Response to the reviewer #1

We wish to thank the reviewer for the positive feedback on this submitted paper and the interesting comments. We are happy to follow the reviewer suggestions in a revised paper as detailed below.

Note: Reviewer's general comments are in "Black", reviewer's questions in **"Bold Black"** and authors comments in "Blue". Figures in the manuscript are referred by their 'Fig. number'. Revised figures are labeled '**Fig. R**'.

This paper presents the application of SnowModel for the Lebanon mountains using the traditional configuration, and a new liquid water percolation into the snow that permits improve the calculations of snow depth and snow density over the study area. The paper is interesting first for showing that liquid water percolation has a major implications in snow modeling; but also to see how with very limited data it is possible to obtain a reasonable good distributed representation of snow in areas where very little information was available. This result itself justifies the publication of the paper in HESS. In my opinion, the paper is convincing when demonstrating that the simulations made with new scheme for water percolation produces better simulations than the traditional configuration. **The only limitation (that is fully understable) is the lack of field data to assess that the water percolation is better simulated actually. In other words, are the better results the consequence of better representing the physical processes within the snowpack, or is just because it just provides less SWE that is closer to observations?** I realize that is not easy to demonstrate this, but perhaps if authors show when the two simulations really differ in the temporal series (may be showing the accumulated differences of both simulations), and at that time percolation plays a major role it could point out that there is a real causal effect.

We did not measure liquid water percolation in the snowpack during the field campaigns because we found out later that this aspect of the model is key to obtain good simulations. However, we can show the accumulated differences in time to illustrate when the liquid percolation scheme creates the differences in the simulated SWE. We propose to incorporate wet snow detection from Sentinel-1 data in the revised manuscript to better justify that we indeed get "the right answers for the right reasons" (Kirchner, 2006) despite the lack of liquid water in situ observations.

Figure R1. shows the time series of wet snow detection from Sentinel-1 over the period October 2014 to June 2016 (method below) and the simulated melt runoff for both model configurations. It can be observed that at CED and MZA, snowmelt runoff using the default model occurs in a short period of time when Sentinel-1 data suggest that the snowpack has completely melted (since dry snow is not possible during the spring season, the absence of wet snow means that there is no snow), as observed with the in situ SWE and HS data (Fig. 3

and Fig. 4, respectively). On the other hand, the runoff simulated using the Pflug et al. (2019) model is better synchronized with the wet snow occurrences, where wet snow occurs earlier in the season and is more temporally distributed. This is consistent with the expected behaviour of the new liquid percolation scheme, which allows a more gradual release of liquid water throughout the melt season when the snowpack is wet.

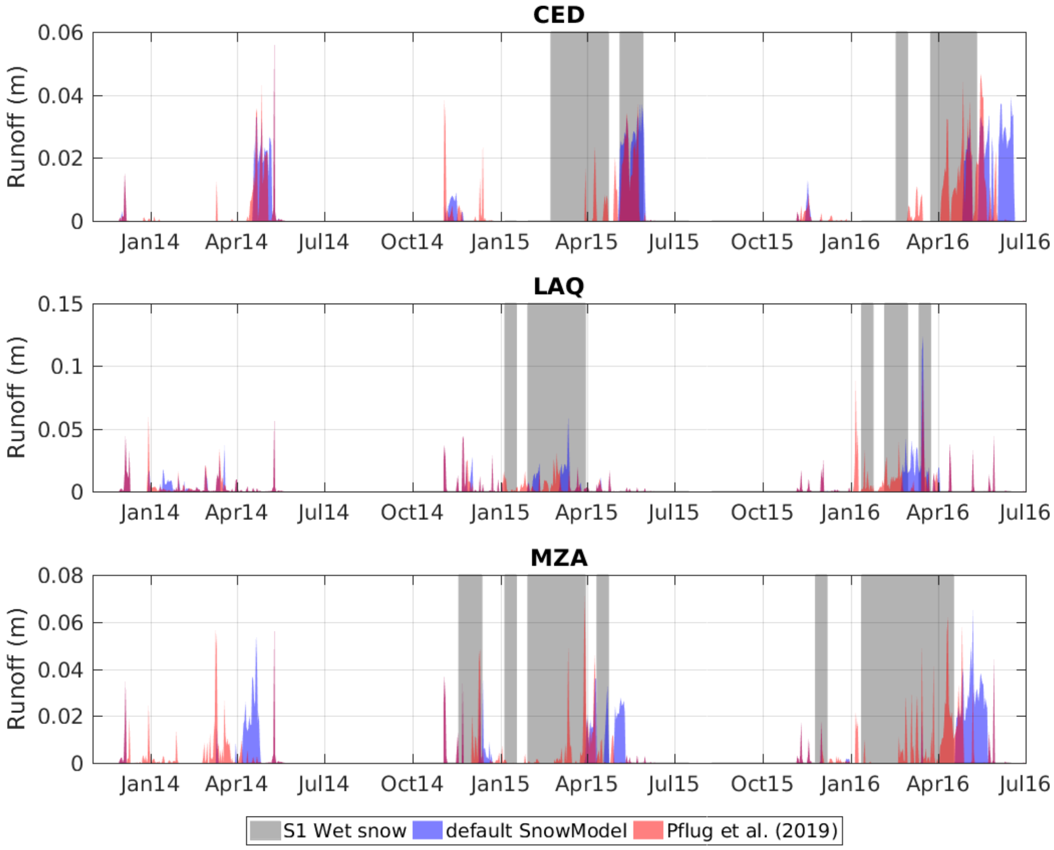

Figure R1: Time series of wet snow occurrences as detected from Sentinel-1 observations and modelled daily snowmelt runoff at each AWS.

Method for detecting of wet snow from Sentinel-1 SAR

The wet snow detection was done using Sentinel-1 SAR observations over the period October 2014 to June 2016. We extracted Sentinel-1 backscatter in VV polarization mode from the Sentinel-1 Ground Range Detected (GRD) collection in Google Earth Engine (Gorelick et al., 2017). We spatially averaged the backscatter at each station using a buffer with a radius of 100 m. Then, we defined a reference “dry” surface backscatter for each station using the 10th percentile of the backscatter time series (CED-8.4 dB, LAQ: -10.7 dB, MZA: -6.10 dB). A negative departure of 4 dB to this reference was used to determine the occurrences of wet snow (Nagler et al., 2016).

It would be also nice to see the differences in the distributed snow duration maps using SnowModel under the two compared model configurations, it may also reveal some interesting finding to see which areas are more benefited from the new percolation model.

We thank the reviewer for this suggestion, which indeed reveals that the Pflug et al. (2019) liquid water percolation scheme had a greater impact in the mid-elevation zones near 2000 m asl. This is consistent with the expected behaviour of this scheme since these areas are prone to continuous melting during the winter. The Pflug et al. (2019) scheme is expected to have a greater impact in areas where the snowpack is wet and isothermal.

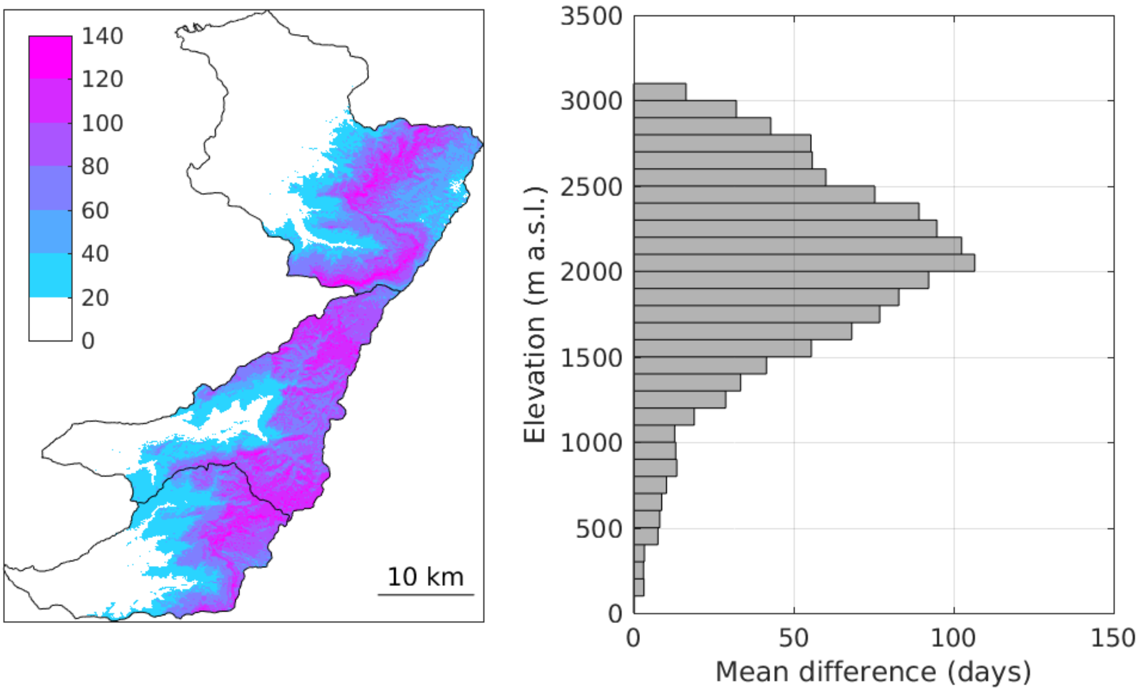

Figure R2. Left: map of the difference in days between the snow cover duration (SCD) simulated by the default model and the Pflug et al. (2019) model. Right: mean difference by elevation band. The SCD was computed over the simulation period (three snow seasons from 01 November 2013 to 01 July 2016).

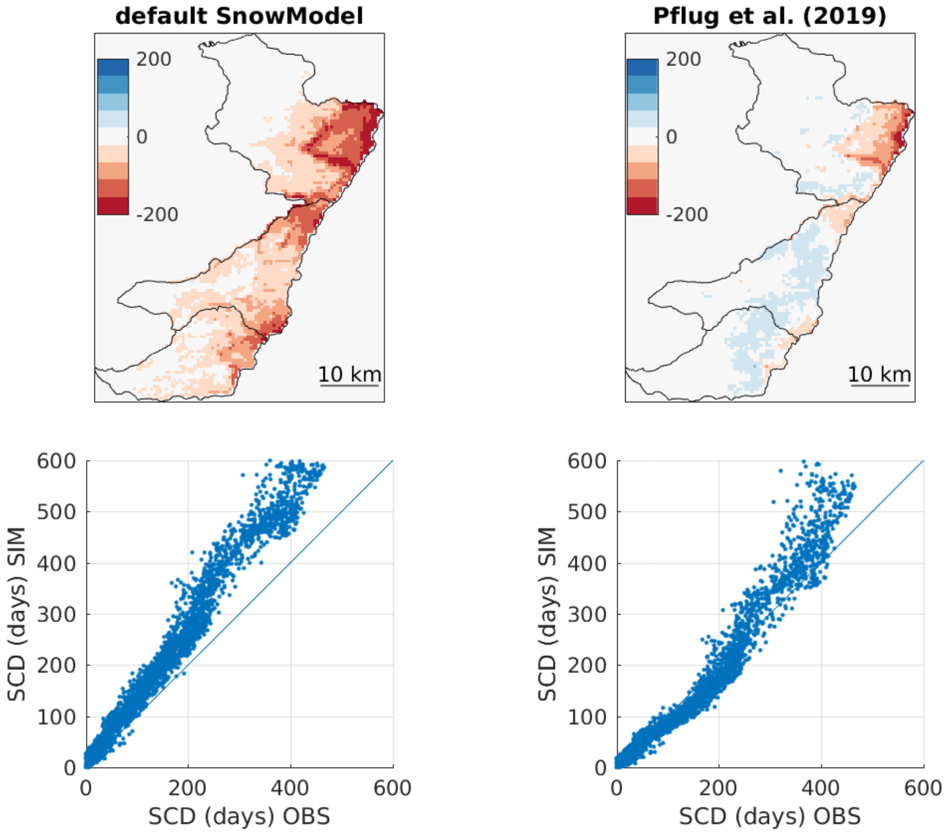

Figure R3. Top: maps of the difference in days between the simulated snow cover duration and the observed snow cover duration from MODIS. Bottom: scatterplots The SCD was computed over the simulation period (three snow seasons from 01 November 2013 to 01 July 2016).

In the revised manuscript we propose to replace Fig. 7 with Figure R3 and add Figure R2 to better discuss the spatial impact of the new liquid water percolation scheme.

Another question, that is out of the scope of this paper but it could be just briefly discussed, is **how much room there is for improving the simulations in the area.**

There is certainly room for improving the simulations.

1) The comparison with MODIS data suggests that the enhanced model performed better at mid-elevations but there remains a positive bias in the snow cover duration at high elevation (Fig. R3). In this study, the model was forced with observations collected from three AWS but precipitation data were not available at the highest AWS (CED). Since then a precipitation gauge was installed at CED and high elevation precipitation data are available. Therefore we suggest that the simulations could be improved by using observed precipitation from all stations to have a better representation of the high elevation precipitation volumes.

2) Given that temperatures remain close to 0°C during winter precipitation events over large areas of Mount Lebanon, a better parameterization of the precipitation phase partitioning is expected to enhance the simulations and help in better capturing the rain on snow events. In general, this is probably a key issue for Mediterranean mountain regions with mild winters (see comment below).
3) As discussed in the manuscript better representation of wind redistribution processes is important in this region. Extremely high variability of snow depth (few meters of difference over a distance of a hundred meters) could be seen over shorter distances (Figure 11, in manuscript). However, with limited information on the wind field and without a high resolution DEM it is difficult to assess blowing snow and its distribution at a finer scale. In addition, a proper representation of the snow redistribution could help in better explaining the role of advective heat fluxes.
4) Most of the energy used from snowmelt is shortwave radiation, with the occurrence of numerous dust storm events in this region, a proper parameterization of surface albedo, through assimilation of remote sensing products, as for example, could help in better capturing the onset of snowmelt especially at mid elevations. Collecting information on the radiative and thermal fluxes and measuring the properties of the different snowpack layers could help in better representing these processes in models.

In the revision, we can incorporate these insights into the Discussion section.

For instance, authors mention the **importance of the determination of liquid/solid phase of precipitation. However, if I understand well it is used a very simple temperature threshold approach, when now there are much more sophisticated approaches.**

The warmer nature of the Mediterranean climate of this study domain makes it challenging to set a proper threshold for the cutoff between rain and snowfall. Setting a proper static threshold for phase partitioning proved to be difficult, especially in the absence of local studies on the partitioning of precipitation in this area. We did use a modified snow-rain static temperature threshold after Harpold et al. (2017). We did not use a more complex precipitation-phase partitioning method such as those described in Harder and Pomeroy (2014) and Harpold et al. (2017) because the parameterization of such methods would require introducing more parameters that we will not be able to determine for this study area. In fact, having a better partitioning of the precipitation phase is one of the options to refining the model performance and improving the simulations in this area.

I also would like to know more about the improvement (or limitations) of the snow blowing and redistribution module used in the model. Does it really help to improve the spatial distribution of snow over the area?.

Blowing snow and its redistribution are important in this region. There is a high variability of snow depth at shorter distances as concluded from the field observations conducted by the authors during two winter seasons. The existence of a large number of sinkholes in the region act as a trap for blowing snow Figure 11. (in manuscript) is an example where snow depth varies from zero to more than 2.5 m over distances as short as a few hundred meters due to snow transport by wind. Previous work showed that the blowing snow and snow redistribution module in SnowModel improves the accuracy of the simulations (see Gascoin et al., 2013). Hence we chose not to focus on this aspect in this study and assume that it contributes to a realistic representation of the snowpack. We actually plan to work on this important aspect in a future study, but this would probably require to focus on a smaller model domain with an accurate fine scale elevation DEM that is currently not available in this region.

Finally, **I also guess that sublimation is another important component of the SEB in Lebanon (as in other Mediterranean Mountains), what does the model inform about this process, is it an important source of uncertainty for snow modeling in this area?.**

We agree that snow sublimation can be an important component of the snow mass balance. Figure R4 below shows the distribution of the mean annual sublimated snow in mm water equivalent. The sublimation reaches 70 mm we in the upper area of the study domain, and accounts for a fraction of about 5 to 15% of the annual peak SWE.

In SnowModel there two sources of sublimation: static surface sublimation and blowing snow sublimation. The relatively low sublimation rates are due to (1) high relative humidity due to the proximity to the Mediterranean Sea, which reduces both sources of sublimation (2) the high snow densification rates which inhibit blowing snow sublimation (Liston and Elder, 2006). We are unable to qualify the simulated sublimation however the point (2) is compatible with our field observations that the snow surface at the mid-elevation is subject to multiple melt and refreeze events. Such snowpack surface limits the capacity for snow removal by wind and hence reduces snow sublimation. To conclude the model suggests that the sublimation is probably not the main source of uncertainty in this area (precipitation remains the main uncertainty), but we suggest this aspect should be the focus of further investigation using in situ measurements by eddy covariance towers or lysimeters.

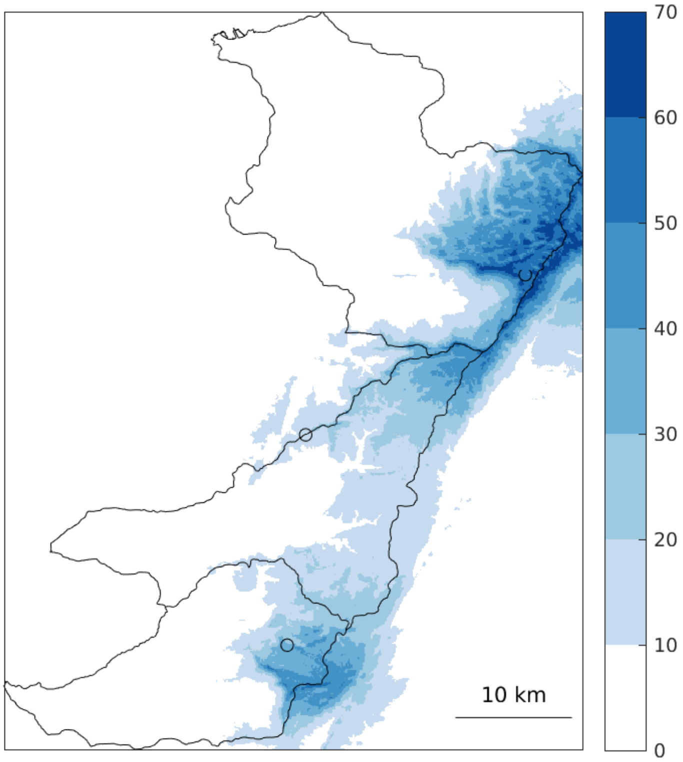

Figure R4. Mean annual snow sublimation in [mm] (Pflug et al. 2019 model).

I have not more significant comments about the manuscript. It is well written and structured and very easy to be followed by readers. Figures are simple and nice.

Again, we thank the reviewer for the kind feedback and insightful comments.

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