Response to Reviewer #2

A) General Comments

This paper presents an assessment of sublimation and melt rates from the snowpack in a semi-arid region of Chile. A distributed physically-based snow model is used to simulate snowpack evolution and quantify losses from sublimation and melt. The model is forced with two datasets that are spatialized on a 100m grid – one created from in-situ meteorological observations, the other from meteorological model output. The simulations are validated against in-situ snow depth measurements as well as snow cover observed using the MODIS platform. The results highlight the complex interactions between meteorological forcing and surface energy and mass balances, along with the effects of spatio-temporal averaging when calculating catchment-wide metrics in areas with predominately ephemeral snow cover. The rationale is well developed, the methods are fit-for-purpose and generally well explained, and many interesting and relevant results are presented.

1) However, there are too many lines of inquiry that compete for the readers attention and the figures presented do not always clearly present the main conclusions reached. In places, the results seem to contradict each other, and it not always clear in what direction different factors affect the sublimation ratio. For example, increased precipitation is attributed to increased melt based on Figure 12, but to increased sublimation based on the WRF simulations). This all contributes to make the paper quite hard to follow and reduces the confidence in the conclusions reached. The aims and hypothesis need to be clarified and perhaps re-assessed given the results available to ensure a coherent story can be told from the results. I would suggest the authors have a few options to refine and narrow the aim of the paper:

- If the aim is to understand the spatio-temporal variability of sublimation and melt, then only the simulations with AWS forcing should be presented as they appear to show the best validation, and the inclusion of the WRF result only confuse the reader. The results relating to season and effect of SCD and elevation can be highlighted.
- If the aim is to compare the effect of using the different forcing datasets (which is a valid aim given that methods to extrapolate beyond areas with in-situ measurements are needed), then the paper should be framed in this way and some of the later results should either be amended to include WRF results or removed (i.e. fig 11 & 12).
- If the aim is to establish the uncertainty in modelling actual sublimation and melt rates, then a more systematic approach requiring further simulations would be needed.

Of course, the authors may have other ways they wish to reframe, these are just some suggestions.

Additionally, some important aspects of the simulation deserve more attention:

2) Validation needs to include comparison of wind speed and incoming radiative fluxes, as well as modelled surface temperature and albedo where possible.

3) Greater discussion of wind speed and surface temperature as a key control on sublimation in the main body of the paper – e.g. the sensitivity of sublimation ratio to 10% change in wind speed noted in S4-15.

4) Clearer description of how average results are calculated, especially the sublimation ratio vs ablation rates, considering the changing temporal and spatial scales associated with the ephemeral snow cover, and their effect on the results. The authors may wish to consider reporting surface energy balance terms as sums in MJ rather than rates (m yr-1 or W m-2) as these can be ambiguous when the you do not average over the full period (e.g. only over snow covered surfaces).

5) Elevational gradients of SEB components need to be shown as it currently very hard to sort out different mechanisms for change in sublimation ratio (e.g. change in meteorology, snow covered area, elevational gradient). Ideally these would come before the catchment-average results to set the context for the observed inter-annual and/or inter-model differences.

With a more focussed aim and the clarification of these points, I have no doubt that this paper will make a good contribution to the literature.

Authors'answer:
Thank you for your positive comments and interest in the paper. We have carefully answered each point presented in the general comments as well as the specific/technical points. Changes made in response to reviewer comments in the manuscript are in green.

1) Authors'answer: To address the comments of both reviewers, the paper has been restructured (including changing the title) to focus more on the differences in terms of snow depth, snow cover and sublimation as a result of the chosen forcing. This modification includes a change in result section, where a better comparison between the two forcings is provided (i.e. section 4.2 has been renamed to "Snow depth and snow cover comparison" for consistency).
The results sections of the original manuscript have also been revised and organized as follows:
4.3 Ablation and energy balance fluxes
4.3.1 Mean annual elevation gradients
This includes changes to Figure 10 and the addition of a new figure (Fig. 9) that show the energy fluxes contribution as a function of elevation (as suggested by reviewer #2)
4.3.2 Monthly evolutions
In this section, Figures 9 and 8 are presented and commented Figures 9 and 8.
The discussion section has also been re-organized with a stronger focus on (i) differences in sublimation as a function of the chosen forcing (ii) differences in sublimation between the two years and (iii) the impact of snow depth on the sublimation ratio and (iii) the limits of the study.
This reorganization was chosen to focus the conclusions on advantages and disadvantages of using different forcing data. For that purpose, differences in terms of meteorological data and the consequences on SD, SC and sublimation are analyzed, as suggested in your comment.

2) The comparison of wind speed and radiative fluxes has been added in section 4.1.2 and Figure 4 (in agreement with the specific comments P13 L1, L21 and L22). For more details please refer to these comments.
Regarding the albedo validation, measurement data have been used to calibrate the model. Information about the available measurements is given by Figure 2. In addition, this point has been clarified according to your comment P10 L30)
Regarding the surface temperature, we have chosen not to add a new graph. For more details please refer to comments P14 L6.

3) Due to the new reorganization of the paper, and a focus on the impact of different forcings on sublimation, we worry that this point will be a little bit disconnected from the main scientific question. Nevertheless since this point is important and interesting, we created a new section in the discussion, named 'Limits of the study', and the results are included in the supplementary material.

4) The calculation is done considering only days with snow on the ground. In order to address your comment and a comment made by the other reviewer, this information is now provided in the methods section: "Note that sublimation and energy balance are only computed over snow surfaces. This means that annual and monthly means are computed at grid-cells with snow only."

In addition, we tried reporting surface energy balance terms as sums in MJ (see Figures below). However, we think that in this case the number of days changes the MJ sum and the graph therefore illustrates the number of days more clearly than the energy flux differences. For instance, for the elevation plot when fluxes at a given elevation are compared, larger values are found at higher elevations and this is related to the number of days rather than changes in the energy flux. Regarding the monthly average, larger values using the unit MJ are found for the months when snow was on the ground for all days.

The alternative would be to compute the MJ over a similar time period, but given the data availability for this catchment, this would reduce the results to a very short time period. Therefore, even though the average cannot be calculated over the entire time period, rather only when there is snow covering the surface, we think that using W m^{-2} allows for a more accurate interpretation of the results.

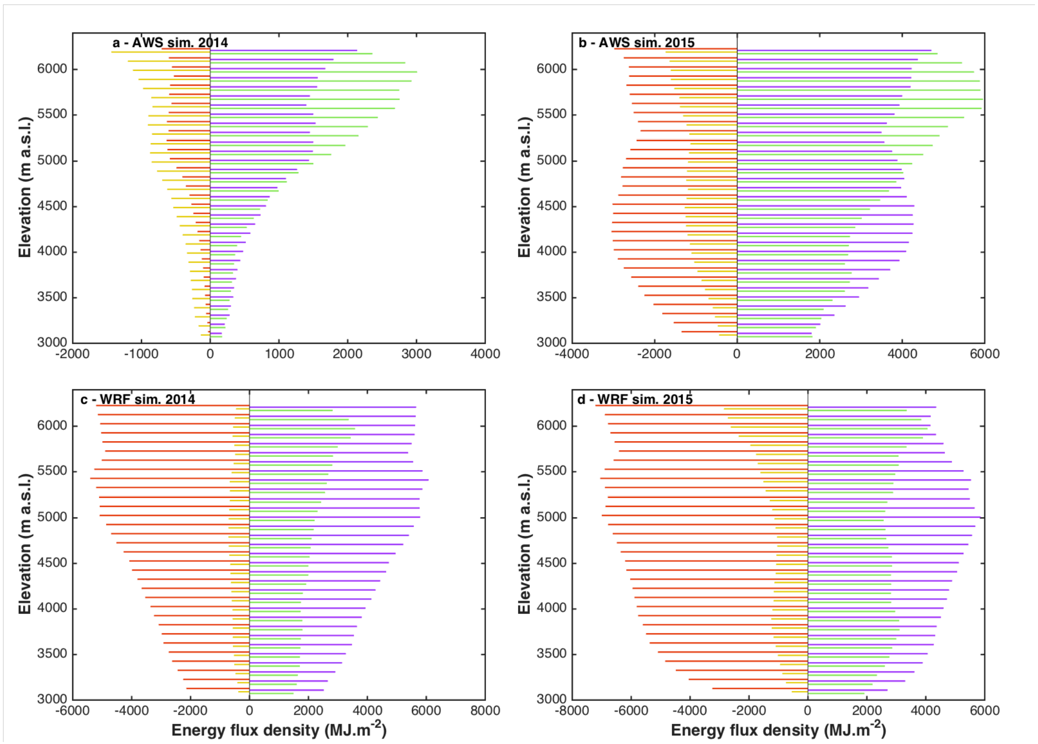


Figure: Annual sum of main modeled energy fluxes (computed over snow surfaces only) for each 200 m elevation band using AWS (a,b) and WRF (c,d) forcing for 2014 (a,c) and 2015 (b,d). SW (green) is net shortwave radiation, LW (yellow) is net longwave radiation, QE (red) is the latent heat flux and QH (purple) is the sensible heat flux.

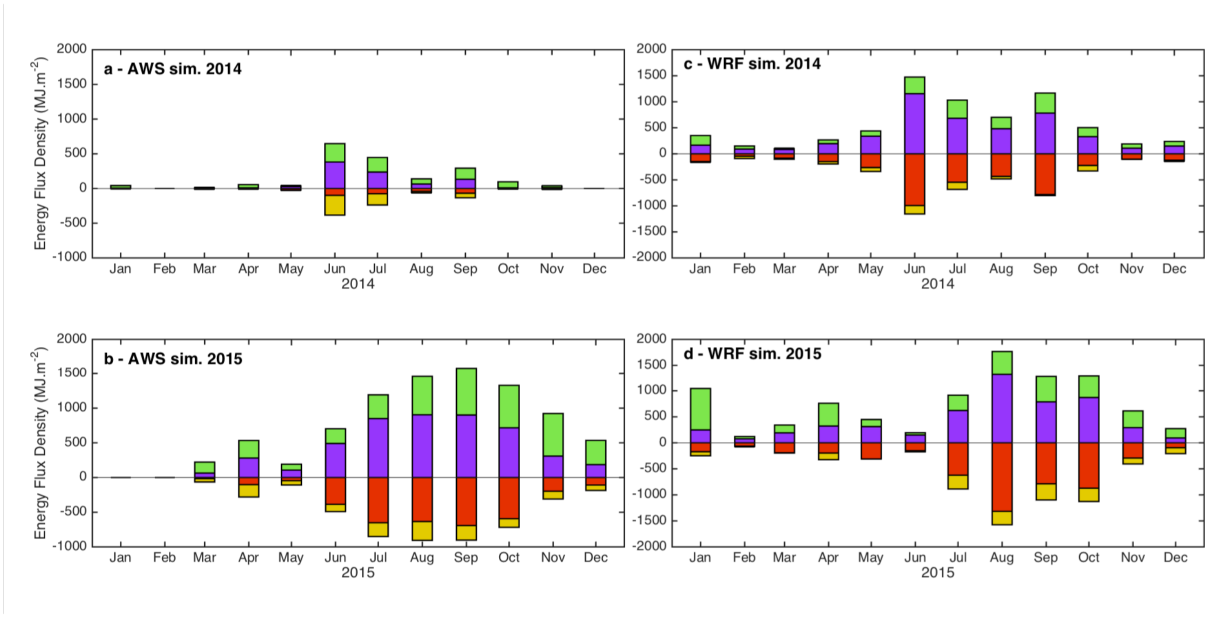


Figure: Monthly sum of the main modeled energy fluxes for the entire catchment, over snow surfaces only. SW (green) is net shortwave radiation, LW (yellow) is net longwave radiation, QE (red) is the latent heat flux and QH (purple) is the sensible heat flux

5) A Figure showing the energy fluxes against elevation has been added to the revised manuscript (Figure 9), before the catchment averages are discussed as suggested.

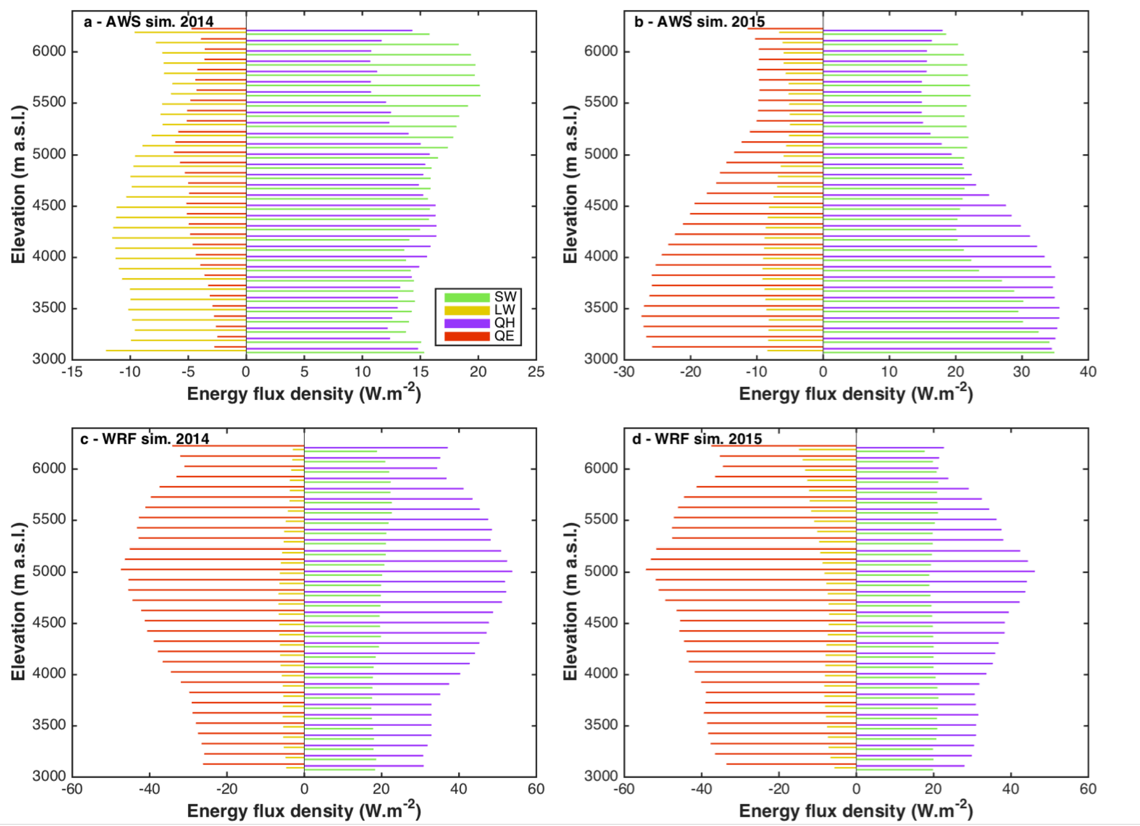


Figure 9: Annual mean of main modeled energy fluxes (computed over snow surfaces only) for each 200 m elevation band using AWS (a,b) and WRF (c,d) forcing for 2014 (a,c) and 2015 (b,d). SW is net shortwave radiation, LW is net longwave radiation, QE is the latent heat flux and QH is the sensible heat flux.

B) Specific Comments

P3-L21 *The maximum elevation of the catchment is listed as 5630 m, but figures 4,7,10 and 11 show elevation bins > 6000m. Please clarify.*
Authors'answer: Thank you for this observation, indeed, the maximum and the minimum elevation of the DEM presented in Figure 1 are respectively 6211 and 3143 m a.s.l.. This has been corrected in the revised manuscript.

P7-L1 *The figure caption describes these as hourly average values, but from the look of things (especially the SW) these appear to be daily averages. Please check and revise text.*
Authors'answer: Thank you for this observation; the caption is incorrect as daily data are plotted here. This has been corrected.

P8-L13 *Please indicate what height were the WRF output were output and if any scaling was used to transform their heights in Micromet.*
Authors'answer: The WRF outputs are 2 m above the surface. This information is an input into the *MicroMet* model, and the meteorological outputs are therefore at 2m. This information has been added in the manuscript as follows:
"The model outputs are at 2m above the surface and are available at 22 km resolution over Chile"

P10-L30 *Please indicate what sites and periods were used to choose the snow albedo values.*
Authors'answer: Albedo measurement availability is mentioned in Figure 2. All the measurements have been used to calibrate the model. This is point is now clarified clarified in the new manuscript:
"The minimum and maximum snow albedo (corresponding to old and fresh snow, respectively) are respectively fixed to 0.6 and 0.9 in agreements with all the measurements performed at the AWSs (Figure 2)."

P11-L26 *Please provide a fuller description of the kappa statistic as it is not a commonly used metric.*
Authors'answer: A more complete definition of kappa statistic has been added as follow:
"The performance was evaluated using a Kappa statistic coefficient (Cohen, 1960) denoted *k,* to measure the agreement between the simulation and the observation, considering the percentage of time with and without snow. The calculation of k is here performed according to the following formula:

$$k = \frac{\Pr(a) - \Pr(e)}{1 - \Pr(e)} \qquad (1)$$

where *Pr(a)* represents the actual observed agreement (i.e. snow or no snow for both simulation and observation); and *Pr(e)* represents the hypothetical probability of chance agreement. Complete agreement is defined when *k*=1."

P12-L5 *The definition of the sublimation ratio is not clear – is it the ratio of ablation totals, or sublimation vs melt rates (which depend on whether the surface is snow covered or not). This ambiguity becomes apparent later (see note 23-1). Please clarify in the text, perhaps with an equation.*
Authors'answer: This is now better defined in the method section:
"The sublimation ratio is defined as a percentage, and equal to the sublimation divided by the total ablation (i.e. sublimation and melt rates). Note that sublimation and energy balance are only computed over snow surfaces. This means that annual and monthly means are only computed at grid-cells with snow."
P13-L1 *The comparison of WRF simulations with AWS measurements needs to be shown if they are to*

be discussed. A table showing validation metrics (mean bias, mean absolute error etc) for different variables at each site used would be useful.

Authors'answer: We agree with this comment. Nevertheless, the direct comparison between the AWS measurements and the WRF outputs remains complicated in this study, mainly due to the spatial offset (and especially the vertical difference) between the AWS location and the closest WRF grid point. Therefore we chose to present the comparison at the catchment scale from the *MicroMet* outputs to overcome this issue. As this information remains important, it is now mentioned in the manuscript and the vertical offset is available in the Table S1 in the supplementary material.

In addition, a Table showing validation metrics (R2, RMSE and Absolute mean error) between the AWS measurements and the outputs from the closest WRF grid-point after running *MicroMet* is provided in the supplementary material.

"Details and statistics information about the comparison at each AWS locations are available in Table S1 (in the supplementary material). Note that here the comparison between the AWS measurements and the closest WRF grid point is not presented due to the significant vertical offset between the two points (Table S1 in the supplementary material)."

P13-L21 *Please include comparison of WS, SWin, LWin in this section and Figure 4. These inputs are critical to the simulation of sublimation through the latent heat flux, surface temperature, and albedo.*

Authors'answer: The figure has been modified (see below) and new panels (Figure 4 e,f,g) have been added to the manuscript:

"The SWi and LWi remain very similar. The wind speed outputs differ (Figure 4e), especially above 4500 m a.s.l. where differences reach a maximum of 4 m s^{-1}. The comparison between the AWS measurements and the closest WRF grid point output yield similar results. However this comparison should be viewed with caution given that there is a spatial offset between the AWS location and the closest WRF grid (Table S1 in the supplementary material)."

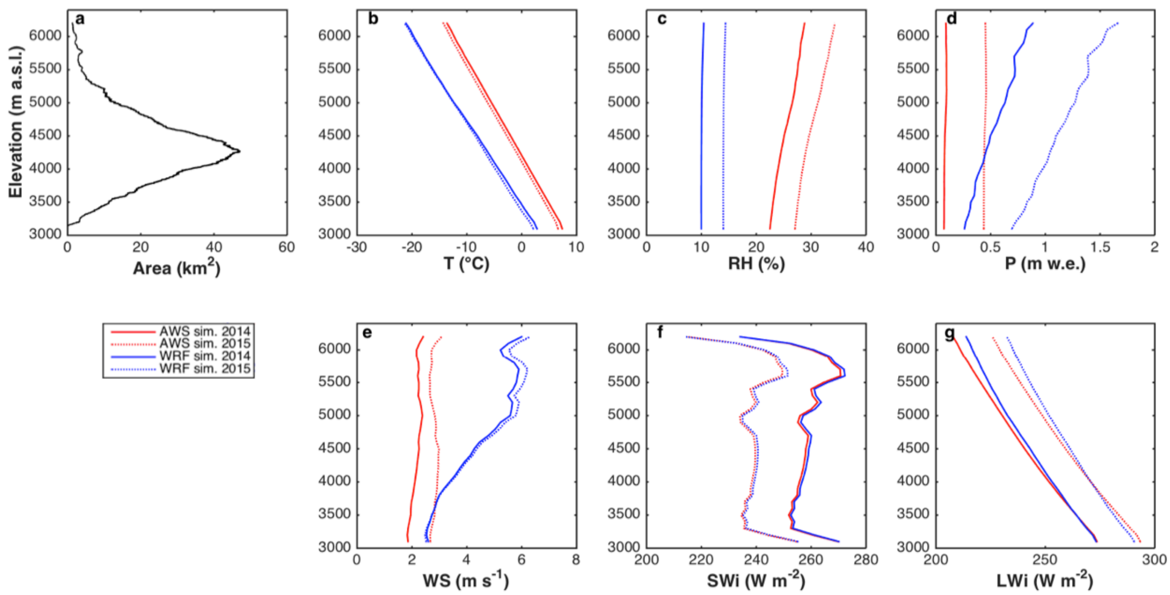


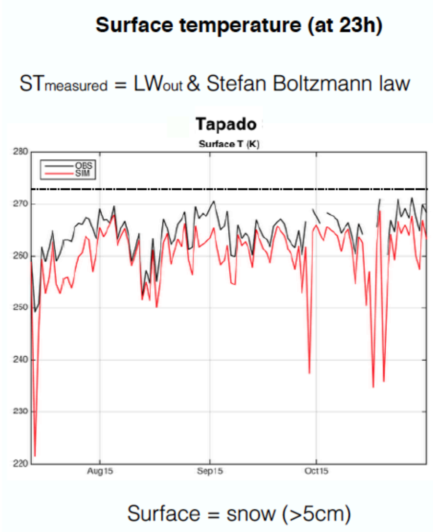
Figure 4: (a) Area-elevation distribution of the La Laguna catchment. (b to g) MicroMet outputs at the catchment scale forced by the AWS (red) and the WRF (blue) for 2014 (lines) and 2015 (dashed lines).

P13-L22 *Figure 4 would be more insightful if the actual mean values for T, RH etc were plotted for each forcing, rather than just the differences. E.g. Is the difference in precip because AWS precip decreases with height or WRF precip increases with height?*

Authors'answer: Figure 4 now represents the actual mean values (see figure above).

P14-L6 *Model validation. Were any LWout or Ts measurements available for validation? A comparison of modelled vs measured surface temperature would strengthen the validation of turbulent flux and subsurface scheme choices, which is a key area of uncertainty (as shown later by the sensitivity to z0).*

Authors'answer: We completely agree with this remark. There are LWO measurements available at the Tapado AWS for the July to December 2015 period. The comparison of these measurements (when snow height > 0.05 m) to the MicroMet output shows that the model generally follows a similar pattern to the measured surface temperature, however consistently underestimates surface temperature (see below Figure). Measured and modeled surface temperatures are consistently below zero, which gives confidence in the use of the parameterization. We have chosen not to include this new Figure in the revised version as it would likely complicate the flow of the paper.



P16-L1 *In the caption, please indicate that periods of the validation data are missing. The green colour is hard to distinguish from the black, thus it appears the AWS simulation performed poorly at the three Tapado sites in late 2015 when there is a data gap. Consider using a different colour for the observed snow depth.*

Authors'answer: The color of the measurement has been changed to red. The areas shaded in green indicate the period with validation data so we are not sure that adding this information in the legend is necessary.

P18-L6 *Please state what threshold was used to designate a snow-covered grid point in the model (e.g. 0.005 m w.e.). This can have a large bearing on the snow cover duration results, especially for small snowfalls such as those produced by WRF.*

Authors'answer: The threshold used to designate a snow-covered grid point is fixed at 3mm w.e.. This information has been added in the method part as new section:

"The snow cover area (SCA) and the snow cover duration (SCD) over the entire catchment were compared to the MODIS product. A threshold of 0.003 m w.e. was used to convert the simulated SWE into snow presence or absence for each grid cell (within the same range as Gascoin et al., 2015). Since the MODIS SCA product corresponds to the maximum visible extent over a period of 8 days, we also computed the maximum SCA over the same 8 day period from the simulated SCA for comparison."

P18-L6 *Please explain how data were averaged spatial and temporally (e.g. the average snow cover* *duration calculated for each individual grid point in the elevation band? Or the average of the grid cells that correspond with the modis pixels in each elevation band).*

Authors'answer: This has been clarified as follow:

"The simulated snow cover duration (SCD) was also compared to the observed duration (from MODIS) by elevation band. For all each 200 m elevation band, the total number of snow-covered days for each grid cell was computed and then averaged for each band."

P18-L7 *"Better performances were obtained for the AWS-forcings" while this is strictly correct, I don't think this comment is balanced. In 2015 the simulations are comparable and the improvement with the AWS forcing is minor.*

Authors'answer: We agree with this comment and the results are described for each year, allowing the reader to make this distinction: "For 2014, better performances were obtained for the AWS-forcing than for the WRF-forcing (Figure 7). For 2015, while better performances were also obtained for the AWS-forcing, the improvement using this forcing was minor."

P19-L9 *Because elevation seems to have a greater effect on the sublimation rate, it would be useful to present these results (figure 10) before presenting the SEB and sublimation ratio results that are calculated over the whole catchment for snow-covered points only (Fig 8, 9). This would give better context for the somewhat complex interactions between SCA, SCD and meteorology. It would also be very useful to show the SEB results averaged in elevation bins after figure 10 to show reasons why the WRF simulations have higher sublimation.*

Authors'answer: To address this comment, and in response to the general comment above, Figure 10 is now Figure 8 in the revised manuscript and comes before the SEB and sublimation ratio results computed for the entire catchment. In addition, the figure showing the energy contribution of each flux against elevation has been added:

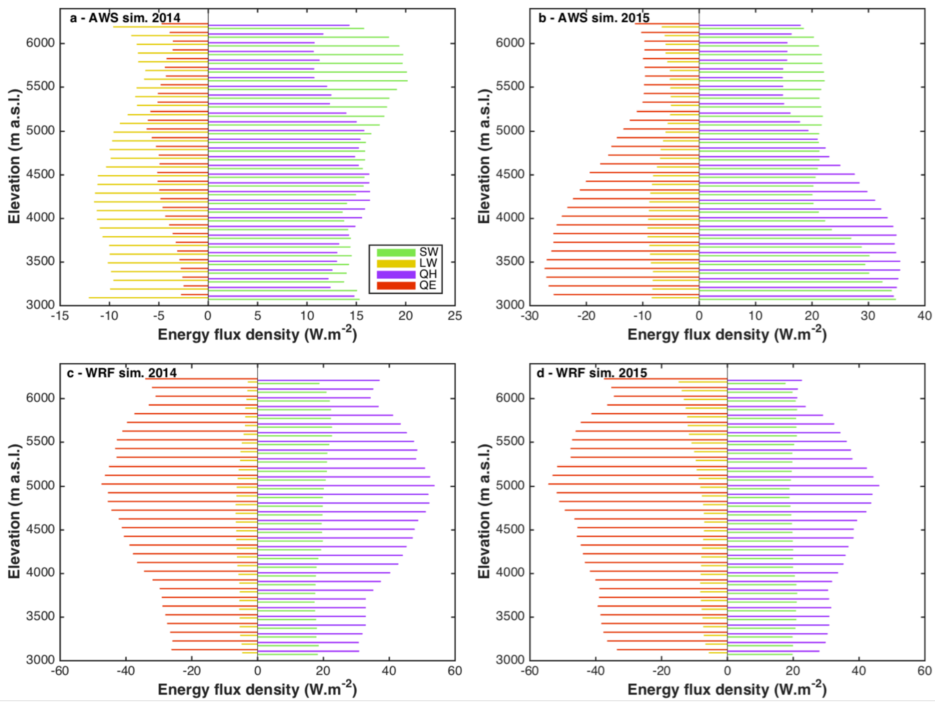


Figure 9: Annual mean of main modeled energy fluxes (computed over snow surfaces only) for each 200 m elevation band using AWS (a,b) and WRF (c,d) forcing for 2014 (a,c) and 2015 (b,d). SW is net shortwave radiation, LW is net longwave radiation, QE is the latent heat flux and QH is the sensible heat flux.

P19-L11 *Do the annual means include periods with no snow as 0 values? Please clarify how the annual means are calculated?*
Authors'answer: The means are calculated only for period with snow. This information has been added in the methods section as follows:
"Note that sublimation and energy balance are only computed over snow surfaces. This means that annual and monthly means are only computed at grid-cells with snow."

P19-L12 *"For the mean AWS-simulation net SW is 24 and 23" do you mean the WRF-simulation here?*
Authors'answer: Yes we mean WRF-simulation. This has been corrected.

P19-L14 *Do you mean -6 and -7 Wm-2? These figures represent fairly small losses compared to mid-latitude sites (e.g. -25 to -20 Wm-2 in Giesen et al, 2009), which is presumably due to the cold surface temperature of the snow surfaces.*
Giesen RH, Andreassen LM, Van den Broeke MR and Oerlemans J (2009) Comparison of the meteorology and surface energy balance at Storbreen and Midtdalsbreen, two glaciers in southern Norway. Cryosphere, 3(1), 57–74, doi: 10.5194/tc-3-57-2009
Authors'answer: Yes, we mean "-6 and -7 W m^{-2}" and this has been corrected. The reference to the study made by Giesen et al., has been added to address the comment above. The text now reads:
"The contribution of net LW on the other hand is low for all simulations (annual mean of -7/-6 W.m^{-2} for AWS/WRF-simulations respectively). Note that these losses are small in comparison to mid-latitude sites (e.g. -25 to -20 W m^{-2} according to the study by Giesen et al, (2009)), because of the very dry conditions of the atmosphere and to the cold surface temperature of the snow surfaces."

P22-L1 *Are these figures monthly average amounts for only snow-covered grid cells or something else? Please explain the averaging procedure in the methods section.*
Authors'answer: Yes the mean calculation only consider snow-covered grid cells. A sentence has been added in the methods section to clarify this: "Note that sublimation and energy balance are only computed over snow surfaces. This means that annual and monthly means are only computed at grid-cells with snow."

P23-L1 *Figure10 – the sublimation ratio and ablation rates do not seem to match up – the ratio at high elevations is ~100% which implies there is little melt, but for both AWS and WRF forcing there is still a significant melt rate, and for AWS in 2015, melt rate=sublimation rate. Is this an artefact of the averaging of melt rate over snow only? Also, why do melt rates increase with height? Perhaps you are better to present the ablation totals rather than the ablation rates? Either way, the top and bottom panels should be consistent.*
Authors'answer: Thank you for your comment, you were right; the ratios were computed incorrectly. This has been fixed and the figure has been changed accordingly:

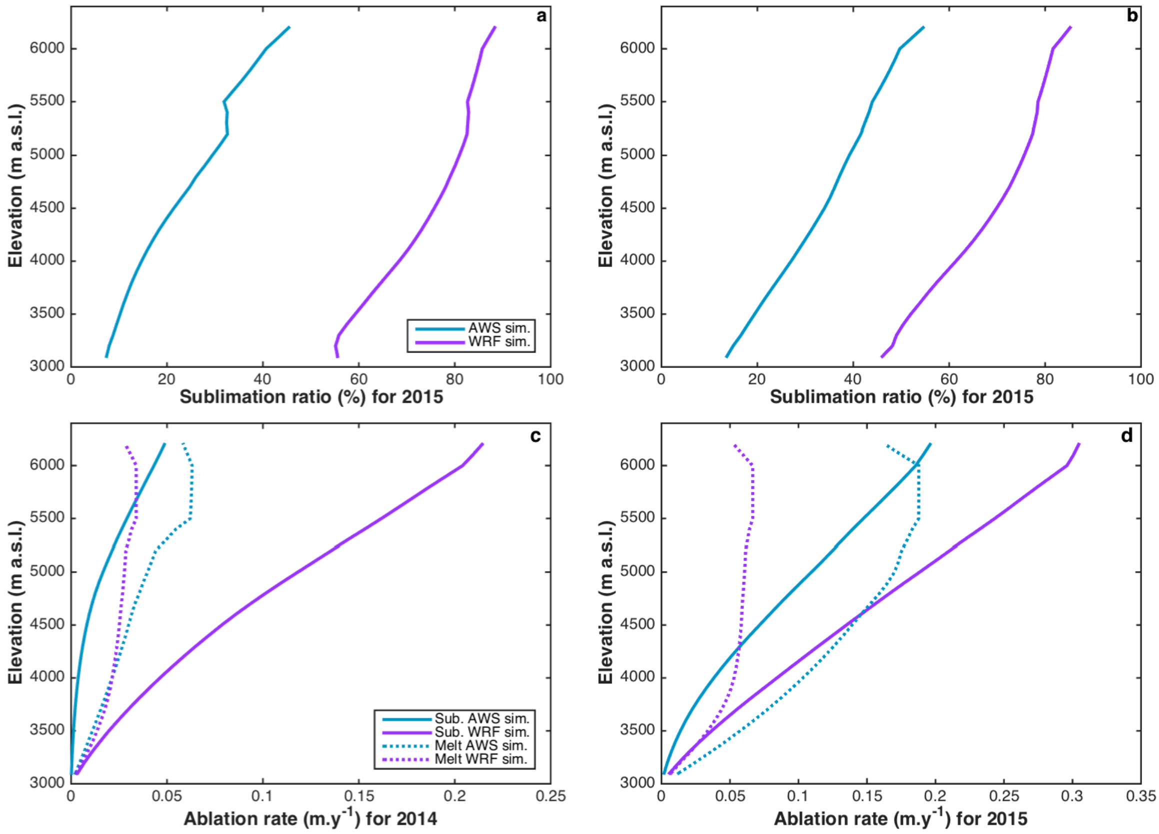


Figure 8: Simulated annual sublimation ratio (a,b) against elevation band using AWS (blue) and WRF (purple) forcing for 2014 (a,c) and 2015 (b,d). Simulated annual ablation ratio (sublimation and melt) against the elevation using AWS and WRF-forcing for 2014 (c) and 2015 (d).

P26-L30 *It is not clear what you mean by evaposublimation? Do you mean the evaporation of liquid from a melting surface or a combination of this process + sublimation from a frozen surface? If you are including evaporation from a melting surface, then doesn't the increased rate occur because the melting lowers the latent heat required to transform the water to vapour? Please be more explicit about the process occurring.*

Authors'answer: To avoid confusion this word has been removed. The sentence now reads: "This may be explained by warmer conditions which induce a warmer snow pack, increasing the saturated vapor pressure at the snow surface, providing energy to increase the sublimation rates (Herrero and Polo, 2016)"

Figure S2 *If this precipitation sensitivity analysis is retained, Figure S2 needs to be included in the results section as it is discussed directly.*

Authors'answer: As the paper has been restructured we decided to keep this figure in the supplementary material.