Review of manuscript "Uncertainties in the spatial distribution of snow sublimation in the semi-arid Andes of Chile" by Marion Réveillet, Shelley MacDonell, Simon Gascoin, Christophe Kinnard, Stef Lhermitte and Nicole Schaffer.

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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.

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:

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

- 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.
- 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).
- 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.

Specific Comments (page-line)

3-21 The maximum elevation of the catchment is listed as 5630 m, but figures 4,7,10 and 11 show elevation bins > 6000m. Please clarify.

7-1 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.

8-13 Please indicate what height were the WRF output were output and if any scaling was used to transform their heights in Micromet.

10-30 Please indicate what sites and periods were used to choose the snow albedo values.

11-26 Please provide a fuller description of the kappa statistic as it is not a commonly used metric.

12-5 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.

13-1 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.

13-21 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.

13-22 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?

14-6 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).

16-1 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.

18-6. 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.

18-6 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).

18-7 “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.

19-9 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.

19-11 Do the annual means include periods with no snow as 0 values? Please clarify how the annual means are calculated?

19-12 “For the mean AWS-simulation net SW is 24 and 23” do you mean the WRF-simulation here?

19-14 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.

22-1 Are these figures monthly average amounts for only snow-covered grid cells or something else? Please explain the averaging procedure in the methods section.

23-1 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.

26-30 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.

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

Additional references

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