Reviewer 1

Review of: "Spatial distribution and controls of snowmelt runoff in a sublimation-dominated environment in the semiarid Andes of Chile" by Álvaro Ayala, Simone Schauwecker and Shelley MacDonell.

This paper presents an interesting case study of a catchment in the Andes, which is a snowmelt-dependent region in which sublimation plays a significant role on the snow cover and water balance. The paper builds on previous studies focussing on modelling performance and underlying snow processes. The authors perform an elaborate analysis on the hydrological importance of the processes occurring in the Corrales catchment, Chile. In general, this is a well-written manuscript. However, parts of the manuscript require some additional attention, so that the overall quality of the manuscript improves. As such, I advise the paper to be revised before publication. Below I have stated more general and specific comments, which I hope the authors consider to be constructive.
We thank the reviewer for their positive comments and thoughtful suggestions. We have carefully revised the manuscript accordingly. Please see below our specific responses.

General:

Results:

The results contain a lot of information and figures, all of which are important. However, it is sometimes hard to make the connection to the other results for me as reader. Each figure is treated separately, and not always clearly connected to previous results. To illustrate, almost each paragraph starts with "In figure x, we compare…" or "Figure x shows …".

I would advise the authors to focus on the point you are trying to make and try to make in-text connections between the separate figures based on the general story. This results in a storyline in which the figures are a helpful tool instead of treating the results as a point-by-point discussion of the figures. Another option would be to merge the results in the discussion, however that is also not sufficiently done currently.
We appreciate your comment as it has allowed us to enhance and better organize our article. In the revised version, we have modified the text to highlight our main storyline, which is the connection between high snow sublimation rates and the spatial distribution of snowmelt in dry mountain environments. 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 and Lehning, 2010), during the melt season the interplay between the surface energy balance components can create large spatial differences in snowmelt rates (DeBeer and Pomeroy, 2017; Pohl et al., 2006). In our article, we argue 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. This response is also in line with the response to the second reviewer.

Figures:

All the figures used in the manuscript are important, and significantly contribute to the manuscript. However, multiple figures are rather unconventional. For example, some figures miss an x-axis and/or y-axis label or contain a strange diagonal line through the colorbar. Also, it seems that part of the figures consist of multiple loose figures, which are not all aligned. I encourage the authors to re-do part of their figures, so that these look more professional. (See the specific comments for examples).
Thanks for your detailed suggestions. We have followed them, and we have also restructured some of the figures to improve the communication of the article's main points. The main changes are:

- Figures 3 and 4a-b-c have been merged to simplify their message.
- Figures 4d-e-f and 7 have been moved to the Supplement as we decided that, although they present valuable information, they partially interrupt the main storyline.
- We have addressed all the specific comments regarding the figures. Please see our detailed responses.

Data and code:

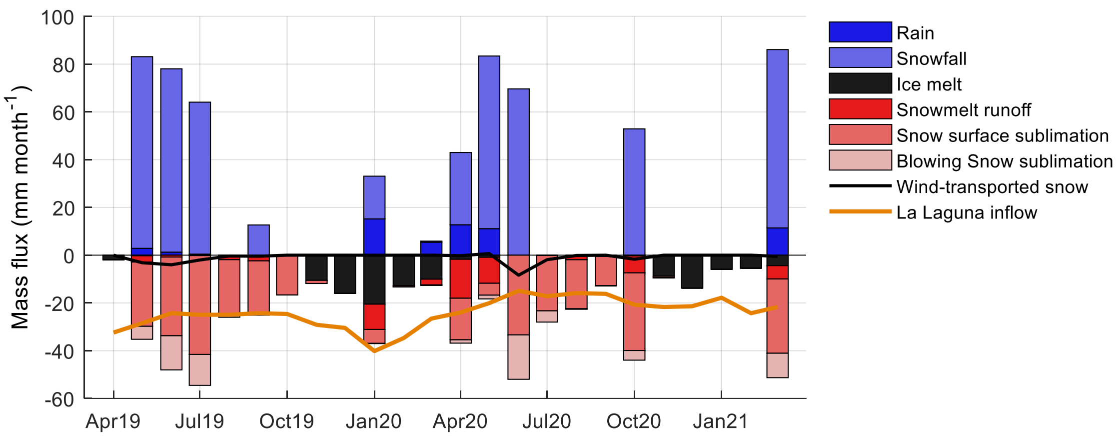
I am happy to see that the data used in this manuscript can be found online. However, I highly encourage the authors to also publish their code used for the data analysis. This would make the research more align with the FAIR principles and also accessible for interested readers.
We are in the process of organizing and commenting on our main codes for interested readers. We will upload these codes as well as the main SnowModel outputs when uploading the revised version of the article.

Specific comments:

L71-74: The definition of snowmelt hotspots is not completely clear for the reader, especially when reading the paper for the first time. The second sentence could also refer to the areas where snow surface sublimation dominates over snowmelt.
In the revised version we are now more explicit and we have changed "these sites" to "the areas producing most of the snowmelt runoff".

L101 – 116 and Figure 1: Is discharge data also available? It seems like that based on Reveillet et al., (2020) (L92-94). This would be beneficial for the understanding of the reader, especially when discussing the hydrological importance. (Also later on in combination with Fig. 8)
The reference to Réveillet et al. (2020) in lines 92-94 is used only to back up the sublimation estimates (50-80% of annual snowfall). We have changed the wording to make this clear. In the revised Figure 6 (old Figure 8) we have included hydrological data corresponding to the inflow to

La Laguna reservoir (see below), which is located some kilometers downstream of Corrales outlet point.



L128: Could you briefly elaborate on what this simple method entails? In general, I agree that this method is in the Supplementary materials.

We have included the following sentence in the revised version: "The method is based on the identification of positive changes in the daily precipitation cumulative record that lead to increases in the 5-day moving average of the same series."

L224-225: Is there a specific reason why you do not consider rain-on-snow events in the snowmelt runoff variable? Previous studies have shown the significant effect rain-on-snow events can have on runoff. Based on Figure 8, I see that rain especially takes place in summer and autumn, during which temperatures are around 0 oC and snowfall also takes place, which could result in ideal conditions for rain-on-snow events to generate relatively high runoff, partly from the snowpack.

Thanks for this suggestion. We have decided to include rain-on-snow events in the definition of snowmelt runoff. In the revised version, snowmelt runoff consists of all runoff from the base of the snowpack, i.e. runoff originated from snowmelt and runoff originated during rain-on-snow events. We verified that after the inclusion of rain-on-snow events the ensemble median of snowmelt runoff increased by 4 mm a^{-1} (from 69 mm a^{-1} to 73 mm a^{-1}). More details have been added to Section 5.3. Some other numbers have changed in the manuscript (e.g. sublimation ratio) without modifying the main conclusions.

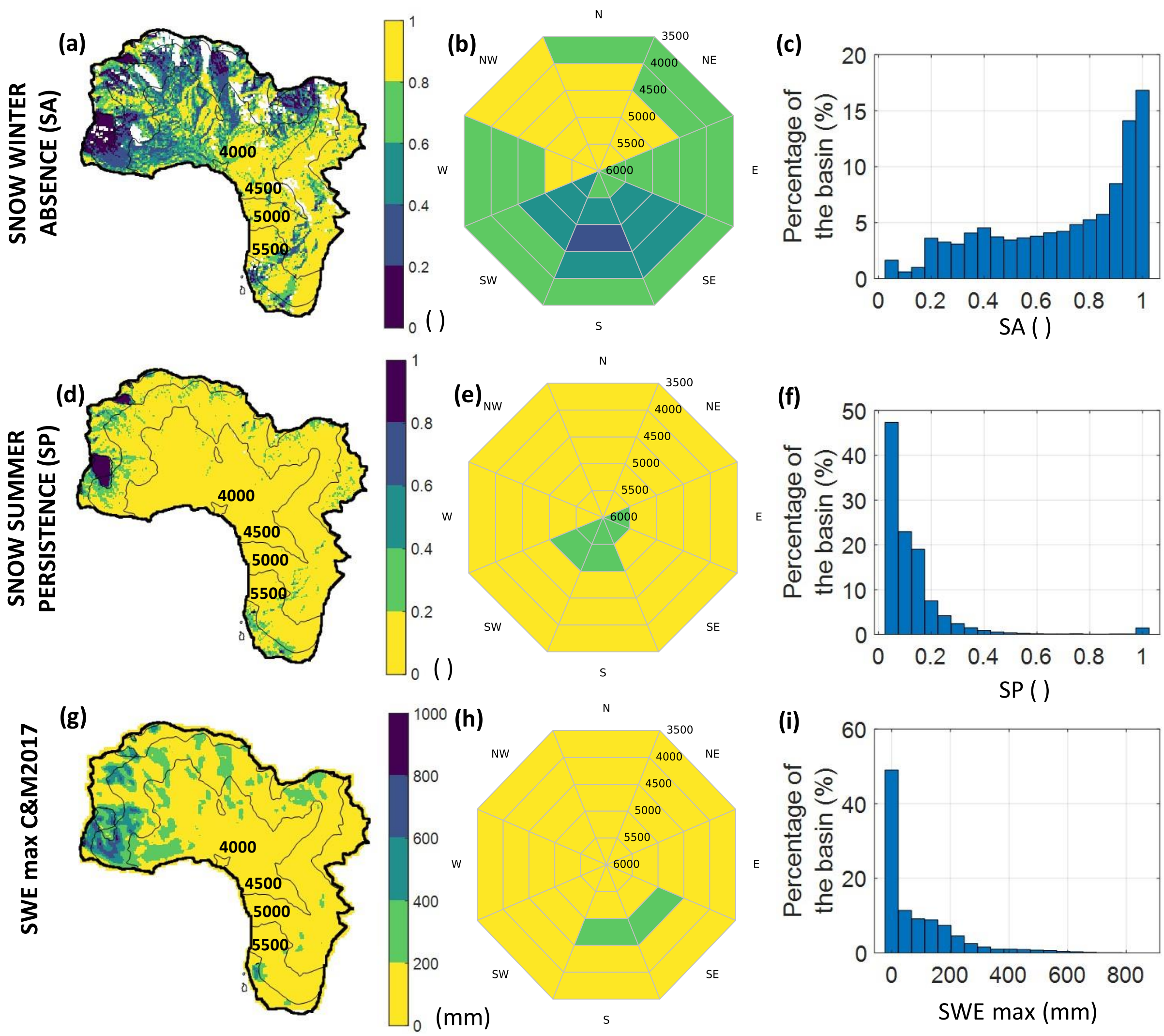
Tables 3 & 4: In these tables the input parameters are presented for the simulations. But it is unclear for me if this results in two "types" of simulations. Do you perform one base simulation (Table 3) and the ensemble runs (Table 4). Or do you vary the parameters in Table 4 as input in the simulations (Table 3)? In the former case I don't understand where you use this "base" simulation. If the latter, couldn't these tables be combined?

We thank the reviewer for noticing this. There is no "reference" or "base" simulation in our study. Table 3 was originally meant for such a case, but this was not included in the article. Tables 3 and 4 appear combined in the revised manuscript.

L245-246: Could you elaborate on the physical meaning of the slope and curvature wind distribution weights?
We have included this sentence in the text:
"The slope and curvature distribution weights increase wind speed in the presence of windward and convex slopes and decrease it in the case of leeward and convex ones (Liston et al., 1998)."

L261: Perhaps I misunderstood the definition of SP, but doesn't a value of 0.2 mean that there is only 20% of the time snow present during the ablation period? If that is the case 0.2 seems to me also mostly snow-free.
We agree with the reviewer that SP=0.2 is mostly snow-free. We reworded this sentence to: "During the melt period the catchment is mostly snow-free as revealed by the SP map (SP<0.2, Figure 3d).".

Figure 3: In the text, you refer to valley bottoms and ridges (L255), but it is hard to come to same conclusions based on your figures. Would it be an option to add isohypses to a and b? Additionally, I would advise to add labels to the colorbars, and add a y-axis label to the c and f figures.
We thank you for your suggestions. We have re-structured the figure and improved the visibility of the maps. Please note that we have merged Figure 3 with Figure 4a-b-c as these figures have similar patterns. Former Figure 4d-e-f- has been moved to the Supplement.



L269-270: I recommend to include the equation used to compute the coefficient of variation and explain how you compute these terms. This will leave no space for any uncertainties on how you computed these.

Thanks. We have included the equation (CV=standard deviation/mean) in the main document.

L285-294: The verification of the model simulations partly is performed based on a single observation site. The authors compare snow depth and SWE observed at Tapado with the modelled version of these variables representing the entire grid. Is there any evidence on how representative the measurements are for the entire catchment? How complex are the surroundings of that specific measurement site in relation to the entire catchment? Is the measurement site at a wind-exposed or wind-sheltered place?

We would like to note that TAP records are not compared against variables representing the entire grid but the variables representing the corresponding grid cell. We have modified the caption of the revised Figure 4 in case the text was not clear about this. In relation to the rest of the catchment, TAP is located in a wind-sheltered area where snow accumulates every winter. The results of our paper about preferential snow accumulation on snowmelt hotspots also reinforce this idea.

L291: What do you mean with the Geonor sensor? I suspect that is the precipitation measurements based on table 1?

Yes, that is the precipitation sensor. We present it in Table 1, but it was not very noticeable in the first version. We have changed the reference in L291 from the “Geonor sensor” to “precipitation sensor”.

L308-309: How do you compare the satellite-derived indices with the model-derived indices? Do you use the model values exactly at the moment of the satellite overpass? Or do you average the model values over a certain period?

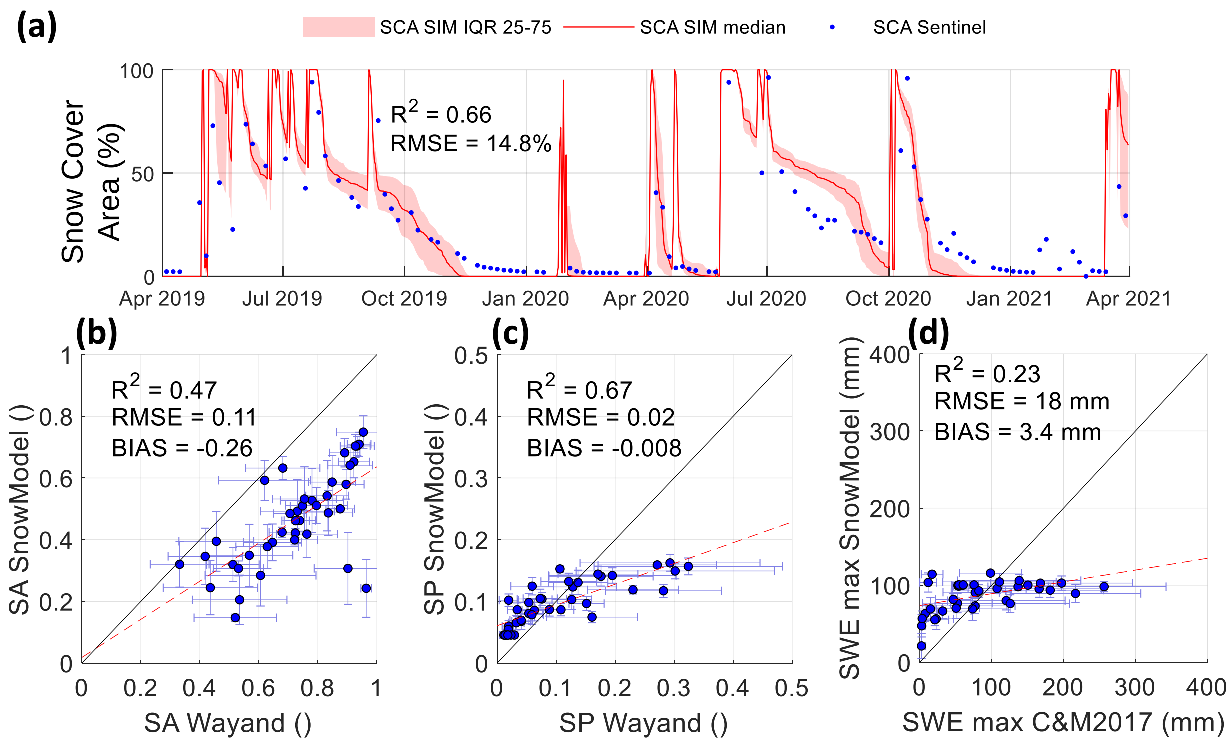
Both the satellite-derived and the model-derived indices follow the same definition, i.e. original lines 184-185: “The snow absence (SA) index is defined as the fraction of time in which snow is absent during the accumulation period, whereas the snow persistence (SP) index is defined as the fraction of time in which snow is present during the melt period.”. We followed the hydrological year to define the accumulation period as April-September and the melt period as October-March. While the satellite-derived indices are calculated using the times of image acquisition, the model-derived indices were originally calculated using every time-step in the corresponding periods. However, following the reviewers’ suggestions, in the revised version we calculate the model-derived indices using only the image acquisition dates. This change has made observed and simulated values more similar in magnitude.

Figure 6: Are SA and SP Wayand the observations?

Yes, we interpret the satellite-derived indices as observations. We have added that to the caption.

Additionally, I would recommend to add a 1:1 line and the equation of the trendline, so it is clear that the absolute values do not match. Also out of curiosity, is there a reason why you do not force the fit through [0,0] (i.e. leave out the intercept). Theoretically, the simulations should be the same as the observations, so would justify removing the intercept.

Following the changes in the calculation of the model-derived SA and SP indices the absolute values match better than in the original version of the manuscript. We have added the 1:1 line. The intercept in the revised SA plot is almost negligible, but we prefer to keep the intercept as an indication of the offset in SP and SWEmax. We clearly acknowledge that in the revised manuscript. We have also included new metrics that help to better understand the relationship between the simulated and reference datasets: root mean square error (RMSE) and mean bias (BIAS). Please see the revised Figure 5 here.



L299-316: In this paragraph (also in the discussion), you refer multiple times to the R2 as correlation. Formally, R2 is the coefficient of determination and not the correlation. Yet, obviously, both are closely related. Additionally, the numbers in the text are not exactly the same as the numbers in the figures.

Thanks for noticing this. We have changed "correlation" to "coefficient of determination" to be precise. We have also double checked the numbers in the text.

Figure 7: What do the different markers mean? Am I correct to interpret these as different stakes?

Yes, each marker represents a different stake. We are now more explicit in the caption. Please note that this Figure has been moved to the Supplement.

Figure 9: I would advise to use the same colorscales for the maps and polar plots. Also, In the colorbars of the maps, some strange diagonal dashed line is present. Lastly, I suspect the caption is not complete.

Thanks for your suggestion. We have carefully revised the colormaps and made them consistent throughout the manuscript. We removed the diagonal bars and completed the caption.

Figure 10c: why is there a message in the figure? I agree that this is an important message, but this can also be inferred without the message (and is also stated in the text).

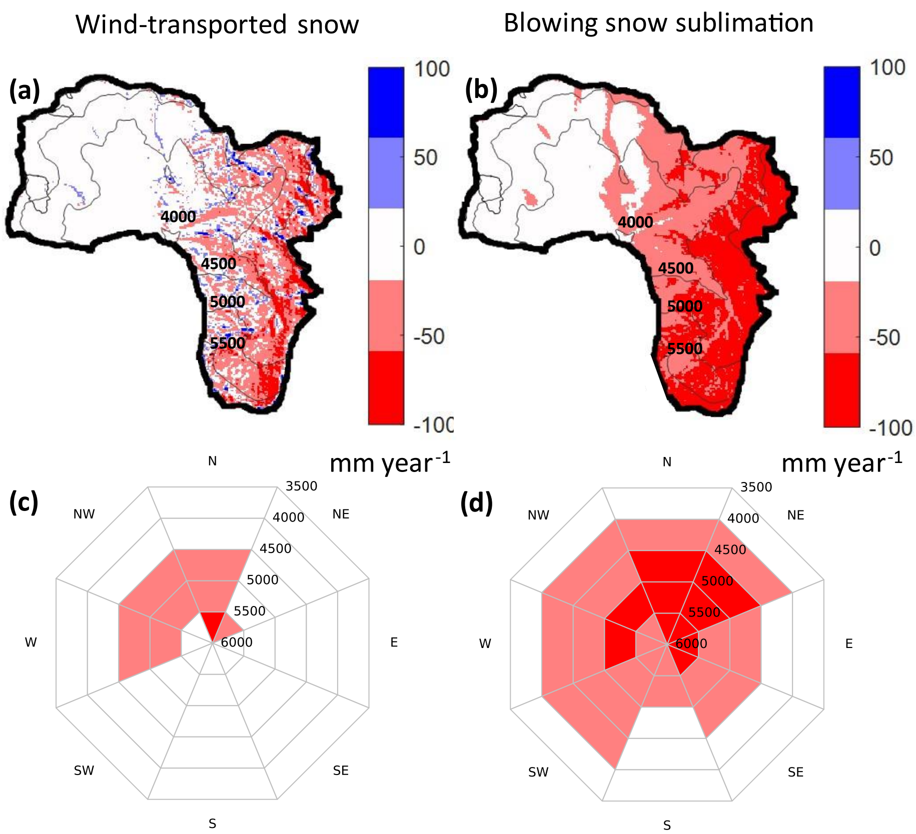
That is a result that we wanted to highlight, but we have removed it as requested by both reviewers.

Figure 12: it is hard to assess which areas are positive and which are negative, due to the chosen colorscales.

We have changed the color scales to facilitate the identification of negative and positive values (from red to blue with white for low absolute values).

Also, I suspect the caption is incomplete.

Thanks for noticing this. Actually, there is an error in the letters of each panel. Those letters referred to another figure. Please see the revised Figure 11.



L433-447: The authors start this paragraph by stating that the model results are in good agreement with the distributed datasets. I only partly agree with them. The R2 shows indeed relatively good scores, but this is not the case for the absolute values, which shows that the simulations underestimate the indices at least by a factor 2.

Since we have changed the calculations of the model-based snow indices, the reference and simulated absolute numbers agree much better than in the original version of the manuscript. We have included the RMSE and the mean bias as additional metrics to better describe the relationship between reference and simulated datasets.

I would recommend the authors to also mention the performance based on absolute values and put both these performances in perspective to previous studies. For example, is this known to be a common case with SnowModel?

To our knowledge, this is the first study that uses the indices proposed by Wayand et al. (2018) to evaluate outputs from SnowModel. However, the study by Vionnet et al. (2021) used the same indices to validate outputs from the Canadian Hydrological Model (CHM). Vionnet et al. (2021)

found that Pearson correlation coefficients of simulated snow depth and SP vary between 0.69 and 0.75, equivalent to R^2 values between 0.48 and 0.56, which is similar to the ones found in our study for observed and simulated SP (revised figure 5, see previous answer). In the case of SnowModel, Réveillet et al., (2020) and Voordendag et al. (2021) have analyzed the snow cover area in the same region. This variable (SCA) has been both under and overestimated and this was mostly attributed to the uncertainty in input data (Réveillet et al. 2020; Voordendag et al., 2021).

And is there an explanation for these mismatches in absolute values?

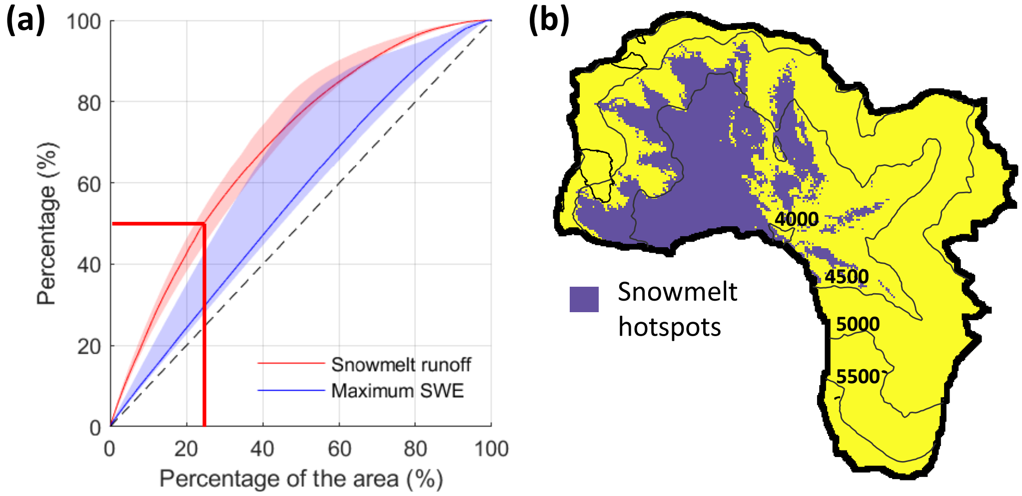
The mismatch in absolute values was corrected after the change in the calculation of the model-based snow indices.

L473-L485: This would be a nice place to discuss the dominant processes that you found in the Corrales catchment and what could be the cause of the snowmelt hotspots. However, you do not go into depth, and only briefly touch upon "the large spatial variability of the physical processes that control snowmelt runoff". I encourage you to elaborate more on what you found, which could serve as an overview of your findings merged into one story. Discussing this, would allow you to also compare your results with other regions in the world, especially where sublimation also plays a significant role.

In the revised version, we have extended this discussion to address the cause of snowmelt hotspots. We argue that the typically large spatial variability of snow accumulation and snowmelt rates in mountain terrain is further enlarged in dry environments by large sublimation rates that are unevenly distributed. These large sublimation rates almost completely remove snow cover from wind-exposed sites leaving very little snow available for melt. This discussion relates with the distribution of turbulent heat fluxes which has been addressed in other study areas, but with a more prominent focus on sensible heat fluxes than on latent heat fluxes. We have included and extended these points in the revised version.

L424-459: I miss a discussion on how well SnowModel generally performs based on the previous studies and how this could relate to your results. For example, could it be the case that SnowModel often overestimates snowmelt in specific parts of a catchment? A discussion on this would clarify whether you actually found snowmelt hotspots or are looking at the modelling uncertainty.

Thanks for this suggestion. We have included a more critical discussion on snowmelt hotspots and model uncertainty. In general, the literature available for this region has suggested that the uncertainty of input data has the largest impact on snow simulations over model selection and most of the model parameters (Gascoin et al., 2013; Réveillet et al., 2020; Voordendag et al., 2021). In our article we attempted to address this uncertainty by creating an ensemble of model runs based on three of the most uncertain parameters (precipitation, roughness length and wind factors). Based on these results, we can say that the heterogeneity of snowmelt and the presence of snowmelt hotspots are not modified within our uncertainty ranges (see revised Figure 9a below). The revised Figure 9b repeats the plot "percentage of the variable" against "percentage of the area" for the map of maximum SWE, showing that this variable is more uniform that total snowmelt runoff. Moreover, despite the uncertain input data in this region, we can be sure that sublimation rates are large and they consume a large fraction of the snow mass available for melt at most sites, except at those identified as hotspots.



L486-488: It is unclear what you mean here? What part of the results do you refer to?

We were referring to the fact that in dry mountain regions sublimation removes large fractions of the snow mass, which would be otherwise available for melt. In these lines we had hypothesized that in more humid environments all snow would eventually melt. In the revised version, we have improved the wording.

References

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