Review of revised manuscript “Impact of forcing on sublimation simulations for a high mountain catchment in the semi-arid Andes” by Réveillet et al.

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General comments

The manuscript is much improved from the initial submission. The paper now focusses more on the differences in sublimation when using different forcing datasets, with a smaller focus on differences in sublimation due to interannual climate variability and elevation. The new and amended figures highlight useful information and the results have been structured in a logical fashion.

The interpretation and discussion around the central result of the paper (increased sublimation rate and ratio when using WRF output) is, however, still lacking and deserves much more attention. The assertion that differences in snow cover are driving the differences is not compelling and is not supported by all the results. For instance, it does not explain why sublimation is still higher in 2015 when the snow-covered area is similar between WRF and AWS simulations. Colder temperature and lower RH will tend to favour sublimation over melt, but these factors do not explain the larger magnitude of both the sensible and latent heat for WRF simulations across all elevations in both years.

I would suggest that wind speed is one of the key reasons for differences in sublimation that needs discussion. Figure 4 shows large differences in wind speed between AWS and WRF forcing (average ~2 vs ~5 ms^{-1}). The turbulent fluxes are very sensitive to variations in wind speed in this range, especially if stability corrections are employed. Figure 9 shows both latent and sensible heat fluxes are increased in WRF simulations compared to AWS simulations, indicating that wind speed is driving an increased magnitude of turbulent fluxes (and hence sublimation). Another piece of evidence that wind speed is driving increased sublimation is the increased sublimation in 2015 for AWS simulations, which have higher wind speed than in 2014.

The interpretation in the results and discussion (including sections 4.3.2, 5.1, 5.1.2 and specific comments) needs revised to align with the new results presented.

In addition, some of the methods that are central to the key results need further description. In particular: the calculation of turbulent fluxes, extrapolation of radiation, definition of mass balance terms, validation of surface temperature (see specific comments).

One further area of concern is the discrepancy between the total precipitation and total ablation simulated by each forcing dataset. In 2015, snow covered area starts and ends the year close to 0 (Figure 6) implying that all the snow that fell during the year has been lost in the year. Both forcing datasets result in a similar magnitudes of SCD and ablation rate with elevation (figures 7 and 8) as well as catchment average total ablation (figure 10), which appears to be around 0.4 m w.e.. Yet the precipitation in WRF is between 0.20 to 1 m w.e. larger than the AWS forcing. What has happened to this extra precipitation in the WRF simulations? Has it fallen as rain? Please explain.

Once these points are addressed the paper will make a valuable contribution to the literature.

Specific comments:

P1ln24 “*melt/sublimation ratios*” Please be consistent with the terms used in the rest of the manuscript i.e. “ratio of sublimation to total ablation (sublimation+melt)”

P1ln25 “*due to…*” This statement needs revising in line with the major comment above. It is not clear that the increased precipitation increases the sublimation ratio. In fact, this is at odds with the last sentence in the abstract.

P8ln10 Given the overestimation of wind speed in the WRF output, it would be worth checking that the data used were in fact supplied at 2m height - standard WRF outputs for surface wind speed are given at 10m and will be markedly higher than 2m wind speed.

Section 3.1 or 3.2 Please describe in more detail the mass balance terms used. I.e. does ‘sublimation’ include evaporation from a melting surface? Does melt include melt that is refrozen in the snowpack or only that which drains from the snowpack? This will make a big difference to the sublimation ratio.

P10ln30 Please describe in more detail the relevant aspects on the turbulent flux parameterisation in SnowModel (i.e. surface temperature calculation, stability correction, whether the option to enhance turbulent fluxes for patchy snow was used etc). This is a key aspect of the methods the deserves more attention.

P12ln30 The variation of LW and SW with elevation in Figure 4f,g does not make sense - if Ta and RH are lower with WRF, then LW should be less but the WRF values are very similar to the AWS values in each year. Similarly, the SW values for WRF and AWS data are very close for each year, despite larger differences in RH between WRF and AWS than between each year for each data source. Can the authors check the lines are labelled correctly and revise comments regarding differences in LW and SW.

Following on from the above point, section 3.2.1 needs to describe (at least in general) how short and longwave fluxes are extrapolated. From reading Liston and it seems that cloud cover is estimated using RH, then predetermined cloud extinction coefficients are used to calculate SW and LW across elevations. Was the option to ‘assimilate’ the observations used in this study? Neither of these is particularly standard practice when SW and LW observations are available to distribute LW and SW across a catchment, and these inputs will have a large bearing on the simulated energy and mass balances.

P13ln10 It is good to see the validation of the surface temperature (albeit at one site only), and it would be useful to include this at the beginning of section 4.2 or at least the supplementary material, given that a correct estimation of surface temperature is key to a correct simulation of melt vs sublimation. The surface temperature simulated with WRF should also be included on this figure too – I expect it will be lower than the AWS measurements given the predominance of sublimation.

P22ln6 “*This [larger turbulent fluxes in WRF vs AWS for 2014] can be explained by the larger snow cover simulated by WRF-forcing with snow cover in the entire catchment while the AWS-forcing only results in snow at higher elevations. Since the fluxes are computed over snow surfaces only, WRF increases the contribution of warmer, low elevation areas.”* Figure 8 shows that the differences in turbulent fluxes are similar at all elevations, which does not support this statement. In 2015, the snow cover is similar between WRF and AWS simulations, yet turbulent fluxes and sublimation are still much larger in the WRF simulations – suggesting that snow cover duration is not driving the differences in sublimation. Please remove or revise this statement.

P24ln26 "*On the other hand biases in wind speed, incoming LW and SWi and air pressure are low for both years (results not shown).*" Figure 4 now wind speed, LWi and SWi, this comment needs revised. A large bias in wind speed is shown in the figure and needs discussing.

P25l6 *"Snow that persists over a longer period results in an increased melt rate and can influence the sublimation rate and ratio (especially in 2015, Figure 10)."* This comment is confusing as SCD in 2015 was similar between AWS and WRF simulations, hence it cannot be the driver for differences between WRF and AWS. Please revise.

P25l8 "*This is the only explanation for the larger melt rate observed with AWS-forcing compared to WRF-forcing at low elevations, given the cold bias in WRF-forcing (Figures 8c and 4b)."* Figure 10c shows that in 2014 the low elevation areas show a similar melt rate between WRF and AWS simulations so it is unclear what is meant here. The cold bias in WRF forcing would tend to favour lower melt, which would explain the higher melt with AWS forcing. Please revise.

Editorial comments:

Figure 4f,g – please check the lines are labelled correctly.

Figure 5a – should the legend read "AWS sim" rather than "WRF sim"

Figure 5 caption – correction to colours – "vs. observed (red)…" and "Grey shaded areas.."

Figure 8a ylabel – "…(%) for 2014"

Figure 8 caption – "… forcing for 2014 (a) and 2015 (b). Simulated annual average total ablation (sublimation and melt) …

Figure 9 – common x limits would make it easier to compare between years as well as with elevation.

Figure 11 – common y limits would make it easier to compare between forcing datasets.