Reply to 1st reviewer

Thank you for your positive and valuable comments. We feel certain that our manuscript has improved by your valuable comments. We changed several reply to your comments from previous our reply in 10 Oct. 2014. We wrote revised part in red in the revised main manuscript and supplement.

We have replied to your comments as follows.
Your comments are written in Arial font (Blue). And our replies are written in Times New Roman (Black).

Main revised parts
1) We used revised glacier outlines. Therefore, some median elevation of glaciers in the Fig. 2 has changed. Then, the RMSE of median elevation and ELA are also changed.
2) We have changed the input reanalysis data from NCEP/NCAR to ERA-Interim according to your comment.

General comments:
One reviewer suggests that this would introduce a bias when analyzing elevation variables (page 2; Paul 2014; http://www.the-cryospherediscuss.net/8/C1415/2014/tcd-8-C1415-2014-supplement.pdf), as Sakai et al. (2014) do in this study with median glacier elevations. The issue of such a potential bias due to the procedure used to generate the GGI outlines as well as how it may have affected the results presented in the paper needs to be addressed by the authors.
In the Nuimura et al. (2014), they compared GAMDAM glacier inventory with ICIMOD inventory (Bajracharya et al., 2011), which has relatively high quality. We added the detail comparison in the head of discussion '4.1.1. Bias of median elevation derived from GAMDAM glacier inventory'.

In addition, I think the manuscript needs (1) further justification for using coarse resolution reanalysis data without correction for a climatic mass balance calculation on a much higher resolution grid,
We have calculated using ERA-Interim in the revised version.

and (2) further discussion about the implications of the methods and assumptions employed in this study on the interpretation of the results. These points are elaborated below in my specific comments.

We reply below to your specific comments.

Specific comments

P3630, L12: What regions in Fig. 1 are referred to with “arid High Mountain Asia?”

This figure shows not only the arid region of High Mountain Asia, but also entire region. I revised the figure’s explanation as follows: "Figure 1. Study area; High Mountain Asia. Region name and location of grid where the GGI occupied."

P3634, Sect 2.2: The assumption that the median glacier elevation coincides with the ELA might be valid if the glaciers are in equilibrium, depending on their hypsometry. Is it reasonable to assume that all glaciers in High Mountain Asia are in equilibrium over the study period? Even all the glaciers in each 0.5° grid cell? How does this assumption affect the validity and interpretation of your results? This needs to be addressed in the discussion section.

Thank you for your comment.

Your comment (the assumption that medina glacier elevation coincides with ELA, if glaciers are in equilibrium condition) is appropriate if both mass balance gradient and glacier area is constant for all altitude. But, actual mass balance gradient is different between upper and lower part of glaciers. Further, glacier area-altitude distribution is not uniform. Then, glaciers do not have to equilibrium state when we assume median elevation is equal to ELA (AAR = 0.5).

In this paper, we calculated precipitation at ELA assuming that median elevation (averaged at each 0.5° grid cell) coincides with ELA (not assuming glaciers are in equilibrium state). We thought that what you would like to say here is that assumption that median elevation of glaciers correspond with ELA can be apply for all glaciers in high Asian mountains or not. We have already shown that median elevation of glaciers are in accordance with observed ELA with the RMSE is 71 m in Fig. 2. But, the observed site on ELA are limited in the high Asian mountains, then, we added section '4.2 Median elevation of glaciers' in the discussion, and discussed on our result using AAR reported by Scherler et al. (2011).

P3635, L9: Since this paragraph is meant to support one of the fundamental assumptions in the analysis, I think a simple statement about the observational data summarized in Table S1 should be added (e.g. about the number of glaciers considered (which is small), maybe the average length of the observation period), to allow the reader to more easily evaluate the comparison.

Thank you for your comment. Here, we added simple statement on the Table S1.

" We have nine observed glaciers, which average observed period was 29 years."

In addition, more information is needed about the methodology of comparing "observed ELA with median elevation derived from each GGI (Nuimura et al., 2014) using ASTER GDEM (ver. 2)."

We added extracted grid size of ASTER GDEM.

L25: On what basis was the resolution of the computational grid selected?

Seasonal variability of precipitation is the most significant factor for reconstructing precipitation at ELA of glaciers. Then, we select 0.5×0.5 degree grid, which is the same resolution of APHRODITE data set. We added those reasons in the revised manuscript.

P3636, L1: Please clarify what is meant by an area-weighted average of the median glacier elevation.

We added following descriptions there.

" Small glaciers have large variation of median elevation since they distribute upper or lower separating from main large glaciers. Further, small glaciers have relatively short response time to the climate change (i.e. they would not record climate in the past (few decades ago)). Hence, in order to represent median elevation of large glacier, we calculate median glacier elevation, area-weighted average at each 0.5 degree grid using GGI in High Mountain Asia."

P3637, L8: See my general comment about addressing concerns raised during the review of Nuimura et al. (2014) that some of these excluded areas are part of glacier accumulation areas.

We have already replied to this comment at your first comment. Please see above.

L20: The example given in Fig. S1 would be strengthened if it demonstrated the result for a real glacier that receives a significant amount of avalanche accumulation. Does it reproduce the observed snow line? If not, what is the potential bias? The impact of the assumptions in the W-median calculation needs to be discussed since (1) it has a strong influence on the median glacier elevation (an increase of ~750 m from L-median) and therefore also on the estimated climatic conditions, (2) important conclusions are being drawn on the basis of the W-median elevation (e.g., all differences between calibrated PL and PW are being attributed to avalanche accumulation in Sect. 3.2), and (3) there is a focus on the W-median results in the discussion section (e.g., Sect. 4.2.2 and 4.3).

Thank you for your valuable comments. We totally agree with you.

Snow line altitude detected by satellite image has large variation, therefore, it is difficult to discuss difference or correspondence between snow line altitude and W-median elevation (average elevation).

We have added on potential bias of W-average elevation in the discussion section (4.1.2). 'we estimate W-average elevation by assuming that the maximum altitude of the ground in the grid cell correspond with the highest altitude of glacier basins. For more ideal estimation for W-average elevation, we should calculate at every basin, not every grid cell. Because mountain peak at glacier headwalls sometimes sort out different grid cell from those glaciers. Those miss segmentations of glaciers and mountain peak would lead to under/over-estimation of W-average elevation. For example, grid B (Fig. 12) with extremely large A_p would be explained by under estimation of W-average elevation by those miss segmentations (see detail in the last section).'

L27: Under what circumstances does the L-median exceed the W-median elevation? How many pixels were corrected?

When hypsometry upper than median elevation has convex curve, the L-median exceed the W-median elevation. Corrected grids number was 413. We add the details in the revised manuscript.

P3638, Sect 2.4:

- Please include the spatial resolution of the NCEP/NCAR reanalysis and indicate which model or pressure levels the temperature and geopotential height data were taken from.

We changed the input meteorological data from NCEP/NCAR to ERA-Interim.

The spatial resolution of the ERA-Interim reanalysis data set was 0.75º×0.75º. The pressure levels of the temperature and geopotential height data were taken from 300 to 850 hpa (300, 350, 400, 450, 500, 550, 600, 650, 700, 750, 775, 800, 825, 850 hpa), which cover all average median elevation of each grid. The temperature was estimated as temperature at free atmosphere by interpolation (not extrapolation), assuming that the temperature gradient was linear with elevation.

- Please include the spatial resolution of the APHRODITE dataset as well as a reference.

The resolution is 0.5º×0.5º. We added in the revised manuscript.

-More justification is needed for using meteorological forcing data at 2.5° resolution without correction to perform a mass balance computation on a 0.5° grid, in particular over regions with complex topography that will be highly smoothed in the reanalysis data. Please see my comments for Sect. 4.1.

Thank you for your comment.

Daily reanalysis data from both NCEP/NCAR and ERA-Interim have compared with the observed data near/on glaciers (Fig. S2). And we concluded that ERA-Interim has better consistency with observed data. Then, we concluded that ERA-Interim will be used for following glacier mass balance calculation.

- More information is needed about how the temperature at the median elevation is computed. To me, it sounds as though free atmosphere air temperatures at the altitude of the median elevation were used, rather than near-surface temperatures?

We used free atmosphere air temperatures since surface elevation in reanalysis data, both the NCEP/NCAR and ERA-Interim, have coarse resolution. Then, surface elevation do not correspond with the actual surface elevation, in particular in high relief mountain regions.

We added equation of air temperature calculation. Eqn. (1)

Sect. 2.5:

- Throughout the manuscript: amend "mass balance modelling" to "climatic mass balance modelling," since only mass fluxes in the top 20 m (according to Fujita et al. 2011) are considered.

Thank you for your comment. We revised them.

- I suggest including the energy and mass balance equations (e.g., Eqns. 1 and 2 of Fujita et al. 2011), since they would help to clarify the first paragraph.

We added those equations in the revised manuscript.

- Given that near-surface humidity is available and the latent heat flux is computed, why are only negative surface vapour fluxes considered in the climatic mass balance model?

We considered both positive and negative surface vapor fluxes. And equation of phase of precipitation has moved to the end of glacier mass balance equation (2).

P3639, L1: P_{cal} and P_{ap} in Eqn 1 are not explicitly defined (I suggest including the symbols in the previous sentence). It's unclear if all-phase precipitation is tuned or only solid precipitation as calculated in Eqn 2?

Thank you for your suggestion. We added those symbols in the previous sentence.

P_{cal} and P_{ap} includes all phase of precipitation. Only separated solid precipitation will be related in calculation for tuning. We added the explanation.

L6: How sensitive is the evaluation of winter balance in Sect. 3.3 to the relationship in Eqn 2?

We have added Fig. S6, which shows the relation between observed winter balance and calculated snow amount with errors caused by not only T_l (temperature that all precipitation become liquid) difference, but also air temperature, solar radiation and median elevation. Error caused by uncertainty of T_l was not so large comparing to errors caused by solar radiation and air temperature.

How is liquid precipitation treated by the mass balance model?

If there is snow layer at the surface, and snow covered ice layer is lower than zero degree, some amount of rain (liquid precipitation) and meltwater will be refreezed on the ice layer, that amount corresponds with the heat by increasing ice temperature. The refreezed ice will contribute to mass balance of glacier.

If there is no snow layer on the surface, or if the ice has zero degree temperature, liquid precipitation and meltwater will release as discharge, and the discharge does not contribute to mass balance of glacier.

L16: Please remove redundant occurrences of this phrase.

Thank you for your comment. We removed this sentence.

P3640, L11: To be clear, does Fig. 6 show all-phase annual precipitation or only that determined to be solid using Eqn. 2? I suggest adding a reference to Eqn. 1 after “calculated precipitations” to clarify that these are the tuned fields.

Thank you for pointing out the unclear part.

We added "solid and liquid precipitation" in the explanation of the Fig. 6.

And we revised the Line 11-12 page 3640 "Figure 6 shows that annual precipitation of APHRODITE and calculated precipitations (:P_{cal} in Eqn. 1) at median elevation ...".

L16: “These calculated precipitations at ELA reflect regional climate in High Mountain Asia.” The calculated precipitation amounts also reflect (1) the imposition of equilibrium conditions and (2) any potential initial errors in the APHRODITE dataset - this needs to be mentioned.

Thank you for your suggestions.

We agree with your comments and we will discuss on inconsistency between median elevation of glaciers and ELA (1).

As for (2), median elevation usually differs from average altitude of each grid. Precipitation has also gradient with altitude. So, it is reasonable that there is difference between original APHRODITE data and calculated precipitation at Median elevation. There might be potential initial errors in the original APHRODITE dataset, but, we cannot say that from our result.

P3641, second paragraph: The first sentence is incomplete and details of Fig. S5 are not discussed in Sect. 4.2.1, as suggested by the text. I suggest moving this paragraph to Sect. 4.2.1.

Thank you for your comments.

We made the sentence complete and then move it to Section 4.3.1, since we insert one section before 4.2 (in previous version).

In addition, I'm confused as to why (1) the L-median curves in Fig. S5 and Fig. 8 differ and (2) error analysis was only performed for the L-median category?

(1) This was our mistake. We have revised both Fig. S5 and Fig. 8 calculated using ERA-Interim.

(2) Because errors of calculated precipitation depend mainly on annual precipitation. So, there is no large differences of error among the result based on G-, L-. W-median. We will add those descriptions in the revised manuscript.

L14: "1979 to 2000." I would clarify this sentence as, "We compared the snow amounts calculated from 1979 to 2000 at the G-, L-, and W-median elevations with observed winter balances, using the value from the corresponding grid cell"

Thank you for your comment. We will revise as your comment.

L18: The evaluation of APHRODITE here is sensitive to (1) the relationship used to distinguish solid and liquid precipitation (Eqn 2) and (2) the low-resolution air temperature data used as input, both of which may contribute to an underestimation of solid precipitation in addition to any original biases in the dataset.

Thank you for your comments. We agree with your comments.

We have added error of calculated snow due to errors of air temperature, solar radiation, average elevation, and equation of distinguishing solid and liquid precipitation in the Fig. S6.

P3642, Sect. 4.1: Small comments:

- I suggest incorporating this section in Sect 2.4, since it is largely a justification of the method.

We moved this section 4.1 to Section 2.4. And we also compared observed data and reanalysis data, ERA-Interim and NCEP/NCAR.

- Please provide the three computed median elevations in Table S2 for comparison with the actual altitude of the AWS (i.e. are the AWS located at the median elevations?).

We added median elevations (G- L- W-) at corresponding grid.

- On what basis were the nine AWS selected?

Observed meteorological data, in particular on glacier or near the terminus of the glacier, are very rare in the High Asian Mountains. These AWS data are not selected, but all data observed by our group. We added the description in the revised manuscript in section 2.4.1..

- Should L12 read, “reanalysis data is greater than observed data,” to be consistent with the next two sentences?

Thank you for pointing out our mistake. We have decided to use ERA-Interim as input meteorological data. Then, this discussion has removed.

In addition to these small changes, I think the evaluation of the meteorological data needs to be greatly expanded. While the agreement between observed air temperature and the value provided by the NCEP/NCAR reanalysis is encouraging, no conclusions can be drawn about the quality of the forcing data over the whole region and study period on the basis of such a small number of weather station records. Depending on what criteria were used to select the weather stations in Table S2, can more records be considered? Additional evaluation could also be performed over a shorter but more recent period (e.g., after 2000) using higher resolution atmospheric datasets (e.g., ERA Interim or the High Asia Reanalysis of Maussion et al. 2014).

We have changed the input meteorological data from NCEP to ERA-Interim as your comment. And we also analyzed error caused by reanalysis data, Ta and SR in Fig. S8.

P3643, Section 4.2.2: The categorization of glaciers in High Mountain Asia into seasonal precipitation regimes has been reported by Bookhagen and Burbank (2010) for the period of 1998 to 2007 based on observations, and by Maussion et al. (2014) for 2000-2011 based on high-resolution atmospheric simulations. A discussion is needed of how their findings relate to those presented here, given (for example) differences in the study period, the resolution of the computational grids, and the methods employed. If possible, the authors should repeat their analysis considering all four seasons, as recent studies have found that spring accumulation is important for many glaciers in High Mountain Asia (Yang et al. 2013; Maussion et al. 2014)

Thank you for your comment.

We have added Fig. S9 (Contribution of DJF, MAM, JJA, and SON to the APHRODITE mean annual precipitation) and discussion on the seasonal change of precipitation in the discussion section.

L22: Remove Gardelle et al. (2013) reference, as the authors of this paper cite Bookhagen and Burbank (2010) for this statement.

Thank you for your comment. We revised.

P3644, Sect. 4.3: I suggest shortening and clarifying this section.

2nd reviewer Prof. Braithwaite has given comments in this section saying that another discussion on A_p (adjusting ratio of APHRODITE precipitation) is necessary. So, this section become longer than the 1st manuscript.

P3645, Sect. 5: The conclusion section does not add anything to the paper, other than rehashing points from the discussion section. It would be good to include perspectives on future research on the basis of this work and mention the wider significance of the results.

Thank you for your comment. We have added future study using our produced precipitation at the last of conclusion section.

P3645, L24: This sentence is misleading, since the calibrated precipitation was obtained by assuming the ELA coincided with the median glacier elevation.

Thank you for your comment. We revised.

P3646, L7: This sentence should be amended, since Fig. 6d very clearly shows extreme values. Perhaps "reduces the number of extreme…"

Thank you for your comment. We agree with your comment. We revised.

Technical corrections

There are a number of single-sentence paragraphs, which should be incorporated into larger paragraphs.

Thank you for your comment. We have incorporated several single-sentence paragraphs into larger paragraphs as you suggested.

P3630, L12: "receive less precipitation", "makes a greater contribution" ?

We revised as your suggestion.

P3631, L1: Move reference to end of sentence (I assume)

Adam et al., (2006) has pointed out that almost all dataset (provided by Chen et al., 2002; New et al., 2000; Huffman et al., 1997) do not consider orographic effects. So, we did not change the location of these references.

L4: "Almost all datasets" We revised.

L29: “had a large discrepancy” We revised as your comment.

P3632, L6: “detailed We revised.

L26: “The centre of our target region is the Tibetan Plateau, whose elevation is…”

We revised.

P3633, L13: “located at,” “westerlies” We revised.

L25: “came from oceanic sources” We revised.

P3634, L3: “The regions” We revised.

P3635, L4-6: I would suggest combining back-to-back brackets throughout the text, i.e.“Paul et al. (2002; Swiss Alps)” We revised.

L8: “Paul et al., 2009” We revised.

L9: “from the GGI” We revised.

P3636, L13: “as glaciers located” We revised.

P3638, L7: “two geopotential heights bounding/containing the median elevation” ?

Thank you for your comment. We revised.

L12: replace end of sentence with something less repetitive, e.g., “at all three median-elevation categories.” We revised.

L24: “0°C” We revised.

P3639, L16: “area-weighted means at each…” is unnecessarily repeated

We deleted this sentence.

L17: “grids points” ? We revised.

L18: Should that read “distribution of L-median elevations”

We have revised substantially this sentence.

P3640, L13: “calculated precipitation amounts” We revised.

P3641, L9: I think “Evaluation” is more appropriate than “Validation” We revised.

L13: Table S3 is mentioned before Table S2

We have moved the comparison between observed meteorological data and reanalysis data in the method section, then Table S2 appear before Table S3. Then, we did not change the table number.

P3645, L1: “show similar altitude” or “are similar” We revised.

L27: “which included“,”< 1 km2” We revised.

Figures

Figure 5: The color scale makes the information very hard to read (especially in print form). For example, glaciers on the Tibetan Plateau appear to be somewhere between 6000 and 8000 m a.s.l.

We revised the color bar.

Figure 6: Same comment as for Figure 5, for panel a) in the 0-500 mm yr-1 range

We revised the color bar.

Figure 9: Include the name of the data set plotted in the caption.

We added name of data set.

Figure 11: The contour intervals between 0 and 1 are difficult to distinguish.

We revised color bar between 0 and 1.

Supplementary information

- Provide the full author list.

We added full author list.

- Can Figs. S5 and 8 be combined?

We have tried, but, it is so complicate. So, we have separated.

- Figure S10: My suggestion for the caption would be, “Relations between reanalysis data and observations for (a) air temperature and (b) downward solar radiation. All data are daily means.”

We revised as your suggestion. Fig. S10 moved to S2 in the revised manuscript.

References

Bajracharya SR and Shrestha B (2011) The status of glaciers in the Hindu Kush–Himalayan region. International Centre for Integrated Mountain Development, Kathmandu

Bookhagen, B. and Burbank, D.: Toward a complete Himalayan hydrological budget: spatiotemporal distribution of snowmelt and rainfall and their impact on river discharge, J. Geophys. Res., 115, F03019, doi:10.1029/2009JF001426, 2010.

Fujita, K., Takeuchi, N., Nikitin, S. A., Surazakov, A. B.,Okamoto, S.,Aizen, V. B. and Kubota, J.: Favorable climatic regime for maintaining the present-day geometry of the Gregoriev Glacier, Inner Tien Shan, The Cryosphere, 5, 539-549, doi: 10.5194/tc-5-539- 2011, 2011.

Gardelle, J., Berthier, E., Arnaud, Y., and Kääb, A.: Region-wide glacier mass balances over the Pamir-Karakoram-Himalaya during 1999–2011, The Cryosphere, 7, 1263–1286, doi:10.5194/tc-7-1263-2013, 2013.

Gardner, A. S., Moholdt, G., Cogley, J. G., Wouters, B., Arendt, A. A., Wahr, J., Berthier, E., Hock, R., Pfeffer, W. T., Kaser, G., Ligtenberg, S. R. M., Bolch, T., Sharp, M. J., Hagen, J. O., van den Broeke, M. R., and Paul, F.: A reconciled estimate of glacier contributions to sea level rise: 2003 to 2009, Science, 340, 852–857, doi:10.1126/science.1234532, 2013.

Maussion, F., Scherer, D., Mölg, T., Collier, E., Curio, J., and Finkelnburg, R.: Precipitation seasonality and variability over the Tibetan Plateau as resolved by the High Asia Reanalysis. J. Climate, 27, 1910-1927, doi:10.1175/JCLI-D-13-00282.1, 2014.

Scherler, D., Bookhagen, B., and Strecker, M. R.: Hillslope glacier coupling: the interplay of topography and glacial dynamics in High Asia, J. Geophys. Res., 116, F02019, doi:10.1029/2010JF001751, 2011.

Yang, W., Yao, T., Guo, X., Zhu, M., Li, S., & Kattel, D. B.: Mass balance of a maritime glacier on the southeast Tibetan Plateau and its climatic sensitivity. J. Geophys Res, 118, 9579–9594. doi:10.1002/jgrd.50760, 2013.

Reply to 2nd reviewer Prof. Braithwaite

Thank you for your very positive and valuable comments. We are happy to receive your comments for improving our manuscript. We changed several reply to your comments from previous our reply in 10 Oct. 2014. We wrote revised part in red in the revised main manuscript and supplement.

Your comments are written in Arial font (Blue). And our replies are written in Times New Roman (Black).

Main revision

We have changed two input data to calculate precipitation at median elevation. One is meteorological data and the other is glacier inventory. Input meteorological data was changed from NCEP/NCAR to ERA-Interim. And revised GAMDAM glacier inventory was used to calculate median elevation of glaciers in this revised manuscript. Then, please notice that the difference of calculated precipitations between first and revised manuscript caused by not only reanalysis data but also revised glacier inventory.

We have used revised version of GAMDAM glacier inventory, which detail was written in Nuimura et al., (2014) " The GAMDAM Glacier Inventory: a quality controlled inventory of Asian glaciers" in TCD.

Looking at the discussion on pages 3635-3637, I think the authors should use the terms G-average elevation, L-average elevation and W-average elevation to make clear that these are different ways of averaging the median elevations of individual glaciers within each 0.5 by 0.5 degree grid square. The glaciers are characterised by size (area), and the different averages G-, L- and W- take account of different effects of local precipitation, avalanching etc.

We revised from G-, L-, W-median elevation to G-, L-, W-average elevation in accordance with your comment.

Important parameters in the classic glacier inventories like the First Chinese Glacier Inventory (Shi, 2008) are primary classification of glaciers, e.g. ice caps, outlet-, valley and mountain-glaciers, and aspects of ablation and accumulation areas but these do not appear to be evaluated in the GAMDAM Glacier Inventory. Aspect has a well-known control on the ELA itself and local precipitation conditions expected there (Evans, 1977 and 2006). Similarly, the primary classification of a glacier has a strong control on both glacier size and precipitation conditions. If these two parameters are not evaluated in the GAMDAM inventory it would be worth considering an update to the inventory.

Thank you for your suggestions and introduction of two interesting papers. We also thinking that analysis on ELA depending on size, glacier type, and aspect are very interesting. Actually, we need revision of our inventory, now. Your suggestion will be addressed in our future study after revision of our inventory.

MINOR ISSUES The paper is generally well arranged in sufficiently good English to understand the main points. There are, however, many “micro-errors” that need correction by an English language specialist. This includes missing or superfluous definite and indefinite articles, verb agreements with nouns etc. I make specific small points on the following:

P. 3631 line 3: I assume you mean “Almost all datasets. . .”

We revised.

P. 3631 line 29-30: The sentence “However, the estimated ELA has a large discrepancy with glacier distribution” needs rephrasing as it is presently meaningless.

We changed the sentence into the past tense as 1st reviewer's comment.

P. 3633 line 4: “Hamper” should be “Harper”.

Thank you for pointing out the error. We revised.

P. 3634 line 6 to 9: This would probably read better as “Most precipitation in the interior of High Mountain Asia originates from recycled evaporation, and such a proportion of continental recycling cannot be found in the other continents”

Thank you for your correction. We revised.

P. 3635 line 9: I suggest “. . .the few observed ELA with . . .”

We revised.

P. 3635 line 25: I suggest “. . .by area-weighted averages of median elevation for individual glaciers”.

We revised as your suggestion.

P. 3637 lines 16-17: I find “The glacier mass balance, however, usually is calculated from only direct precipitation as an input meteorological data” nearly meaningless. Are you talking about mass-balance models?

Yes, we are talking about mass-balance models. We agree with your comment. We deleted.

C1802

P.3637 lines 27 to 28: I suggest “If calculated, W-median elevation is less than or equal to L-median elevation”.

It was our mistake. We will correct as follows, also taking into account the 1st reviewer's comments:

"When glacier hypsometry upper than median elevation has convex curve, the L-average elevation exceeds the W-average elevation, in that case, we assumed that W-median elevation is equal to L-median elevation."

P. 3638 line 17: Kondo (1990) is not listed in reference list.

That was our mistake. We should refer Kondo and Xu (1997). We changed the text and added in reference list.

P. 3642 lines 3 to 4: Fujita and Ageta (2000) may have said what you say, but they are over-simplifying as there will be different ablation rates for ice, snow and debris-covered ice, so ablation must depend upon precipitation as well as air temperature and solar radiation.

We added one sentence ' Ablation of glaciers depends on several elements, such as snow, albedo of the glacier surface, air temperature, solar radiation, longwave radiation.'

We have moved this section to 'method' section because first, we evaluated the reanalysis data sets to select input meteorological data.

P. 3642 line 24: You quote Braithwaite et al. (2006) but Braithwaite (2008) would also be a useful reference.

Thank you for your comment. We have added new section '4.2.4 Annual temperature range' referring Braithwaite (2008).

P. 3643 line 5: Braithwaite (2008) is a useful extension of the paper by Ohmura et al. (1992) as it takes account of the different climate settings of the glaciers with a family of curves.

Thank you for your comments. We also analyzed the relation between summer temperature and precipitation, classified by annual temperature range (Fig. 5 in Braithwaite (2008)).

Please, read below comments on P. 3660 Fig. 8.

P. 3643 line 19. I once tried (unpublished!) to map glacier precipitation across High Mountain Asia using the degree-day model extrapolated to the median elevation in the World Glacier Inventory, and I found problems in especially the Mount Everest region. This was because air temperatures were too low to give any meaningful melt at the ELA in this region. Presumably zero balance at the ELA is maintained by relatively large sublimation.

Thank you for your very interesting comment.

In our result, calculated precipitation using heat balance method in Everest region has reasonable results (200-300 mm/yr) comparing to Pyramid data (Italian group). In more arid regions, for example, West Kunlun, East Pamir, and southern central Tien Shan receive much less precipitation (less than 200 mm/yr). Zero mass balance at the ELA would be maintained by large sublimation.

P. 3644 lines 20 to 25: It is interesting that you adjustment ratios less than unity in some parts of your region. Braithwaite et al. (2002) compared “glacier precipitation” with precipitation from a gridded climatology (“regional precipitation”) and found ratios of about 2 to 2.5 for many regions, but the ratio was closer to 1:1 for the Alps. Braithwaite et al (2002) suggested that the Alpine part of the climate dataset contained relatively higher-lying stations than other parts of the dataset. Could that be true of the regions where you find a lower adjustment ratio?

We have added discussion about the adjustment ratio with less than unity in the section 4.3. We focused at Himalaya and Karakoram.

P. 3645 line 27: You probably mean < 1 km2 here?

Thank you for your comment. This is our mistake. We revised.

Pages 3647 to 3652 Reference list. I have checked your reference list and only missing item is Kondo (1990).

Thank you for your attentive check. We added Kondo and Xue (1997) instead of Kondo (1990) because it was written in Japanese.

P. 3653: Nice map! Thank you!

P. 3654: To what do the coloured dots refer? Are these decade averages of observed ELA? On how many glaciers?

We added the explanation of colored circles, and number of glaciers.

P. 3657 Fig. 5: I cannot see much difference between these three maps. What about mapping (a) differences L-median minus G-median, and (b) W-median minus G-median?

Actually, grid numbers of G-average elevation and L-average elevation is different. Therefore, we did not make figures different average elevations. We changed color scale. Difference between L-average elevation and W-average elevation is shown in the Fig. S2.

P. 3658 Fig. 6: It is interesting (and important) that (a) misses the high precipitation in SE High Mountain Asia.

Thank you for your important comment. Calculated precipitation using ERA-Interim at SE High Mountain Asia was not so large. Because NCEP/NCAR has relatively large solar radiation (less cloud intercept effect) than ERA-Interim. In the SE Asia (Hengduan Shan), difference between P_L and P_W were large, in other words, the avalanche effect on mass balance of glacier is large as shown in Fig. S5.

P. 3660 Fig. 8: Note that Figure 5 in Braithwaite et al (2008) shows how the Ohmura dataset can be split into high- and low-accumulation situations using annual temperature range.

Thank you for your comment. We added a T-P plot at different annual temperature range in Fig. 11. Please note that in the previous reply, we calculated annual temperature range using daily temperature. In this revision we calculated them based on the monthly temperature.

Then, we found that recent mass gain area (Karakoram, Pamir, Tibetan Plateau) correspond with the high annual temperature range regions (low sensitivity to climate change regions). We also added those discussion.

P. 3661 Figure 9: Very good! Do you discuss why you use 40% summer precipitation and not 25% or 50 %?

As we wrote in the first manuscript, "Hengduan Shan, Bhutan, Everest, and West Nepal are strongly influenced by the Indian and Southeast Asian summer monsoons, and glaciers are summer-accumulation type glaciers (SAG). On the other hand, the climate in Pamir, Hindu Kush, and Karakoram are dominated by the westerlies, and glaciers there are winter-accumulation type glaciers (WAG). Himachal Pradesh and Jammu Kashmir (included in the W Himalaya in Fig. 1) are in transition zones, influenced by both the monsoon and the westerlies (Bookhagen and Burbank, 2010)"

We added contour lines with interval of 10% in Fig. 9 in the revision.

P. 3662 Figure 10: Interesting! Thank you!

P. 3663 Figure 11: Please confirm that there are some places where adjustment ratio can be 15 to 20 times.

Adjustment ratios on the basis of ERA-Interim was changed from the result based on NCEP/NCAR. We have added discussion on three grid cells with large adjustment ratio (>10) in the end of section 4.3.

References

Yatagai, A., Kamiguchi, K., Arakawa, O., Hamada, A., Yasutomi, N., and Kitoh, A.: APHRODITE: constructing a long-term daily gridded precipitation dataset for Asia based on a dense network of rain gauges, B. Am. Meteorol. Soc., 93, 1401–1415, doi:10.1175/BAMS-D-11-00122.1, 2012.