Reply to editor and reviewer's comments

We appreciate editor and reviewer's comments about our manuscript.

(Original comments are in italic, our replies are in bold)

Editor, Tobias Bolch

The manuscript has significantly improved and I am in line with the reviewers who still see some minor issues but finally recommend publication.
Please consider the reviewer's comments carefully in your revised version.
In addition to the reviewers comments, I ask you the following:

- Please mention in the abstract the total area of the RGI including the uncertainty.
We added total area and error for entire glaciers in Asian mountains (L31-32).

- Figures: Please avoid long captions and please include a legend so that the figure is understandable without reading the captions (e.g. "Green lines denote glacier outlines" can and should be easily substituted by a legend)
We shortened them by adding legend or partly moving caption into main text (Figs. 1-3, 5-8, 10-11, 16).

- Fig. 16, L 718: Should be Bolch et al. (2012)
We revised it (Fig. 16).

- Table 1-3: Please include the uncertainties where available (e.g. the RGI, see Pfeffer et al. 2014).
We calculated the uncertainty of RGI 4.0 using error estimation equation (eq. 1) in Pfeffer et al. (2014) (Tables 1-3).

The revision should contain a response to each of the comments from the reviewers and explain how the comment was handled. If you disagree with a comment, please explain why. If the text was modified as a response to a comment, please indicate how the text was modified, including the new text and its position in the manuscript (line numbers).

Reviewer#1, Graham Cogley

General Comments

This paper is a revised version of an initial submission on which I commented earlier. The revision appears to have covered most or all of the points raised in the first round of review.

The paper is well written, and is commendably short. There remain some problems to do with clarity of style and some minor substantive points to be clarified, but these criticisms, detailed below, are not fundamental. I think that this important new inventory should be documented in the literature, and I would be happy to see it published in The Cryosphere.

Substantive Comments

P2

L31 The authors must have worked extensively on revision of their original glacier outlines, with the result that the difference (GGI–RGI)/GGI is now –24% rather than –31%. Thus a significant difference remains. There is a lesser difference of –7%, perhaps not statistically significant, with respect to the ICIMOD inventory of the Himalaya and Hindu Kush. I think that the discussion of reasons for these differences in the body of the paper is a fair summary of the "state of the art" in large-scale glacier inventory research. It should stimulate further work on the problems of clarity of definitions and accurate recognition of glaciers in remote-sensing imagery.

We appreciate your kindly comments.

We agree with your comments. Not only making glacier inventory, but also utilization of inventory is important for glaciology. Our inventory's recognition depend on our future works.

P3

L61-62 These fractions are out of date. Assuming that the authors have not made their own special calculation for High Mountain Asia, the correct percentages for undated glaciers in RGI version 4.0 are 6% by area and 12% by number. (See Figure 2 of Arendt et al. 2014.)

We revised the phrase as "Furthermore, small portion of glaciers used in the RGI is undated (Pfeffer et al., 2014)." considering decrease of undated data in the RGI (L62-63).

P4

L66 "since 2011" could be deleted. The GGI is based on imagery from 1999–2003 (L85).

This sentence means that we started the project in 2011. We revised this sentence for avoiding misunderstanding (L64).

P6

L122 "(excluding the Greenland and Antarctic ice sheets)". The RGI includes peripheral glaciers in Greenland and on the Antarctic Islands.

We have made mistakes here. We would like to indicate ice sheets, not glaciers.

We revised ' (excluding the ice sheets in Greenland and in Antarctica)' (L128).

P7

L137 If the three false-colour bands were weighted unequally, give the weights.

The composite color bands weight had been automatically adjusted based on each image contrast by GIS software. Therefore, we have not applied unified weight coefficient to all images. We added explanation into the sentence (L143-145).

P8

L180-181 Clarify. I would delete "probably", first checking one or two representative tiles of the SRTM without void-filling if you have not done so already. Then after "numerous voids" you could add something like "due to shadowing and layover".

We checked numerous voids in high relief mountainous region. And Frey et al. (2012) mentioned it in their paper. We removed "probably" from the sentence.

P11

L247-249 Clarify. I do not understand how changing the image "corrects" the first polygon. Presumably it is the digitized outline that needs to be changed.

If source image used by first operator is ambiguous (e.g. casting shadow), second operator checked the glacier polygon using another satellite image. We revised the phrase more clearly (L250).

P12

L263 "mid": this should be expanded to "mid-range elevation", if that is what it is.

We revised it (L268).

P13

L298-299 First, these are differences rather than errors. Second, consider expressing the rms area difference as a percentage (of the average area).

We revised it (L304-305, Fig. 14).

P14

L307-309 Perhaps remark that these differences may be due to the exclusion of headwalls in the GGI. Clarify the discrepancy in the central Himalaya; presumably its sign is the same as that for the northern Hindu Kush and Karakoram, but the reader cannot tell.

The reason of those discrepancy of median elevation of glaciers in the northern Hindu Kush and northern Karakoram would be our miss-delineation of glaciers at the upper shadow part. Please check detail of required revision of GGI were listed in the Table S5. While, the discrepancy in the central Himalaya would be due to exclusion of headwalls in GGI. (L315-318)

Please notice that we have made mistaken the region name in Table S5. We have revised from 'Eastern Pamir' to 'northern Hindu Kush and northern Karakoram' (Table S5).

P19

L438-440 This sentence is garbled. Say something like "a repeated inventory using Landsat 8 imagery will …". But also consider deleting the sentence, which adds no value to the text. What is more, if the sentence is kept it would have to address the matter of accuracy. If the uncertainty in GGI areas is of the order of 15% (L348), then given typical shrinkage rates of –0.5 % a^{-1} (say) a reliable estimate of change may be difficult in the "near" future.

We added because of M. Pelto's comment. But, now we agree with your comment. We deleted this sentence.

P27

Figure 2a,b Why were the smaller red (RGI) objects excluded from the GGI? Was it because they are smaller than 0.5 km^2?

They are smaller than 0.05 km^2, which is the minimum size of our inventory.

At L583, add a comma after "imagery" (or delete the one after "2001").

We revised all of similar phrase in caption of figures (L105-106, Figs. 2-3, 5).

P33

Figure 7d I do not understand how non-glacial lakes are of any use for identifying debris-covered glacier surfaces.

We revised the phrase as "used to differentiate debris-covered glacier surface or not" (Fig. 7).

P40

L702 This sentence does not make sense. Presumably what is meant is "The dashed lines indicate 1:1 correspondence between ICIMOD and GGI. The solid lines are the best-fitting linear equations."

We revised it (Fig. 14).

Stylistic Comments

P3

L54-55 I would say "while uncertainty in glacier outlines influences estimates of mass changes …".

We revised it (L52-53).

L56 "To support" would be better than"To contribute to".

We revised it (L54).

P4

L68-69 "to evaluating", and delete the unnecessary "(imbalance of glaciers)".

We revised it (L68).

P6

L117-120 I think this repetitive sentence (see L56-57) could be deleted.

We deleted the repetitive sentence.

L127 Do not capitalize "basins".

We revised it (L134).

L128 "… and the Qinghai-Tibetan plateau" (not one basin, but many).

We revised it (L134).

P7

L149 Add a "±" sign before "1–2 grid cells".

We revised it (L156).

L151 For clarity, I would say "often failed" or "sometimes failed". Otherwise the reader might understand this as "always failed".
We revised it as "often failed" (L158).

P8
L169-171 Delete the first sentence, which adds little or no value, and begin "We compared glacier outlines delineated from the gap-filled SRTM …".
We deleted the first sentence.
We did not compare glacier outlines but elevation derived from SRTM and GDEM2. Therefore following phrase is retained with small revision from "We then tested" to "We tested".

P9
L193 Add "ground-surface" before "elevation changes" for clarity.
Ground-surface elevation change might cause misunderstanding of elevation changes on exposed ground. Therefore, we revised the phrase as "surface elevation change" (L191).

L196 Perhaps add "and valleysides" after "headwalls".
We revised it (L194).

P10
L212 The best word here is "inflections", rather than "turnoff points". It could also be used to replace "folding points" at L634.
We revised them (L210, Fig. 7).

P13
L288 Change ">" to "<". Confusion between the greater-than and less-than signs occurs more than once in this manuscript; double-check all of them.
I am sorry for the carelessness mistake. We revised here and similar miss using at P15L344 too (from "<2.5" to ">2.5") (L294).

P14
L316 This sentence is constructed awkwardly. Simply say "Glaciers in the RGI are larger …", and mention the size constraint at L312: "we compared glaciers larger than 0.5 km2 in the GGI and RGI …".
We revised them (L325, 321).

L323 Change "1st" to "First".
We revised it (L332).

L326 Do not capitalize "basins".
We revised it (L334).

P15
L344 Change "<" to>".
We revised it (L352).

P16
L355 "close (93% similar) to" is unclear. Say "is only 7% less than".
We revised it. And we also corrected table number in the sentence from Table 2 to 1 (L363-364).

L371 I would add "long-term" before "changes" for clarity.
We revised it (L378).

P17
L388-389 Delete "path row".
We revised it.

P19
L428-429 I do not see why "omitted" is needed. Say "revision of glacier boundaries that are in shadow will".
We revised it.

P20
L460 "König".
We revised it (L479).

L464 "Sigurðsson". This Icelandic character (eth) is Unicode 00F0.
We revised it (L483).

P31

Figure 5 I think that "(c,d)" is needed before "Steep" at L618.

We revised it (Fig. 5).

P33

L634 "inflections", not "folding points".

We revised it (Fig. 7).

P38

L686 "Dashed", not "Dotted".

We revised it (Fig. 12).

L687 "elevations from the First Chinese".

We revised it (Fig. 12).

P39

L695-696 "Only glaciers larger than 0.05 km^2 are included in the calculations for each inventory."

We revised it (Fig. 13).

P43

Figure 17 The debris-covered symbols need to be in a brighter colour. Or perhaps they could just be made solid. Consider replotting the graph with a logarithmic scale for the horizontal axis.

We revised the figure with logarithmic scale of x axis (Fig. 17).

P45

L742 Change "mean" to "means of".

We revised it (Fig. 19).

P46

L748 Delete "in area".

We deleted it.

Supplementary Information

Table S5 I do not understand "pass over". Does it mean "Revise <shadowed parts/some glaciers/seasonally snow-covered areas>"?

We tried to say "failed to delineation". We deleted "pass over" for avoiding confusion. We revised as your comments.

Figure S1 Add information to the caption about what is shown in Figures 15 and 18 of the paper.

I guess this comment is about Figure S2. We added information into the caption (Fig. S2).

Reviewer#2, Frank Paul

1. General comments
In my opinion the ms has greatly improved compared to the first version (e.g. regarding structure, description of relevant details, and illustrations). With the better justification for the different definition of a glacier applied here (excluding steep parts), I can also live with the revised dataset. The authors have described and illustrated what the impacts of their specific glacier definition are compared to other datasets, also indicating that there might not be a universal right or wrong approach. I also appreciated the thoughtful point-to-point reply to all reviewers, indicating that the review effort was justified.

What I miss is the critical reflection about the impacts of the decisions taken or what happens in case of failure. The authors stay rather close to their datasets / methods and report what has been done with a limited consideration of alternatives. Below are some examples of issues that I think deserve additional discussion:

The authors explain their decision to remove steep (and partly also shadowed) glacier parts with the intended purpose of the inventory (determination of mass changes). While I assume this might work, it creates a problem when the dataset is included in global databases like GLIMS or the RGI. I agree with the authors that a related confusion of outlines in the accumulation area can also be found in current studies (see P21/22 of their first reply), but the question remains why adding yet another definition? There might be scientists investigating glaciers in a climate change context (e.g. area and length changes) or as a source of natural hazards. With the steep glacier areas removed this is likely not really possible. Furthermore, when glacier outlines vary with the extent of terrain shadow, our datasets might have a credibility problem. To avoid this, it could have been suggested that the final outlines will come with an attribute describing the intended purpose of the dataset and their limitations.
Here, we should confirm our definition that we did not exclude all steep glacier area. Please check '3.3 Criteria for manual delineation'.
Our definition of glacier area (excluding steep headwalls) is surely different from those of GLIMS and RGI. Then, it is impossible to get information of area shrinkage by area comparison between RGI and GGI. We add intended purpose and the limitation of our GGI in conclusion.

Kääb et al. (2012) (Nature) (supplement) described that they have tried to estimate elevation change of 'clearly be assigned glacier' excluding steep flank, ice patches and ice-cored moraines and rock glacier included in debris-covered glaciers. Those exclusions have no effect on elevation change in Karakoram and HinduKush Jammu Kashmir, but, decline of glacier surface has increased in Himachal Pradesh, Uttarakhand and West Nepal (25%) and East Nepal and Bhutan (50%). We have no information on the value of excluded steep area, but, exclusion of uncertain glacier polygon should have large effect on elevation change of glaciers in the Himalayas.

Another example is the use of contour lines for glacier delineation. Though this might work for many glaciers, not all shadowed and/or debris-covered glaciers are laterally delimited by steep rockwalls where 'clear turnoff points' (L212) can be identified. What has been done in these cases?

We have already described in '3.3 Criteria for manual delineation'.

We have used several images, thermal infrared band, rugged surface topography and exposed ice cliffs on Google Earth in order to identify glacier outlines of debris-covered glaciers.

On the other hand, it could have been mentioned that slope and curvature (as derived from a DEM) have been used in previous studies to determine glacier boundaries. And this could have resulted in a discussion of the benefits when using manual interpretation of elevation contour lines vs slope or curvature (first or second derivative) with a threshold.

We agree with your comment.

The threshold of snow-accumulatable slope (or curvature) is also interesting subject for us in the high relief regions, if we can get several cloud-free and seasonal snow-free images. Unfortunately, it is difficult to get clear and high resolution images in high relief regions (Himalaya and Hengduan Shan) at present.

The outline review process is now much better described, but for me a new question is now why analysts with field experience produce more accurate outlines than the others? Of course it is good to know how a glacier looks like in reality, but transferring this knowledge back to a 30 m pixel remote sensing perspective can also be challenging, in particular when even field-based determination of glacier boundaries fail. To my knowledge, best outlines result when different possibilities are jointly discussed with all analysts and a compromise is used.

The field experienced operators, who involved to revise glacier outline, in our project are T. Nuimura, A. Sakai, and D. Lamsal. We selected the operators because of field experiences to glaciological region in high mountain Asia, glaciological knowledge, publications of glacier study using remote sensing. In the five delineation tests, we evaluated delineation by operators and made feedback to each operator for minimize delineation difference and improve accuracy of delineation. The process after each delineation test corresponds to your best procedure. We added this explanation (L240-242).

The authors describe that they have used manual delineation for glacier mapping and excluded glaciers < 0.05 km^2. This is fine but requires a-priori knowledge about the glacier size, i.e. they have to be digitized first and then removed again? Or has - as described in the first response - the length and width of each glacier been measured beforehand to determine if it is larger or smaller than this threshold? This sounds like enormous work to avoid the use of automated mapping and a size threshold to automatically remove polygons smaller than this.

We have also delineated glaciers slightly smaller (see Table 1) than the threshold (0.05 km^2) by rough estimation based on pixel numbers (0.05 km^2 nearly equals 55 pixels in 30 m resolution satellite imagery). However, the judgement was not performed strictly by measuring accurate length, width or number of pixels. The judgement of smallest glacier would vary depend on operator. Therefore, we standardized the smallest glacier by applying conservative area threshold (0.05 km^2). (L294-295)

In the same sense I wonder about the conclusions (L425-430): The simple band ratio method works very well for a precise classification of ice and snow in shadow as several studies have shown. In contrast, the authors stress that only a field-experienced operator can improve the outlines without even mentioning the alternative possibility. I fully agree that mapping of ice and snow in shadow requires a careful selection of the best threshold, but it has been written already in the 2002 papers that thresholds should be selected in a way that manual editing is reduced to the largest possible extent, i.e. in shadow regions that can hardly be accurately mapped otherwise. I would suggest that the authors are at least testing this in a sub-region where precise classification suffers from shadow.

We revised the sentence about "field-experienced operator ..." (see reply in specific comments) (L246-249, 359-360, 445).

Thank you for your suggestion of using simple band ratio to avoid miss delineation at shadow part. Those method are very useful at Karakoram, Pamir, Tien Shan and Altai, where relatively less roughness and clear (seasonal snow-free, cloud-free) images taken during summer season are available, and there are relatively less debris-covered glacier. We will consider your suggestion for further improvement of GGI in these regions.

But, in the Himalaya, problems (for our criteria of glacier area) are mainly judgement of inclusion (or exclusion) at steep headwalls and shadowed debris-covered glaciers. Then, we selected manual delineation rather than simple band ratio method.

Anyway, we have added following sentence in the end of section 5.2. 'For further revision at shadowed glacier outlines, not only simple band ratio (band 3/band 5), but also additional threshold in band 1 (blue) will help to delineate shadowed debris-free glaciers (Rastner et al. 2012).' (L400-403).

I stop here and provide some further comments in the specific comments section below. I hope the authors can address the points listed above and below to get the study acceptable.

Thank you for your comments to our study. We replied your comments point by point in following specific comments section.

2. Specific comments

L61: I think the new Chinese glacier inventory is available as well in the meantime. To avoid that your study is out-dated too soon, I suggest adding a note that only the old Chinese inventory was available for the RGI at the time of data compilation for GAMDAM.

We added a note about new Chinese glacier inventory (L59-61).

L62: I suggest removing this statement as this is constantly improving. Otherwise it should be mentioned that this was the status at the time of the GAMDAM compilation and that efforts are taken to constantly improve it.

We revised the phrase as "Furthermore, small portion of glaciers used in the RGI is undated (Pfeffer et al., 2014)." considering decrease of undated data in the RGI (L62).

L122: I suggest writing "(excluding the ice sheets Greenland and Antarctica)."

We revised it (L128).

L132: I would add here that also the ICIMOD Inventory is excluding very steep glaciers. Another possibility would be using the Frey et al. (2012) inventory for the western Himalaya (see link in L356 comment below).

As we showed in Fig.5, ICIMOD inventory includes steep headwall. We retain here as is.

L152: This argumentation is not really a working, as clouds and debris have to be corrected in both cases. If there is really an advantage of complete manual digitizing vs using automated classification of clean ice as a starting point for the further editing, the workload for removing the seasonal snow polygons (that are included with the automated method) should be higher than the digitizing of all clean glaciers. Is this really the case?

We removed the sentence.

L163-166: I do not understand this: On the one hand basin polygons have not been derived automatically, ok. But in the next sentence basin polygons have been used to identify glacier divides. So there must be basin polygons. Where do they come from?

It is our mistake. We tried to explain as "Contour lines were referred to identify glacier divides". We revised it (L166).

L193/4: '… do not occur': I would not say it that exclusive. This might be well the case for most of the glaciers, but there are also hundreds if not thousands with thick and changing ice masses on steep headwalls (e.g. hanging glaciers). They do not only provide snow avalanches but they also contribute ice (either by avalanching or by flow).
We weakened the assertion (L192).

L212 and 222: 'exhibit clear turnoff points': Again, this might be true for most of the glaciers but certainly not for all. How have these been delineated?
As you noted. It is not applicable to 100% of debris-covered glacier. However, the inflection of contour lines are generally shown during inspection in our project. We revised our assertive phrase to more conservative (L210).

L256: Just as a note of caution: It might be the case that the glacier parts that are finally considered in this study might no longer be seen as real glaciers.
Thank you for caution. Your assertion is understandable. The "real glacier" changes depend on definition of each researcher.

L266: I am somewhat sceptical here. Validation might be difficult if others do not apply the same definition of a glacier. So whenever real changes of glacier extent should be followed with the outlines provided here, the same group of persons must provide the new outlines. All others would only derive virtual changes due to differences in interpretation. So I suggest writing this differently to be clear.
Here, we added a limitation that 'if others follow our definition of glacier area excluding steep headwalls (unable to accumulate snow).' (L271-273).

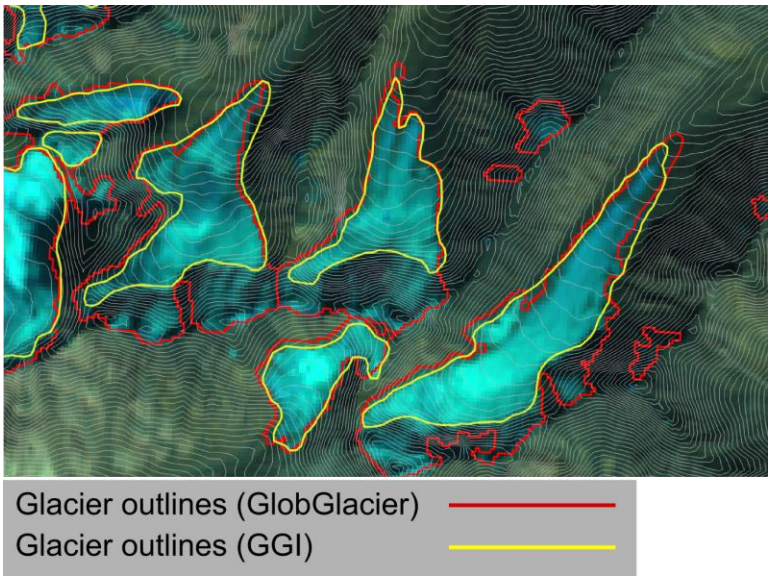
L277: ... and the missing parts of the accumulation region will also play a role.
We agree with your comment that missing parts of the accumulation region will affect on the median elevations of glaciers. But, here we would like to explain why we calculate 'area-weighted average' of median elevations, (not simple average of median elevations), and small glaciers have large variation of median elevation affected by drift snow. Therefore, we added ' resulting in an underestimation of total ice volume and median elevations in the GGI. ' in the section 5.2 (L395).

L288: I assume excluding glaciers <0.05 km^2 is meant here?
I am sorry, it is our mistake. We revised it (L294).

L294: '… with sizes between 16 …'
We revised it (L300).

L351: Field experience might not be very helpful when it comes to mapping of ice in shadow, as the related regions might be invisible from the ground.
The field experienced operators, who involved to revise glacier outline, in our project are T. Nuimura, A. Sakai, and D. Lamsal. We selected the operators because of field experiences to glaciological region in high mountain Asia, glaciological knowledge, publications of glacier study using remote sensing. We revised the phrase "field experienced operator" more accurately (L246-249, 359-360, 445).

L356: The comparison with the ICIMOD inventory might give a good agreement as they have excluded steep accumulation areas as well. Hence, the 'despite' does not really apply. Is there any chance of performing a comparison with an inventory that does not exclude steep parts a priory? Maybe the one we have created for GlobGlacier can be used instead (http://www.globglacier.ch/outlines/himalaya)?
We agree with a part of your comment. ICIMOD glacier inventory do not include steep slope at some part of Karakoram but include in the part of Himalaya and the Hengduan Shan. The quality also different from region to region. Anyway, we will delete L356-357 'despite the exclusion of steep headwalls from the GGI'.
We also performed area comparison between GGI and GlobGlacier inventory (Frey et al., 2012) over the region same with GlobGlacier coverage. GGI and GlobGlacier have area of 8007 and 9270 km^2 respectively. Area difference of them are 1263 km^2 and 15%.
The area difference (GlobGlacier is 15% larger than GGI) is consistent with the glacier definition of GlobGlacier which includes upper steep headwall area same with RGI (lower figure). We added explanation of this comparison (L435-444).



L434-437: I think this is not the point. ICESat data are filtered in regions of steep terrain so that wrong elevation changes in this region are unconsidered anyway. Instead, we have observed that strong changes in the derived mass budgets result from errors in the ablation region, for example when the outlines have a shift, were generalized too much (e.g. including rock outcrops), or not precise enough (e.g. incl. the lateral moraine). So the general idea that precision of these mass change estimates can be increased when just removing the steep parts of the accumulation region does in my opinion not really hold.

Thank you for important information on the elevation change of glaciers. We deleted the sentence. Here, we do not specify on the steep slope, but we described on general things that there are difference in glacier elevation change including steep flank, ice patches and ice-cored moraines and rock glacier and excluding one (referring Kääb et al. (2012)).

L 438ff: I hope that area changes in this case will then not be due to changes in shadow or seasonal snow cover.

We deleted this sentence.

Figures and Tables

L575: Insert before 'Zero' that "Colours refer to the number of scenes used [N] and that zero (black squares) …"

We revised it (Fig. 1).

Fig. 2: This caption is too long. Please move the interpretation (e.g. L585/6, L590-592) to the main text.

We moved it in main text (L100-109). And we shortened caption of figure by adding legend (Fig. 2).

Fig. 6: Maybe apply some contrast stretching to these images to see something.

We applied contrast stretching (Fig. 6).

Fig. 8: The green outlines in the upper left part of the image look rather conservative. Would it be possible to add a Fig 8b image showing a screenshot with the outlines draped over the high-resolution imagery available in Google Earth (such as in Fig. 10b)?
We added Google Earth imagery as Fig. 8b.

Fig. 11: The text in L678-680 should be in the main text rather than in the figure caption. I do not see any glaciers in Fig. 11c.
We moved it in main text. And we replaced Fig. 11c more clear image (another time in Google Earth) and lap over glacier outlines (Fig. 11).

Fig. 14: Can we agree to remove the regression equations in this figure? They make no sense in my opinion.
We deleted.

Fig. 16: I suggest removing the upper half of the figure, as it is not used.
We revised it (Fig. 16).

Fig. 17: I suggest using a different symbol for the debris-covered glaciers (star or cross) and map them on top of the other class to be visible.
We revised the figure with logarithmic scale of x axis (Fig. 17).

Table 2/3:
These tables are a little bit critical as the RGI will soon be updated with improved outlines, i.e. the provided numbers will no longer apply. The table captions should also mention the RGI if the values are reported.
As your comment. The RGI is updating and the quality progress improving. The RGI in this study is snapshot of this time. Therefore, we strength version of RGI in caption (Tables 2-3).