# Point by point reply (in red) to Anonymous Referee # 2 by Azam et al.

**General comments**

This paper describes automatic weather station measurements at Chhota Shigri Glacier in the Western Himalaya and an analysis of the surface energy balance with a model. Given the sparse observations on glaciers in this region, this paper provides very valuable information on meteorological conditions and surface energy balance characteristics for the Himalayan glaciers. The paper is well-written and generally uses good English. However, in many sentences, there is a lack of usage of 'the' and 'a/an'. I give a few examples in the Technical Comments below, but there were too many to list them all. I recommend to have the text proof-read by a native English speaker.

Many thanks for your constructive comments that substantially helped us to improve our manuscript. The revised manuscript has been proof-read by one of the co-authors Jose Pottakkal who has pursued his entire academic career in English-speaking countries, and whose preferred language of communication is English.

The methods used are mostly sound, but two points need attention, as described below.

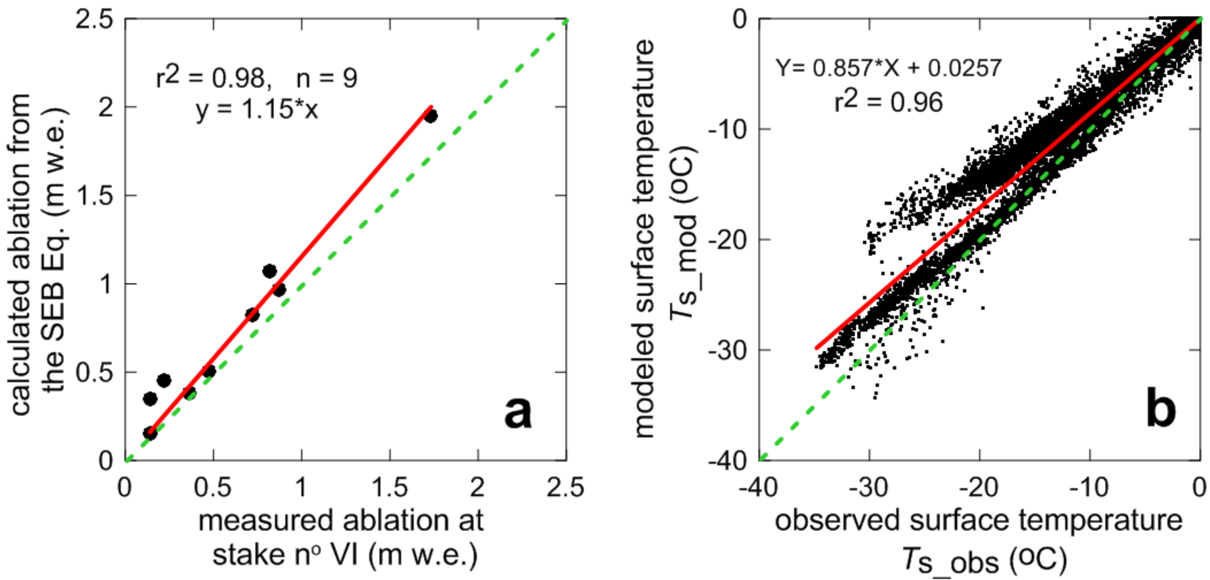
In the given SEB equation, the conductive heat flux G is given, but the text mentions that this flux is small enough to be ignored. First of all, I wonder whether this is a valid assumption, since the surface temperature is well below zero outside the summer season indicating considerable heat loss from the subsurface layers. As noted in the text, without G the SEB is a balance between the radiative and turbulent fluxes. Only when melt is allowed, Q is positive. However, in later parts of the manuscript, Q also has non-zero values for periods without melt. The authors explain these non-zero values with conductive heat transfer and change of the surface temperature, which is not consistent with the neglectance of G. In fact, the non-zero values are a measure for the inaccuracies in the model assumptions, which is not a problem as long as the values are small. The authors either need to present the non-zero values for non-melting conditions as the model error, or use a model including G. The latter is preferred, because the assumption that G = 0 does not seem to hold outside the summer season

We agree with these comments that have also been raised by Reviewer 1. 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.

The model validation presented in the paper is rather weak and should be improved. The authors compare computed ablation with a number of stake measurements in the direct vicinity of the AWS. They however also have the continuous height change detected by the sonic ranger, which should also be used for the model validation. Even though the record seems to have data gaps, a significant part of the ablation season with ice at the surface remains. If the authors would include a subsurface model in their SEB calculations, they can also calculate Tsurf with the model and compare it to Tsurf derived from LWO.

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 n° 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 n° 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."*

**Detailed comments**

2869, 13-16: I do not agree with this statement. There was clearly an error in the IPCC AR4, but as suggested by Cogley et al. (2010), this is likely a typing error. Although this points out the inadequate checking of references by the IPCC, this particular error does not show the poor understanding of Himalayan glacier changes.

We agree and revised the sentence. Now it is:

*"The erroneous statement in the Intergovernmental Panel on Climate Change (IPCC) Fourth Assessment Report (Solomon et al., 2007) about the future of the Himalayan glaciers dragged the attention of the scientific community towards the behavior of the these glaciers in relation to climate."*

2872, 29-20: Just a remark, perhaps useful for the future: I would recommended to use half-hourly averaged wind directions instead of instantaneous measurements. This however requires a more complicated data logger program, since the instantaneously measured wind speeds and directions should be added vectorially. Especially at low wind speeds, the wind vane is moving a lot and this introduces unwanted noise if only one measurement per half hour is taken. In addition, wind speed is a half-hourly average, so wind direction and wind speed represent different periods.

Thanks for the advice. We will keep this in mind to improve our data records in the future.

2873, 18-22: What is the range of values this cloud factor can have, is it between 0 and 1? So for clear-sky days, 93% of the STOA reaches the glacier surface and for cloudy days, this is 21%? It would be good to add at least the range, for clarity.

The cloud factor ranges between 0 and 1. It is added in the revised version:

*"In the present study cloud factor is calculated by comparing SWI with solar radiation at the top of atmosphere (STOA) according to the Eq.: cloud factor = 1.3−1.4*(SWI/STOA) that represents a quantitative cloud cover estimate and ranges between 0 and 1."*

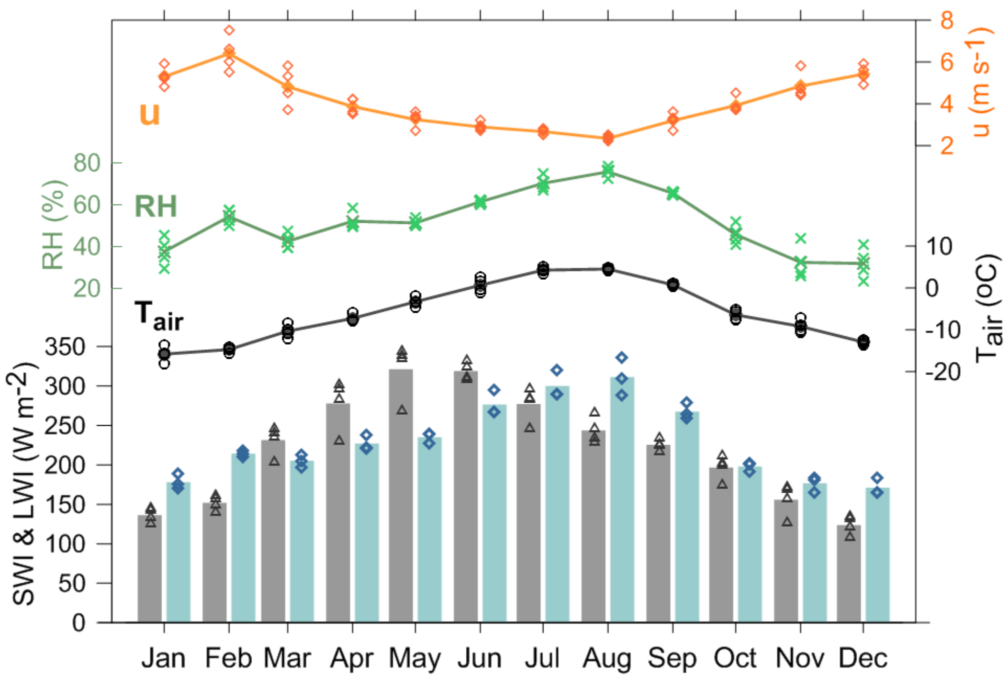
2874, 7-8: I assume this is a result of the temperature effect on the sonic ranger signal? Perhaps the authors can slightly rewrite to make clear that this sentence follows up on the previous one.

Yes, this is the result of temperature effect. The sentences are re-written for clear understanding. The revised sentences are:

*"This sensor does not involve an internal temperature sensor to correct for the variations in speed of sound as a function of $T_{air}$. Without this correction the measured distance may reduce during the evening which could be misunderstood as a snowfall event (Maussion et al., 2011). Therefore, temperature corrections for the speed of sound were applied to the sensor output using $T_{air}$ recorded at the higher level. Besides, to reduce the noise, a 3-hour moving mean is applied to smooth the SR50A data."*

2875, 11-14: Perhaps the authors can include all monthly values alongside the monthly means in the figure, for example as dots. This will then nicely demonstrate that the seasonal cycle is larger than the inter-annual variability in the monthly values.

Thanks for the suggestion. It greatly improved Figure 3 (see below):




**(Revised) Fig. 3.** Mean monthly values of $T_{air}$ (black dots), RH (green crosses), u (orange squares), SWI (grey bars) and LWI (light blue-green bars) at AWS2 (4863 m a.sl.). $T_{air}$, RH, u and SWI are the mean monthly values of four hydrological years between 1 October 2009 and 30 September 2013 while LWI are the mean monthly values of three hydrological years between 1 October 2010 and 30 September 2013. Also shown are the monthly values of $T_{air}$ (black circles), RH (light green crosses), u (orange hollow squares), SWI (black hollow triangles) and LWI (blue hollow squares) used to derive the mean monthly values.

2876,1-4: I do not think the measured RH gives much information about moisture received at the glacier. If the authors are speaking of precipitation, then the next section shows that winter precipitation is higher than summer precipitation, while RH values are lower in winter than in summer. If the authors are speaking of sublimation/re-sublimation, the SEB analysis shows that the glacier only receives moisture summer, in winter it looses mass. I suggest to remove this speculation about moisture sources.

We agree. Now the related sentence is removed from the revised manuscript.

2876,8-10: Does the 33 W m−2 come from the difference between the pre-monsoon and the summer-monsoon SWI? Then better write something like 'The summer-monsoonal mean is 33 W m−2 lower than the pre-monsoonal average'. By writing 'is reduced', readers might think that the loss of radiation on its path through the atmosphere is 33 W m−2 . Furthermore, I do not think that

RH is a good indicator for cloud conditions, better use the cloud factor. Especially, a mean RH of 68% is not particularly high and does not necessarily indicate overcast conditions.

Done. The revised sentence is:

*"The summer-monsoonal mean is 33 W $m^{-2}$ lower than the pre-monsoonal mean because of high cloud coverage in the summer-monsoon."*

2880,11-12: What is meant with 'in these conditions'? Why is the sensor mainly receiving isotropic radiation, is this because of overcast conditions? In case the cloud factor has values between 0 and 1, the reported mean value does indicate that cloudy conditions prevailed most of the time.

'In these conditions' means = "the phase shift and poor cosine response of the sensor."

It is true that we were not clear enough here.

On one hand, the SWO sensor (measuring the reflected SW radiation) faces towards glacier surface and doesn't receive the direct light beam (anisotropic radiation) from the Sun but mainly receives isotropic radiation. On the other hand, the SWI sensor (facing towards the sky) may receive anisotropic or isotropic radiation depending on clear sky or overcast conditions, respectively.

The sentences are rearranged and rephrased to avoid any confusion.

*"At high elevation sites, such as Himalaya, measured SWO can be higher than SWI (2.6% of total data here) during the morning and evening time when the solar angle is low because of poor cosine response of the upward-looking radiation (SWI) sensor (Nicholson et al., 2013). Besides, as AWS1 was installed on the middle of the ablation area, the unstable glacier surface during ablation season conceivably gave rise to a phase shift by mast tilt (Giesen et al., 2009). SWO sensor mostly receives isotropic radiation and consequently is much less sensitive to measurement uncertainties of poor cosine response and mast tilt compared to SWI sensor (Van den Broeke et al., 2004). Therefore, SWI is calculated from SWO (raw) and accumulated albedo ($\alpha_{acc}$) to avoid the impact of the phase shift because of tilting during the daily cycle of SWI and poor cosine response of the SWI sensor during the low solar angles."*

2884,5: I assume that the z0m values cannot be negative, which the STD values suggest?

Yes we agree, z0m values cannot be negative. If the STD values are higher than the mean, it shows that most of the data are close to the extreme values in the data set. There are several examples when STD is higher than the mean, indicating a large scatter in the values but not meaning that there are negative values. Please see the figure below (from Azam et al., 2014). The STD for October-December period is higher than the mean of the corresponding months, but it does not mean that precipitation display negative values.

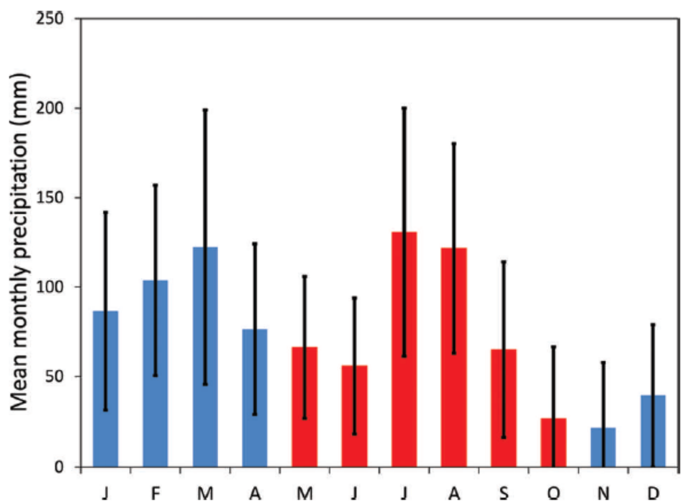



**Fig. 3.** Mean monthly precipitations between 1969 and 2012 at Bhuntar Observatory. Summer precipitation (red bars) predominantly derives from the Indian summer monsoon, whereas winter precipitation (blue bars) predominantly derives from mid-latitude westerlies. The error bars represent the standard deviation ($\pm 1\sigma$) of the monthly precipitation mean.

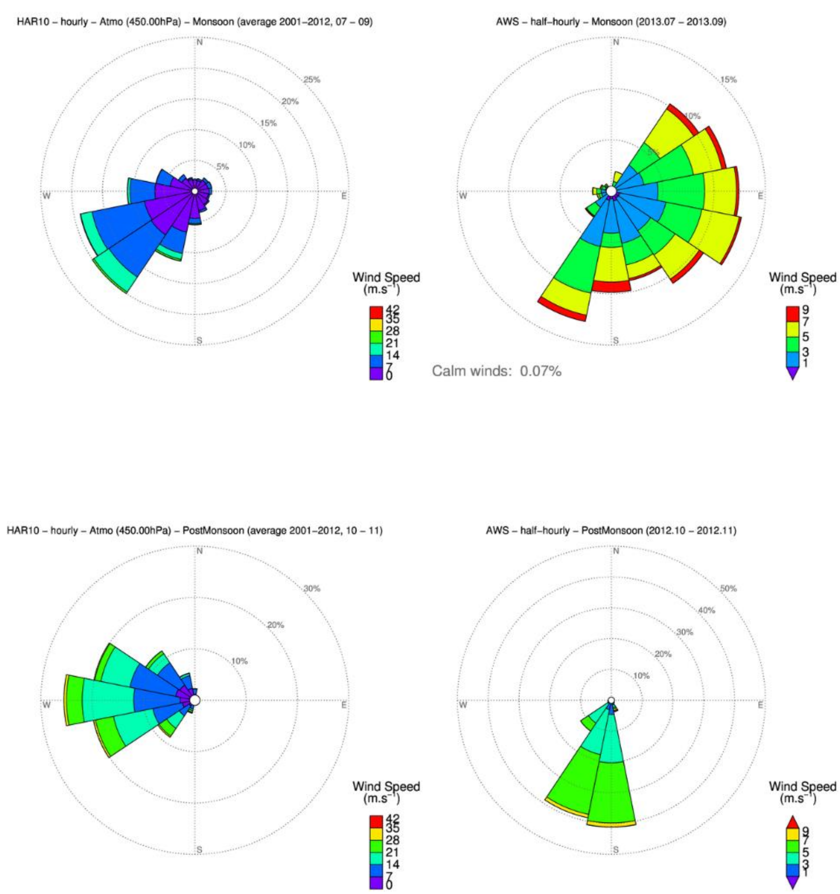
2885, 27: This is a bit confusing, because in Sect. 2.4.1 it is said that the highest SWI values are measured in the pre-monsoon season. This season is not included in the comparison for the glacier values, but perhaps it can be mentioned that the pre-monsoon values are presumably larger with a reference to Sect. 2.4.1.

Done. The sentence is rephrased as:

*"SWI is high during the summer-monsoon period (however, the highest SWI is expected in pre-monsoon season, section 2.4.1) with a mean value of 248 W m$^{-2}$ (Table 3)."*

2886, 20-24: What are the dominant wind directions of the large-scale circulation in the summer and winter seasons? Even though the glacier may be surrounded by high valley walls, the authors cannot conclude that the observed winds are mostly katabatic winds without mentioning the large-scale circulation winds. A first-order analysis can for instance be made from meteorological reanalysis products.

Thanks for the comment. Following the suggestion, we compared the winds (velocity and direction) at AWS1 with those of 450 h Pa pressure level obtained from High Asia Reanalysis (HAR, Maussion et al., 2014) data at hourly scale. HAR wind data is available at 10 km resolution for different pressure levels. We chose 450 hPa winds (assuming 450 hPa equivalent to ~6350 m a.s.l. as synoptic winds just above the glacier) for the AWS1 wind comparison with synoptic scale winds. Figure A shows a comparison of AWS1 and HAR winds.



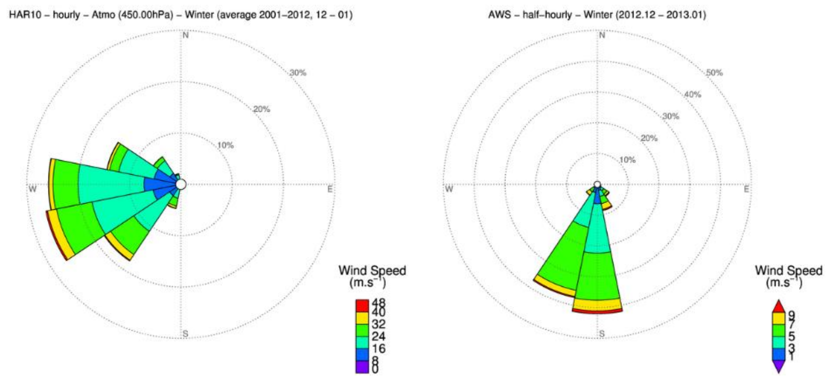



Figure A: Comparison of AWS1 winds with High Asia Reanalysis (HAR, Maussion et al., 2014) winds for monsoon, post-monsoon and winter seasons. HAR rose diagrams show the mean seasonal winds from 2001 to 2012. HAR winds are available at hourly time scale while AWS1 are the half-hourly means. Note that scales are different between AWS and HAR wind speeds in order to keep information on wind velocities much lower at AWS site than from HAR data.

Synoptic (HAR, 450 hPa) wind comes mainly from west or south-west direction depending on the season. Given that on its eastern side the glacier is bordered by a high N-S ridge (up to elevations higher than 6000 m a.s.l.) the synoptic wind is potentially deflected down to the valley providing winds coming from south at AWS1. Therefore we completely agree that the impact of synoptic scale winds at AWS1 cannot be ignored and the south wind at AWS1 is the cumulative effect of katabatic and synoptic winds. Accordingly we re-phrased the text in the manuscript and added a small paragraph concerning synoptic wind impact.

*"AWS1 is surrounded by steep N-S valley walls. In order to analyze the impact of synoptic scale circulation at AWS1 site, we compared the wind directions at AWS1 with those at 450 hPa pressure level obtained from High Asia Reanalysis data (HAR, Maussion et al., 2014) at hourly scale. HAR wind data is available at 10 km resolution for different pressure levels for 2001-2012 period. The pressure level of 450 hPa (equivalent to ~6350 m a.s.l.) has been chosen as representative of the synoptic circulation above the glacier (whose highest elevation is 6263 m a.s.l.). Synoptic (HAR, 450 hPa) wind comes mainly from west or south-west directions, depending on the season. Given that on its eastern side the glacier is bordered by a high N-S ridge (often above 6000 m a.s.l.), this synoptic wind may be deflected down to the valley providing winds parallel to the katabatic flow at AWS1. Therefore at AWS1 site the wind coming from south to southwest is probably the result of both katabatic and synoptic effects."*

2888, 8-10: The latent heat values of condensation and sublimation are different (2.50 and 2.83 · 10 6 J/kg, hence gained mass estimates will also vary. Please specify which processes are considered, it seems that (re)sublimation is assumed under all conditions.

Yes, it is true, we assume re-sublimation in all conditions when the latent heat flux is positive. We revised the sentence as:

*"Because of this positive LE, glacier gained mass through condensation or re-sublimation of moist air at the surface (Table 3). Assuming re-sublimation as the main process an amount of 0.3 mm w.e. $d^{-1}$mass gain is calculated during the summer-monsoon period."*

2888, 25-28: I would be interested to see the relative amounts of mass loss/gain by melt and (re)sublimation, to illustrate whether (re)sublimation is important in the surface mass balance of Chhota Shigri. Please include total numbers here.

Done and a sentence is added as:

*"During the summer-monsoon, the glacier lost mass with a daily mean melt rate of 61.3 mm w.e. $d^{-1}$ while a mass gain of 0.3 mm w.e. $d^{-1}$ was observed through re-sublimation (Table 3). Compared to melt, re-sublimation and sublimation were negligible during the summer-monsoon."*

2889, 17-18: There is a very prominent minimum wind speed in the morning in the post-monsoon season, any idea what causes this?

We could not identify the reason for this wind speed minimum in the morning time of post-monsoon. We added a line in the manuscript.

*"A wind speed minimum is observed in the morning time of post-monsoon but no reason for this could be identified."*

2891, 15-21: Was a gradient in precipitation with elevation included here? It is probably not correct to assume that precipitation amounts at AWS1 and AWS2 are equal. If no information is available, the authors should be more careful with mentioning snow amounts on the glacier.

We did not use any precipitation gradient here. Unfortunately we don't have any information of precipitation from AWS1 or AWS2. The Geonor rain gauge is installed at the base camp (3850 m as.sl.) and provides the only available information about precipitation amounts close to the glacier. We agree that precipitation at 3850 m a.s.l. may be different from that at AWS1 (4670 m a.s.l.)

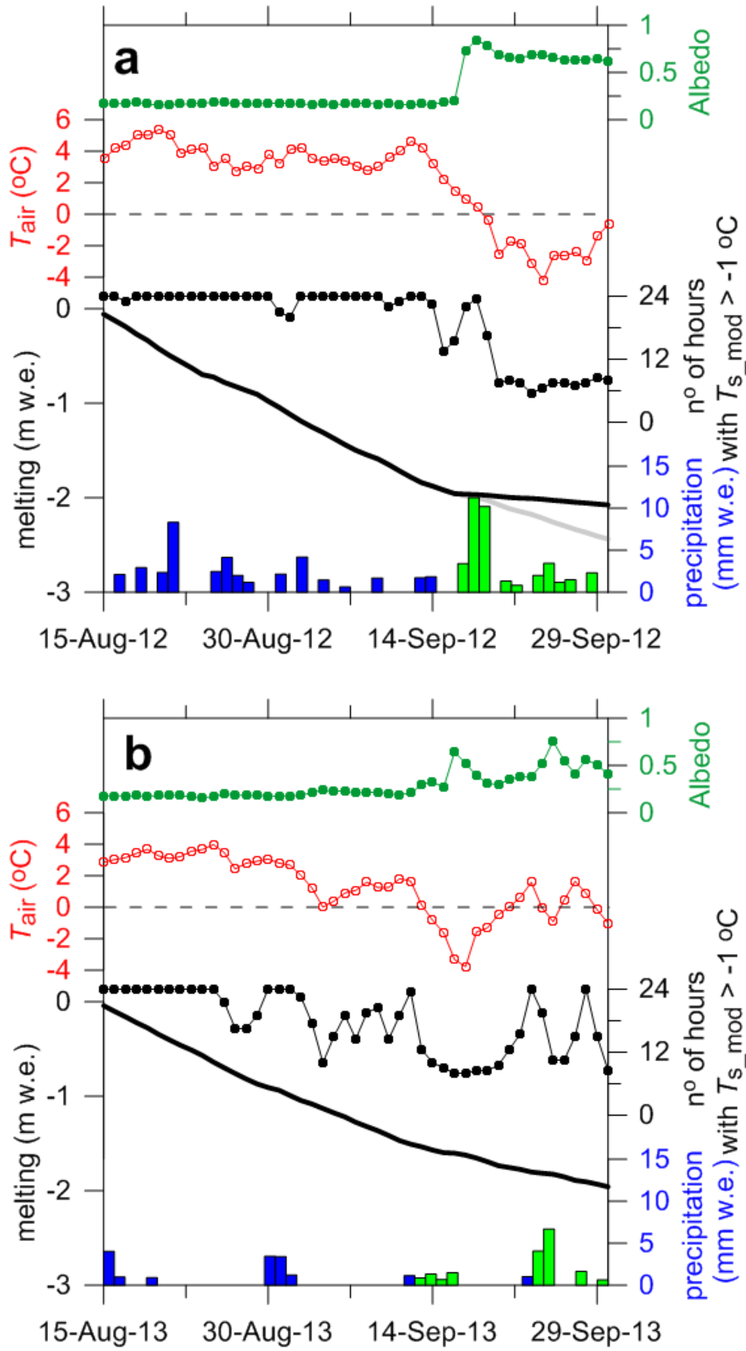
but assuming a 0-gradient was the best available option. We also agree that we should be more careful while mentioning snow amounts on the glacier; therefore, we clearly provide this assumption in the sentence. The revised sentence is:

*"These precipitation values are extrapolated at AWS1 assuming a zero-precipitation gradient and are considered as rain (snow) at AWS1 site when $T_{air}$ at AWS1 is above (below) 1 °C (e.g., Wagnon et al., 2009)."*

2892, 1-24: It would be interesting to assess the effect of the summer snowfall by running the SEB model for the 2012 period without the increased surface albedo, so constantly kept at 0.19. This gives more quantitative information than the comparison with 2013, when meteorological conditions were also different. However, this requires the computation of Tsurf (and LWO) in the model, which is currently not the case.

Thanks for this interesting suggestion that efficiently helps to quantify the effect of albedo on glacier melting during over 15 August-30 September 2012 period. We ran the model, assuming a constant albedo (0.19) over the entire 2012 summer period. This new simulation is now included in Fig. 13 (see below – grey thick line on panel a), and discussed in the corresponding text with an added paragraph (below):

*"In order to better quantify the albedo effect of the mid-September 2012 snow falls on the glacier melting, the model was run again assuming a constant albedo (=0.19) over the entire 2012 summer period, all other meteorological variables being unchanged meanwhile (Fig. 13a). As expected, the overall melting with constant albedo is enhanced (2.44 m w.e.) with a moderate difference of 0.36 m w.e. (+17% compared to a simulation with real albedo) between 15 August and 30 September 2012, but very significant when considering only the period when the observed albedo differs from 0.19 (i.e. after 17 September 2012). Certainly, between 17 and 30 September, the computed melting using a constant albedo (0.19) is 0.48 m w.e., 4 times higher than that with the observed albedo (0.12 m w.e.). Even though Chhota Shigri Glacier is a winter-accumulation type glacier, this analysis highlights and quantifies the role of snowfall events during the summer-monsoon on albedo and, in turn on melting."*




**(Revised) Fig. 13.** Comparison of computed cumulative melting (black thick line) between 15 August and 30 September from summers 2012 (a) and 2013 (b). Also shown are the mean $T_{air}$ (red open dots), the number of hours in a day when $T_{s\_mod}$ is > −1 °C (black dots), daily albedo (dark green dots) and the precipitations as rain/snow obtained from records at base camp (blue and green bars, respectively). The grey line in panel (a) is the computed cumulative melting between 15 August and 30 September 2012 assuming a constant surface albedo of 0.19.

2892, 25-2893, 24: This is a brave attempt to transfer the point-scale results to glacier-average mass balance. However, I do not think the input data used are good enough to give reliable results. For example, the authors show in Sect. 2.4.2 that the precipitation regime of Bhuntar and AWS1 is noticeably different, not only regarding the amounts, but also the seasonality. Do they have any indication, for instance for the 2012/2013 season, that precipitation events on the glacier correspond to the Bhuntar record? In addition, the effects of summer snowfalls cannot be separated from the effect of simultaneous low air temperatures on the mass balance. I therefore think that the authors should leave out this analysis and stick to the point-scale analysis.

We agree that, due to the orographic barrier, the precipitation regimes for 2012/2013 year at Bhuntar and AWS1 are noticeably different and need to be better understood with several years of precipitation data from glacier base camp as mentioned in the section 2.4.2. We already discussed in details this issue in our reply to the anonymous reviewer and we invite Reviewer 2 to read this reply. The conclusion of this reply is that the precipitations at Bhuntar are not totally decoupled from those at the glacier and are probably good enough to provide reliable results on the glacier.

Wulf et al. (2010) conducted a thorough study using the precipitation data of 80 stations from the northwest Himalaya including Chhota Shigri Area and concluded that in Baspa Valley (~100 km southeast to Chhota Shigri Glacier):

"The two most prominent 5-day-long erosional events account for 50% of the total 5-year suspended sediment flux and coincide with synoptic scale monsoonal rainstorms. This emphasizes the erosional impact of the Indian Summer Monsoon as the main driving force for erosion processes in the orogenic interior, despite more precipitation falling during the winter season."

This study (Wulf et al., 2010), in agreement with our present study, concluded that the ISM has important impact in the orogenic interior.

Furthermore, the impact of the summer-monsoon snow falls on the glacier-wide mass balance is not a transfer of point scale SEB results. In fact this was initially discussed in section 5.1 in Azam et al. (2014) but given the lack of physical basis it was not discussed in details there. Certainly, the present SEB study provides a detailed understanding of the impact mechanism of ISM snow falls on mass balance. Therefore we believe it is logical to discuss the qualitative impact of ISM snow falls on the glacier-wide mass balance in the present manuscript. However we also confess that a distributed glacier-wide SEB study is still needed to understand this mechanism quantitatively.

We agree with the reviewer and understand that the effects of summer snowfalls cannot be separated from the effect of simultaneous low air temperatures on the mass balance. This issue has already been mentioned in the initial manuscript in section 5.1:

*"Indeed, the summer-monsoon air temperature is as crucial as summer precipitation mainly because it controls the amount of rain versus snow received at the glacier surface and in turn, has an important control on glacier albedo and thus on the amount of short-wave radiation absorbed by the glacier surface, which is the main heat source for Himalayan glaciers."*

In order to be clearer, we supported our analysis (section 5.1) with the study of Wulf et al. (2010) and added a small paragraph in section 5.1:

*"The choice of using precipitation data from Bhuntar meteorological station to assess precipitation on the glacier might seem unfortunate at first glance because, as already discussed in section 2.4.2., both sites are separated by an orographic barrier inducing a different precipitation distribution. However, these sites are only 50 km away from each other, and we believe that meteorological conditions are not totally decoupled between the windward and the leeward side of the mountain range, especially in the case of precipitation events strong enough to cross this orographic barrier. Fortunately, Wulf et al. (2010) conducted a thorough study using the precipitation data of 80 stations from the northwest Himalaya including Chhota Shigri area and concluded that in Baspa Valley (~100 km southeast to Chhota Shigri Glacier) "The two most prominent 5-day-long erosional events account for 50% of the total 5-year suspended sediment flux and coincide with synoptic scale monsoonal rainstorms. This emphasizes the erosional impact of the ISM as the main driving force for erosion processes in the orogenic interior, despite more precipitation falling during the winter season".*

2908, Table 4: The percentages of the fluxes in this table do not seem correct for the locations with negative fluxes. I would think Q should always be 100% and that fluxes with negative values have a negative contribution. For example, for Laohugou No 12 the percentages should be 108% for R, 9% for H, -17% for LE and 100% for Q. This method is at least common to compute the contribution of the different fluxes to melt, which should ideally only include those periods when the surface was actually melting. All studies in Table 4 are conducted over the summer season, so this seems a reasonable assumption. But perhaps the authors used a different definition of the flux contribution, it would be good to explain this.

Thanks for suggesting the method. Your method seems more appropriate and is adopted here. In fact we adopted the different methods described in the corresponding papers explaining why Table 4 did not show consistent percentages of heat fluxes. Now the Table 4 is revised accordingly and a sentence is also added in the text.

*"When an energy flux was negative, we assigned a negative contribution to this specific flux in order to have the resulting flux* $F_{\text{surface}}$ *always equal to 100%."*

**Technical comments**

Thanks for the technical comments. They all were constructive and are included in the revised manuscript.

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