Reviewer 2

In the manuscript "Spatial distribution and controls of snowmelt runoff in a sublimation-dominated environment in the semiarid Andes of Chile", the amount of snowmelt and the snow sublimation are quantified for a catchment of 78 km2 using the SnowModel and two years of measured meteorological data. The paper aims to present the spatial distribution of snowmelt and snow sublimation processes and to do that, it defines so-called 'snowmelt hotspots' in the catchment. The study concludes that 50% of the snowmelt occurs in only around 20% of the catchment.

Overall, I found the study interesting, mostly well written and the topic well-suited for HESS. However, I have some doubts about the novelty of the study. It is not surprising that snowmelt is spatially heterogenous, and the paper also cites quite some studies that already looked at the contribution of sublimation exactly at this location. In the introduction it is written that these studies rather focus on models and uncertainty, rather than hydrological importance. However, in this study the 'hydrological importance' part is unfortunately not so clear either: only a short statement about the implication of snowmelt hotspots on recharge areas is given. I think the study would clearly benefit from describing more explicitly the added value of this study in the introduction, discussing in more depth the implications for snow science (in semi-arid areas) and explain more explicitly the hydrological importance of the findings.
We greatly thank the reviewer for their thoughtful comments. In the revised version, we have stated more clearly the novelty of this study based on the following points:

- The spatial variability of total snowmelt runoff in mountain terrain is large due to the complex patterns of snow accumulation and snowmelt. While snow accumulation is controlled mostly by preferential deposition, wind redistribution and gravitational transport (Freudiger et al., 2017; Mott et al., 2010), during the melt season the interplay between the surface energy balance components can create large differences in snowmelt rates across a certain domain (DeBeer and Pomeroy, 2017; Pohl et al., 2006).
- In the Introduction, we hypothesize that the resulting spatial variability of total snowmelt runoff in the semiarid Andes is further enlarged by unevenly distributed large sublimation rates that greatly reduce the snow mass available for melt and define relatively small areas that concentrate most of the snowmelt runoff.
- In the Discussion, we argue that this situation is distinctive of dry mountain ranges. In mountain ranges with low sublimation rates, the ablation of the end-of-winter snow cover is dominated by melt and consequently total seasonal snowmelt runoff is expected to have practically the same spatial distribution as snow accumulation, although the snowmelt timing can be very variable from site to site due to differences in the spatial and temporal distribution of melt rates.
- Additionally, we have added a more in-depth analysis of the implications of our findings for snow science and hydrology, such as the differences in the energy balance of snowmelt hotspots and the rest of the catchment; the role of wind in the exchange of turbulent fluxes that increase and decrease melt rates through sensible and latent heat fluxes, respectively; the difficulties to identify adequate snow monitoring sites; the possible connection between snowmelt hotspots and soil moisture and vegetation; and

the expected differences between the end-of-winter SWE and streamflow volumes at downstream locations.

- The hydrological significance of our findings has been further assessed based on two datasets: i) a comparison of snow and ice melt from the Corrales catchment against records of inflow to La Laguna reservoir (revised Figure 6), and ii) contrasting the location of snowmelt hotspots and soil moisture and vegetation in the catchment derived from satellite images (Figure S7). The first analysis shows the low snowmelt during the study period due to below-average precipitation and how most of the summer streamflow increase could be explained by ice melt. The second dataset suggests that the areas with the largest values of soil moisture and vegetation indices in the study period might be related with the location of snowmelt hotspots.

My other main concern is the presentation of the results and the figures. Sometimes units are not described, the same color bars for everything are confusing, text is added at strange places and captions are not always informative enough. The manuscript presents a lot of figures, which are sometimes only described in very few sentences in the results section and rather in a disconnected way. It would be helpful if the figures and the text together form a story and are answering a question or research gap that is presented in the introduction.
We thank the reviewer for the suggestions. In the revised version, we have corrected all the presentation problems identified by the reviewer. The disconnection between the text and figures was also raised by the other reviewer, and we have worked on the Results section to emphasize the general storyline. As a result, parts of the text have changed, and we have restructured some figures. Figures 4d-e-f and 7 have been moved to the Supplement as we decided that, although they present valuable information, they do not fit entirely into the main message.

Please see below for a point-by-point response including the new figures that will be presented.

Please find below more detailed comments:

L14: ‘satellite-derived …….product’ – suggest to leave out, because it comes also a few sentences later
We have removed the first use of “satellite-derived” to avoid repetition.

L17: ‘absence and persistence’ – maybe add the season?
We have reworded it to “snow winter absence and summer persistence”. We have also added “winter” and “summer” to the labels in the revised Figure 3.

L19: here the characteristics of the snowmelt hotspots are shortly summarized. Maybe you could indicate which of these elements are likely also applicable to other regions/catchments.
We now describe snowmelt hotspots using characteristics that are more transferable to other regions: “Snowmelt hotspots are located at mid-to-lower elevations of the catchment on wind-sheltered, low-angle slopes”.

The following sentence about "we suggest that snowmelt hotspots play a key hydrological role" is a too strong statement for the abstract as this is not shown in the current study. I suggest to reformulate.

We have reworded the last sentences of the abstract to "Our findings suggest that sublimation rates play an important role controlling the spatial variability of total snowmelt runoff in dry mountain areas. Snowmelt hotspots might be connected with other features of dry mountain regions, such as areas of groundwater recharge, rock glaciers and mountain peatlands, and recommend more detailed snow and hydrological monitoring of these sites, especially in the current and projected scenarios of scarce precipitation."

L41: shouldn't it be the other way around? i.e. decreases the energy and therefore lowers the temperature?

We have clarified changing these sentences to: "turbulent latent heat fluxes associated with the solid to vapor transition use energy from the snowpack, lowering its temperature and decreasing the energy available for melting".

L66: "From another perspective" – not clear what is meant here

We have changed "From another perspective" to "From a geostatistical perspective" and provide more a few more details about Mendoza's study: "From a geostatistical perspective, Mendoza et al. (2020) analyzed the spatial properties of a set of Lidar snow depth measurements across several catchments of central Chile and found a strong relation between snow depth and local topographic and land cover properties."

L116: here I wondered why the groundwater and hydrological data are not used in this study?

Unfortunately, the quality of the available hydrological data is still not that good to be included in the paper as some major gaps make the assessment and interpretation difficult. We have decided to change the last sentence to: Since 2009, the Corrales catchment has been instrumented with meteorological equipment and several glaciological field campaigns have been carried out (e.g., Figure 1f).

L132-133: Maybe shortly explain how the measured precipitation relates to the mean annual precipitation given for La Laguna earlier in the manuscript (i.e. why 3 times higher, elevation?)

Precipitation at La Laguna DGA was of 63 and 82 mm in 2019-2020 and 2020-2021, respectively, which means that precipitation at TAP was 4 to 5 times higher than at La Laguna. We added these numbers to the manuscript.

L142: somewhere in this paragraph or earlier, suggest to explain winter/summer and the corresponding months

We have added the corresponding months (winter: June-August, summer: December-February) to the first paragraph of section 2 (Study Area).

Section 3.2: it would be good to explain why a model is used when daily SWE maps are also available. Now the reader has to guess based on the Results section. The same explanation is lacking for the SA and SP indices, why are these indices relevant for this study?

The snow evolution model is needed because it provides several variables apart from SWE, being snowmelt and sublimation the most relevant ones for this study. Also, the SWE reconstruction from Cortés and Margulis (2017) ends in 2015.
The SA and SP indices and the SWE reconstruction are used as verification datasets for the model results.
Apart from the Introduction, this information has been included in a more explicit way at the beginning of the corresponding sections (sections 3 and 4).

3.3 Not a great name for a section and confusing as it still describes another snow product. It would be useful to describe what different information can be obtained from the SA and SP indices and the SCA
We have decided to remove this section. We have moved the SCA data to the previous section (Snow products) and the SRTM DEM is now presented in the SnowModel section. The SA and SP summarize information over several months, whereas the SCA values are used as instantaneous information.

L239: why 5 mm and in table 3 1mm?
In the original manuscript, Table 3 was showing only one of the values of surface roughness that are used in this study, whereas Table 4 was showing the three values that are used to build the ensemble results. We note that the albedo fit was made using the middle value (5 mm). To improve clarity, in the revised version we have merged Table 3 and 4 (as suggested by the other reviewer).

4.2: is there any routing of the snowmelt runoff?
No, in our study there is no routing of the snowmelt runoff.

Table 3: why is the precipitation lapse rate 0?
We have included this information in the revised version:
"Although there is an annual precipitation lapse rate from the lowlands of the Coquimbo Region up to La Laguna DGA station (3160 m a.s.l.), we used a value of zero because we do not have data to support a precipitation lapse rate above that elevation, particularly within the relatively small area of the Corrales catchment. In general, snow distribution at high-elevation catchments is governed mostly by wind transport (e.g. Lehning et al., 2011)."

Table 4: how were these values determined? And is the precipitation correction coming on top of the 30% increase (bias correction) in the precipitation measurements?
Yes, the precipitation factor is on top of the 30% increase estimated due to undercatch (Macdonald and Pomeroy, 2007). As the 30% value was chosen based on previous studies, we selected precipitation factors that can be interpreted as an undercatch uncertainty range. The chosen surface roughness lengths vary within the typical ranges given in literature for snow and ice surfaces (e.g. Brock et al., 2006; Fitzpatrick et al., 2019). The slope and curvature weights for wind distribution were chosen to explore the sensitivity of snow ablation to these parameters in the Andes mountains. This information has been added to the manuscript in Section 4.3.

Figure 3: some suggestions:
Add row names with Snow absence and snow persistence, and add in caption the period for which this was calculated (months, years)
Could one color bar be used for all graphs? Why are color bars for c and d smaller? Could the bars be made such that the colors are more intuitive, i.e. the same color to indicate snow accumulation “hotspots” (high SP values and low SA values?)
Add axis labels on the axes and not at the top
What are the white areas in a?
What are the numbers in b and e? elevation?
Add explanation about the units of each graph in the caption, i.e. indicate that it is a fraction of time in a,b, d and e and percentage of space in c and f.
Thanks for your suggestions. This figure has been merged with Figure 4a-b-c and we have performed all the suggested changes in the figure and the caption. Please see the new figure and caption below.

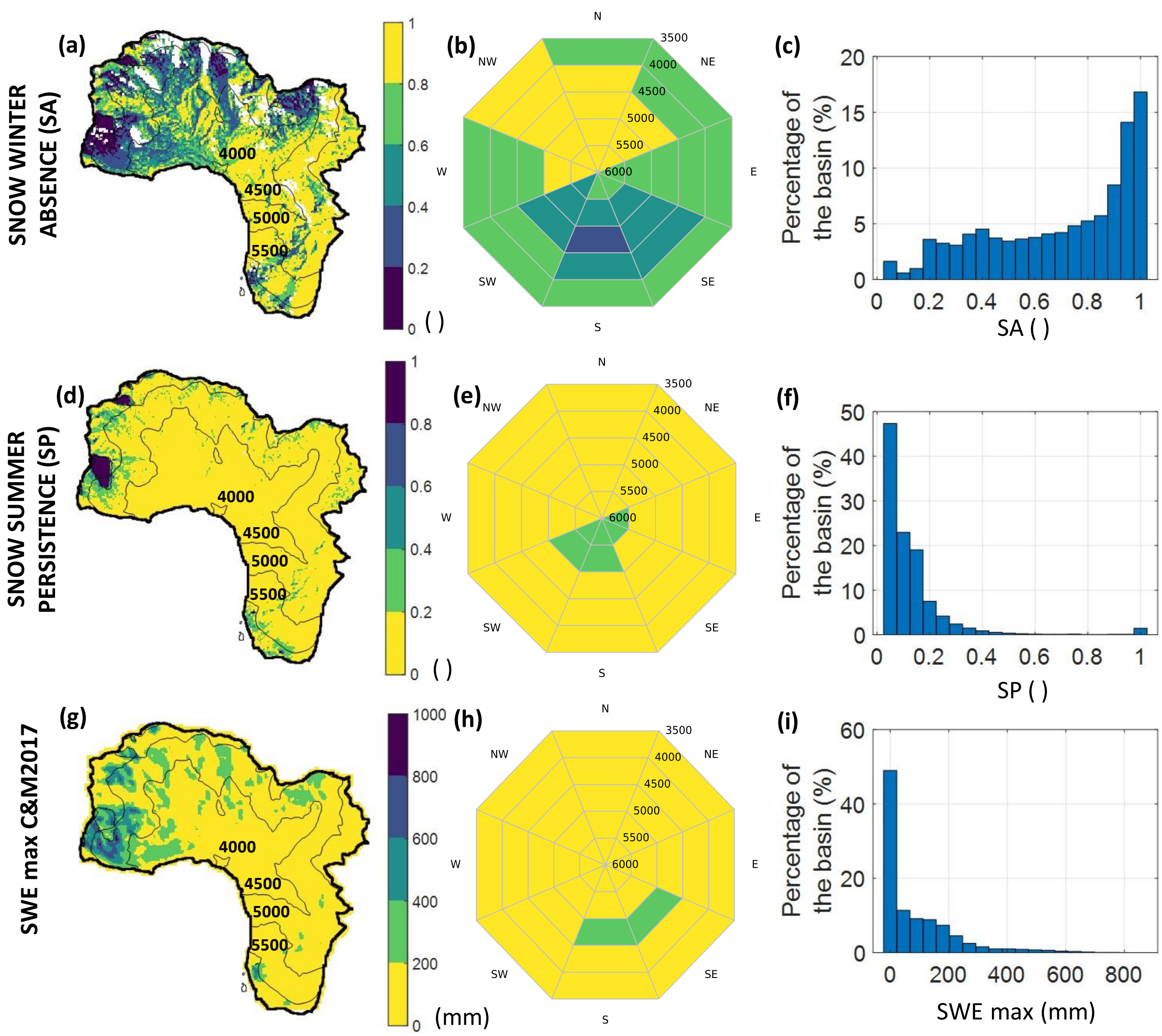


Figure 3: SA, SP and SWEmax in the Corrales basin. (a-d-g) Maps, (b-e-h) Polar plots, (c-f-i) Histograms. In the polar plots (b-e-h) the angles represent the orientation, and the elevation is represented by the inverse radial distance. While SA and SP refer to the percentage of time when snow is absent or present in the accumulation (April-September) and melt (October-March) periods, respectively, the histogram provides information about the spatial distribution of the variables across the catchment. Glacier outlines and contours are shown in (a), (d) and (g). Blank areas in (a) represent sites where the SA index cannot be calculated due to an insufficient number of cloud-free images in April-September.

L269: "interannual median" – per pixel or for the catchment?
Per pixel. We have included this information.

Figure 4, similar issues as with Figure 3. In particular, please provide units in the graphs.
Figure 4a-b-c has been merged with Figure 3. Figure 4d-e-f (coefficient of variation) has been moved to the Supplement. We have corrected the problems found by the reviewer.

L290: where does "also" refer to?
It was superfluous, we have removed it.

Figure 6 – it would be more logic to have a maximum y-axis of 100% in figure a.
Agreed. We only added it to accommodate the legend, but we now placed it above the chart.

L307-309: Why was the comparison not based on simulations at the same time as the satellite data? This would be a more fair comparison and indicate if the model is under- or overestimating snow persistence. The same comment for the SWE estimates in the next lines.
To make a more fair comparison, we have calculated the SA and SP indices for winter and summer, respectively, using the same dates as the satellite data. As a result, the observed and satellite-based absolute values are now much similar (see below). Unfortunately, in the case of SWE we can't do the same because the SWE reconstruction from Cortés and Margulis (2017) ends in 2015. The comparison is only to assess whether the relative distribution of SWE across the catchment is the same for the SnowModel simulations and the SWE reconstruction product. We now explain this more explicitly in the main document.

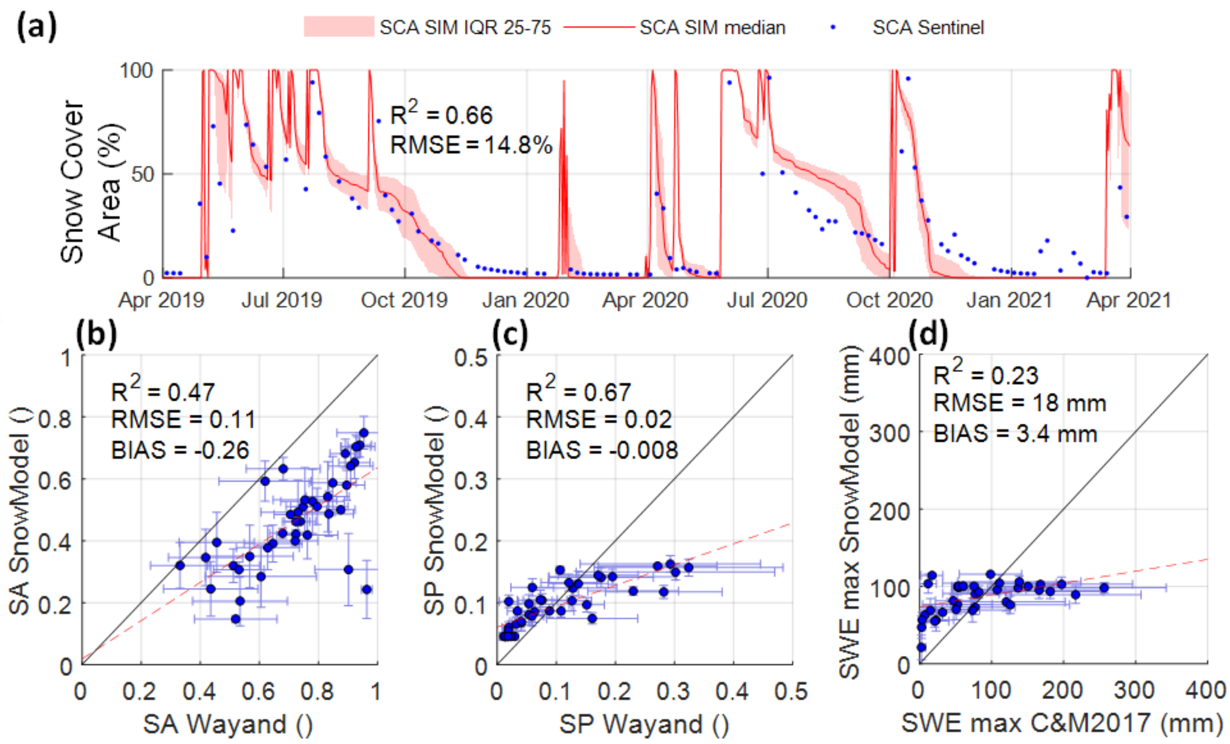


Figure 7: What is meant with "to help comparison" in the caption?

Please note that Figure 7 has been moved to the Supplement of the revised version. We did this to keep the focus of the main document on the validation of snow variables. "To help comparison" was meant to explain why the first reading was placed at the simulated value on that date and not at zero. Alternatively, we could start the plot at the date of the first reading, but the comparison will look the same, but with an offset. The main message is that the difference between readings is well simulated by the model.

Why is there only one stake for 2020 at the beginning of the season?

We drilled a couple of more stakes at the beginning of the 2020 season (December 2019), but they had already collapsed when we visited them in January 2020. In that same visit we drilled two additional stakes.

Why is the first value not the measured value but the simulated value.

Please see our previous response. This was just meant for visual purposes, but the main message is that the difference between readings is well simulated.

Figure 8: some suggestions:

Add more x-axis labels

Indicate somewhere that these are stacked bars

We have performed the suggested changes. Please note that this is now Figure 6.

During summer snowfall events (Jan 2020) it looks like the ice melt flux is largest – why is that?

Although there was a snowfall on January 25-27 2020, the month was dominated by clear-sky conditions that led to large ice melt rates. We have included this in the text.

L361: “where snowmelt is more important than sublimation” – if values are above 50% than this is nowhere the case?

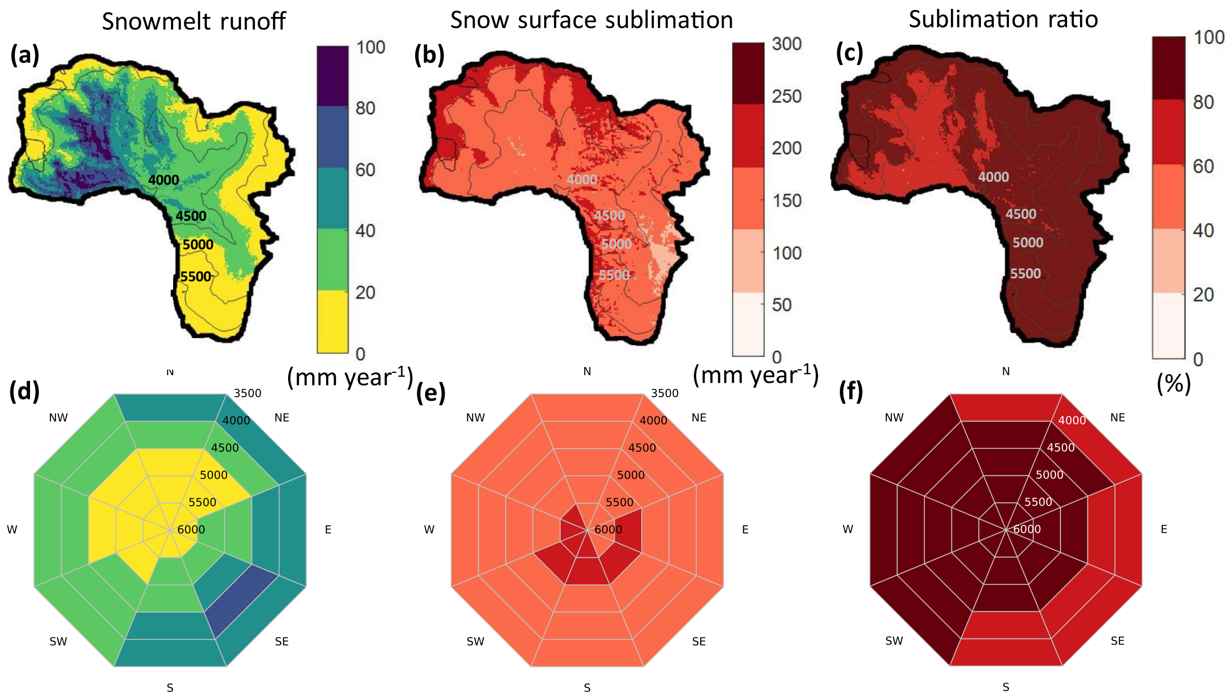
Agreed. We have changed this to “i.e. where snowmelt is more important than sublimation” by “where snowmelt runoff is highest”.

L365: 4.3 mm3 a-1 – to give a hydrological meaning to the number, wouldn’t it make more sense to give it in the same units as P, i.e. mm per catchment?

Agreed. We have changed the units to mm a^{-1}, but we have kept the Mm3 value in parenthesis as these units are useful to make comparisons with the capacity of La Laguna reservoir.

Figure 9 – Please introduce some consistency in the color bars and ranges. Also the headers could be improved which are now sometimes over two lines and sometimes centered but not always.

Thanks for your suggestions. We have improved the visualization of this figure.



L368 “total ablation” and L365 “total ablation” – please reformulate as they refer to something different. This also applies to the conclusion, point 1 which is now a bit ambiguous

We have reformulated the text. In the revised version the term “total ablation” refers only to the sum of melt and sublimation.

L370-375 how are the location of the snowmelt hotspots determined? Is there a clear ranking of which cells are included and which not? The line in Figure 10 c looks rather linear, at least between 10 and 30% of the area?

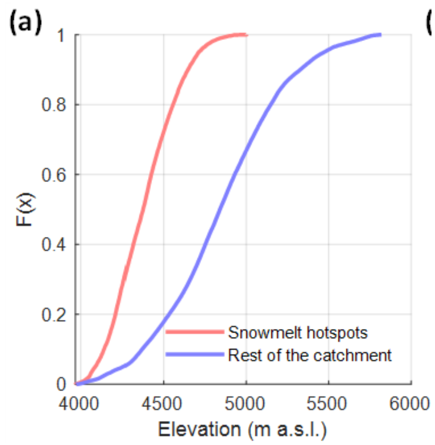
We first rank the grid cells based on their snowmelt runoff and then we define as snowmelt hotspots the first grid cells of the ranking that produce 50% of the total snowmelt runoff. We have included this information in the revised text.

Figure 10: please remove the text from the figure C
Agreed. The text has been removed.

L387: Do the SP and SA values refer to the observations or the simulations?
The values refer to the simulations. We have included this information in the revised caption.

L392 "consequently" – Please explain this last sentence in more detail
This was not clear. We have reworded it to "Sublimation ratio is largely dependent on z0 ($R2=0.85$), which controls the magnitude of turbulent latent heat fluxes."

Figure 11: why are cumulative distributions used here? Please also add in the caption which data is in the graphs. For example for the maximum snow depth, is it the maximum depth over the whole simulation period?
We use cumulative distributions because we think that they allow a fast identification of percentages. For example, in the new Figure 6a (below), "80% of the snowmelt hotspots are located below 4600 m a.s.l."



The role of Figure 12 in the study does not become clear from the text.
In the revised version we have improved the text to better justify the inclusion of this figure. Most importantly, these results go in the same direction as the rest of the variables, i.e. snowmelt hotspots are located on sites where snow erosion is lowest, whereas the opposite slopes present large values of snow surface sublimation, erosion and blowing snow sublimation.

L453: "similar AWS forcing" – what is meant here, as the lines before just explain that there was a different availability of AWS data. "Similarly" – similar to whom?
We have removed and reworded: "In fact, some differences between our simulations and those of Réveillet et al. (2020), who also used AWS forcing in the same catchment, might be caused

by gaps in the PAN records during their study period. For example, we obtain much larger sublimation ratios (~85% versus ~35%). "

L477: "In this direction…..this type of environment" – it comes across as if this sentence does not fit the text
We have removed it.

L487: "We here show that …" – please describe more explicitly what is the case here in this study, also referring to figures.
The revised Figure 9a now shows more explicitly what we meant. If sublimation rates are low, the ablation of the end-of-winter snow cover is largely dominated by melt and total seasonal snowmelt are expected to show almost the same spatial distribution as snow accumulation. The new text in the Discussion addresses these findings in more detail.

L491: "where large part of snowmelt is generated" – repeat where this is and if this is general for semiarid Andes
We referred to the snowmelt hotspots. We have reworded these sentences to make them clearer.

L507: can ice also sublimate?
Yes, but it seems that the large surface melt rates caused by the low ice albedo dominate over ice sublimation. Please note that SnowModel does consider ice sublimation and this was much lower than ice melt once the snow disappeared from the surface. We have added a short sentence about this in parenthesis: "ice sublimation calculated by SnowModel was much lower than ice melt".

In section 6.2, I was expecting a discussion about the "glacier hotspots" too, as they turned out to provide even more melt, but have an even smaller area. It is shortly mentioned, but what is the hydrological implication of glacier hotspots versus snowmelt hotspots?
Thanks for this interesting suggestion. Indeed, Tapado Glacier can be considered a hotspot as well. We have incorporated this point to the discussion based on the revised Figure 6. The role of ice melt during dry years is indeed very important in this region.

References

Brock, B. W., Willis, I. C. and Sharp, M. J.: Measurement and parameterization of aerodynamic roughness length variations at Haut Glacier d'Arolla, Switzerland, J. Glaciol., 52(177), 281–297, doi:10.3189/172756506781828746, 2006.
Cortés, G. and Margulis, S.: Impacts of El Niño and La Niña on interannual snow accumulation in the Andes: Results from a high-resolution 31 year reanalysis, Geophys. Res. Lett., 44(13), 6859–6867, doi:10.1002/2017GL073826, 2017.
DeBeer, C. M. and Pomeroy, J. W.: Influence of snowpack and melt energy heterogeneity on snow cover depletion and snowmelt runoff simulation in a cold mountain environment, J. Hydrol., 553, 199–213, doi:10.1016/j.jhydrol.2017.07.051, 2017.
Fitzpatrick, N., Radić, V. and Menounos, B.: A multi-season investigation of glacier surface roughness lengths through in situ and remote observation, Cryosphere, 13(3), 1051–1071, doi:10.5194/tc-13-1051-2019, 2019.

Freudiger, D., Kohn, I., Seibert, J., Stahl, K. and Weiler, M.: Snow redistribution for the hydrological modeling of alpine catchments, Wiley Interdiscip. Rev. Water, 4(October), e1232, doi:10.1002/wat2.1232, 2017.
Lehning, M., Grünewald, T. and Schirmer, M.: Mountain snow distribution governed by an altitudinal gradient and terrain roughness, Geophys. Res. Lett., 38(L19504), 1–5, doi:doi:10.1029/2011GL048927, 2011.
Macdonald, J. and Pomeroy, J.: Gauge Undercatch of Two Common Snowfall Gauges in a Prairie Environment, Proc. 64th East. Snow Conf. St. John‘s, Canada., (1974), 119–126, 2007.
Mendoza, P. A., Shaw, T. E., McPhee, J., Musselman, K. N., Revuelto, J. and MacDonell, S.: Spatial Distribution and Scaling Properties of Lidar-Derived Snow Depth in the Extratropical Andes, Water Resour. Res., 56(12), 1–23, doi:10.1029/2020WR028480, 2020.
Mott, R., Schirmer, M., Bavay, M., Grünewald, T. and Lehning, M.: Understanding snow-transport processes shaping the mountain snow-cover, Cryosph., 4(4), 545–559, doi:10.5194/tc-4-545-2010, 2010.
Pohl, S., Marsh, P. and Liston, G. E.: Spatial-temporal variability in turbulent fluxes during spring snowmelt, Arctic, Antarct. Alp. Res., 38(1), 136–146, doi:10.1657/1523-0430(2006)038[0136:SVITFD]2.0.CO;2, 2006.
Réveillet, M., MacDonell, S., Gascoin, S., Kinnard, C., Lhermitte, S. and Schaffer, N.: Impact of forcing on sublimation simulations for a high mountain catchment in the semiarid Andes, Cryosph., 14(1), 147–163, doi:10.5194/tc-14-147-2020, 2020.