Review of Sakai et al.: "Climate regime of Asian glaciers revealed by GAMDAM Glacier Inventory"
August 2014

General comments:

The study of Sakai et al. (2014) estimates climate conditions at the median elevation of glaciers in High Mountain Asia over the period of 1979 to 2007, using a combination of reanalysis and gridded-precipitation data. By assuming that the median glacier elevation corresponds with the equilibrium line altitude (ELA), the authors tune the precipitation input data to yield neutral conditions in a climatic mass balance calculation at this elevation. The computations are performed on a 0.5° grid and three definitions of the median glacier elevation are considered: one that includes all glaciers in the inventory, one that excludes small outlying glaciers, and one that attempts to remove the influence of avalanche accumulation.

The analysis is performed using a new inventory of Asian glaciers called the GAMDAM Glacier Inventory (GGI), which is presented in a companion paper currently under review for publication in *The Cryosphere* (Nuimura et al. 2014). During the review process of Nuimura et al. 2014, two of the three referees raised concerns about the exclusion of steep headwall regions in the inventory that could belong to glacier accumulation areas. One reviewer suggests that this would introduce a bias when analyzing elevation variables (page 2; Paul 2014; http://www.the-cryosphere-discuss.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 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, 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.

The study is topical and contributes to a growing number of studies assessing climatic influences on the glaciers of High Mountain Asia. Subject to addressing the specific comments below, I think the paper is publishable in *The Cryosphere*.

Specific comments

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

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.

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. 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)."

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

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

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

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 P_L and P_W 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).

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

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.

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

-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.

- 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?

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.

- 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.

- 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?

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?

L6: How sensitive is the evaluation of winter balance in Sect. 3.3 to the relationship in Eqn 2? How is liquid precpitation treated by the mass balance model?

L16: Please remove redundant occurrences of this phrase.

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.

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.

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. 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?

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"

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.

P3642, Sect. 4.1: Small comments:
- I suggest incorporating this section in Sect 2.4, since it is largely a justification of the method.
- 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?).
- On what basis were the nine AWS selected?
- Should **L12** read, "reanalysis data is greater than observed data," to be consistent with the next two sentences?

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).

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)

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

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

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.

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

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

Technical corrections

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

P3630, L12: "receive less precipitation", "makes a greater contribution" ?
P3631, L1: Move reference to end of sentence (I assume)
L4: "Almost all datasets"
L29: "had a large discrepancy"
P3632, L6: "detailed
L26: "The centre of our target region is the Tibetan Plateau, whose elevation is…"
P3633, L13: "located at," "westerlies"
L25: "came from oceanic sources"
P3634, L3: "The regions"
P3635, L4-6: I would suggest combining back-to-back brackets throughout the text, i.e. "Paul et al. (2002; Swiss Alps)"
L8: "Paul et al., 2009"
L9: "from the GGI"
P3636, L13: "as glaciers located"
P3638, L7: "two geopotential heights bounding/containing the median elevation" ?
L12: replace end of sentence with something less repetitive, e.g., "at all three median-elevation categories."
L24: "0°C"
P3639, L16: "area-weighted means at each…" is unnecessarily repeated

L17: “grids points” ?

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

P3640, L13: “calculated precipitation amounts”

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

L13: Table S3 is mentioned before Table S2

P3645, L1: “show similar altitude” or “are similar”

L27: “which included“,”< 1 km^2”

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.

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

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

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

Supplementary information

- Provide the full author list.
- Can Figs. S5 and 8 be combined?
- **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.”

References

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.

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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.

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