Response to the reviewer #2

We wish to thank A.N. Arslan 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**'.

The paper investigates the spatial distribution and evolution of the snow water equivalent (SWE) during three snow seasons (2013-2016) in the coastal mountains of Lebanon. A recent upgrade of the liquid water percolation scheme in SnowModel, which was introduced to improve the simulation of the snow water equivalent (SWE) and runoff in warm maritime regions was evaluated. The performance of the model was evaluated against continuous snow depth and snow albedo observations at the AWS, manual SWE measurements, and MODIS snow cover area.

Chapter 3.3: SWE estimation is very important as the main focus of this paper. But this chapter written very shortly. It would be good to make it more detailed like "how to estimate the evolution of SWE over the three basins were done using the model outputs and etc.?

We thank the reviewer for this suggestion and we propose to update the section 3.3 as follows:

We used the model outputs to estimate the evolution of the SWE over the three basins. The basins surface boundaries were used to estimate the SWE contribution for each basin by computing the mean daily SWE for all pixels within each basin. To account for the hydrological contribution of SWE from the snow dominated regions, the daily distributed SWE was spatially integrated for elevations above 1200 m a.s.l..

From the temporal evolution of these basin-scale SWE time series we derived the following key indicators: date and value of the peak SWE (maximum SWE during a water year), snow melt-out date (the first day of the calendar year on which SWE gets below 1 mm w.e.), distributed SWE for the peak SWE data (this map is important from a hydrological perspective as it shows the amount of snow available for melt at the end of the snowfall season for each hydrological year).

We used the snow course locations to approximate the pixels that are representative for each snow course. The simulated SWE were evaluated by comparing the observed SWE (revisit time ~ 11 days on average for all snow courses according to Fayad et al. (2017b)) with the simulated SWE value corresponding to the same dates when the in situ observations were taken. Five snow courses located near the three stations are used to showcase the evolution of SWE across different elevation bands (snow courses near the three AWS have elevations at 2823 and 2834 m a.s.l. for CED, 2297 and 2301 m a.s.l. for MZA, and 1843 m a.s.l. for LAQ).

How manual SWE measurements are conducted, what instruments are used?

A detailed description of the methodology and the protocols used for measuring SWE is described in Fayad et al. (2017b). We did not cover this in detail in this manuscript to minimize information redundancy.

In chapter 4.1: It is said that "Figure 3 compares the observed and modelled SWE evolution using both SnowModel configurations. The Pflug et al. (2019) model provides a better simulation of SWE during the melt season in CED and MZA. At LAQ, both models are positively biased." I am not sure this is totally true. It seems that it is true for year 2016 but not for 2015!!! It is important to discuss this why it is like that any reasons? Why model works better for 2016 not for 2015?

We thank the reviewer for this comment. In the revised manuscript we propose to change the text as follows: "At LAQ, both models are too biased to draw a robust conclusion on the effect of the percolation scheme. Both models are negatively biased in W1415 and either positively biased (default model) or negatively biased (Pflug et al., 2019) in W1516."

There are a number of variables that can influence the performance of the model such as blowing snow and snow redistribution at the pixel scale, snow course representativeness for each location, and precipitation observations used for forcing the model. This is why we present the results in Tab. 3 for the full simulation periods and all stations to make an assessment based on the overall model response. We think if is a strength of our study to have collected observations over three snow seasons at three AWS to reduce the effect of unexplained local uncertainties.

What is the purpose of comparing snow depth measurements in Figure 4? This should be explained. Same thing is valid for SCA. What is the purpose of this comparison in terms of estimate SWE in this paper?

The main reason for using HS and SCA in the validation of the SWE is attributed to the scarcity of the spatially distributed SWE data needed for model validation. In fact, HS is a good proxy of SWE in many cold regions climates (Strum et al., 2010). Furthermore, Fayad et al. (2017b) demonstrated that the estimation of SWE from HS could be achieved with acceptable accuracy using two years of observations in Mount Lebanon. Both, the SWE measurements that are available at a temporal resolution of ~ 10 days and the daily HS observations from

AWS are collected at point locations in Mnt. Lebanon (Fayad et al., 2017c) and only provide an idea on the evolution of the snowpack at that particular location.

The spatial distribution of HS, and to a similar extent the SWE, vary over short distances in Mediterranean Mountains (e.g. López-Moreno et al., 2011, 2013) and the spatial variability in snow depth is much greater than that of snow density (e.g. López-Moreno et al., 2013). The use of SCA is justified by the fact that extrapolating point SWE observations, to cover the high heterogeneity in mountain topography, is not possible given the limited number of point observations (~30 snow courses in total). Hence, in the absence of sufficient distributed SWE or HS, needed to validate the modeled distribution of SWE, the use of SCA as a proxy for SWE or HS was shown to be useful in similar mountain environments (e.g., Gascoin et al., 2015; Baba et al., 2018).

In generally I found this paper focuses on comparison of the performances of default model and the model with upgrade of the liquid water percolation scheme. Either title should be modified or focus should be more on the estimation of SWE in Mount Lebanon.

A previous version of this paper was focused on the SWE estimation but based on earlier discussions with the Editor we chose to change the focus on the finding that the percolation scheme has a significant impact on the simulation. We believe that this aspect can indeed be of interest to a broader audience. Yet, we use the “best” model configuration to compute the SWE in the final section as it is the societal question which motivated this study. The title “The role of liquid water percolation representation to estimate snow water equivalent in a Mediterranean mountain region (Mount Lebanon)” reflects these two aspects. We will do our best to clarify this double motivation in a revised manuscript, but we would like to keep this title if the Editor agrees.

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