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Interactive comment on "Snowfall increase counters glacier demise in Kunlun Shan and Karakoram" *by* Remco J. de Kok et al.

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General comments

The authors of this study address the reasons behind the spatial patterns of glacier mass balance as observed in High Mountain Asia (HMA) over the last decades. While many of the glaciers in HMA follow the world-wide trend of losing mass, there are some regional exceptions like the so-called Karakoram anomaly, the Kunlun Shan and parts of Pamir, where glaciers are stable or even growing.

The authors used the Weather Research and Forecasting model (WRF) to downscale ERA-Interim reanalysis data from 1980-2010 to a domain of 20 km grid spacing covering HMA and adjacent regions, and then applied a glacier mass balance model forced

by the downscaling results. By this approach they have been able to reproduce the observed patterns of glacier mass balance in HMA. They found that the low temperature sensitivities of glaciers and increases in snowfall have caused positive mass balances in the western Kunlun Shan and Karakoram ranges (WKSK). They also detected that increases in snowfall are to large degree stemming from increases in evapotranspiration from irrigated agriculture in regions adjacent to HMA.

Dynamical downscaling was performed by using grid nudging of the upper 35 (of 50) vertical levels for horizontal wind, temperature, and humidity, which preserves the large-scale meteorological tendencies of the ERA-Interim reanalysis. The WRF model was re-initialised each month (plus 10 days of spin-up) with appropriate data reflecting seasonal changes in snow cover, surface and soil temperature, and surface moisture. Glacier mass balance was modelled by employing a glacier mass balance gradient model to all individual glaciers in HMA larger than 0.4 km^2 transiently for the period 1980-2010 assuming steady-state conditions in the beginning. They performed three separate glacier simulations using different forcings (precipitation and temperature, only temperature, and only snow) from the WRF data set. A moisture tracking algorithm was applied to identify the source areas of precipitation.

The study is comprehensive, well-performed and mostly well-presented in the manuscript, and the results are highly interesting. A mistake in one of the figures has already been corrected by the authors prior to my review. The study definitely deserves publication in The Cryosphere.

Specific comments

I do have only a few specific comments, which could be easily addressed by the authors.

First of all, the entire study depends on the accuracy of downscaled precipitation. It would therefore be of utmost interest to better understand the uncertainties in the WRF output. As the authors correctly state, in-situ meteorological observations are scarce, and there is almost complete lack of data in the WKSK ranges, which makes it difficult to compare the WRF output with independent observations. This is especially true for high altitudes, i.e., the glacierized areas, where observational data are not available. Nevertheless, there are gridded data sets that could be used for comparison. Although they do not cover the entire study period (so far) and thus cannot substitute the ERA-Interim data used for downscaling, they could anyway be compared with the WRF results for shorter periods (as the authors have done with GLEAM data). The new ERA5 reanalysis and the High Asia Refined analysis (HAR) data set (Maussion et al., 2014) are suitable data sets in this respect. ERA5 data, especially the newest ERA5 land data set (https://cds.climate.copernicus.eu/cdsapp#!/dataset/reanalysis-era5-land?tab=overview), and the HAR data set do have very high spatial and temporal resolutions, such that they resolve mesoscale atmospheric processes, and thus orographically induced precipitation. HAR data are freely available at www.klima.tu-berlin.de/HAR. I would ask to authors to include a comparison of WRF output with these gridded data sets in the article. This could be put into a supplement with only a short paragraph in the main text.

The authors shall not only provide Pearson correlation coefficients but also further metrics like mean biases, r.m.s. deviations, regression slopes, etc., when comparing their WRF results with those from GHCN stations. I am not convinced that it is necessary to exclude so many GHCN stations by requesting at least 20 year of data coverage. This could be relaxed, or further comparisons may be added. I am also not convinced that it is sufficient to present results only for aggregated time periods, i.e., for annual mean air temperatures, May-September air temperatures, and July precipitation. Depending on the details of forcing the glacier model by WRF output, more detailed analyses of the WRF uncertainties are required, since snow- and ice melt can be rather variable from year to year, although years might have shown similar mean seasonal values for air temperature and precipitation.

In this respect, I would ask the authors to add more details on the WRF output and

its application for forcing the glacier model simulations and the moisture tracking algorithm. In particular, I would like to know the output time step (one hour?).

Reference

Maussion F, Scherer D, Mölg T, Collier E, Curio J, Finkelnburg R, 2014: Precipitation Seasonality and Variability over the Tibetan Plateau as Resolved by the High Asia Reanalysis. Journal of Climate 27(5), 1910-1927. https://journals.ametsoc.org/doi/full/10.1175/JCLI-D-13-00282.1

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