The Cryosphere Discuss., 8, C960–C963, 2014
www.the-cryosphere-discuss.net/8/C960/2014/

The Cryosphere
Discussions
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Interactive comment on "Processes governing the mass balance of Chhota Shigri Glacier (Western Himalaya, India) assessed by point-scale surface energy balance measurements" *by* M. F. Azam et al.

Anonymous Referee #1

Received and published: 22 June 2014

Review of

Processes governing the mass balance of Chhota Shigri Glacier (Western Himalaya, India) assessed by point-scale surface energy balance measurements

by M. F. Azam and others

General

This interesting paper addresses the point (specific) surface energy balance (SEB) of a location in the ablation zone calculated using several years of automatic weather

station (AWS) measurements on Chhota Shigri Glacier (Western Himalaya, India). The scientific quality of the paper is good, but several issues need to be addressed, see major comments below. The paper is well referenced but the English is poor and needs reparation. The figures are of very good quality. The structure needs improving (see below).

Major comments

The paper is not logically structured: in section 2, some data corrections are described, but this is again done in section 3. Please collect all data treatment in a single section (Data and Methods), then go on to describe results (Climate setting and SEB). The Conclusion section reads more like a summary.

P. 2879, l. 15: "The conductive heat transfer within the snowpack or the ice is also ignored as it tends to be small when compared to radiative or turbulent fluxes (Marks and Dozier, 1992). Consequently the SEB is described by the sum of radiation fluxes and turbulent heat fluxes." I guess this assumption was made because the SEB model of the authors does not have a subsurface part, but this assumption is quite severe when the subsurface is not isothermal and must be much better supported or not made altogether. Although the conductive heat flux in a dry, homogeneous soil and averaged over longer time scales (> 1 year) may be small, this changes for soils that are snow-covered for part of the year and for shorter time scales. At sub-daily and inter-daily time scales, the conductive heat flux may become a major heat sink/source for the surface, depending on the sign of the sub-surface temperature gradient. Refreezing and subsequent latent heat release in the snow makes the conductive heat flux a heat source in the mean. So please provide quantitative support for this assumption, or, better, include sub-surface calculations in the SEB model.

Were AWS relative humidity data corrected according to method described in Anderson (1994)? See: Anderson, P. S., 1994: A Method for Rescaling Humidity Sensors at Temperatures Well below Freezing. J. Atmos. Oceanic Technol., 11, 1388–1391.

The SEB evaluation can be extended. First, it is not clear why SR50 data were not directly used to check the melt calculation (when ice is at the surface and density known). Moreover, outgoing longwave radiation is measured and corrected: it is therefore possible to directly compare modelled and observed surface temperature at the model time step (half hour). Such a comparison and its discussion of bias and RMSE is a necessary complement to the comparison between calculated and observed ablation by scarce stake measurements.

P. 2881, l. 18: "At AWS1 site, u at the upper level (initially at 2.5m) is always higher (99.6% of all half-hourly data) than that at the lower level (initially at 0.8m) suggesting that the wind speed maximum is almost systematically above 2.5m and justifies the choice of the bulk method." Remove this sentence: the fact that 2.5 m wind speed is larger than 0.8 m is generally true and is no proof that the katabatic wind maximum is above 2.5 m. To justify this statement involved a more in-depth analysis of the wind speed gradient.

Minor/technical comments

Abstract l. 11: "During THE summer-monsoon period..." Please check erroneous omission of THE throughout the MS, it occurs frequently.

Abstract l. 13: complimented -> complemented

p. 2869, l. 13: Please specify which working group of the IPCC made the mistake

p. 2870, l. 11: of crucial importance -> crucial

p. 2870, l. 21 and further: A negative mass loss implies mass gain. Remove minus sign.

p. 2871, l. 3 and l. 9: decadal level -> decadal time scales

p. 2873, l. 9: near surface snow?

p. 2885, l. 26: hot?

p. 2886, l. 20: "...katabatic wind flow is more expected during summer season than in winter..." This is not always so: in winter, a surface radiation deficit can also force persistent and shallow katabatic winds.

Interactive comment on The Cryosphere Discuss., 8, 2867, 2014.