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Interactive comment on "Factors influencing spring and summer areal snow ablation and snowcover depletion in alpine terrain: detailed measurements from the Canadian Rockies" *by* Michael Schirmer and John W. Pomeroy

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We thank the reviewer for the constructive comments. We copied the reviews comments in this reply for a better readability and marked them using italic fonts.

The objective of the study is to analyse factors which control areal snow ablation and snow cover depletion in a small study area in the Canadian Rocky Mountains. The analysis is based on very detailed maps of snow depth and snow depth differences obtained by several flights of UAV in one winter season 2014/2015. The results indi-

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Discussion paper

cate that ablation rates differed in space and were mainly related to the spatial patterns of solar irradiance and albedo. The most important factor controlling snow cover depletion was the initial distribution of SWE, which was five time s more variable than melt variability. The authors conclude that in near summer solstice conditions the snow cover depletion curves can be calculated only from SWE spatial distribution. Generally, the topic of the manuscript is interesting and within the scope of the journal. The manuscript has a good structure and is clearly written. However, the analysis is based on only a few observations in one winter season, so the significance and generality of results are rather small. This is very well documented by the authors, who conclude that: ". . . clear advice to modellers is still not possible" and "Thus longer time series of spatially detailed SWE observations need to be made . . .". Moreover the methodology used (UAV snow depth mapping) is not new.

We agree with the reviewer that there are papers on UAV snow depth mapping, but mainly on the accuracy of this method. This is one of the first using this methodology to answer snow-hydrological questions.

These facts raise a question whether the presented results provide a significantly novel contribution satisfying the HESS requirements for a scientific paper. In my opinion, the presented results are in its current form rather premature and more systematic and longer datasets are needed to justify interpretations made and to allow a transferability of results to other regions.

The novelty is given with the high-resolution data set. Since snow depth is known to vary largely below a scale break of tens of meters in alpine environments we think that a replication of studies which relied on manual probing in a much coarser resolution is needed. Manual probing with a spacing at or larger this scale break may complicate the interpretation of the results (Clark et al., 2011), since the dominant spatial structure can hardly be captured. A similar high resolution data set was only presented by Grünewald et al. (2010) and Egli et al. (2012), which only covers one study region and two different seasons. A replication of these studies to other areas is urgently needed to show the transferability of results. Furthermore, we present an explanation on the missing correlation between SWE and melt. This is now more precisely mentioned in a more focused way in the revised manuscript.

While we feel that this paper makes a significant movement forward in our understanding of the relationships between snow accumulation and snowmelt patterns at multiple scales, final answers can only be given if more replications like this study are available. Given the large effort in field data acquisition and processing of this high-resolution data set to good quality levels (the signal of a melt period needs to overcome the noise) and the rare availability of long melt events without snowfall, this can only be a stepwise process. However, this study may initiate more replications with indicating the urgent need for this to better guide snow-hydrological model design.

During our field work in 2015 there was nearly no knowledge of the spatial noise inherent to this method. Thus, we ended up with more field days than in the final paper (9 days instead of 5). Since the noise is site and weather dependent, as well as the signal, i.e. the melt amounts, also in 2018 we would probably include much more flight days than what can be finally use. Post-processing of one field day can be done within one day, however, only if the data is acceptable with standard settings. In our case, it took us several days to achieve acceptable data quality with changing post-processing settings. This shows the tremendous effort to achieve good results, which implies that single studies using this technology can only add to existing knowledge.

The potential of transferability will be discussed in the final version.

"In alpine terrain a large part of SWE variance is generated at scales below a scale break of tens of meters (e.g. Deems et al., 2006; Schirmer and Lehning, 2011). If melt reacts mainly to solar irradiance changes on larger scales similar to our study, a scale mismatch between ablation and SWE might be a general feature of alpine terrain."

1) I found a little bit confusing connecting snow depth change directly to snow water equivalent. How valid/uncertain is the assumption of uniform snow density at 10cm *spatial scale?*

First of all we only use HS and dHS in the final manuscript. However, this still implies that they can be used as proxies for SWE and melt. In the newly written introduction we point to other studies which rely on spatial model results. Spatial modelling of snowmelt in complex terrain inherits a large amount of uncertainty. For example, energy balance modelling relies on assessment of turbulent fluxes, which is dependent on local wind fields. Those wind fields in complex terrain are very difficult to estimate (e.g. Mott et al., 2010; Musselman et al., 2015). To be independent of model uncertainties, we chose semi-direct measurements of melt and SWE. These also have uncertainties, e.g. one has to either apply no density (depth only) or just a few density measurements or modelled densities. However, it is well known that snow depth varies to a much larger degree than density (e.g. Pomeroy and Gray, 1995; López-Moreno et al., 2013). As a consequence, it is common to estimate areal SWE with a small number of representative density measurements and a high number of snow depth data (e.g. Steppuhn and Dyck, 1974; Pomeroy and Gray, 1995; Rovansek et al., 1993; Elder et al., 1998; Jonas et al., 2009). We were able to take a few SWE measurements, but we did not multiply dHS with snow density to estimate the ablation rate, because we think that our SWE measurements were neither representative for the whole area nor for the time between the measurements. This is why we analysed and interpreted HS and dHS as proxies of SWE and ablation rate.

This does not answer your question on snow density variations at a 10 cm scale. There are a view measurements assessing the density variability at smaller scales. For example, Proksch et al. (2015) showed SnowMicroPen measurements along a transect with 0.5 m spacing in the Antarctica (cp. their Fig. 12), which shows density variations resulting from different deposition and metamorphic processes. Given this lack of knowledge we think that in our study region, however, with large differences in HS and rather deep snowpacks density differences are mainly driven by larger scale deposition processes (tens of meters) rather than smaller scale metamorphic processes driven by e.g. summer terrain or vegetation differences as it may be the case in shallow snowpacks. Thus, we argue that the results of López-Moreno et al. (2013) also apply in our study area at 10 cm scale as an increase of variability of density at this scale seems unlikely.

2) P.7, l.27: ". . .increase? of R2". Please check.

Many thanks for finding this error. However, we decided on excluding the topic stepwise regression in the revised version.

3) Fig. 5. Perhaps consider to switch x and Y axes (to plot dHS on Yaxis as a prediction variable). Is a simple linear relationship robust enough?

We will switch the axis and delete the regression lines. A new Figure 5 including the relationship to HS0 was requested by reviewer 2.

4) References: I understand that the authors wrote many papers about the subject and are expert in the field, but I feel that the references are too biased to their own work. I wonder whether all the cited works of the authors are really relevant for the topic and/or if there are some other relevant studies evaluating snow cover depletion curves and factors controlling them on different scales.

The introduction has been changed substantially following this comment and the suggestions of reviewer 3. This will include a diversification of cited authors, as well as focusing on relevant work for this topic.

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