

## Response to Reviewer #1

### A) General Comments

*In the manuscript “Uncertainties in the spatial distribution of snow sublimation in the semi-arid Andes of Chile” Réveillet et al. present results of their study aiming to simulate melt and sublimation rates over the instrumented watershed of La Laguna. They present the relative importance of sublimation versus snow melt using a distributed snowpack model for two meteorologically contrasting years. They detected a large difference in modelled sublimation rates forcing the model with data of Automatic Weather Stations (AWS) and with Weather Research and Forecasting (WRF) model data. This difference is caused by (a) the different meteorological input, particularly in precipitation and temperature, and (b) by the modelled snow cover persistence. The objective of the study of Réveillet et al. is to assess the uncertainties in melt and sublimation arising from modelling snow evolution using AWS and/or WRF-model generated meteorological datasets. Since the seasonal snow cover and glacier melt as processes of the cryosphere have locally a high contribution to available fresh water in this region. The study of Réveillet et al. contributes to gain knowledge of snow depth distribution and snow cover processes in the semi-arid Andes of Chile, and thus the manuscript is in the scope of TC. In general, this study shows again the importance of a critical interpretation of model results with respect to model input data. The introduction is complete, the applied methods are appropriate and comprehensible, and the results are compared to referenced work. However, the manuscript at its present stage summarizes results of interesting work packages and analysis, but lacks in the overarching aim with a clear problem statement, the research questions and the respective conclusions. This is also obvious in the high number of subsections presenting methods and results which not necessarily contribute to the conclusions. My suggestion is to restructure the manuscript with a clear focus on formulated research questions and to revise the title reconsidering the term “uncertainty”.*

### Authors’ answer:

We thank the reviewer for this constructive and thorough review of the manuscript.

As requested, and in agreement with comments made by the reviewer #2, the manuscript has been restructured and now focuses on the differences in simulated sublimation depending on the forcing used. More information is provided in the detailed response to Specific comment #1.

### B) Specific Comments

*(1) As addressed in the general comments, the manuscript shows an interesting work but without presenting an overarching aim and respective research questions. Forcing the model with different data from AWS and WRF does not result only in differences of sublimation rates, but also differences in e.g. snow covered area, snow persistence and snow melt. So I would suggest that not the uncertainties in the spatial distribution of snow sublimation are shown. Rather the variation of snow related parameters and processes forcing the applied model with different input is presented. Since the weaknesses of the WRF model output are known (cold bias, precipitation overestimation) and the AWS data might be the more appropriate model input, the study shows the error of forcing the model with WRF data for snow parameters (i.e. the overestimation of the snow cover duration by 2 month). The impacts of these errors are particularly obvious in the sublimation rates and ratios, which again are a function of snow coverage, elevation, temperature, etc. If the (model?!) uncertainty in sublimation should be addressed in more detail, I would suggest to show at least one additional figure presenting the calculated and simulated sublimation rates at AWS locations. In my opinion, the manuscript at its*

*present stage presents "Differences in simulated sublimation in a high mountain catchment of the semi-arid Andes of Chile using AWS data and WRF meteorological forcing". Possible research question may address the main differences (errors?) in sublimation simulated using the WRF forcing, the impact of SCA and SCD under/overestimation, and the effect of different meteorological conditions of the two contrasting years on sublimation ratios. This can lead to conclusions about advantages and disadvantages of using the different forcing data. Most of the results are already presented, but the paper should be restructured accordingly to answer the formulated research questions and further drawing the conclusions.*

**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 renamed to "[Snow depth and snow cover comparison](#)" for consistency).

The results sections of the original manuscript have also been revised and organized in:

#### [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 with respect to elevation (as suggested by reviewer #2)

##### [4.3.2 Monthly evolutions](#)

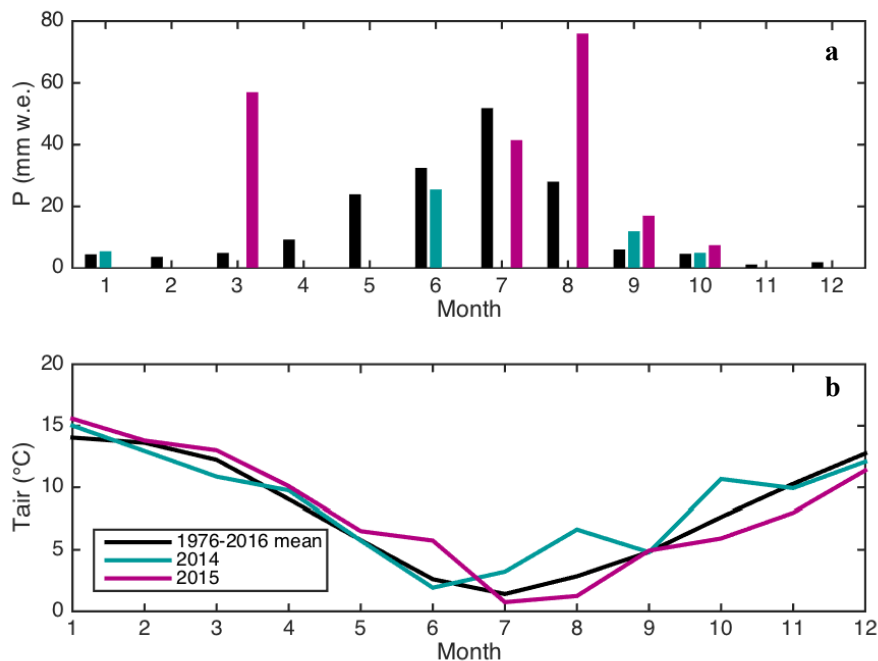
In this section, Figures 9 and 8 are presented and commented Figures 9 and 8.

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 re-organisation was chosen to focus the conclusions on advantages and disadvantages of using the different forcing data. For that purpose, differences in terms of meteorological data and the consequences on SD, SC and sublimation are studied, as suggested in comment #1.

*(2) Two years with contrasting meteorological conditions have been chosen for this study. This has the advantage of testing the simulation results, but also seems to be restricted to AWS data availability. To get an overview of the overall climate in this region and to classify the two selected years in a climatological context, please present a short climate overview of the last 30 years from a nearby station, or at least some short statistics for the AWS with the longest data history (La Laguna?)*

**Authors'answer:** To address comment #2, a figure showing the monthly mean air temperature and precipitation over the 1976-2016 period recorded at la Laguna has been added to the supplementary information. The 2014 and 2015 measurements have been included in this figure.



*Figure S1: Monthly precipitation (a) and air temperature (b) recorded at La Laguna station. The monthly mean (black) is computed over the 1976-2016 period.*

(3) The downscaling of the WRF data still appears opaque to me. Is the relatively large  $T$  difference (Page 13 L22) before or after the adjustment? If before, what is the temperature offset after the adjustment? Please also present some standard deviation of the hourly/daily/monthly  $T$  values using the mean monthly gradients. What is about thermo-dynamics considering relative humidity and saturation for calculating lapse-rates. Please give some more detailed information in section 3.2.1. and the results.

**Authors' answer:** We agree that the section 4.1.2 dealing with the comparison between the forcing was confusing. As the paper has been re-structured and now focuses in more details on the impact of the forcing choice on the simulation, the forcing data comparison is important. Modifications have been made in this section accordingly.

1- We agree that having the comparison before running the *Micromet* subroutine is interesting. Nevertheless, the direct comparison 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. This is why we chose to present in the paper 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 ( $R^2$ , 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).”

2- We don't really understand your request. Could you please clarify this point? Are you talking about the standard deviation for each station for each month between the AWS measurements and the *Micromet* outputs forced by the AWS-forcing? Or a cross-validation study?

In addition, measurements have been used to compute the lapse rates, but as results were close to the default values, it has been chosen to keep the default parameterization to run the model. These information are given in section 3.2.1 :

“Spatial interpolation using the Barnes scheme was used to distribute the nine AWS measurements of T, RH, LWi, SWi and pressure over the model domain. As relative humidity is a non-linear function of elevation, the relatively linear dewpoint temperature is used for the elevation adjustment. For more details refer to Liston and Elder (2006). In this study the *MicroMet* subroutine has been run with the default setting for the Southern Hemisphere, for air temperature and dewpoint temperature monthly lapse rates (Liston and Elder, 2006b). Monthly lapse rates computed from the available measurements are dependent on the year considered. As the mean is close to the default settings, it has been chosen to conserve these values.”

3- Section 3.2.1 has been clarified as follow:

“The 3km WRF outputs (i.e. the 240 points (Figure 1) described in section 2.2.1) were used as inputs for *MicroMet* which considers that each WRF cell corresponds to a virtual weather station located in the center of the WRF cell, following Mernild et al. (2017) and Baba et al. (2018a). In other words, this means that MicroMet has been forced by 240 virtual stations containing the WRF outputs meteorological data. As an vertical offset exists between the WRF grid point elevation and the DEM, MicroMet adjusts this offset at the corresponding coordinate and downscales the data to a 100 m grid.” The result part was also modified as mention above.

### C) Detailed Comments

**P1 L23:** Please present the longitude in addition to the latitude.

**Authors’ answer:** This information has been added: 70°W

**P1 L23:** Here an throughout the text: Above sea level can be abbreviated by "a.s.l."

**Authors’ answer:** Done

**P1 L23:** Are the two years contrasting in hydrology or meteorology (or both).

**Authors’ answer:** This is a difficult question given that long-term gauges are located downstream of a dam (La Laguna). It’s likely both, but in this study we are more focused on precipitation amounts. We have specified this in the manuscript:

“...La Laguna (3150–5630 m a.s.l., 30°S 70°W), during two hydrologically contrasting years (i.e. dry vs. wet).”

In addition, in the introduction and section 2 a reference to the figure S1 has been added.

**P1 L27:** Replace "increased by 100%" with "doubled"

**Authors’ answer:** Done

**P2 L3:** Replace "cryosphere" with "glaciers(?) and the seasonal snow cover"

**Authors’ answer:** Done

**P2 L5:** "winter months". Please consider to present these months in the introduction (June, July, August?!)

**Authors’ answer:** It has been defined as follows: “that are largely limited to winter months (i.e. June, July and August)”

**P2 L5:** *"intermittent": This can also mean at regular intervals, but I think you mean "erratic"?*

**Authors' answer:** Yes, "intermittent" has been changed to "[erratic](#)"

**P2 L24:** *Delete "evolution"*

**Authors' answer:** Done

**P3 L6:** *Revise to one relatively wet and one d*

**Authors' answer:** The sentence was modified accordingly.

**P3 L15:** *Remove "over time". Instead you can present a date to which the snow cover duration persist (which month/season?)*

**Authors' answer:** "over time" has been changed to "[at the end of the winter season \(i.e. in August, September\)](#)", to address this comment.

**P3 L18:** *Add the longitude.*

**Authors' answer:** The longitude has been specified as in the abstract.

**P3 L20:** *"best instrumented": Please explain in more detail. In contrast to which other catchments? Or just write "well-equipped" or "mounted"*

**Authors' answer:** To address this comment, we have specified as follows: "...[is the most instrumented within the region](#)"

**P3 L21:** *Remove the "~" (also throughout the manuscript)*

**Authors' answer:** "~" has been removed throughout the manuscript.

**P3 L23:** *Remove "clean"*

**Authors' answer:** Done

**P3 L25:** *Please use 10x m3 instead of Mm3*

**Authors' answer:** Done, in addition the value was wrong. Therefore, "200 Mm<sup>3</sup>" has been change by "[38.10<sup>6</sup> m<sup>3</sup>](#)".

**P3 L26:** *Revise "rate" to "mean annual precipitation"*

**Authors' answer:** Done

**P3 L31:** *Is "area" the "study site"?*

**Authors' answer:** Yes, this has been specified.

**P3/ffP4:** *This sentence is hard to understand. Are these trajectories "storm paths". Please give some more detail. Also the sentence can be condensed to "The seasonal variability and frequency of precipitation events is also affected by precipitation trajectories..."*

**Authors' answer:** The sentence has been re-written: "[Seasonal precipitation variability and frequency are also complicated by individual storm trajectories \(e.g. Sinclair and MacDonell, 2016\) which can cause large differences in relative precipitation distribution across the catchment, a phenomenon also described in central Chile \(Burger et al., 2019\).](#)"

And the following reference has been added:

[Burger, F., Ayala, A., Farias, D., Shaw, T. E., MacDonell, S., Brock, B., McPhee, J. and Pellicciotti, F.: Interannual variability in glacier contribution to runoff from a high elevation Andean catchment:](#)

[understanding the role of debris cover in glacier hydrology. \*Hydrological Processes\*, 33\(2\), 214-229. <https://doi.org/10.1002/hyp.13354>, 2019](https://doi.org/10.1002/hyp.13354)

**P5 L7:** Remove "Finally"

**Authors' answer:** Done

**P5 L5:** Replace "for a specific campaign" by "next to the glacier"

**Authors' answer:** According to your comment, "for a specific campaign" has been replaced by "[in the debris-covered part of the glacier](#)"

**P8 L12:** Shift "the" to the front of La Laguna

**Authors' answer:** Done

**P8 L13:** Shift the reference to Figure one to the sentence before "the La Laguna catchment (Figure 1)"

**Authors' answer:** Done

**P9 L9ff:** Please consider to remove the subsections and to highlight the sub-model description by paragraphs and the sub-model names by italic font.

**Authors' answer:** Done

**P10 L8:** Please unify "T" or "Tair"

**Authors' answer:** "T" has been chosen and is now used over the entire manuscript.

**P10 L10:** Replace "please" by "we"

**Authors' answer:** Done

**P10 L28:** Add "snow albedo" to "minimum"

**Authors' answer:** Done

**P12 L3:** Add "sublimation" in front of "rate"

**Authors' answer:** Done

**P12 L19:** Since the abbreviations have been introduced, use SWi here. Please check this throughout the text.

**Authors' answer:** "SWi" is now used in the manuscript (and in the Figures) after being defined, as well as "LWi".

**P12 L19:** You present absolute values here, but how much is this in % of mean SWi?

**Authors' answer:** This information have been added for SWi and LWi. The paragraph has been re-written as follows:

"According to the AWS measurements, [Jan-Jul 2015](#) was warmer than [Jan-Jul 2014](#). Conversely, observations indicate lower temperatures for [Aug-Dec 2015](#) than for [Aug-Dec 2014](#) ([daily](#) mean difference of -2.6°C). Relative humidity [was higher for 2015 compared to 2014](#) ([daily](#) mean difference of 11%) [whereas SWi was lower](#) (mean difference of -18 W m<sup>-2</sup>, [i.e. 6% of the mean SWi](#)), with larger differences in [Jul-Dec](#) ([daily](#) mean difference of -32 W m<sup>-2</sup>, [i.e. 12% of the daily mean SWi](#)), [and LWi was higher](#) ([daily](#) mean difference of 20 W m<sup>-2</sup>, [i.e. 7% of the daily mean LWi](#)). [This decrease SWi and increase in LWi can be explained by a larger number of clouds in 2015.](#)"

**P12 L20:** *Why is it in agreement? Why are weather conditions with more clouds necessarily colder? Whats about clear sky conditions at night causing very low temperatures?*

**Authors'answer:** We agree that the relationship between SW and air temperature is more complex. To avoid confusion and considering that this information is not central to the paper, the sentence has been deleted.

**P13 L1:** *Is this comparison performed before or after the Barne-downscaling? Please clarify here.*

**Authors'answer:** Yes, it was performed before the Barne-downscaling. Nevertheless this section is confusing. In agreement with your specific comment 3 and remarks made by the second reviewer, this information is now in the supplementary information as a Table."

**P13 L25:** *Correct "annual"*

**Authors'answer:** Done

**P14 L8:** *Revise this sentence to: "Simulated snow depths using. . . agreement with measured snow depth values.*

**Authors'answer:** The sentence has been changed according to your comment. It now reads: "Simulated snow depths using the AWS-forcings (Figures 5 a-f) are in good agreement with measured snow depth values (mean  $k=0.14$  and mean  $RMSE=0.15$  m)."

**P15 L1:** *"forcings indicates": Remove one "s"*

**Authors'answer:** Done.

**P19 L9:** Please give more detail ion the time period and spatial extent of the averaged values here

**Authors'answer:** This sentence has been restructured and the issue in question is no longer relevant here. However, in order to address this comment and remarks made by reviewer #2, more details have been added in the methods section as follow: "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."

**P21 L7:** *Delete "when the snow. . ."*

**Authors'answer:** Done

**P25 L5:** *Revise to: Precipitation is known to be over-estimated using the WRF model*

**Authors'answer:** Done

**P25 L6:** *Correct "exist"*

**Authors'answer:** Done

**P25 L10:** *Please revise this sentence. Suggestion: Precipitation measurements using rain gauges can be biased towards an underestimation because of an undercatch particularly of snowfall due to wind influence.*

**Authors'answer:** Done

**P26 L26:** *Here the sublimation rate (absolute values) should be compared.*

**Authors'answer:** We agree with your comment and also think that having these rates will also be very useful for the discussion. Therefore, this information has been added to the results section as



follows:

“The mean daily rate is 0.6 mm w.e. d<sup>-1</sup> and 3.6 w.e. d<sup>-1</sup> for 2014 and 2015, respectively, when the model is forced with the AWS-forcing. Values are larger and reach 3.1 mm w.e. d<sup>-1</sup> and 4.1 mm w.e. d<sup>-1</sup> for 2014 and 2015 when simulations are performed with the WRF-forcing.”

The discussion has been entirely re-organized and all the sections in 5.2 have been re-written. Sublimation rates are now discussed in this section.

***P27 L3:** There will be no sublimation without a snow cover. Thus, this sentence is redundant. Perhaps you want to say that the snow cover duration SCD has a significant influence on sublimation/melt ratio.*

**Authors’ answer:** This sentence has been removed due to the restructuration and to avoid confusion.

***P28 L17:** It is rather the mass and energy balance of the snow cover, which includes sublimation.*

**Authors’ answer:** This sentence has been modified accordingly.

***P29 L19:** Precipitation data is not an uncertainty, but the uncertainty of measured/modelled precipitation is.*

**Authors’ answer:** The sentence has been re-written to address this comment: “First, the main forcing uncertainty is from precipitation due to measurement errors and lack of spatial representation as precipitation data was only available for two stations.”

#### **Figure 1:**

- Please include the reservoir and the glacier in the legend. Please adjust the elevation in the legend to the colour transitions.

**Authors’ answer:** Done

#### **Figure 3:**

- Please unify the units. I would suggest Wm<sup>-2</sup> and ms<sup>-1</sup>

- Please present in addition the snow depth, since this is an important parameter of this study

- Caption: Since only two years are presented, no "climatic" conditions are shown.

Please revise to "meteorological conditions at the . . ."

- Please uniform the radiation abbreviations throughout the figures/manuscript to avoid confusion between incoming (index i) outgoing etc.

- Remove the "s" from "precipitations"

**Authors’ answer:** Figure 3 and the caption have been revised to address this comment.

#### **Figure 4:**

- Caption: Replace ‘studied’ with "La Laguna"

- Please describe in the caption which output is subtracted from the other for interpretation of the sign on the differences?

**Authors’ answer:** The caption has been changed. The Figure has been modified according to the reviewer #2 comment and the variables are now plotted instead of the differences.



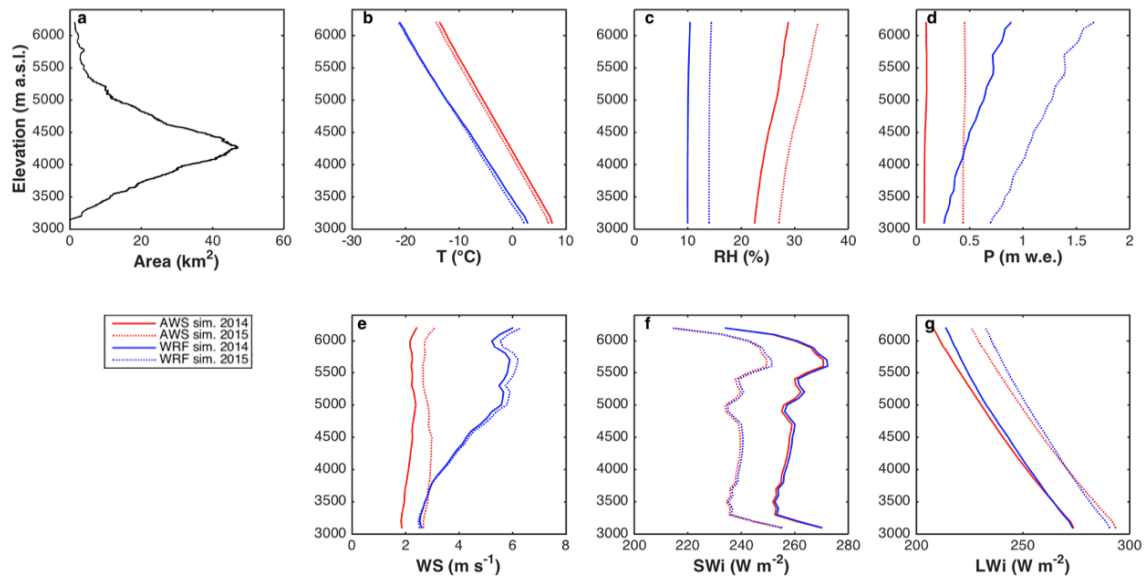


Figure 4: (a) Area-elevation distribution of the *La Laguna* catchment. (b to g) MicroMet outputs at the catchment scale forced by the AWS (red) and the WRF (blue) for 2014 (lines) and 2015 (dashed lines).

#### Figure 5

- I would suggest to bring the graphs of AWS/WRF-forced SD and observations in one figure for each station, and thus reduce the number of subfigures to 6.

**Authors'answer:** We initially did this, but it was difficult to distinguish AWS-forced SD and WRF-forced SD and almost impossible to compare it to the observation. So we decided to split it into different graphs.

#### Figure 6

- Please bring the decimal order ( $10^8$ ) to the label of the y-axes. I suggest to use km<sup>2</sup> like in the RMSE

**Authors'answer:** Done, km<sup>2</sup> is now used.

#### Figure 7

- Modelled "energy" fluxes are shown

**Authors'answer:** Done