Reply to reviewer's comments

We would like to thank the reviewer's valuable comments about our manuscript.
Revised text in main manuscript are in red colored. And we will submit all supplement table data after publication.

(Original comments are in italic, our replies are in bold in this reply letter)

Reviewer#1, Graham Cogley

Thank you for your positive and shrewd comment.

We have two main revisions, as follows.

1) **Revision of our inventory at upper part and at shadow part. In the previous our reply, we reported that we have following two underestimated area for glacier inventory. In the previous version of our inventory, a) Higher elevation with relatively steep slope area. We underestimate glacier area at upper region of glaciers than our rule, and we have eliminated upper glacier area, even where snow can accumulate. b) Shadow part. We have overlooked the glacier at shadow part, because we used Landsat image taken in winter season (with low solar angle).**
   We have revised our inventory in this half year by adding Landsat images taken in summer, as much as possible. Original manuscript reported that the number of Landsat image was 322, but now the number has increased to 356. Still there are excluded shadow part, so, we discussed the required revision by comparing with ICIMOD glacier inventory in the revised manuscript.
2) **We have deleted the comparison of glacier inventories between GGI and AGI (ALOS- glacier inventory at Bhutan Himalaya) according to Frank Paul's comment. Instead, we compared the GGI and the ICIMOD glacier inventory (Bajracharya and Shrestha, 2011). We discussed on the revision required part of our GGI comparing to ICIMOD glacier inventory. Further, we eliminated detail comparison GGI against RGI for example distribution of glacier area difference for each 0.5 grid cell.**

P2800
L4 Why not give the exact number of scenes?
We provided exact scene number as "356 Landsat ETM+ scenes in 226 path-row sets".

P2801
L13 The distribution of dates in the Chinese Glacier Inventory is actually from ~1956 to ~1983, with the median at about 1970. So repeated references in the paper to the 1970s should probably

be to “the 1950s-1980s”, although space could be saved by saying here that “for brevity we refer to the Chinese inventory as being from the 1970s.”.

Thank you for the precise information. We revised as your comment.

P2804

L13 This protocol for quality control is commendable and very impressive. One point about which more detail is required is the stage in which outlines were “if necessary, revised by a second operator”. Although the earlier part of the paragraph describes a sort of training programme, and introduces the delineation tests that are the subject of section 4.2, it sounds as though the final result was determined simply by the second operator. Given irreducible ambiguities of the kind discussed below (P2809 L28), this somewhat reduces confidence in the protocol (although it is not obvious how to improve it given that the final outline has to be the subject of a binary choice).

Delineation works were carried out by field work experienced operators and non-experienced operators. If the glacier polygon was delineated by non-experienced workers as first operator, field work experienced workers reviewed as second operator. Further, we added detail revision on the underestimation of the shadow part with figures.

P2805 As described, the “unique” ID is non-unique. Each Landsat scene may contain hundreds of glaciers. Explain the ID more fully.

The ID is unique because of sequential number begin with id=000001 in p130r037 and it end with id=087084 in p154r033. The largest ID corresponds total number of glaciers in GGI (Table 1). We revised the explanation about the ID.

P2806

L2-3 These biases are ambiguous. With respect to what reference? The other DEM (in which case they are differences, not biases)?

We added ‘against to ICESat’ in text, then references have been clarified.

P2808

L2 It would be helpful to give the equivalent RGI area for comparison. Its uncertainty is of the order of 10%, so there is a clear discrepancy.

We deleted the comparison of three inventories in Bhutan Himalaya as Frank Paul’s comment.

P2809

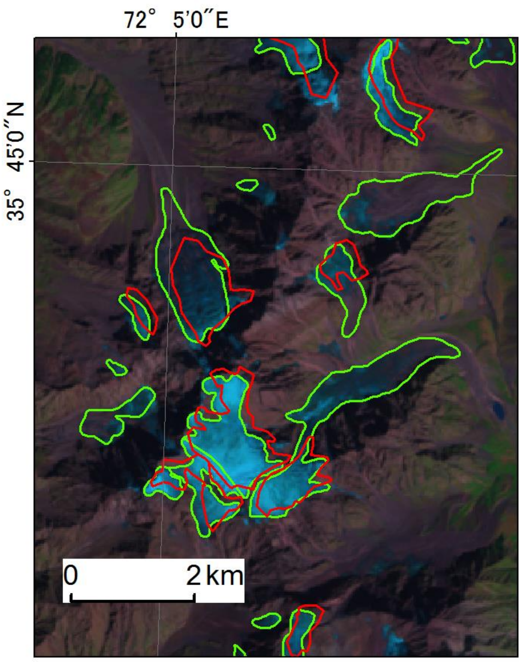
L3-5 This is not true of the RGI coverage of China, only of non-Chinese parts of High Mountain Asia (in the Altai, Tien Shan, Pamir and Himalayas).

This is our mistake. We eliminated detail comparison GGI against RGI in the revision. And only entire comparison of hypsometry and area size have been included. Therefore, we deleted above discussion.

L12-13 The assertion that there are no RGI glaciers in the western part of High Mountain Asia surprises me. As accurately as I can read Figure 11b, the bright red pixel in northern Pakistan is *at (72.0, 35.5) (southwest corner) on the Chitral–Swat divide, and in RGI version 3.2 that 0.5-degree cell contains lots of glaciers, including several valley glaciers.*

We are sorry for making your misunderstanding due to our short explanation. Our explanation does not mean glaciers do not exist in western part of high mountain Asia, but mean some glacier was not detected by RGI.

Below figure shows the scale-upped images at 72.5-73.0° E, 35.5-36.0° N (large area difference (thick red grid)). Green line indicates GGI, and red line shows RGI (ver. 3.2). Glacier outlines of the RGI are delineated carefully, but some debris-covered glaciers were missed in the RGI. Then, glacier area of the RGI would be less than that of GGI in this region. But, as we described above, we eliminated detail analysis (Fig. 11 in the previous manuscript) in the revised version. Instead, we will add detail comparison GGI against ICIMOD inventory.



p151r035 Green line: GAMDAM, Red line: RGI

L28 "high-relief headwalls": an important reason for the greater glacierized area in the RGI than in the GGI must be the ~30-year difference in their dates (P2810 L3-5), but the assumption in this sentence that high-relief headwalls ought not to be included raises a complex and open question which needs further discussion in the paper.
One has to decide, usually on the basis of a single satellite image, whether the steep slope is ice-covered; commonly this decision is unreachable because the slope is snow-covered, and a further decision is required about whether the slope is so steep that all the snow will fall off between the image date and the end of the mass-balance year. If it were to do so, and whatever was beneath the snow were thus exposed (and observed, which is unlikely), the question would have an answer. Short of this ideal, I think there is genuine ambiguity given the present state of observational knowledge. Perhaps there is a role for time-lapse photography of steep valley walls in resolving the problem.
A further difficulty is that it is not clear how those who worked on the Chinese Glacier Inventory approached the problem, or even whether the problem was recognized at the time.
Thank you for your comment.
We eliminated the detail comparison GGI against RGI in the revised manuscript.
We added difficulty of glacier delineation at steep headwalls at the section 3.3 Criteria for manual delineation. Further, our criteria on glacier delineation at steep headwalls are also added.

P2810
L5 Shangguan et al. 2007 offer only weak support for glacier shrinkage as an explanation of the RGI/GGI discrepancy (10 km^2 of shrinkage, or 0.4% of an initial glacierized area of more than 2700 km^2 in the Kun Lun, in 31 years). Possible alternatives with broader geographical scope are Li, X., et al. 2008 (Global and Planetary Change, 62, 210-218) and Ding, Y.J., et al. 2006 (Annals of Glaciology, 43, 97-105).
Thank you for your information. We added Li et al. (2008) and Ding et al. (2006) instead of Shangguan et al. (2007).

L16 "since the 1970s". But which discrepancy is being discussed here? If it is the discrepancy with Bajracharya and Shrestha 2011, "the 1970s" should be "about 2000".
A related point, which also diminishes the usefulness of Table 3, is that the numbers in Bolch et al. 2012 derive largely from the RGI.
We deleted the description on comparison between GGI and RGI. And we did not discussed on the Table 2 and 3, as your comment. We will add "Those discrepancies between the GGI and Bajracharya and Shrestha (2011) (Table 2) or Bolch et al. (2012) (Table 3) are caused by under-estimation of glacier area at steep slopes in the GGI.".

Stylistic Comments
P2800
L2 Delete “the” before “High Mountain Asia”, and make this change throughout the text.
We revised.

P2801
L15 “Pfeffer”.
L26 “… error respectively in these regions”.
We revised.

P2803
L16 “identification of glacier divides”.
We revised.

L21-22 Change “glacier area” to “ ‘glacier’ ”.
We eliminated this sentence ‘we defined glacier as a continuous body of ice.’

P2804
L20 “glacier boundaries were misidentified”.
We revised.

P2805
L7 Change “attribute datasets” to “attributes”.
We revised.

L12 Change “are” to “is”.
We revised it.

P2806
L5 The English spelling is “Karakoram”.
We revised it.

L9 Change “less” to “lesser” and “field” to fields”.
We revised it.

P2807
L20 These percentages are ambiguous. The consistency should be described in terms of a percentage difference between the inventories, making sure that the reader knows which is which.
We removed comparison of glacier number based on comment by reviewer#4 (Frank Paul).

P2808

L6 “grid cell”. Make this change throughout the text, e.g. at L19 and frequently later.

We revised all “grid” to “grid cell”.

L16 “overestimated”, not “over-delineated”. This too needs to be changed throughout, as well as “under-delineated”.

We revised them.

P2810

L7 “glaciers”.

We revised “glacier” to “glaciers”.

L9 “over the Himalaya”. “summaries”.

We revised as your comments.

L17 Figure 10a, not 10c.

Thank you for your comment. But, we deleted Fig. 10 according to Frank Paul’s comment.

L25 Change “in parts of” to “in most of”. In the RGI only small parts of Chinese territory (e.g. part of the Nyainqentanghla Range) are more recent than the Chinese inventory.

We revised.

P2816

Table 2 Add “inventory” after “(2011)”, and right-justify all columns but the first.

We revised. And we also added “inventory” after Bolch et al. (2012) in Table 3.

P2817

Table 3 Move “the” to follow “and”.

We revised the caption based on comment by reviewer#4 (Frank Paul).

P2827

Figure 10 End the first sentence at “boundaries”, then say “Glacier outlines are from the RGI (red) and the GGI (green).”.

We deleted Fig. 10 as Frank Paul’s comment.

Supplementary Information

The supplementary information is not adequately documented. The filenames should ideally include the name of the first author and the date, and in any case should be reproduced accurately in the table headers. The main PDF file should begin with full bibliographic data (full list of authors, title of the paper, etc.).

We revised.

Figure S1 This is potentially quite valuable, but needs further documentation.

a: Add an outline of the steep headwall, which I cannot find. If it is the bright white patch at right centre, I need more information before accepting its exclusion from the glacier; it looks like an ordinary accumulation zone to me.

We revised the figure. We used accumulation area of the Khumbu Glacier comparing to ICIMOD inventory to explain our criteria of glacier area on steep headwalls as shown in Fig. 5 of revised manuscript.

c: Define "true-colour" and "false-colour" (as in the main text, in terms of Landsat bands), and say which panel is which. In fact, say which panel is which in each of b to g.

We have added detail explanation in each figure (Fig. 2, 3, 5 - 9, 11) .

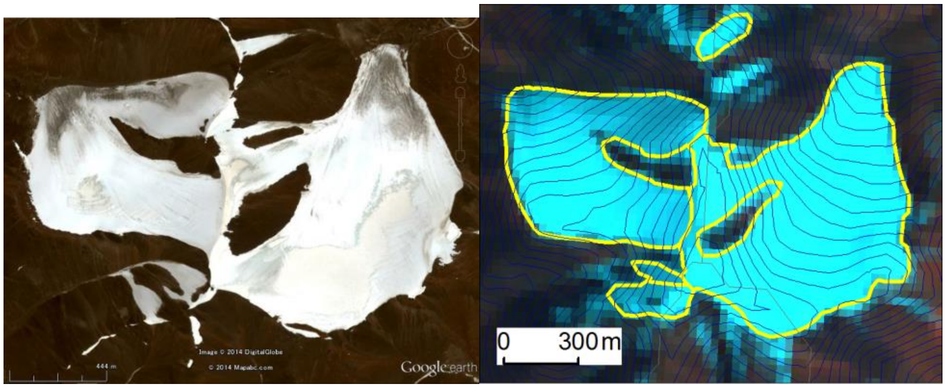
d: Is the lake in the right panel really "non-glacial"? It looks like a supraglacial lake.

As your comment, the right lake is surrounded by ice cliffs. So, the lake is supraglacial lake. We replaced the picture as shown in Fig. 7d.

f: The scales appear to differ between the two panels. I can only guess that the orange rectangle in the left panel represents the extent of the right panel, and I do not know what conclusion I am being invited to draw – that the blue patch on the left is or is not a glacier.

We replaced both images as shown in Fig. 9. And interpretation of Google Earth image has added in explanation.

g: "… in Google Earth imagery of appropriate date."; the typically high resolution of Google Earth imagery is of little value for distinguishing seasonal snow from perennial snow or firn.
On the left of the north-south divide in the left panel, the larger of the two green glacier outlines omits a grey (i.e. not brown) patch that seems to be part of the glacier in the right panel. Why?



Actually, we have found better source image for Fig. S1g (in previous manuscript), and revised the polygon based on the better image. The glacier polygon have excluded uncertainty by changing source image. Therefore, we removed this figure in the revised manuscript.

Figure S2 Again potentially valuable, but I do not know which colours represent right and which represent wrong decisions about delineation.
a: In the deep shadow on the south side, I would accept the orange outline (following the topographic divide) as correct, and I can see no basis for the decisions marked in red, green and blue.
The southern part of steep walls have partially exposed rock surface by close investigation of Google Earth™ image. We, therefore, judged that the orange line is not correct. We added a Google Earth™ images in Fig. 10 with explanation.

b: Here I would also reject the red, green and blue decisions. The main paper seems to suggest that they correctly exclude a lateral moraine, but I am not sure of the basis for this.
Lateral moraines do not contain glacier ice under the debris and do not flow down with glacier ice. Therefore, in order to evaluate glacier mass change, we have to exclude lateral moraine surrounding debris-covered glaciers. We changed dotted circle location and added clear explanation which lines are correct in Fig. 10 with closeup shot of Google Earth™ image.

Reviewer#2, Mauri Pelto

Thank you for your kind comments.

We have two main revisions, as follows.

1) **Revision of our inventory at upper part and at shadow part.**
   In the previous our reply, we reported that we have following two underestimated area for glacier inventory. In the previous version of our inventory, a) Higher elevation with relatively steep slope area. We underestimate glacier area at upper region of glaciers than our rule, and we have eliminated upper glacier area, even where snow can accumulate. b) Shadow part. We have overlooked the glacier at shadow part, because we used Landsat image taken in winter season (with low solar angle).
   We have revised our inventory in this half year by adding Landsat images taken in summer, as much as possible. Original manuscript reported that the number of Landsat image was 322, but now the number has increased to 356. Still there are excluded shadow part, so, we discussed the required revision by comparing with ICIMOD glacier inventory in the revised manuscript.
2) **We have deleted the comparison of glacier inventories between GGI and AGI (ALOS-glacier inventory at Bhutan Himalaya) according to Frank Paul's comment. Instead, we compared the GGI and the ICIMOD glacier inventory (Bajracharya and Shrestha, 2011). We discussed on the revision required part of our GGI comparing to ICIMOD glacier inventory. Further, we eliminated detail comparison GGI against RGI for example distribution of glacier area difference for each 0.5 grid cell.**

1) GGI identifies more glaciers than RGI, give us a visual example of where this occurs and that can help explain why. Figure 1 is in the Zanskar region and is not specifically recommended but just a typical area that would be useful in looking at differences in area and number of glaciers where steep slopes are not an issue, yet the glacier count is not straightforward.

Thank you for your recommendations. As Frank Paul's comments, we will eliminate number and detail comparison between GGI and RGI. Instead, we will add the comparison between GGI and ICIMOD in area. The partitioning of glacier polygons in ICIMOD inventory is well done. Therefore, we do not need visual example to compare partitioning of glacier polygons.

2) The GGI despite more glaciers has much lower glacier area. Part but not all of this result from using imagery of differing dates. Again provide a visual example indicating how RGI and GGI deal with glacier boundaries in a specific location that has steep avalanche slopes that GGI does not typically classify as a glacier. This comment contains three figures that illustrate the level of visual detail needed for an adequate comparison. Figure 10 and S1 currently serves that role, but there are too many examples with too poor resolution in each. Figure S1 does not compare RGI versus GGI for a specific area. Figure 10 has too many examples and does not provide the detail needed, or supporting tabular results. The steep slope example does not adequately portray which *approach is better given the nearly complete snowcover. Figure 2 and 3 in this comment use Digital Globe and Landsat imagery looking at same area to point out specific locations where steep slopes could be differently interpreted. This is the level of detail needed to delineate the ability of the method chosen and contrast it with the RGI. The output from a specific glacier or watershed comparing GGI and RGI in tabular form is needed. You could focus on a single glacier, such as the Durung Drung Glacier shown in these figures.*

Frank Paul has made comments that comparison GGI with RGI is no suitable since the RGI will be soon revised. And visual examples like Fig. 10 b and c were already published at Pfeffer et al. (2014). Therefore, we will not compare GGI with RGI. Instead, we will compare GGI with ICIMOD inventory.

2807-25: Be more precise here since all inventories rely on satellite imagery, what imagery did RGI use beyond China that would lead to this?

We removed comparison of glacier inventories in Bhutan Himalaya as Frank Paul's comment.

2811-14: Given the completed inventory value is as a baseline, authors should comment on how easily the inventory can be replicated with Landsat 8 imagery in the near future.

Thank you for your comment. We added " In the near future, setting using GGI as the initial glacier area and re-compiling the glacier inventory using Landsat 8 imagery will yield the temporal change in glacier area in high mountain Asia." in the last.

Table 3: Does not add value beyond that of Table 2. Are the Bolch et al (2010) numbers different than RGI? Instead or in addition to this a table for a specific watershed such as in Figure 1 where the count, area and boundaries of glaciers could be shown and reported from GGI and RGI.

We have added RGI(ver. 4.0) area in Tables 2 and 3 for comparison in watershed unit and comparison between Bolch et al. (2012) and RGI. And we also modified difference based on comments from you, reviewer#3 (Samjwal Bajracharya) and reviewer#4 (Frank Paul).

Figure 4: The contours detract from actually seeing the colored elevation depiction.

We reduced number of those contours as shown in Fig. 12.

Reviewer#3, Samjwal Bajracharya

Thank you for your positive comments.

We have two main revisions, as follows.

1) **Revision of our inventory at upper part and at shadow part. In the previous our reply, we reported that we have following two underestimated area for glacier inventory. In the previous version of our inventory, a) Higher elevation with relatively steep slope area. We underestimate glacier area at upper region of glaciers than our rule, and we have eliminated upper glacier area, even where snow can accumulate. b) Shadow part. We have overlooked the glacier at shadow part, because we used Landsat image taken in winter season (with low solar angle).**

   We have revised our inventory in this half year by adding Landsat images taken in summer, as much as possible. Original manuscript reported that the number of Landsat image was 322, but now the number has increased to 356. Still there are excluded shadow part, so, we discussed the required revision by comparing with ICIMOD glacier inventory in the revised manuscript.
2) **We have deleted the comparison of glacier inventories between GGI and AGI (ALOS-glacier inventory at Bhutan Himalaya) according to Frank Paul's comment. Instead, we compared the GGI and your ICIMOD glacier inventory (Bajracharya and Shrestha, 2011). We discussed on the revision required part of our GGI comparing to ICIMOD glacier inventory. Further, we eliminated detail comparison GGI against RGI for example distribution of glacier area difference for each 0.5 grid cell.**

To reply your general comment 'Why we selected manual digitization (not automated mapping)'.
The Asian region tend to covered with seasonal snow or partly cloud cover during the high solar angle season (summer) because of the monsoon. So, it is difficult to get clear seasonal snow-free, cloud-free and shadow-less images. Eventually, a lot of manual corrections for clouds, shadow, debris cover and seasonal snow are required after automated processing. We used multiple Landsat images to complete one path-row set.

Some comments P2800 and in other area Instead of "glacier in the high mountain Asia" "Asian glaciers" sounds better
We can not find the phrase "glacier in the high mountain Asia" in our manuscript. Probably you mentioned "glacier inventory for the high mountain Asia" at P2800L2. We retain the phrase as is for consistency through the manuscript.

P2801 L15 "Pfeffer".
We revised.

L22 Why not give the exact extension of glaciers. The glaciers below 27.5 deg latitude does not exist in Asia. (Bajracharya and Shrestha, 2011)

The southernmost glaciers locate in the Yulong snow Mountain (27.06° N, 100.18° E). Those glaciers are also included in the RGI. We added exact location in the revised manuscript (27.0-54.9° N, 67.4-103.9° E).

P2804 L26-28 Though 11 operators had delineated the glacier outline in 20 months with review of initial delineation but the error will be minimized if peer reviewed by limited number of reviewers.

Thank you for your comment. We agree with your comment. First delineation works were carried out by both field work experienced operators and non-experienced operators. And glacier polygons, those are delineated by non-experienced operators, were reviewed by field work experienced operators. But, not all glacier polygons were checked by field work experienced operators. Therefore, we included errors by all operators at present. We added the less uncertainty by limited operators as future plan.

P2815 Table 1 show very high glacier number and area compared to the report Bajracharya et al. 2014. The number and area shows not only the glaciers within the territory of Bhutan but also included from the adjoining areas. The inventory of glaciers in Bhutan in 2010 shows 885 glaciers with total area of about 642 km2. (Bajracharya et al. 2014)

Yes, the difference was caused by the difference of target area. But, we deleted the comparison of glacier area and number in Bhutan Himalaya according to Frank Paul's comment.

Table 2 difference (%) subtract from 100 and provide the difference in + and - %

We revised the difference value in Tables 2 and 3.

Reviewer#4, Frank Paul

Thank you for your shrewd comment. We considered your comments once again, and revised our manuscript.

We have two main revisions, as follows.

1) **Revision of our inventory at upper part and at shadow part. In the previous our reply, we reported that we have following two underestimated area for glacier inventory. In the previous version of our inventory, a) Higher elevation with relatively steep slope area. We underestimate glacier area at upper region of glaciers than our rule, and we have eliminated upper glacier area, even where snow can accumulate. b) Shadow part. We have overlooked the glacier at shadow part, because we used Landsat image taken in winter season (with low solar angle).**

   We have revised our inventory in this half year by adding Landsat images taken in summer, as much as possible. Original manuscript reported that the number of Landsat image was 322, but now the number has increased to 356. Still there are excluded shadow part, so, we discussed the required revision by comparing with ICIMOD glacier inventory in the revised manuscript.
2) **We have deleted the comparison of glacier inventories between GGI and AGI (ALOS-glacier inventory at Bhutan Himalaya) according to Frank Paul's comment. Instead, we compared the GGI and the ICIMOD glacier inventory (Bajracharya and Shrestha, 2011). We discussed on the revision required part of our GGI comparing to ICIMOD glacier inventory. Further, we eliminated detail comparison GGI against RGI for example distribution of glacier area difference for each 0.5 grid cell.**

Reply to general comments

(1) general comment, this (exclusion of steep rock walls) makes the new outlines incomparable to other existing datasets and is strongly limiting their usability for other applications when made available.

We have described on our criteria on steep headwalls in the section '3.2 Criteria for manual delineation' . And also we added how we decided to include/exclude the steep headwalls into/from glacier area showing visual examples (Fig. 5)(accumulation of the Khumbu glacier)

(2) the rather detailed comparison with the poor-quality glacier outlines of the RGI in this region

ICIMOD inventory was compared to GGI in detail. We compared GGI and RGI 4.0 in only hypsometry and in area class for entire high mountain Asia.

(3) why automated processing was not applied here

We added the reason that automated processing is not suitable at high mountain Asia in the section '3.1 Pre-processing' with visual example (Fig. 2)

(4) All methods need to be properly described. As an example: just writing (P2803, L13): 'Contour lines were used to identify glacier outlines' is not sufficient.

We added detail of delineation method in the section '3.3 Criteria for manual delineation' with visual examples.

(5), the quality control procedure seems to rely on one (?) person ('revised by a second operator') who - in the end - always knows where a glacier outline needs to be and can reshape them to the one and only true position. It is unclear to me how the judgement of this second operator can always be superior and 100% correct?

Delineation works were carried out by field work experienced operators and non experienced operators. If the glacier polygon was delineated by non-experienced workers as first operator, field work experienced workers reviewed as second operator. Even if the glacier polygon was made by experienced workers, other field work experienced operators reviewed as much as possible. So, quality of inventory depend on mainly the field work experienced operators who checked at final. We added in the section '3.4 Quality control'

page 7
Fig. 10a

We appreciate your attentive comments on each ice surface. We revised as previous reply letter and we also revised other glacier polygon as much as we could.

We have shown this figure (Fig. 10a in TCD) in Fig. 2b(revised manuscript) as visual example that multiple imageries are required for one path-row scene in the revised manuscript.

I have some further points to criticise (see specific comments), with the above being the most general ones. Of course, it is now difficult to recommend 'Please do the inventory again and apply the correct glacier definition this time'. This might be senseless as the dataset (GGI) might be useful for its intended purpose, but it is important to pay attention to the other points, in particular explaining why certain decisions have been taken and how the applied methods work. As this study is describing a basic dataset (creation, accuracy, key parameters) I suggest moving Figures S1 and S2 (the revised ones) to the main text as they answer key questions about the approach taken. The comparisons with the entire RGI (which is frequently updated so the comparisons presented here will quickly be outdated) should be replaced with more detailed comparisons of independent studies (having provided high-quality datasets) and results from automated mapping. And please do not compare glacier numbers from different inventories. These are basically arbitrary numbers depending on several external factors (e.g. minimum size, drainage divide rules, separation of tributaries) with a very limited scientific meaning. I hope that my suggestions are helpful in revising the ms.

We have moved all visual example to the main text.
And we replied this general comment in specific comments.

2. Specific comments
P2800

L6: This is not the reason; I think the explanation for the good match is that the same DEM is used here. In the void-filled regions of the SRTM DEM the geolocation of the Landsat scenes could be off by about 5 pixels (150 m) resulting in a relatively poor match with other geocoded datasets (e.g. the GDEM).

The void-filled region in SRTM DEM could cause offset as you noted. However, the part of void in SRTM DEM is not so large. Therefore, we revised the explanation as "Geolocations are mostly consistent between the Landsat imagery and DEM due to systematic radiometric and geometric corrections made by the United States Geological Survey".

L16: For this reason more precise (regional) inventories should be used for a comparison (however, the here applied glacier definition requires to only compare complete glaciers).

We compared GGI with ICIMOD inventory as shown in Fig. 13-15.

P2801

L17: With this purpose in mind I would also include the upper parts of all glaciers as they might belong to their accumulation area (see page 8 example). In particular when later operating with elevation related variables (like in Sakai et al. 2014) the missing accumulation areas would cause a bias. How have glaciers with an interrupted profile been considered?

We added 'Our initial and direct purpose of creating glacier inventory is to estimate elevation change of glaciers in Asian Mountains, which is equivalent to evaluate the glacier volume change on river runoff (imbalance of glaciers) (Kääb et al., 2012) . ' in the introduction.

We evaluated median elevation derived from our GGI by comparing with those of ICIMOD inventory in Fig. 15c.

P2802

L1: This section should introduce the RGI and AGI datasets used for comparison. However, as mentioned above I recommend not using the entire RGI for this due to its obvious regional deficits and ongoing improvement. Please select high-quality outlines from individual (citable) studies (that can also be found in the RGI dataset).

We introduced ICIMOD inventory and RGI at the end of this section.

L6: The DEM used for orthorectification is not everywhere identical to the void-filled SRTM DEM but is a merged product (called GLSDEM).

Thank you for your comment. We will revise here as "geometric corrections were performed for the L1T imagery using Global Land Survey digital elevation model (DEM) 2000, which is merged product using SRTM DEM (http://landsat.usgs.gov/Landsat_Processing_Details.php) and other DEMs".

L11: This is fine but it requires that snow and cloud conditions are substantially different (what is difficult for orographic clouds and perennial snow fields). It needs to be described what the differences among the multiple scenes are and how they were combined. To me it seems that in many regions it was not possible to find scenes with appropriate snow conditions (despite the combination of scenes), as the 3-year period is simply too short for this. Just as an example: For *the M. Everest scene 140-041 the supplemental xls table indicates that two scenes from 17.10. 2001 and 5.1. 2002 were selected. The first one shows severe seasonal snow cover hiding the real glacier perimeter. Why has this scene be used? Because of the smaller regions in shadow? Please explain the selection/combination process.*

We added visual examples of glacier outlines delineated based on two imageries (Fig. 2) and also added how we combined. (we made multiple shape-files in the delineation phase, and added Landsat image ID as attribute data when the multiple shape-files are merged.) in this 'Data sets section'.

L14/15: I have no idea how this can work. Please explain it in the methods section.

In the method section, we explained how we used contour lines at terminus of debris-covered glacier and at shadow part with visual examples.

P2803

L6/7: The discrimination of snow from clouds is working because of the strong absorption of ice/snow in the SWIR compared to clouds. The moderate absorption in the NIR has nothing to do with it (and actually snow reflectance is still high in the NIR).

We revised as "false-colour images enabled us to differentiate ice from cloud because of the strong absorption of ice/snow in the SWIR compared to clouds"

L10: This is fine, but why has automated mapping not been used, at least to have an accurate base for the clean ice? This should be explained here. I assume it has something to do with the poor snow conditions in several images and the difficulties in interpreting them?

We replied in the above "Reply to general comment" (3). We added the description in the revised manuscript.

L13: As indicated above, this is not a description of a method. Please add all relevant details to understand how this is working (maybe better: do not use it).

We added how to use the contour lines in the method section.

L20-24: This is not the definition as given in the GLIMS Analysis Tutorial. First, all connected feeders (above the Bergschrund) need to be included, and second, also the unconnected glaciers in the steep headwalls are glaciers (maybe hanging glaciers). Finally, several glaciers might have interrupted profiles (e.g. due to a steep slope) and receive ice through avalanches. Of course, the upper parts of these glaciers have to be mapped as well. Only ice-free rock walls (and those covered by seasonal snow) need to be excluded.

Thank you for your comment. We added difference of our rule from GLIMS Tutorial in section 3.3.

L25: According to my experience, it is much more easy to delineate clean glaciers from the false colour composites (where glaciers appear light blue and thus have good contrast) and interpret debris from the true colour composites (also to have a better comparison with the high-resolution data available in Google Earth). In the example of Fig. S1 b) and c) the contrast issue is well visible: The glaciers in c) are clean but the ice is dirty (polluted) and has thus a much lower *reflectance. By the way, also these dirty (but debris-free) glaciers can be accurately mapped automatically (e.g. with a simple band ratio).*

Thank you for your suggestions. We added the reason that automated processing is not suitable at high mountain Asia in the section '3.1 Pre-processing' with visual example (Fig. 2).

P2804

L2: 'delineated by reference to topographic data'? How does this work? Does the 100 m SRTM DEM show the glacier boundary clearer then the Landsat imagery at 30 / 15 m resolution? This is hard to believe. Has a hillshade been used or just the contour lines mentioned before? In the latter case: How have they been used to decide where the boundary is? This needs to be described.

We will add how to use the contour line with a picture in the revised manuscript. And also added that the glacier outline at hillshade have low accuracy in the discussion section.

L4: What happens when Google Earth images are snow covered (which is often the case)? Or have all high-resolution images from this source been perfect for all decisions?

We added that 'Additionally, SLC-off scenes (Landsat ETM+ post-dating May 2003) were used to identify ambiguous glacier boundaries in the case that Landsat L1T imagery and seasonal snow-free Google Earth™ imagery were not found, though we note their acquisition dates are different from those of L1T scenes.' in section 3.3.

L6: What is seasonal ice cover?

We revised it as 'seasonal snow'.

L7: 'we referred to topographic data': this is not a description of a method. Apart from the fact that it is an unfortunate decision to define glaciers different from the standard by excluding their steep parts, it needs to be described HOW this method works (I can imagine a threshold value applied to a slope grid, but contour lines?).

We replied in the "replay to general comment" (1).

L13ff: This section sounds like there is one person (second operator) knowing everything precisely and thus being able to always give advice for correct interpretation to all others. Given the sometimes wide range of interpretation that is possible (e.g. in cold-dry regions where debris-covered glaciers often have no clear boundaries to rock glaciers), I doubt that such a person (or several?) exists. The examples on pages 7 & 8 also illustrate severe difficulties in correct interpretation. In any case, a comparison with the outlines derived from automated mapping is missing (for clean glaciers) and should be provided. This can even be used as a reference dataset (for accuracy determination) as it is free of generalization effects. Furthermore, other regional studies should be considered (see P2802, L1).

We added detail of check procedure in section 3.4.

Further, we added visual comparison between Automated mapping and manual delineation in Fig 3.

L19/20: When specific surface features are obscured by shading, there is no need to assume that there is no ice underneath and exclude these regions. Very likely (as I can judge from Fig. S2a) the orange line is much closer to the correct outline than all others, i.e. that was not a misidentification of the glacier but the correct one.

We agree with your first comment 'When specific surface features are obscured by shading, there is no need to assume that there is no ice underneath and exclude these regions.'. But, in this case we can judge that rock surface partially exposed at the southern steep headwalls by Google Earth™. We added Google Earth™ image.

L22/23: Where these tests performed independently? Please provide details of the method.

We added detail information of tests as TableS2 in revised manuscript.

P2805

L1: This reads like the second operator has special knowledge that cannot be shared beforehand with those doing the work and that this expertise is always the correct one. Please show examples of what this second operator is correcting to learn from it (or even better: use the same rules for all operators).

We added explanation of second-check system and visual example (Fig. 11) of 1st and 2nd revised glacier outlines in the revised manuscript.

L14: There could be a mismatch with the outlines derived from Landsat as the SRTM DEM with the wrongly interpolated data voids has been used for orthorectification.

We discussed on those error might cause wrong orthorectification in the section 5.3.

L20-25: I would place this into the methods section. It is a description of how calculations have been performed.

We moved this to the methods section including later part (P2806 L1-12), since we set 'Distribution of glaciers and their median elevations' to the first result (as your comment).

L23: Hayakawa et al. have not investigated the performance over glaciers. This is in general a different type of terrain due to lack of contrast (snow), more gentle slopes and self similarity of surface features (debris). A subtraction of both DEMs should reveal which DEM is more appropriate for the specific purpose and provide better evidence for the selection.

We deleted this sentence in the revised manuscript. And we calculated subtraction of median elevations derived from SRTM and that from GDEM as next below reply.

P2806

L1-12: This discussion of DEM uncertainties is not really a result. I would either describe this when introducing the datasets or mention it in the discussion section. I am also not sure if this evaluation (ICESat comparison) really matters when considering the applications shown in Figs. 4, 7 and 12.

We moved the discussion of DEM uncertainties to the discussion section.

We added Fig. 19, which shows the difference of area-weighted mean median elevations derived from SRTM and that from GDEM in 0.5 degree grid cell. And we did not removed

the ICESat comparison since we cannot evaluate SRTM and GDEM without ICESat comparison (Fig. 4).

L13: I would not place this section before 4.3. The main results are in 4.3 and they should be described first. Section 4.2 itself starts with a description of methods (that might be better placed in the methods section). I would move the reminder of it to the discussion section as it belongs to an overall evaluation of the results. But this is maybe a matter of personal taste. In any case, sections 5.1 and 5.2 are results of this study and have to be in the results section (with the suggested changes) rather than in the discussion.

We have moved section 4.3 to the first of result section. And also 5.1 and 5.2 have moved to the result section. Section '4.2 Evaluation of uncertainties' has moved to the discussion section.

L17: I suggest using the mean value of all digitizations as a reference for calculating the standard deviation. Otherwise it would imply that the digitiztions are not independent and one is always better than all the others (which seems to be confirmed in L21). When the quality in test 5 is very different from all others, I would assume that the rules for digitization have changed (?) and results are not comparable. Please clarify why this final test was superior in quality to all others and only compare what can be compared.

We revised this figure based on your suggestion that "mean value of all digitization as reference" as shown in Fig. 17. Then, there was no much difference between 5th test and other test. Therefore, we deleted the description on the improvement of the final (5th) test.

L25/26: If these would be 'real' uncertainties, the outlines would not be worth considering in an inventory (as they should be better than 5% in the mean). But as mentioned before, there is likely a bias in the calculation of the accuracy and this should be corrected first (applying the same rules for all digitizations and then use the mean value as a reference).

We evaluated GGI by comparing with the ICIMOD inventory. And the uncertainties has derived from difference of delineation.

P2807

L4/5: How can median elevations be area-weighted? I would understand that only glaciers larger than a certain size are used to calculate a mean value (to reduce the influence of local topographic factors which have a stronger influence on small glaciers) but area weighting?

We would like to represent the median elevation of whole glacier ice in certain grid by area-weighted median elevation. If we eliminate small glaciers, some grids would have no glaciers.

L13: As mentioned above, I see section 5.1 and 5.2 of this study as results rather than a discussion and would suggest moving larger parts of it to sections 4.2 and 4.3 (the current 4.3 should be 4.1). As this would result in a missing discussion, the key findings (e.g. the differences between the compared inventories) should be critically assessed in a revised discussion section. I suggest including a discussion of uncertainties and how they impact on the results (e.g. how does median elevation change when 'correct' glacier outlines are used?), how glacier area *changes due to a different interpretation of what a glacier is, and where the largest real differences in interpretation are (when comparing inventories of similar quality), among others (e.g. the derived topographic parameters).*

We have moved section 5.1, 5.2 as your comment.

And we added discussion on regional discrepancy between GGI and ICIMOD inventory.

Actually, median elevation difference caused by both exclusion of steep slopes and excluded shadow part. We could not evaluate each contributions. We described in the discussion part.

L13ff: As mentioned before, please use regions for comparison that are worth a comparison rather than those who are wrong for obvious reasons (and do not compare numbers).

We eliminated detail (regional) comparison between GGI and RGI. Only entire comparisons between RGI and GGI have retained as shown in the Fig. 13b, d.

P2808

L11: The RGI has only assimilated the existing datasets rather than interpreting them (i.e. the obvious errors were in the source material).

We deleted the inventory comparison at the Bhutan Himalaya as your comment. Then, this sentence has also deleted.

P2809

L4/5: What has the spectral mapping of glaciers to do with the partitioning? The latter is performed with a DEM.

As your comment, automatical spectral mapping has no relation with the partitioning. And Fig. 10 in previous manuscript does not indicate incomplete partitioning. We deleted this sentence.

P2810

L21/22: For clean ice manual delineation is not better than automated methods but more inconsistent and not reproducible. The peer-review process is fine but intransparent (i.e. I do not understand how this works, see comments above). It needs to be explained where the first delineation failed and why the 'second operator' is always right with the interpretation.

Because second (final) operators have experience of field work at glaciers.

p2811

L2/3: 'potentially accounts' sounds like if it is not clear that removing large parts of the accumulation area results in smaller glaciers. I assume that real changes of glacier size since the 1970s are comparably small?

We have revised the shadow part of glaciers as much as possible. But, we could not get quantitative evaluation on glacier changes, since still we have revision required shadow part at some regions. We added the revision required part in discussion and conclusion part.

L6: Fig. S1c is not about seasonal snow.

This is our mistake. Here, we have to cite Fig. 10b or c. But, we deleted this sentence and Fig. 10b and c as your below comment.

L6: Misinterpretation by whom? The RGI or this inventory? What about comparing glacier outlines in a region with good quality and snow conditions (see suggestions above)?

We will delete this sentence.

L12: Please be aware that the excluded headwalls also include glacier (parts) under the seasonal snow (see page 8 example).

As your comment, if steep headwalls include glacier ice, we underestimate glacier area. We will add in the fault of our inventory.

L13: I do not understand this sentence. What is meant by 'projections of mass balance by in situ observations'? Does this refer to differences in calculated mass changes due to the different techniques of spatial interpolation and averaging applied? How can exclusion of headwalls improve this? As far as I know, these regions are filtered when altimetry is used, are seldom measured in the field, and have small changes anyway.

We added following explanation in the conclusion 'Gardner et al. (2013) reported that the glacier surface decline values estimated using GRACE and ICESat were less than that observed by field observation. We suggest that one of the reasons would be glacier inventory used for elevation analysis include those no elevation change area (due to glacier mass fluctuation). Hence, the exclusion of no elevation change area, such as upper steep headwalls and seasonal snow in the GGI has great potential to correct any discrepancy among projections of surface decline of glaciers by in situ observations and laser altimetry (Gardner et al., 2013).'.

Tables

Table 1: Please replace 'Number of excluded small glaciers' with 'Excluded glaciers'

We revised it. And we deleted number of glaciers based on your other comments.

Table 2: Please use the abbreviation AGI also here (instead of the citation). The last column is not the difference in percent (that would be the difference in km2 divided by the total area), but a normalized value (with negative values being changed to positive). If this way of presenting differences should be retained, I suggest using +4 instead of 104 and -4 instead of 96, etc. This would allow for a more easy comparison. It also needs to be shown on a map (in a new Fig. 1) where these subregions refer to (catchment boundaries). The further study sites for area comparison should be marked in this new Figure as well. And please use a more descriptive caption (see Table 3 comment).

AGI covers only Bhutan Himalaya. And we delete comparison between GGI and AGI in the revised manuscript.

We revised last columns based on comments from you, reviewer#2 (Mauri Pelto) and reviewer#3 (Samjwal Bajracharya). (see reply to comment about Table 2, 3 by reviewer#2)

Table 3: Please use a more descriptive caption, e.g. 'Comparison of regionally aggregated total glacier areas from Bolch et al. 2012 and the GGI'.

We revised it as your comment.

Figures

Please insert an overview Figure showing important subregions/test sites and outlines of catchments (listed in Tables 2 and 3).

We added a figure as shown in Fig. 16.

Figs. 2a/b, 3, 5, 6, 8, 9 (with their respective revised content): Please add minor tick marks and show them on both sides. I suggest placing the a), b) etc. annotation outside the plot.

We revised as your comments.

Figs. 10 and S1: Green on light blue is difficult to see, please use another colour (yellow?).

We revised image in the revised manuscript.

Fig. 2: As a justification for selecting a specific DEM, I suggest to simply subtract the GDEM from the SRTM DEM, add a colour coding in classes of standard deviation, and a layover of glacier outlines (of course, this could only be shown for a subregion). The comparison with ICESat is interesting in general, but in the framework of this study I would suggest showing something more relevant (glacier specific).

We depicted differences and standard deviations of median elevation derived from SRTM and GDEM in each 0.5 degree grid cell in Fig. 19.

Fig. 3: This plot should be recalculated, after applying the same rules to all tests (also number 5) and using the mean area as a reference.

We revised the figure based on your suggestion (see reply to your comment to P2806L17). The result of uncertainty have not changed as shown in section 5.1.

Fig. 4: This plot might change when 'correct' outlines are used. The impact of such a change should be determined for a test region and discussed in the main text.

We revised our glacier polygon. And comparison of median elevation between ICIMOD and our revised GGI are also discussed in the Section 5.2.

Fig. 5: I suggest removing this figure. For the GGI vs AGI comparison I would like to see an example (close-up with a few glaciers) showing an overlay of outlines.

As we described above, we deleted the comparison between GGI and AGI.

Fig. 6: When retaining this figure, I suggest removing the comparison with the entire RGI and focus on more regional comparisons with other high-quality datasets. When hypsometry is shown, it would be nice to indicate how the sampling is done (e.g. in 100 m bins?).

We have no whole area comparison with our inventory without RGI. Therefore, we have used RGI as entire comparison.

We added the interval of altitude. (In the previous manuscript, the bin was 200m, but in this revised manuscript, we made hypsometry in 100 m bin.)

Fig. 7: I suggest removing the comparison with the RGI dataset and instead of c) showing the difference between b) and c)
We removed this figure.

Fig. 8: General remark: For scatter plots with a high correlation, it is most often more insightful to plot the difference between the values vs the values. As mentioned above, I see little value in comparing glacier numbers (what is the message here?) and I would not compare areas or elevations of the entire RGI. Please use a few subregions where the RGI data are of sufficient quality and compare these. The effect of not considering parts of the accumulation area on median glacier elevation should be analysed in detail, in particular when it is foreseen to use this parameter for climatic interpretation (see Sakai et al. 2014).
We compared the number, area and median elevation derived from GGI and those from ICIMOD in Fig. 14. In this figure we would like to indicate the overview of the relation between GGI and ICMOD inventory. The detail difference were shown in Fig. 15 with spatial distribution. We thought that spatial distribution of those difference would have more information than the simple scatter plots of difference.

Fig. 9: I suggest removing this Figure.
We removed the comparison of glacier number between GGI and RGI, but, we would like to retain area comparison by the gross. Because, these figures (13b and 13d) can evaluate 'whole' our inventory. But, if this paper will be reject with this figure, we will delete this figure.

Fig. 10: The comparison with high-quality outlines as shown in a) worries me. I have compiled comparisons with high-resolution screen shots from Google Maps on page 7, showing that the GGI quality is partly rather poor compared to the RGI. It seems that GGI is highly generailzed (like the DCW) and that the second operator has also problems in interpreting glaciers correctly. The poor-quality regions shown in b) and c) have already been documented by Pfeffer et al. (2014) and have been revised in the mean time (for RGI 4.0). There is no need to show them here. The red line on d) is certainly not correct, but why has the glacier in the upper centre and at the Lhotse westface been removed? Please also check the Mt. Everest map from swisstopo or the National Geographic Society for a correct interpretation of glacier extents in this region and see comparison on page 8. Caption: I think the normal way of providing geographic coordinates is latitude, longitude.
We removed Fig. 10 b) and c). As for Fig. 10 d), we have revised our glacier outline according our definition. Further, we explained our criteria on steep headwalls using this figure with Google Earth images and geographic coordinates in Fig. 5.

Figs. 11/12: I suggest removing these figures.
Instead of this figure, we added comparison GGI against ICIMOD (Fig. 15, 18).

Figs. S1 and S2: Some coordinates must be given for all images to have a chance to find these example glaciers (as in Fig. 10). As mentioned above, please integrate these figures in the main text. This is the key work of this study and should be properly documented. Fig. S1: a) Where is the not included steep headwall and the glacier (outline)? b) How is snow discriminated from clouds then? c) glaciers are not debris covered but dirty (i.e. it is clean ice for automated mapping), d) where is the glacier outline here? (this is the interesting stuff!), e) as the thermal signature is not unique and the spatial resolution is much coarser, how is this compensated?, f) where is the glacier in the right hand image?, g) the left image has lots of snow, please adjust the caption for f) and g) to be clear what are the issues here.
We have moved all visual examples to the main text. Those images have to be described properly(long space are necessary for explanation) as your comments, then we separated figures.

Fig. S2: The orange line might be more correct than all others (see example on page 8). Is there an image available showing that there is bare rock under the snow?
We have added Google Earth™ image and explanation in Fig. 10a.

Typo issues
P2801, L4: A glacier inventory …
We revised it

P2801, L15: Pfeffer et al (with f)
We revised it

P2801, L19/21: over high mountain Asia (remove 'the', here and elsewhere)
We revised it

P2802, L7: We selected Landsat scenes
We revised it

P2802, L9: Reference System 2 (WRS1 is for MSS)
We revised it

P2804, L17: were delineated differently (not all are inaccurate)
We revised it

P2806, L4/8: biases: I would call these errors
We will use errors instead of biases here since the differences against elevation from ICESat are both under/over-estimation.

P2809, 16/17: DN is a unfortunate abbreviation as it is already occupied by 'digital number'.
We do not use DN here as abbreviation.

P2818, caption: 'Footprints of Landsat scenes ...'

We revised it

P2827, caption Fig. 10 (last line): These are false-colour composites

We revised it

References

Bolch, T., Pieczonka, T., and Benn, D. I.: Multi-decadal mass loss of glaciers in the Everest area (Nepal Himalaya) derived from stereo imagery, The Cryosphere, 5, 349-358, doi:10.5194/tc-5-349-2011, 2011.

Kääb, A., Berthier, E., Nuth, C., Gardelle, J., and Arnaud, Y.: Contrasting patterns of early 9 twenty-first-century glacier mass change in the Himalayas, Nature, 10 doi:10.1038/nature11324, 2012.