Reply to 1st reviewer

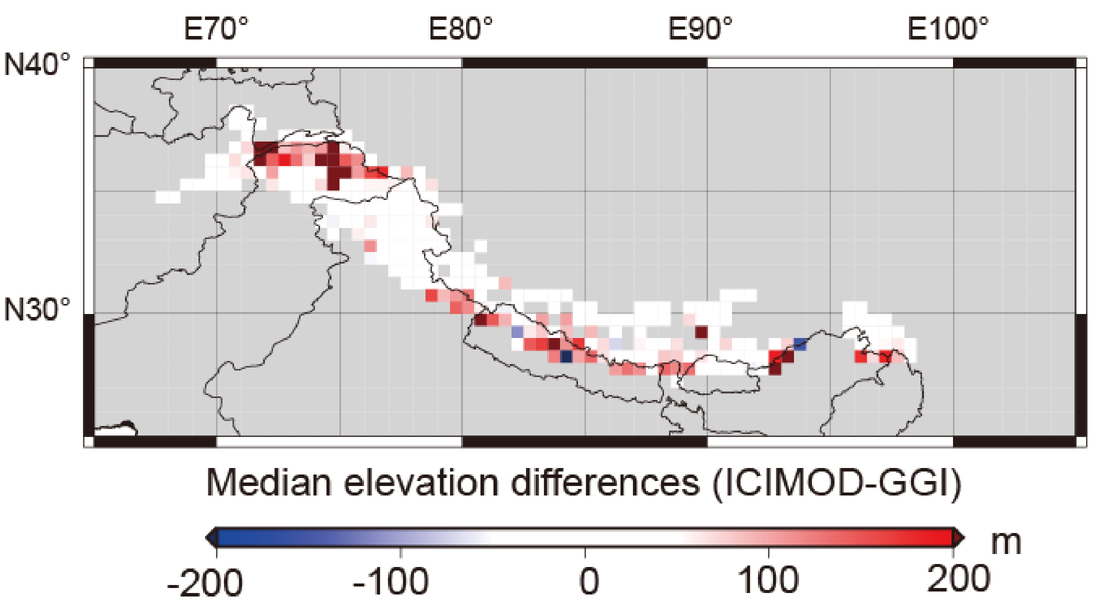
Thank you for your positive and valuable comments. We have replied to your comments as follows. Your comments are written in Arial font (Blue). And our replies are written in Times New Roman (Black).

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.

ICIMOD glacier inventory (Bajracharya et al., 2007; 2011) covers Brahmaputra, Ganges and Indus Basins, which corresponds to southern part of GGI and those regions have high relief mountain ranges. ICIMOD glacier inventory has relatively high quality, so we compared average median elevation at each 0.5 gird.

Below figure shows the difference of median elevation (ICIMOD-GGI). The white grids indicate that the difference is less than 50 m. The median elevation of ICIMOD inventory in the Karakoram, central Himalaya, and a part of SE Tibet have higher median elevations. Then, calculated precipitation which maintain zero mass balance at median elevation would be less than our results in these regions.



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,

Thank you for your comments. We will calculate 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 will 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 rigion. I will revise 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 shrewd comment. The following discussion will be added in the revised manuscript.

Scherler et al., (2011) reported that AARs (they estimated the snow line altitude using satellite imageries) are greater than 0.5 in the Karakoram and the West Kunlun Shan, ~~which~~ these regions correspond with mass gain region reported by Gardner et al., (2013) and Gardelle et al., (2013). And other regions (western Himalaya, central Himalaya) have AAR of less than 0.5, which correspond with glacier shrinkage regions (Gardner et al., (2013); Gardelle et al., (2013)). Then, the ELAs tend to be lower than median elevation in the glacier shrinkage regions, and vice versa. Those non-equilibrium state would effect on our result. In the Karakoram and Pamir (mass gain regions), actual annual precipitation at median elevation would be larger than the calculated value. On the other hand, in the Himalaya, and the Hengduan Shan actual annual precipitation at median elevation would be less than the calculated value.

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 will add simple statement on the Table S1.

And we revised some glacier polygons, so, Fig. 2 will be revised.

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 will add " Each median elevation derived from each polygon of GGI was calculated from ASTER GDEM (ver.2) extracted by 30×30 m grid cell."

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

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

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

We have tried to represent relatively large glacier by area-weighted average of median elevation because we have following two reasons behind it. Small glaciers have large variation of median elevation since small glaciers used to distribute upper or lower separating from main large glaciers. Second reason is that small glaciers has short response time to the climate change, so, small glaciers would not record past (few decades ago) climate.

Then, we used area-weighted average median elevation as representative value at each grid.

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).

We totally agree with your comments.

One of the assumptions to estimate W-median elevation is that the maximum altitude of the ground correspond with the highest altitude of glacier basins. It is possible that the highest altitude of glacier basins would be lower than the maximum altitude of the ground. So, the upper ground area of glacier basins might be overestimated. Then, our calculated W-median elevation might be overestimated. We will add them in discussion.

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 47. We will add 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.

The spatial resolution of the NCEP/NCAR reanalysis data set was 2.5º×2.5º. And the pressure levels of the temperature and geopotential height data were taken from 300 to 850 hpa (300, 400, 500, 600, 700, 850 hpa), which cover all average median elevation of each grid. The temperature was estimated by interpolation, assuming that the temperature gradient was linear with elevation. We did not estimate temperature at each median elevation by extrapolation. We will change the reanalysis data from NCEP/NCAR to ERA-Interim. So, we will add those information of ERA-Interim in the revised manuscript.

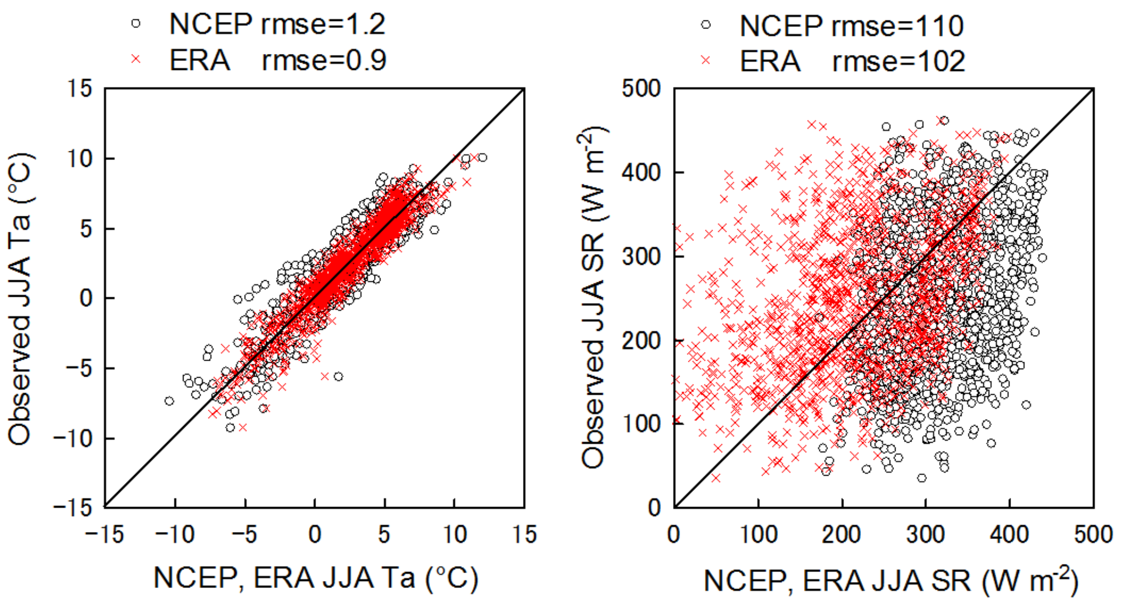
- Please include the spatial resolution of the APHRODITE dataset as well as a reference.

The resolution is 0.5º×0.5º. We will add it 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.

Here we compared observed temperature and solar radiation and those of ERA-Interim with the resolution of 0.75°×0.75°. In particular, accuracy of temperature was better, so, we will use ERA-Interim in the revised version.



- 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.

In Fig. S6a, Reanalysis Ta was calculated from temperature at each geopotential height.

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 will revise it.

- 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 will add 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 also considered negative surface vapor fluxes = condensation. We will add 'condensation' in the revised manuscript.

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 appropriate suggestion. We will add the symbols in the previous sentence.

Pcal and Pap includes all phase of precipitation. Only separated solid precipitation will be related in calculation for tuning. We will add the explanation.

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

The calculated snow amount have to be tuned to ablation (= incoming heat), therefore, the Eqn (2) would not affect directly on the snow amount. But, the equation have effect on snow amount through surface albedo change (snow or ice).

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 ~~at~~ 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 will remove 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 will add "both solid and liquid precipitation" in the explanation of the Fig. 6.

And we will revise 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 the imposition of equilibrium conditions (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 will make the sentence complete and then move it to Section 4.2.1.

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) We have presented previous version in Fig. S5. It's our mistake. We will calculate those T-P plot using ERA-Interim, so, all figures on the result will be revised.

(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 you suggested.

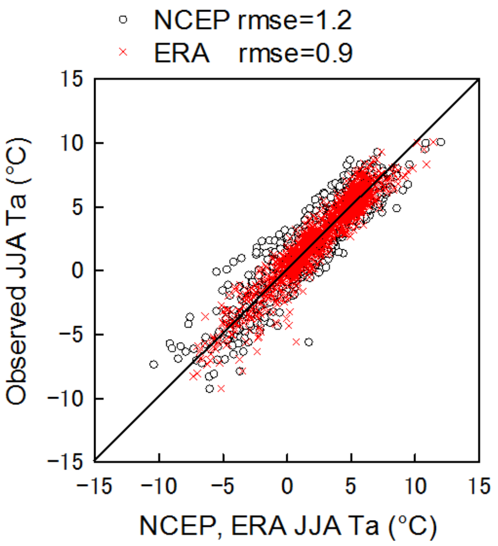
L18: The evaluation of APHRODITE here is sensitive to (1) the relationship used to distinguish solid and liquid precipitation (Eqn 2) and (2) the low-resolution air temperature data used as input, both of which may contribute to an underestimation of solid precipitation in addition to any original biases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

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

We will add the following possible reasons for the under estimation of solid precipitation in the discussion.

(1) We have no enough observed data for the relationship to distinguish solid and liquid precipitation in the entire Asian mountains. So, we will add them in the discussion that equation (2) would be one of the reason of underestimation of snow APHRODITE.

(2) We will calculate the precipitation at median elevation using ERA-Interim as you suggested. Anyway, temperature data of ERA-Interim have some error as shown in the below figure. So, temperature error might cause underestimation of solid precipitation.



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 will incorporate Section 4.1 into Section 2.4.

- 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 will add 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 limited in the High Asian Mountains. These AWS data are not selected, but all data observed by our group. Those daily data are available for us. We will add the description in the revised manuscript.

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

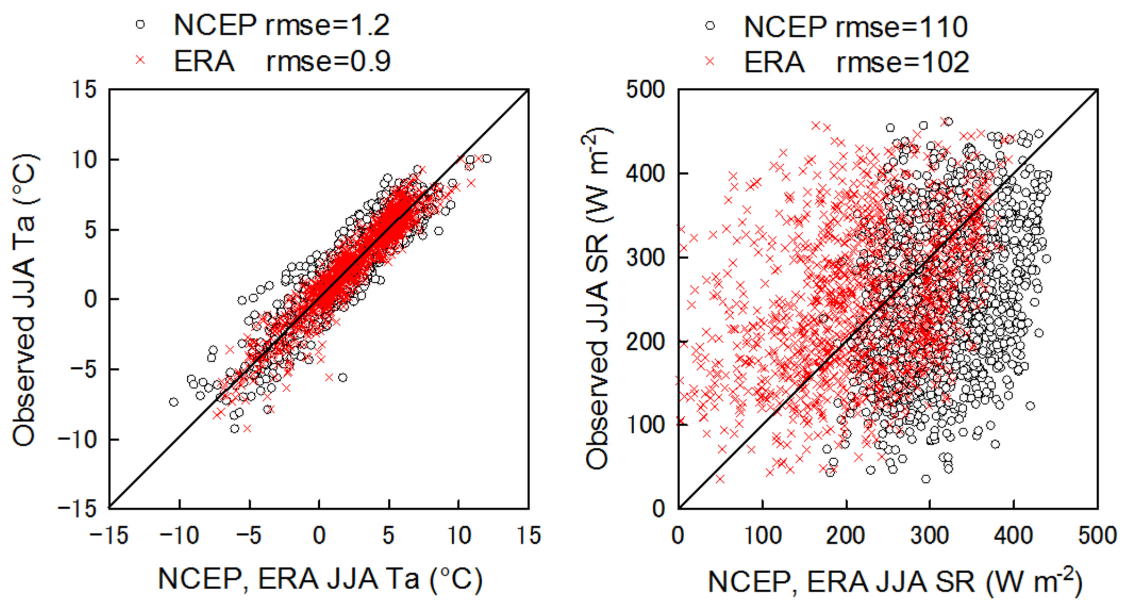
Thank you for pointing out our mistake.

We will correct as follows: 'solar radiation of reanalysis data is greater than observed data, especially in the summer time'

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).

Meteorological data summarized in Table S2 is all data measured at terminus or on the glaciers by our group. No more observed daily data near glaciers are available for us.

We have compared observed temperature and solar radiation (only JJA) with those reanalysis of ERA-Interim ($0.75^{o} \times 0.75^{o}$). As you commented, performance of ERA-Interim was better than NCEP/NCAR. For the solar radiation of NCEP/NCAR was relatively larger than observed data, on the other hand, solar radiation of ERA-Interim was less than the observed one.



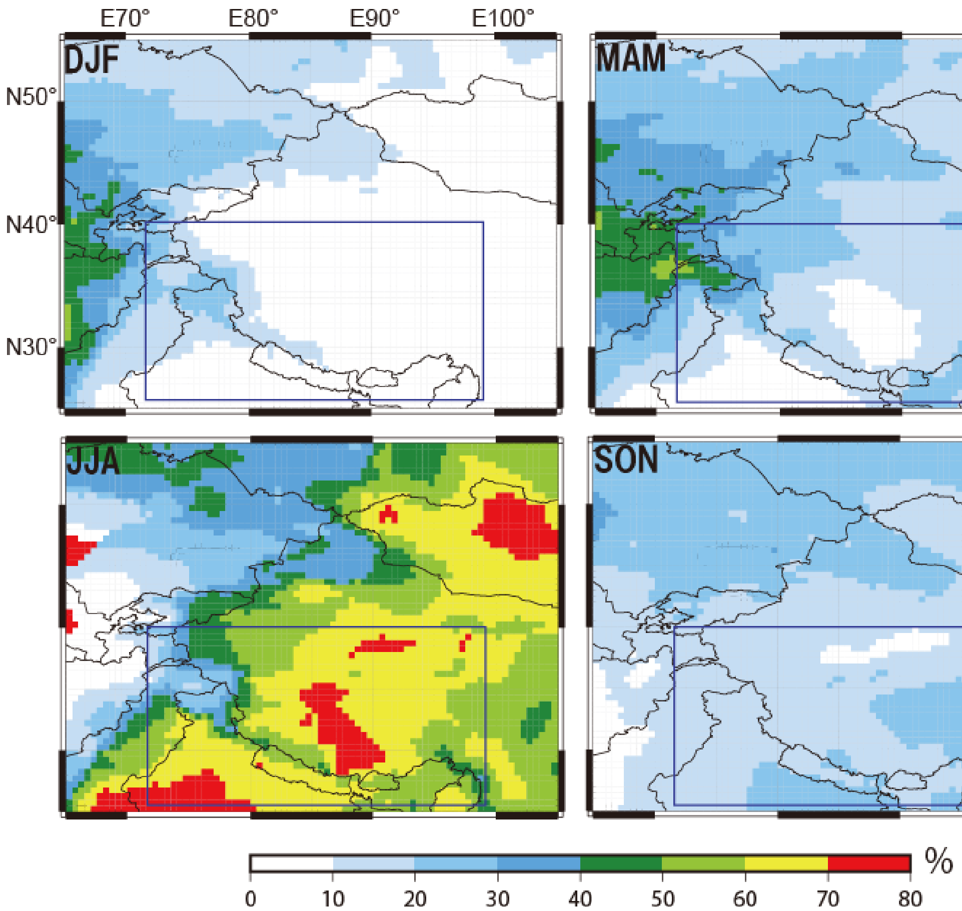
Then, in the revised version we will calculate precipitation at median elevation of glaciers using calibrated ERA-Interim data for temperature and solar radiation.

Still we have not calculated precipitation, but it is possible to expect better results. Thank you your suggestion.

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)

Bookhagen and Burbank (2010) calculated discharge using evaporation products, snow cover (MODIS) and rainfall (TRMM). And rainfall does not include snowfall. Then, we compared with Maussion et al. (2014)'s precipitation distribution.

Next figures show contributions of DJF, MAM, JJA, and SON to mean annual precipitation based on APHRODITE from 1970-2007.



Contribution of DJF, MAM, JJA, and SON to the APHRODITE mean annual precipitation from 1970 to 2007

Purple rectangle indicates the target area of HAR (Maussion et al., 2014). This figure has almost same color scale with Fig 8 in Maussion et al., (2014). The main differences of seasonal contribution between HAR and APHRODITE are West Kunlun in JJA and MAM and Karakoram in DJF. According to Yatagai et al. (2012) (Fig. 1), there are gauge stations (contributed to APHRODITE) in the Karakoram, but very few at Kunlun. Therefore, reliability of APHRODITE data are high at Karakoram, but less in the Kunlun.

Further, Maussion et al. (2014) found high variability of precipitation seasonality along the Central and East Himalayas. We could not find such high variability of precipitation seasonality in the APHRODITE products because of its coarse resolution. Such discrepancy of precipitation seasonality might cause errors in calculated precipitation.

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 will correct it.

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

We will try to shorten it. But, 2nd reviewer Prof. Braithwaite has given comments in this section saying that discussion on A_p (adjusting ratio of APHRODITE precipitation) is necessary. So, it might 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 will incorporate perspectives on future study using our current work in the revised manuscript.

As for the wider significance of the results, we will add that those calculated precipitation at the High Asian mountains would reveal the sensitivity of glaciers to climate change, and further study will contribute to estimate variation of discharge involving glacier fluctuations.

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 will revise this sentence.

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 will revise this sentence.

Technical corrections

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

Thank you for your comment. We will check all sentences, and incorporate them into larger paragraphs as you suggested.

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

We will revise as per 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 will not change the location of these references.

L4: "Almost all datasets" We will revise it.

L29: "had a large discrepancy"

We will delete this sentence according to 2nd reviewer's comment.

P3632, L6: “detailed We will revise it.

L26: “The centre of our target region is the Tibetan Plateau, whose elevation is…” We will revise it.

P3633, L13: “located at,” “westerlies” We will revise it.

L25: “came from oceanic sources” We will revise it.

P3634, L3: “The regions” We will revise it.

P3635, L4-6: I would suggest combining back-to-back brackets throughout the text, i.e.“Paul et al. (2002; Swiss Alps)” Thank you for your suggestion. We will revise it.

L8: “Paul et al., 2009” We will revise it.

L9: “from the GGI” We will revise it.

P3636, L13: “as glaciers located” We will revise it.

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

Thank you for your comment. We will revise it.

L12: replace end of sentence with something less repetitive, e.g., “at all three median-elevation categories.” Thank you for your comment. We will revise it.

L24: “0°C” Thank you. We will revise it.

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

We will delete this sentence.

L17: “grids points” ? Yes, we will revise it.

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

We will revise in accordance with your comment.

P3640, L13: “calculated precipitation amounts” We will revise in accordance with your comment.

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

We will revise in accordance with your comment.

L13: Table S3 is mentioned before Table S2 We will revise the order of these Tables.

P3645, L1: “show similar altitude” or “are similar” We will revise as you suggested.

L27: “which included“,”< 1 km2” We will revise as you suggested.

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 will try to revise the color scale. The 2nd reviewer Prof. Braithwaite has suggested that Fig. 5

should be the figure showing difference of medians, L-median minus G-median, and W-median minus G-median. So, we will add figures of their difference.

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

We will try to revise color bar of Fig. 6.

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

We will add it

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

We will try to revise color bar between 0 and 1.

Supplementary information

- Provide the full author list.

We think the author list, which your point out, is for first page of PDF file of supplement. The first page was provided by TCD side. So, we cannot touch to provide all author's name.

- Can Figs. S5 and 8 be combined?

We will try after calculation using ERA-Interim.

- 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 think you pointed out to Figure S6. Thank you for your comment. We will revise in accordance with your suggestion.

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