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Hydrology and Earth System Sciences
Discussions

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

Anonymous Referee #2

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The study of Schirmer and Pomeroy used high-resolution aerial photographs of a mountain ridge to determine the spatial distribution and height of the snowpack (HS) during the melt season. Several surveys were undertaken at different times and the differences in snow height measurements (dHS) were used as proxies for ablation. The spatial patterns of ablation (dHS) was compared with pre-melt snow water equivalent (SWE, measured manually) and several topographical variables. Albedo (i.e., brightness of the snow) and solar radiation differences (i.e., deviation from North or soral irradiance) were identified at the dominant controls on dHS, whereas there was

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no correlation between dHS and initial SWE. The authors explain this lack of correlation with the difference in spatial scales at which dHS and initial SWE are affected by topographic and climatic variables. The high-resolution measurements of dHS further allow to estimate the spatio-temporal variability of ablation. The authors find that the variability in ablation (dHS) is much smaller than that of initial snow depth (HS0). Consequentially, snow cover depletion curves (SCD) are less sensitive to the spatio-temporal variability of ablation and most sensitive to the HS0 of the area. The authors show this by determining and comparing SCD's from combining either uniform or variable initial HS0 with uniform or variable dHS.

The high-resolution data set of spatial snow depth distribution is unique and potentially allows interesting analyses, however, I find it difficult to identify the novel scientific contribution of this study. One of the main findings, that initial snow depth (HS0) is not correlated with changes in snow depth over time (dHS) is only briefly mentioned in Sect. 3.3. The second finding, SCD curves for the study area are largely affected by HS0 and less by dHS, has been studied extensively previously (P2L21-24, P3L31-32). I would thus recommend to revise the manuscript in a way that brings out the novelty of the authors' findings more clearly. and to help the reader to learn something. In addition, the language of the manuscript needs to be improved as some sentences are confusing and hard to understand (e.g., p11L27: "However, no study showed consistent and persistent fine-scale association between ablation and SWE suggested that they can be considered uncorrelated in modelling at fine scales."). Please find some more detailed comments below.

Introduction:

P2L30 – P3L33: It is difficult to follow the authors' train of thought here as this paragraph seems like a random collection of studies without an overarching theme that help the reader to get to the same conclusions as the authors. Is the overall point of this paragraph to show that SWE and melt are variable over time and space or that it is challenging to determine an accurate SCD curve? If so, it would help to state this as a

theme at the beginning of the paragraph.

Please explain briefly what a SCD curve is.

Methods: 2.1 Site description: Why was this study site chosen, given that the snow distribution was strongly affected by ski slopes and strong winds?

2.2 UAV Data acquisition: please don't use abbreviations in the headings or write as "Unmanned aerial vehicle (UAV) data acquisition"; How many flight were made in total? Can you please provide the dates of the individual flights in this section? Your statement on p5L20 is not clear enough: "Ideally, four flights in total were made each sampling day, two for each subarea with perpendicular flight plans. Weather conditions and technical problems often allowed only a part of this program."

2.3 Accuracy evaluation and manual measurements: From this description of the methods I understand that for each (4?) sampling day, snow depth and density (i.e., SWE) were measured at up to 7 locations over the entire field site. Were these SWE estimates assumed to be representative for the times between measurements? Did you multiply snow density with dHS to estimate the ablation rate? This is not at all clear from your statement: p6L5-9 "At these GPS measurement points, snow depth was also manually measured and snow density was measured at approximately each third of these points. Density measurements were not sufficient to confidently estimate SWE from snow depth into SWE and ablation rates from differences in snow depth. As such the originally measured quantities are analysed and interpreted as proxies for ablation and SWE in the text." Also, what do you mean by "originally measured quantities"? Please be more precise.

Results and discussion: 3.3 Spatial differences in dHS: It would be nice if you could also provide the correlation results for the remaining variable Slope, as well as the p-values for all correlations, for completeness (Table 1).

In Section 3.3. you use dHS (change in snow depth) equivalent to melt (or ablation),

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although nowhere before was explained what this assumption is based on and how melt was estimated. This important bit of information only comes later (Sect. 3.4, P12L28-30); please include this description into the Methods section. Also, if you simply multiply HS and dHS with a uniform and temporally constant snow density, the variability of the resulting SWE values and melt volumes are the same as for HS and dHS multiplied by snow density.

P11L29-32: Your main finding, that is that initial HS is not correlated with dHS, is somewhat hidden in section 3.3. Given that this is a major result of your study, I would suggest to include a figure similar to Fig.5 that actually shows this lack of correlation. Also, your conclusion "These values indicate much larger SWE variability than ablation variability in this period." is equivalent to a larger variability of HS relative to dHS (in other words: the relative standard deviations of HS and dHS are same as for SWE and melt volumes). Thus, it seems confusing to use SWE instead of HS and melt instead of dHS, because SWE and melt are not measured the same way as HS and dHS.

Minor comments: - P4L5: What results of what models? A reader not familiar with snow hydrology literature has no idea what it meant by that. - P6L18: Please explain what SfM means. - P11L24: "The correlation of. . ." What? ". . . with. . ." - P16L22: ". . .varying exposures of vegetation, which is not a factor in this study." Earlier in the manuscript you state that vegetation has a strong effect on snow distribution. Please explain. - P7L27: Shouldn't it be "decrease of R2"? - P12L23: "Relative importance of ablation and initial SWE" Relative importance for what?

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