Point by point reply (in red) to Anonymous Referee # 1 by Azam et al.

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.

Done. Thanks for this comment, we agree, it is logical to put all data corrections together. Now all the data corrections are described in section 2.2. We feel that the conclusion section justifies its structure. We could not find a way to re-organize the conclusion which provides an extensive summary of the results and also brings some perspectives to this work.

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 snowcovered 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.

We agree with these comments that have also been raised by Reviewer 2. In order to address all the comments satisfactorily, we applied Favier et al. (2011) model that includes a scheme dealing with sub-surface heat fluxes (conductive heat flux, and penetration of short-wave radiation inside snow/ice). V. Favier is now one of the co-authors of this paper. We decided to adopt the same terminology as given in Favier et al. (2011) to avoid any confusion and a large part of the methodology section has been re-written accordingly. The whole manuscript has been revised (all changes appear in red in the revised manuscript) and the results have only slightly changed (also highlighted in red in Table 3). The conductive heat flux (G) and short wave penetration heat flux (SW_{sub}) are now shown in Fig. 10 & 12 (newly added panels) and in Table 3 and they are discussed in the related sections of the revised manuscript.

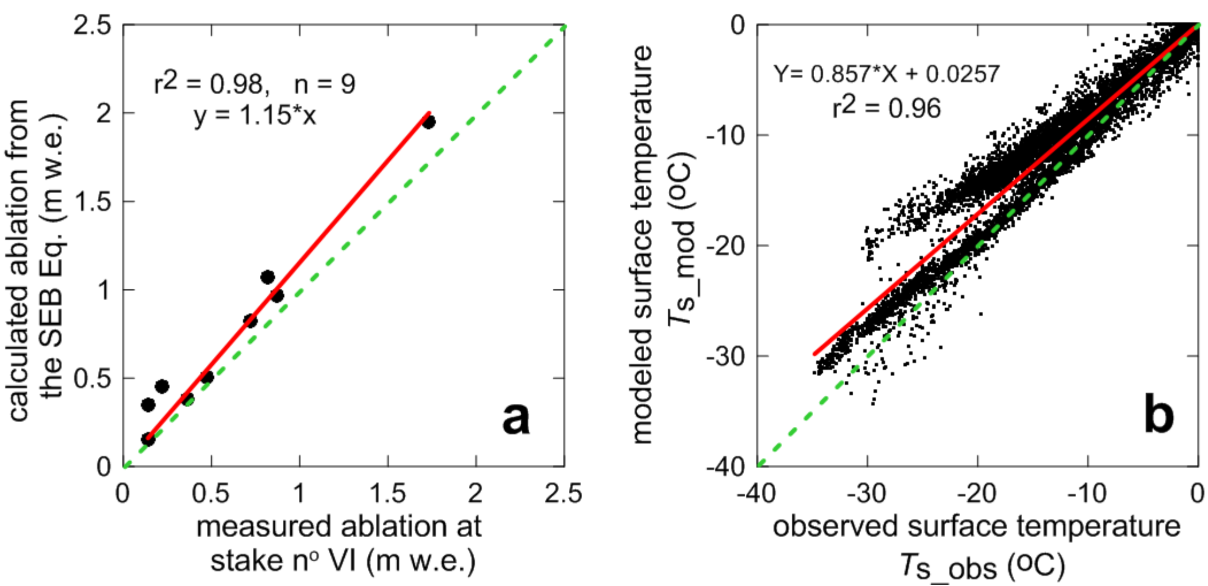
The conductive heat flux is most of the time negligible compared to the other terms of the surface energy balance, but is still responsible for 2% of the total summer-monsoon melt. Using a model able to simulate sub-surface heat fluxes greatly helped to understand the sub-surface processes and in turn greatly improved our analysis but did not change our initial results significantly. The main results as well as the conclusions of this paper remained unchanged.

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.

Anderson (1994) did some improvements in the output data of 'HMP35A' sensor because this sensor is calibrated only between -20 and 65 ^{o}C and was used to measure RH at much lower temperatures than -20 ^{o}C. In the present case the temperatures lower than -20 ^{o}C are observed only during winter season for limited days. Therefore RH data was not corrected for low temperatures.

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.

We agree with the weakness of model validation. As presently we are applying Favier's model that allows the computation of surface temperature (T_{s_mod}), we could compare it with observed (derived from LWO) surface temperature ($T_{s\_obs,}$ in the revised manuscript). This comparison is now used as a second independent model validation, additionally to the initial validation using an ablation stake. Fig. 11 (below) displays T_{s_mod} as a function of T_{s_obs}, at half-hourly time step. Given that the SR50A dataset has a long gap (from 08/09/2012 to 09/11/2012) and that it is not always easy to extract the ablation signal (from compaction) when the surface is snow, this dataset has not been used to check the melt calculation. The stake observations covering the whole period have been preferred for this purpose.



(Revised) Fig. 11. Comparison between ablation computed from the SEB Eq. and measured at stake nº VI (a) during several few-day to few-week periods of 2012 and 2013 summers where field measurements are available. (b) Comparison between half-hourly modeled (T_{s_mod}) and observed (T_{s_obs}) surface temperatures over the whole simulation period. Also shown are the 1:1 line (dashed line) and the regression line (solid line).

The revised text in the manuscript is:

"To validate the SEB model, computed ablation (melt + sublimation – re-sublimation) was compared with the ablation measured at stake nº VI in the field (section 2.3). The correlation

between computed ablation from the SEB Eq. and measured ablation at stake n° VI is strong (r^2 = 0.98, n = 9 periods), indicating the robustness of the model. Although, the computed ablation is 1.15 times higher than the measured one (Fig. 11a), this difference (15% overestimation) is acceptable given the overall uncertainty of 140 mm w.e. in stake ablation measurements (Thibert et al., 2008). Furthermore, surface temperatures at half-hourly time step (T_{s_mod}) were calculated by the model without using measured LWO (or associated surface temperatures, T_{s_obs}). Figure 11b shows that the half-hourly T_{s_obs} and T_{s_mod} are highly correlated (r^2 = 0.96), with an average difference of 1.2 °C. This temperature difference corresponds to a mean difference of 4.6 W m^{-2} between LWO_{mod} and observed LWO, showing that the modeled surface heat budget is reasonably computed. Moreover, if we run the model with an additional 2-cm snow layer at the surface when measured albedo values are higher than 0.7, the mean difference between T_{s_mod} and to T_{s_obs} drops to 0.2°C, showing that this difference does not come from a bad performance of the model, but from a bad estimation of the surface state (snow or ice) and thus of precipitation during low intensity events (explaining the bi-modal scatter observed in Fig. 11b i.e. surface state correctly reproduced or not). Thus when the surface state is appropriately assessed, the model provides a good estimation of T_{s_mod}. In conclusion, given that the model is able to properly compute surface temperature or ablation at point-scale, we believe that it can reasonably calculate all the SEB fluxes."

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.

At the AWS1 site we had velocity measurements from two levels (0.8 and 2.5 m) only that cannot allow us to go in in-depth analysis of the wind speed gradient. Although we rephrased the sentence here following the reviewer comment, because we agree that u(2.5m) > u(0.8m) does not systematically imply that the wind speed maximum is above 2.5m, it may still be between both measurement levels.

"*Wind speed, T_{air} and RH were measured at two levels (0.8 and 2.5 m) at AWS1. 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). For the turbulent heat flux calculations, the bulk method was used……."*

Minor/technical comments

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

Checked and revised.

Abstract l. 13: complimented -> complemented

Done.

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

Done. We provided the reference (Solomon et al., 2007).

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

Done.

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

Done.

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

Done.

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

Done.

p. 2885, l. 26: hot?

'Hot' is replaced with 'warm'.

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.

Done. The sentence is removed.

References

Anderson, P. S.: A method for rescaling humidity sensors at temperature well below freezing, Journal of Atmospheric and Oceanic Technology, 1388-1391, 11, 1994.

Favier, V., Agosta , C., Genthon , C., Arnaud , L., Trouvillez , A., and Gallée , H.: Modeling the mass and surface heat budgets in a coastal blue ice area of Adelie Land, Antarctica, J. Geophys. Res., 116, F03017, doi:10.1029/2010JF001939, 2011.

Solomon, S., Qin, D., Manning, M., Chen, Z., Marquis, M., Averyt, K. B., Tignor, M., and Miller, H. L.: IPCC: Climate Change 2007: The Physical Science Basis. Contribution of Working Group I to the Fourth Assessment Report of the Intergovernmental Panel on Climate Change, Cambridge University Press, Cambridge, United Kingdom and New York, NY, USA, 2007.