Review of the study by Usmanova et al.

General comments

The study by Usmanova et al. is presenting an analysis of glacier area changes in the Tekes Basin (Tien Shan) for 118 glaciers from 1992-2013 and a smaller subset of 28 glaciers from 1956 to 2013. They use Landsat data from 1992 and 2013, KH-9 imagery from 1976, the Russian glacier inventory from 1956 and outlines from the RGI for comparison. A detailed analysis of climate station data for the 1947/1952 to 2015 periods is presented as well. Particular emphasis is given to results from previous studies in nearby mountain ranges (that are compiled in an additional Table) and accuracy analysis. The study is well written (but I am not a native speaker), logically structured and some glacier outline overlays are provided as well. This gives overall a good impression. On the other hand, I also have a few larger objections that question the overall value of the study. The most important are:

(1) The scene from 1992 is in my opinion not suitable for glacier mapping, even if only glacier tongues are digitized manually. Most glaciers are completely snow covered and seasonal snow is hiding many glacier perimeters (see Fig. 1 in my Appendix). Using this scene would result in an overestimation of glacier area for many glaciers. In this regard I have a problem in matching the subset shown in Fig. 3b (top row) for 1992: There must be a massive snowfield in the lower right corner (where the '1992' is printed) but there is nothing (see Fig. 1 in my Appendix). Which leads to the question: Has this scene actually been used for the mapping or the one from 1989? Which brings me to my next point: Why has the poor scene from 1992 be used rather than the one from 22.8.1989? This scene is still not perfect but massively better than the one from 1992 (see my Appendix Figs. 1 and 2). Actually, the top row figure 3b (please use (a) to (f) next time) looks very much like the one from 1989. So is this now only a confusion of the figure by the authors (as they do not want to show the poor snow conditions in 1992) or has the 1989 scene actually been used (which might require to change all numbers)? Please note, for this point alone I would recommend rejection of the ms so that the authors have the possibility to clarify everything and perform the work properly.

(2) Furthermore, the glacier sample analysed here is really small (today's studies presenting change assessment typically cover some 1000+ glaciers) and biased towards a specific aspect sector. Whereas the latter restriction might be useful for a hydrologic analysis (or to reduce workload), such a restriction is close to senseless for area changes (and comparison to other regions), as these should always include the entire mountain range (considering all aspect sectors) to be comparable (e.g. glaciers exposed to the north might be larger and have thus a smaller area loss, see Fig. 6a/b). As the comparison to other studies is a central point of this study (Appendix A), the small and biased sample analysed here is not really suitable for this purpose. Hence, also this point puts a big question mark behind the usefulness of the study. I am aware that doing this analysis for the entire mountain range depicted on scene 147-031 (are there any further glaciers on 147-030?) would be a large amount of extra work, but otherwise the entire comparison part (which I like very much) would break down. Maybe results from colleagues can be included for a broader picture and a more useful analysis?

(3) Compared to my points (1) and (2), it is likely more easy to remove the detailed climatological analysis. Whereas this analysis is fine in itself, it has nothing to do with the here observed area changes, as glacier response times are not considered. I am aware that response times have also been neglected in other studies on area and length changes, but this does not mean it is a valid concept. And even for the 1D case (length change) the relation with climate

is complex and requires a (simple) glacier flow model to bring them together. For area changes the situation is even worse as the unknown ice thickness distribution plays a critical role for the observed changes (e.g. a glacier can be very flat or thick near the terminus). So please either skip the detailed climate analysis or look at time series of glacier mass balance (where the details would make sense). There is no problem in stating that the observed glacier shrinkage might be related to increasing temperatures (as in L439/440), but that's it.

(4) Finally, the accuracy analysis is strange and leads to very high uncertainties. It seems the authors have subtracted resulting glacier maps from each other to determine area change (what should never be done) so that geolocation plays a critical role in their assessment? The correct way of doing this would be to subtract the resulting (scalar) area values of each glacier polygon so that geolocation issues do not play a role. In consequence, all uncertainties given have to be revised and maybe the conclusions will then also be different. I also miss results of a multiple digitizing experiment to determine the real uncertainty of the derived outlines. This is really important when all outlines are based on manual digitizing. I give further details to this point in the specific comments below.

There are two smaller general issues requiring consideration:
(a) The area loss rate in km^2/a has not really any meaning as it strongly depends on the total area covered and is thus incomparable among different regions and even within this region. I suggest removing these values throughout the entire ms and provide only relative area change rates (and the total glacier area at each point in time).
(b) The terminology should be more precise to avoid confusion. In my opinion the wording 'retreat'/'recession' (and advance) should be used when length changes are analysed (1D case). Change in glacier area (2D case) could be named area loss/gain or shrinkage/growth (for mass loss/gain: thinning/thickening). A consistent application of this terminology would also facilitate understanding of the text (e.g. in L47: why should runoff change when a terminus is retreating?). On P19 mass balance is frequently confused with area changes.

Overall and considering also my specific comments I would recommend rejection of the ms at this stage. This is basically to give the authors sufficient time to consider the comments, maybe redo larger parts of the calculations and write it up properly. If the changes obtained in this study were sensible and comparable with the others (presented in the Appendix), the study would be a welcome addition to the knowledge of glacier changes in the region.

Specific comments

L34-36: Though it is likely true that increasing temperatures are the reason for the shrinking glaciers, there is no way to relate a specific climatic forcing to glacier behaviour without considering response times (that might strongly vary from glacier to glacier and require consideration of the mass balance history). The details on temperature and precipitation (T/P) trends given in L30-34 suggest that details of glacier area change can be related to this variability. I suggest mention here only the more generalized conclusion about glacier shrinkage and increasing temperatures.

L74ff: There might be good reasons for selecting only this basin. But this study has a focus on comparing area changes across regions (Table 1 in the Appendix). For this purpose the selection of a drainage basin with an aspect bias gives incomparable and thus misleading results. For example, area changes can be smaller because glaciers exposed to the north are generally larger and smaller ones are better radiation protected. I thus recommend extending the analysis to the entire mountain range until all aspect sectors are included.

L90: Chinese sector: As before: I would remove (or mark) all studies from the comparison that potentially have aspect biases with likely impacts on the reported change rates. Otherwise a comparison makes little sense. If the data are available, one can also compare the changes per aspect sector and size class. At least it should be indicated in the Table, which studies suffer from such aspect biases due to drainage dive or country borders.

L119: Minimum elevation is not a good descriptor for the climatic regime. Can you give instead mid-point, mean or median elevation? These are closer to ELA and have thus a climatic meaning.

L119-123: These details about the glacier types are interesting but the differences are not fully clear to me (e.g. compound-valley, valley, cirque alley). If this differentiation is required for the later interpretation, can it be illustrated somehow (graphical/photos)?

L123: Are there any mass balance glaciers nearby or geodetic measurements that could be added here?

L134: What is the 'glacier tongue elevation'? Can this also be related to mean elevation (as a proxy for ELA)? This would be relevant for a climatic characterization of the glaciers.

L146: Nothing from Corona acquired in the 1960s here?

L155: 'at the end of the ablation period' does not mean that snow conditions are sufficiently good to map glaciers. This is only possible in years with a very negative mass balance and at best no seasonal snow left outside of glaciers. Otherwise the impact of the snow conditions on the mapping accuracy should be described as well. I think the OLI scene from 2013 can be used, but conditions are on the edge of being acceptable.

L157: 'suitable for glacier mapping': I disagree that this scene can be used. It only works for the lower parts of the largest tongues; all the smaller ones are snow covered. If this scene has really been used (Fig. 3b suggests something different, see above), please perform the analysis again with the scene from 22.8.1989 (see Fig. 2 in my Appendix). This scene is also not perfect, but much better than the one used here.

L160/1: Relative error also strongly increases towards smaller glaciers when manual digitizing is applied (just test it!), so this is not a good argument. The reason for using automated mapping as a first step is to get at least for the clean ice reproducible results. This applies in particular for the region presented here, where most of the glaciers are debris free and only very few (larger ones) have to be manually corrected due to debris. When all glaciers in scene 147-031 would have been mapped, the story is a different one. But also here the automated classification would provide a robust and reproducible first estimate of glacier coverage. Moreover, with this approach the (optically thick) percentage of debris cover can be determined for each glacier afterwards by grid subtraction. This information would be valuable for modellers (e.g. mass balance, future glacier development).

L166: Yes, and then you cannot use GAMDAM because it applies a different glacier definition. This is exactly the reason why automated mapping should always be applied as a first step (if a SWIR band is available). How does manual digitizing help when half of the glaciers are missing? Please note: as far as I know the GAMDAM inventory will be repeated using a more common definition of glaciers.

L169: Checking debris cover with HR images in Google Earth is fine, but where the available scenes suitable (e.g. regarding snow conditions)? Please describe this shortly.

L176: This is also a reason why automated clean ice mapping is helpful: rock outcrops are recognized and automatically excluded from the glacier map, independent of their size.

L188: Have both Landsat images been used to collect GCPs? As far as I know the higher resolution pan-chromatic sensor on ETM+ or OLI is preferably used for this. Please clarify.

L201ff: The Granshaw and Fountain (2006) study uses the buffer method to consider the thickness of a glacier outline on a map and the positional uncertainty of a map from 1958.

Both types of uncertainty do not really apply to the satellite images used here that are likely orthorectified with the same DEM and the same set of GCPs. But the key question is, why should geolocation uncertainty impact on glacier area? When glacier areas are determined independently for each glacier and the scalar values are subtracted to determine the change, where is the issue with geolocation? Unfortunately, it is not reported how area differences are determined here (please add!), but the description in L210-214 indicates that glacier grids have been subtracted after co-registration? If this is the case, please never do it this way as it blurs the quality of the results with the geolocation uncertainty.

When manual digitizing is performed, the - in my opinion - only useful way to determine uncertainty of the obtained glacier areas is from a multiple digitizing experiment (10-20 glaciers 3 to 5 times). This is a value specific to the analyst (and thus the study) that cannot be "taken" (L206) from another study or neglect the much higher uncertainty when it comes to the delineation of debris cover glaciers as mentioned in L216. The result of this exercise will be a size-dependent uncertainty envelop as shown in Fig. 3 of Granshaw and Fountain (2006). At best, size-class specific mean values of this uncertainty are used to obtain the correct value for the entire sample. Please also discuss the impacts of wrongly mapped glaciers due to seasonal snow hiding the glacier perimeter, in case it matters.

L231/2: As mentioned before, co-registration uncertainty should not play a role when area values are determined independently. What really counts is the uncertainty of the glacier margin delineation. And this should also be determined for some (isolated) glaciers on the Hexagon scenes. It would be interesting to see if these are lower than for Landsat due to the higher spatial resolution.

L236ff: As mentioned in the general comments, I think the detailed analysis of meteorological data is not required as a clear link is only given to mass balance measurements that are not presented here.

L256/7: The uncertainty given for the three numbers is now 7.5, 5.2 and 36%. Where do these numbers come from? I cannot link them to what is described in section 3.2.1. In particular the 36% uncertainty for the area change is strange. Why is it so large?

L274: I would not consider Simonov as having 'extensive debris cover'. There is some widening of the medial moraine near the terminus, but apart from this the ice is clean. The eastern tributary of Simonov, which is currently building the terminus, also seems to be very steep, with likely short response times. I doubt that its retreat is 'slowed down by the debris'. The same might apply to Bayankol, despite its more extensive debris cover.

L275/6: Terminology: by using here 'recession rates' and 'wastage' I would assume that length changes are compared to mass changes. Please use area loss rates when area loss rates are compared. In this regard, why can glacier changes in this region be compared to the one studied by Osmanov et al. (2013)?

L286: And due to their small elevation range they are likely much more sensitive to climate change (i.e. a small upward shift of the ELA).

L302: This is an interesting analysis of climatic variability, but the link to the observed area changes cannot be made. When precipitation variability is analysed over the same period as glacier change (L328/9), please look at mass balance time series (response times!).

L352ff: I think this comparison is free of scientific reasoning and not useful to demonstrate cause and effect. How should this work with two arbitrarily selected glaciers without considering flow velocities, surface ablation, ice thickness distribution and response times?

L357: slow retreat: does this mean here length, area or mass change? Are the glaciers in this region really comparable? They should at least be in the same size class.

L358: As long as area change rates are also a function of glacier size (e.g. Fig. 4 in this study) and debris-covered glaciers tend to be larger and longer, different retreat rates must not necessarily be a result of the debris cover, but can also be due to their larger size.

L370-376: This is basically a repetition of results. The key conclusion about the acceleration trend could also be provided in the results section. So I suggest deleting it here.

L378/9: When it is only a 'slight acceleration' I would not state that this is 'in contrast' to this study. The differences might be minor (or within the uncertainty).

L403/4: This is certainly a glaciological far stretch as it neglects everything we know about glacier response to climate change (see discussion above, I will not repeat it here).

L415: I think the point in excluding these areas was not that they are without permanent ice cover, but that these regions do not really contribute to mass loss and run-off. And this was the purpose for the inventory. Maybe better compare with the new Chinese Glacier Inventory (CGI2) as this would be more interesting.

L419/20: This likely applies to all inventories compiled elsewhere. This is also the reason why a new version of GAMDAM is currently in preparation (incl. accumulation areas).

L421ff: This is more a presentation of results rather than a discussion. But I would replace it anyway with a comparison to the CGI2. The differences to GAMDAM are well known in the mean time and are currently corrected (so the information is out dated soon).

L439-442: I think this generalized statement is fine, but I would not go beyond it.

L442-452: This is interesting in itself, but not required for the area changes presented here.

L453-457: This sounds very speculative. Are there any measurements to prove it?

L468/9: This is certainly true - when mass balance is analysed! When length changes are analysed, the time step should at least match the response time, for the area changes analysed here, do not expect to see anything, as non-climatic controls (ice thickness) take over.

L470: 'glacier recession rates': please never mix it up again: they look at mass balance!

L479/484: I expect the accuracy estimates and the conclusions to change with a correct accuracy assessment (and the values itself when the scene from 1989 is used).

L475: This might be true but it is not relevant here (as 'wastage' means mass balance).

L487: Yes, in case 'glacier change' means mass balance!

Smaller issues

L27/8: in the glacier recession => in glacier area loss

L28-30: I think this sentence is not required in the abstract.

L31: per 10 years => per decade

L39: glaciation => glacierization

L44/49/51: recession/retreat => area loss or shrinkage

L46: 'potentially ... lakes' => remove (changes in extent might not impact on run-off). Is formation of lakes an 'impact of glacier retreat'? I would argue that it is a consequence.

L135: explain JJA

L149: when cited here (even in brackets) it should be Fig. 3 rather than 9

L171: just write: "The ASTER GDEMv2 (http://...) ..."

L176: deduced: subtracted?

L187: Why OLI TIRS? Has the thermal infrared sensor been used as well? The optical land imager is just named OLI (without TIRS)

L261-264: Absolute area losses are not really comparable. I suggest deleting.

L337: retreat rate: I assume this is the area change rate rather than length change rate?

L342: glaciated should be glacierized for contemporary glaciers.

L352: retreat rates: this is like the area loss rate?

L357: (2105) is likely (2015)

L358: retreat rates: length or area changes?
L367: ... as well as many other factors such as special topographic conditions (shadowing, snow avalanching).
L382: retreating: losing area
L413: underestimated: I suggest writing 'lower' as this is more neutral.

References

Just as a remark: To increase access to the references, it is possible to use indentation from the second line on.

Tables

Below I comment on the tables as they are. These comments will mostly also apply for a new/revised version of the ms.

Table 1: I would suggest removing this table. Without analysing mass balance the investigation of seasonal trends in T/P is not relevant for this study.

Table 2: The Landsat 8 sensor used here is OLI, please remove TIRS. Please clarify in the main text why row 30 is also required. It is unclear from Fig. 1 which of the glaciers are not covered by 147-31. Please also give the image ID of the Hexagon scene used (DZB1211-500142L008001?).

Table 3: When the heading row gives area changes, all values miss a minus sign. It is unclear what 'Average area change' means. This has to be described in the caption. Is it the combined value divided by the number of glaciers? This works for the km^2 column, but what is here in the % column? I would also provide annual change rates, i.e. column 6 divided by 21 (or 24 when the better scene from 1989 is used). And please add a row with totals.

Table 4: There should be two further columns: per cent of the total area covered) between column 4 and 5) and the per cent area reduction per year (at the end). Here the values do not need a minus sign as 'Area reduction' is already including this. However, I suggest being consistent and using 'Area change' on top and add minus signs to all change values.

Table 5: a) Please use a dot instead of a comma (86.3) for all values and consider to just listing them in the main text or in the 5b) table. Overall, I think there is no need for the 5a) table. For b) I suggest removing the 'per year' line and add the value for the change rate as an extra column only for the per cent column. And please use area change on top and add minus signs.

Figures

Below I comment on the figures as they are. This might also be helpful for any future version of the study. In case the authors considerably extend the size of the study region, other/additional figures might be required.

Fig. 1:

The image shows major hydrological basins with glaciers squeezed in the lower right corner. As this study has a focus on glacier changes rather than hydrology, I suggest showing a close-up of the glaciers instead. Please also indicate the location of subsets on this map and show which glaciers are covered by the Hexagon scene (footprint). Annotations of specific glaciers could be indicated as well. Please note: there are many glaciers near the numbers 2, 3 and 4. When they are not shown without a note in the figure caption, the map is highly misleading. I suggest showing all glaciers in the region and highlight those being investigated (whatever the study region will be). The two long black lines are wrong by location and are not required. Please indicate in the figure caption what the subset and the black square on the subset should indicate. Figure caption: Please write a complete sentence (and give full details).

- Can the monochrome colour table of the background map be replaced with something more atlas like (starting with light green)? And Altitude in the legend should be elevation (and asl. should be a.s.l., but together with elevation a.s.l. is not required).
- The map looks like if there is a hill shade in the background, which question if the colours are only related to elevation. Please clarify and adjust the colours accordingly
- Where are the boundaries between the basins 1-4 and Tekes?
- I do not see any glaciers on the map that require scene 147-30. Are there any? If yes, please show the scene footprint (or the relevant part of it).

Fig. 2:

I think this figure can remain for the general climatologic characterization of the study region, but please add the elevation of the station, and that month is depicted on the x-axis. Caption: It is not only T/P that is shown: It is "Mean monthly temperature ...". Add annual mean T and annual sum of P in the caption.

Fig. 3:

Please use (a) to (f) when labelling 6 figures. The glacier in the lower row is very small compared to image size, please show a close-up. '(b) figures': please never use red outlines on a red background, white or light green might be better choices. I mentioned already above that the 1992 images cannot be from 1992, there is much more snow (see Fig. 1 in my Appendix).

Fig. 4:

I suggest using a font without serifs (such as Arial or Helvetica) for the labelling of all graphs. Please also add minor tick marks on both axes. Instead of 'Individual glacier area' I would write 'Glacier size in 1992'. The two dots smaller than 0.01 km^2 are likely snow patches rather than glaciers (that flow). I suggest using a lower limit of 0.01 km^2. The caption should read "Scatter plot of glacier size versus relative change in area from 1992-2013".

Fig. 5:

As mentioned above, absolute changes in glacier size have a very limited meaning. I think they can be shown here to illustrate the contrast. However, I think it would be more interesting (or maybe just add it) showing the contribution of each class to the total area change, maybe expressed as a ratio of their percentage to the total glacier area divided by their percentage of contribution to the change. This would indicate which glacier types contribute disproportional to the overall loss. Of course, such a graph would be more interesting when the sample size is larger.

Fig. 6:

Although a figure of the aspect distribution is not shown, this figure clearly illustrates a key problem of the study: The sample is strongly biased towards glaciers with a northerly exposition whereas the strongest area changes come from glaciers exposed to the SE. Mean area changes would likely be different without this bias.

Fig. 7:

Please do not use red outlines on a red background as this is difficult to see; maybe white, black and yellow works. There is limited change in the lower half of the picture. I suggest shifting the close-up to the north to show more of the study region or even go to portrait format and show the changes of the entire study region? Caption: Provide glacier names on Fig. 1. The sensor name is Landsat OLI. I suggest writing: 'The Landsat OLI image from 2013 is shown in the background.'

Fig. 8/9:

Please remove, there is no relation of this variability to the reported area changes.

Fig. 10:

I think we are all aware that the GAMDAM inventory has used a different interpretation of glacier extents. This will soon be changed (also in the RGI) so that the information is out-dated from the beginning. I thus suggest removing the figure (or show them on top of Fig. 7, e.g. as a black-doted line). A close comparison with the CGI2 would be interesting instead.

Appendix Table
I like this overview very much but find the one to ten glacier samples not really comparable to those with a few hundred or even thousands of glaciers. In view of my critique on this study, I suggest either removing or at least marking those studies who have also an aspect bias or the other way round mark those who cover entire mountain ranges. For all studies I would add the area change rate per year in a further column, as only these values are comparable across regions. Maybe a graphical representation of the values listed in the table can be added as an overview (e.g. period vs. relative change rate with colour coded regions and line-style coded sample size (<100 dotted, 100-1000 dashed, >1000 solid))? This would give an easy access to the observed variability of change rates and the investigated time periods.

My Appendix

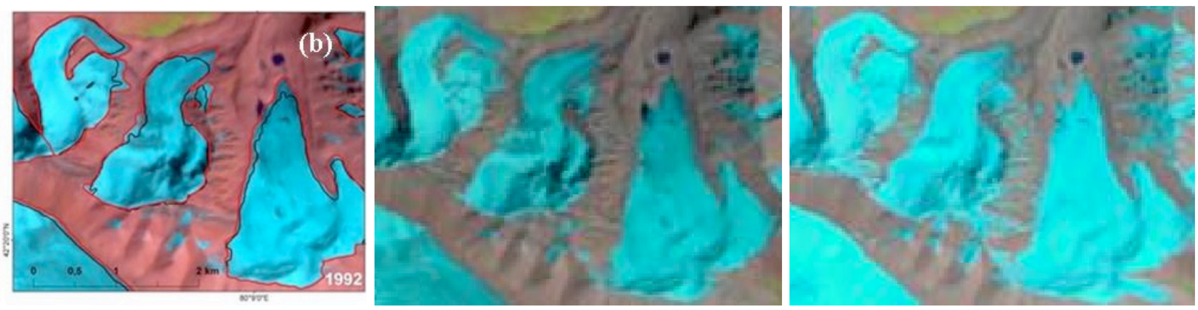

Figure 1: For point (1) of the general comments showing a subset of the study region. Left: The image in Fig. 3b of the study that should be from 1992 but is looking like the one from 1989 (middle). The real snow conditions in 1992 are shown to the right suggesting that the 1992 scene is indeed unsuitable for mapping glacier extents.

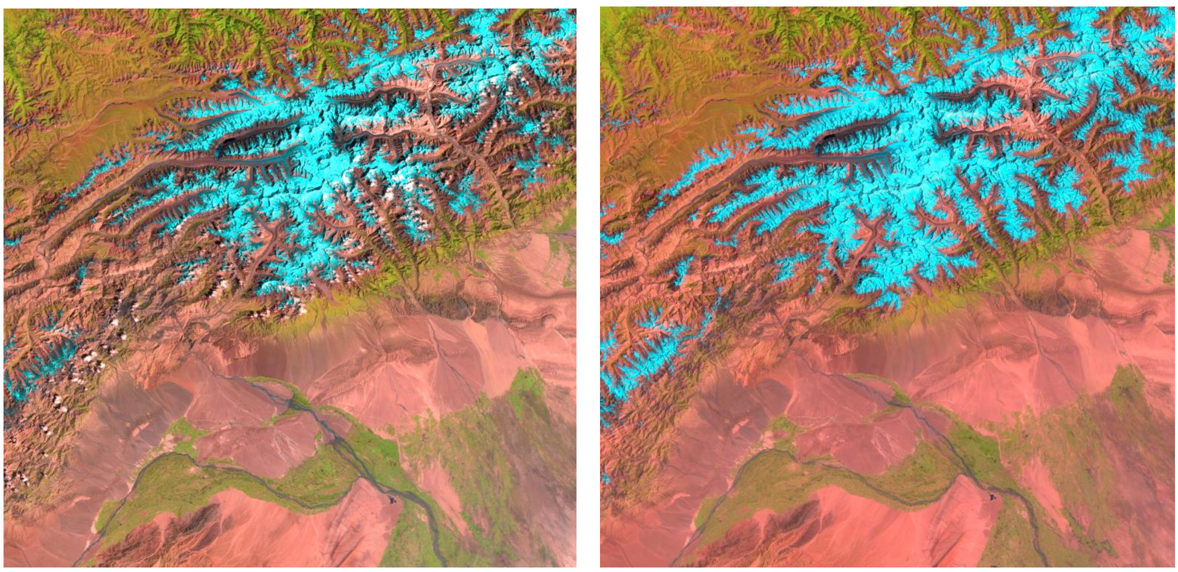
Fig. 2: Comparison of snow conditions: Left the scene from 1989 that should have been used, right the scene from 1992 that (maybe) has been used. The much more extensive snow cover (light blue) in the 1992 scene is easily recognizable.