COMMENTS TO REFEREE #1

In this paper the authors use terrestrial photogrammetry of a small area (30 by 30 m) to measure snow depth (h) and the associated snow cover fraction (SCF). They then use the results to define accumulation and four styles of depletion (DC) curves. These curves are ingested into a snow melt evolution model in a way that updates the fraction of area over which the model is applied. The results are found to improve the model performance.

This is a clearly written paper and a nice tidy study. It has two main conclusions which need to be explored a bit more.

The first is that there were 4 styles of melt over the domain and these were a function of the antecedent history of snowfall as well as the time of year. That is an interesting and potentially useful finding, but it was derived for a near postage-stamp sized domain. So the question is "How robust and general is the finding?" Are the four styles universal for the Sierra Nevada, all snow landscapes, or just the local area? The current set of conclusions is really a reiteration of the abstract… instead the conclusions should be about these styles and what they might mean in a more general way. That would make the conclusion relevant to snow researchers not working in the Sierra Nevada. It would also greatly strengthen the paper if the authors had any data from nearby (but not in the training area) domains that could be used to validate that the DC styles have at least local widespread validity. I have reason to think they might.

The second finding is that the ingestion of the DCs into the melt model improved the model. This is not totally novel, nor is it surprising, but it is useful. I would have liked to see a bit more quantitative assessment of the extent of the improvement. The metrics are all there, but, for example, how much better would the improvement have been if a single style of DC was used for all cases. So in summary, I find this paper worth publishing, but I would ask that the authors revise the text in ways that address in greater detail the styles of melt depletion observed, whether those styles can be extended beyond the training domain, and if so, how far, and delve a little deeper into just how much improvement the ingestion of the DCs made to the model (for example, what if the model was just adjusted with a fixed linear depletion...would it compete well with the 4 styles?).

First of all, we would like to thank Referee #1 his comments and suggestions on the paper. The authors fully agree with Referee #1 in the validation of the findings at different locations over Sierra Nevada. In fact, replications of the monitoring system, that is, camera+weather station, have been recently installed in selected sites throughout the area of Sierra Nevada, but there are not data series long enough so far to perform the analysis. Other data sources such as remote sensing and field measurements of snow depth values were used in a previous effort to this work with the terrestrial images, but the scarcity of available measurements and the cloudy conditions during most of the periods of interest (with snow presence) made us focus of ground sources. Nevertheless, we think that the results of this work lead to relevant conclusions under the scope of its goals: to incorporate the effects of the spatial variability of the snow distribution at subgrid scale for a Mediterranean site. The subgrid scale effects may affect significantly the extension to a raster-model of the physical modeling of the snow, especially in regions where both the spatial and time variability are high, in two ways: first, when applying the physical equations from point to cell-size calculations; this is what we are focusing on in this work. Secondly, the application of these results in larger areas; this is what we are working on at the moment.

With this in mind, despite the DCs were obtained over a 30x30m area as control point, there are some results that allow us to conclude on the usefulness of this approach and its potential representativeness beyond the local scale. On one hand, the four years of data in our study (3 calibration + 1 validation) exhibited a high variability of both the states in the monitoring point during each year, and the annual regime of snow, and thus they cover a wide domain of daily states of the snow distribution. The curves were described from dimensionless expressions of the selected variables, snow depth and snow cover fraction, so that they could be analyzed under the light of the physical conditions prevailing during each analyzed melting cycle and some relationship could be explored (as it turned out). The fact that we could finally explain the observed variability from four different patterns of the snowmelt dynamics associated to specific conditions, which was validated during an additional year, is an indirect but useful support to the potential application of the DCs not only in different points in Sierra Nevada, but also in other regions and/or similar snow states cycles, once the local order of magnitude of snow depth is estimated.

We tried to show this in the text and in the results showed in Figure 9. Following this comment, further explanation in the discussion has been included (see page 10, lines 19-34, and page 11, lines 1-14 in the revised version) together with a new table (Table 6) that gathers the factors and processes dominant in each pattern of melting dynamics. We have also emphasized the scope of the work and the on-going work to directly validate the general applicability of the results (see page 11, lines 25-34, in the revised version).

On the other hand, regarding the need for a single or multiple curve approach, the conclusions of a previous study (Pimentel et al., 2015) suggested that the use of a unique depletion curve was not enough to capture the complex dynamics of snow on this scale in these regions. In this study, we tested the use of a single depletion curve during the study period from the same data set. Three different parameterizations of a lognormal distribution, described by their coefficient of variation (CV=0.4, CV=0.8, CV=1.2) were assessed following the curves proposed by Luce and Tarboton (2004). The results pointed out the likely improvement to be obtained if different parameterizations were used during the snow season or depending on the year, but the use of predetermined curves did not lead to their respective association to certain conditions during the melting cycles. Our current work is the consequence, and the results show an improved performance of the snow model, with RMSE values of 84.2 and 105.8 mm during the calibration and validation periods, respectively, below the RMSE values of 321.3 mm, 285.4 mm and 556.0 mm obtained in the previous work for each CV-parameterized curve tested in the work, respectively. The performance of the multiple-DC choice in the snow model is also better than the results obtained with a single-DC choice plus the assimilation of the observed SCF values (also in Pimentel et al., 2015).

	Single DC			4 DCs	
	CV=0.4	CV=0.8	CV=1.2	Calibration	Validation
Without assimilation	321.26	285.37	555.96	84.2	105.8
With assimilation	279.23	191.18	523.14		

Following the Referee's comment, further explanation has been included both in the Introduction and Discussion sections (see page 2, line 29, and page 12 lines 1-14, in the revised version)

Detailed comments follow:

Page 1, Line 31: I disagree: cold northern regions also have extremely heterogeneous snow covers due to both wind-drifting and canopy interception.

The authors fully agree, and the sentence has been rewritten following this comment (see page 2, lines 1-2, in the revised version).

Page 2, Line 5: There could be considerably more discussion in the text on the microtopography of the domain. With respect to the above comment, see: See Sturm & Holmgren (1994). Effects of microtopography on texture, temperature and heat flow in Arctic and sub-Arctic snow. Annals of Glaciology, 19(1). You will see there that northern landscapes also have heterogeneous snow.

We have included some text in this section (see page 1, lines 7-8, in the revised version)

Page 2, line 26: Here the authors speak of processes. . ...this is a good lead in to what is needed in the expanded discussion about the 4 DCs: what processes of melt (and albedo etc.) are different in each curve and why? Perhaps a table of these processes differences would be useful. They have made a good start on this in the current text, but have not really made a succinct summary of the styles and the reasons for them.

This is certainly a key point and we have failed to fully highlight it. Following the suggestion of the Referee #1, a new table that sums up selected descriptors of the main processes involved in each DC pattern identified (Table 6 in the revised version) has been included.

Page 5, Equation 2: It is not totally clear from the text (or the figure) how these regressions of mean depth vs. pole depth were derived. It is not even clear how many poles were in view and measured. Some additional details would help here.

To clarify this, we have included some text in this section (see page 5, lines 22-34, in the revised version) regarding how many rods were used, the conversion of pixel to meters, and how to derive a distributed value from the point measurements. A new version of Figure 2 associated to this has been also included.

Page 6, Line 11: In the equation precipitation is R not P. Correct.

This typo error has been corrected in the revised manuscript (see page 6, lines 15, in the revised version).

Page 8, Lines 10 to 20: Good. More of this is needed.

Page 10, Line 15 and Page 12, Line 5: These assertions have no backup. . .no evidence that they are true. It would be very helpful to show that this is the case... or at least that there is some evidence it is true. Just stating so doesn't make it so.

As we have commented in the initial general comment, we cannot validate directly with measurements these assertions. However, the way in which the curves were described, see previous answer to the general comments. Some clarifications have been added to the Discussion section (see page 11, lines 25-34, in the revised version).

Figure 5: Axis Labels are too fuzzy and small to read. Also, add a table (or schematic) that summarizes how the 4 melt curves differ and why.

The font size has been increased and a summary table has been added (see Table 6 in page 24 in the revised version).

Figure 6: The decision diamonds in the figure don't seem quite right. For example, what if the snow is >0.6 m and older than 30 days? This figure needs some more thought and revision.

This figure tries to explain the decision tree included in the snow model to choose a given curve during the calculation process. Each curve represents a different melting behavior which is in practice highly conditioned by the previous states of the snowpack. The condition formulated in the diamonds translate these previous snow state conditions into test variables the model is capable to check out. For example, the model does not simulate/monitor the rate of compaction, but since this process is closely related with the age of the snow this condition has been simplified by using the number of days with continuous presence of snow. The same takes place with the effect of the micro-topography, for which a reference depth above which its impacts on the snow change is negligible, since the micro-topography is completely covered by snow has been used. The numerical values for each test variable were estimated from the observations. 30 days is the maximum number of days that make the different between the Curve 1 and 2, and 0.6 m is the 95th percentile of the distribution of height in the micro-topography in the study area. In future extension of the model to different areas, these values are prone to change and should be adapted to particular conditions. This arrangement in the tree comes from the observations and therefore, from the importance of the process on the melting parameterized in each of them. Some clarifications have been added to the Discussion section (see page 9, lines 12-16, in the revised version).

Answering the direct question if the snow is >0.6 m and older than 30 days, the age of the snow simply prevails and the selected curve may be the 1.

Figure 9: Nice figure. I think looking at it, some of the physical reasons for the 4 DC styles are suggested