By using simply root mean square?

From my view, the glacier area uncertainty should better to be presented in the form mentioned in above comment, and also for each single glacier mapped, rather than the total glacier area of the study region. Besides, the resulted 18.6% and 13% area uncertainty in 1992 and 2013 are too larger to study the glacier area change, because the uncertainty of change must reach to $\sim 23\%$, which was much larger than the area change (13.5%) itself.

Line 210-211: Same as comments on this section, the co-registration uncertainty should better not to be considered as a part of the glacier area change uncertainty except the area uncertainties caused by using the common ice divides, which should be seperately calculated by proper way. The method suggested by Guo et al. (2015) may became an alternative way.

Guo, W., Liu, S., Xu, L., Wu, L., Shanguan, D., Yao, X., Wei, J., Bao, W., Yu, P., Liu, Q. and Jiang, Z. (2015). "The second Chinese glacier inventory: data, methods and results." *Journal of Glaciology* **61**(226): 357-372.

Line 211-214: The method to calculate glacier area change uncertainty is also problematic. Both glacier areas containing uncertainties, which certainly will be propagated into area change results. From my knowledge, the glacier area change uncertainty should be calculated based on the uncertainties in the glacier areas to calculate the change, rather than use a totally different method.

Line 215-224: The uncertainties in delineate debris-covered glaciers should not be so underestimated. Even by using high resolution Google Earth images, you cannot accurately identify where is the margins of debris-covered ice under many circumstances (e.g. as following Google Earth snapshots of debris-covered glaciers in the study region). So I suggest the authors to give a full consideration on the uncertainties of mapping debris-covered glaciers.



Line 229: The glacier mapping uncertainty from KH-9 imagery must be different from Landsat imageries because of the higher resolution, i.e. it should not be 3.5% anymore, and need to be properly re-evaluated.

Line 244: "... no urban heat island effects were assessed in ...".

Line 247: Remove two "period".

Line 256 and below: How the absolute area uncertainty ($x \pm y \text{ km}^2$) was calculated was not clearly described in section 3.2 (it is all about the relative uncertainties from my understanding). Please clearly specify it in corresponding section.

Line 257: Similar to comment on Line 211-214, the area change uncertainty should be calculated from area uncertainties by error propagation theory, which will result in $\pm 10.7 \text{ km}^2$ in this case, much larger than current $\pm 5.9 \text{ km}^2$.

Line 265-266: "The size of vanished glaciers ranging between 0.02 and 0.19 km^2 ".

Line 267-268: Replace "and in 2013, they" by ", however, they only", move "in 2013" to the end of this sentence.

Line 268-269: Remove this sentence.

Line 270-272: "The largest three glaciers, i.e., which locating on the Meridional Range and the northern slope of Saryjaz, respectively, are all compound-valley type glacier". The first current sentence conflicts with Table 4, where only 3 glaciers belong to compound-valley type.

Line 272-273: Replace "positioned at higher elevatoin reaching" with "located in the elevation range of".

Line 273-277: "The tongues of these three glaciers are extensively covered by debris, which slows down their retreat rate (here give their retreat rate), and consistent with the rate (it's better to give the reported rate here as well) reported by Osmanov et al. (2013) in nearby Saryjaz basin".

Line 277-278: "The valley type glaciers exhibit the largest absolute area loss, and the cirque type glaciers also show higher relative loss despite their shaded locations (Table 4; Fig.5). The larger relative retreat of cirque glaciers was probably ...".

Line 281-282: "The ice aprons have the largest relative area loss ($\dots \pm \dots\%$) followed by the ice-caps (please make sure that this is the correct terminology) ($\dots \pm \dots\%$)".

Line 287: Replace "aspect" with "orientation", and "faced" with "face".

Line 288-290: Breakup this sentence at "and this is why", replace the "combined area loss" with "absolute area loss", add "(Fig.6a)" at the end of this sentence, and add "By contrast, " ahead of next sentence. Replace the "(Fig.6)" with "(Fig.6b)".

Line 292: See general comment 2.

Line 305: Please clearly notation the corresponding abbreviations of periods in Figure 8 for autumn (SON?) and winter (DJF?).

Line 310-312: Where the "coefficient of variation (CV)" and the results for "CUSUM and Mann-Kendall sequential tests" are presented in the manuscript? Please clarify.

Line 313-315: Rewrite this sentence to make it structurally reasonable. Add "(see Fig.9)" at the tail.

Line 315-317: Similar to Line 310-312, where the results for "CUSUM and Mann-Kendall sequential tests" are presented?

Line 343-345: "This is similar to the southern neighbouring basin Saryjaz, where glaciers are relatively larger with higher elevation range, and retreated $3.7 \pm 2.7\%$ during 1990-2010 ($0.19\% \text{ a}^{-1}$) (Osmonov et al., 2013; Table 1A)".

Line 346-348: "The area of all other glaciers except the three largest glaciers has declined by $20.8 \pm 7.5\%$ ($0.99 \pm 0.36\% \text{ a}^{-1}$) during 1992-2013, and comparable to the results of Severskiy et al. (2016) for other regions of northern Tien Shan that mostly featured by smaller glaciers".

Line 349: Provide the value of "the mean retreat rate" in brackets.

Line 353-355: Add uncertainty to two area values in this sentence. See comments on Line 215-224, it's unreasonable that the clean-ice glacier has similar area change uncertainty with debris-covered glacier.

Line 362: Replace "in excess of" with "exceed" or "greater than", simply ">".

Line 363: I believe that the “uncertainty of measurements” will be much larger if using more rational uncertainty assessment methods.

Line 365-367: Double check this conclusion. The cirque glaciers have lost much higher proportions of their area than any type of valley glaciers, whatever in yours and Kutuzov and Shahgedanova (2009)!

Line 370-372: “These glaciers have a larger average size (... km²).”. Remove the words after “whose” because the comparison of the change rate between two different periods are meaningless.

Line 375: Use “a deceleration” rather than “no acceleration” here. It is very clear that the annual recession speed has decreased more than 1/5. Remove the words after “and that the temporal changes ...” because it’s very clear in Table 5 and this part makes no sense here.

Line 376-378: Replace the “low temporal variability in the” with “decelerated”. Similar trend can be found in Severskiy et al. (2016), which the decelerations in annual rate after 2000 can be read in most regions.

Line 383-384: The part after “however” has less sense. The deduction of low measuring uncertainty according to similar change rate at different period is fairly speculative thus problematic. Actually, there was only very poor comparability between them. Besides, blank space was missed in “glacierretreat”.

Line 385-387: “The retreat rate for the period of 1956-2013 is 22.0% (0.39% a⁻¹), which very close to the rate of $0.37 \pm 0.22\% \text{ a}^{-1}$ in nearby Chinese sector of Tekes basin during 1960-2009 (Xu et al., 2015).”

Line 391-392: “..... presents a large scale glacier inventory in 1950s-1960s for Central Asian Tien Shan, which was often used”.

Line 402-408: Similar to Line 383-384, the authors should be careful to give such speculative conclusion without any substantive evidence. The “consistent rate” theoretically can nearly prove nothing, neither to indicate the low uncertainty of USSR glacier catalogue, just as mentioned in last sentence. A more substantive and direct validation on the USSR glacier catalogue is strongly suggested, as discussed in General Comments.

Line 410: It is suggested to use GAMDAM only rather than RGI5.0/GAMDAM, also for other texts. The inclusion of GAMDAM in RGI 5.0 only need mentioned once in previous text.

Line 417-418: The reason for includes the steep headwall in glacier area should be clearly specified to provide a reference to other users of GAMDAM glacier inventory.

Line 418-420: It’s ambiguous that what kinds of comparisons in whose studies are problematic. Please clarify it by clearly specify the problematic comparisons in corresponding studies.

Line 424-426: “... and 93 were absolutely not included. The area of these six glaciers was $47.4 \pm 1.9 \text{ km}^2$ in 2013 according to our measurements, 12.7 km^2 larger than GAMDAM inventory, which account for 12.1% of all glacier area in Tekes Basin in 2013.”

Line 428-429: “, which excluded by GAMDAM inventory”.

Line 434: “... total area of ... not compiled in GAMDAM inventory”.

Line 434-436: “As a whole, the glacier area in GAMDAM inventory in Tekes basin for 1999-2003 is 25 km^2 (24%) smaller than the area in 2013 of this study.”.

Line 439: Add “e.g.” before “Farinotti”.

Line 441-442: Replace “10 years” with “decade” here, and for all other places. No related contents can be found in Table 1 for “linear trends explaining 25% of the total variance”. Besides, what is the significance level of the trend of 0.18°C per decade should also be mentioned. According to Fig.8c, the increasing trend is fairly insignificant after 1985. More phase-based trend analyses are suggested (also see comments on Fig.8).

Line 442-444: It seems that this sentence want talk about the driving factor of climate change. I suppose this study is focusing on glacier changes and their climate driving factors, rather than the climate change itself. So I suggest to remove this sentence.

Line 447-450: More phase-base analyses are needed for three other seasons. It is very clear in Fig.8 that the increasing trend of other three seasons also have stage differences.

Line 450: Use “decade” rather than “10 a⁻¹”.

Line 453-455: The first sentence of this para is currently ambiguous. Increase in liquid precipitation of higher elevation and in “transitional months in the future”? How to understand it? Please specify it clearly.

Line 461: Breakup this sentence into two at “while”, and replace it with “By contrast,” or “However,”.

Line 462-464: This sentence is conflict with the first sentence “no statistically significant linear trends”. Please revise it to properly describe the insignificant trends.

Line 468-469: This is just one of the fatal shortcoming of this study. Also see General Comments.

Line 469-471: This result is questionable and worthy for more discussions in my opinion. The larger change rate (-1.67%/a) during 1975-1979 revealed in Severskiy et al. (2016) is too larger for glaciers with mean size of greater than 0.8 km², and actually it also need more conformation. Or give the value of the change rate as a source of visual comparison for readers if you insist.

Line 473-476: It need to be careful to draw such conclusion. The increasing trend of temperature after 2005 is fairly unclear for nearly all seasons according to Fig.8 (a panel for annaul change trend is needed as commented on Figure 8). There’s also no data to support the acceleration wastage of glaciers after 2010s.

Line 485-488: It seems that you the authors have clear recognition on the largest shortcoming of this study. Please revise you study following this recognition, i.e. promote the time step of glacier inventories.

Line 488-491: From my view, the analyses on climate change are currently very shallow. The values of the precious meteological data have not been completely digged out. Some in-depth analyses are still required, e.g. change phases.

Comments on References (In selective way):

General comments:

- 1) Hanging indents are needed for each literature;
- 2) DOI is needed for every literatures.

Line 498-499: Use abbreviated journal name for “Journal of Climate”.

Line 500-502: Use abbreviated journal name for “Annals of Glaciology”.

Line 503-505: Use abbreviated journal name for “Global and Planetary Change”.

Line 524-526: Use abbreviated journal name for “Journal of Applied Remote Sensing”.

Line 535-537: Use abbreviated journal name for “Nature Geoscience”.

Line 659-642: Wang is the family name, and Puyu is the given name, also for other authors.

Line 689-694: The two literatures of Wang et al. (2014) should be differentiated by 2014a and 2014b.

Comments on Tables:

Table 1: The contents of this table are largely repeated with Figure 8. It is suggested to label all the mean values, trends, and R² on Figure 8, and then remove this table.

Table 2: The information of KH-9 scene, e.g. the scene ID, is also need to be provided in the last row.

Table 3: An additional row of “Total” should better to be added in this table.

Table 5: The data in two sub-table need to be rearranged and better to be placed in one bigger multi-column table.

Table A1: The surface area reduction rates should better to be presented in annual. Besides, the results of current studies on the study area of this paper should be clearly presented for comparison.

Comments on Figures:

Figure 1: It's more intuitive to replace 1, 2, 3, and 4 with properly placed mountain name, and also for simplify the legend. Besides, the altitudinal legend seems not necessary because it fuzzily coupled with hill-shade effects. The scale of the map should be enlarged and the glacier IDs (or at least the IDs of glaciers mentioned in the text), and the glacier names as well, after minimize the legend. The country names should better to be marked on the main map.

Figure 2: More details are needed in this figure, e.g. the variation range or standard deviation of temperature and precipitation of every month as error bar. The data type or source should also be clearly marked as legends, e.g. "Mean precipitation (1952-2015)", "Mean temperature (1947-2015)".

Figure 4: The logarithmic units for horizontal axis should be clearly noted in the figure or axis caption.

Figure 5: Error bars are needed for each glacier types, both for absolute and relative changes

Figure 6: Two additional panels for glacier area and number distributions in different aspects are suggested to clearly illustrate the reason for different levels of glacier change.

Figure 7: See comments on Figure 10.

Figure 8:

- 1) A panel for annual temperature is needed, just similar to Figure 9.
- 2) The lines for linear trend of all phases should be added, with the regression formula and R^2 .
- 3) It is obvious that the temperature for almost all seasons (except spring) kept relatively stable after 1975, which certainly will have influences on glacier change. Some phase-based trend analyses are thus strongly suggested to reveal more details about the temperature change. The results should also be marked on the figure.

Figure 9:

- 1) This figure can be merged into Figure 8 to compose a larger figure with two columns (left column for temperature while right for precipitation). This is better for the comparisons between the variations of temperature and precipitation.
- 2) Similar to Figure 8, the results of linear trend analyses should be marked on the figure.
- 3) More phase-based trend analyses are also suggested to give more details of the precipitation change.

Figure 10: This figure can be merged into Figure 7 by add the GAMDAM glacier outline into it. The yellow color for glacier outline is also unclear and should better to be changed.