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Hydrology and Earth System Sciences Discussions

Interactive comment on "Applying a time-lapse camera network to observe snow processes in mountainous catchments" *by* J. Garvelmann et al.

Anonymous Referee #3

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Using a network of time-lapse cameras to observe and quantify snow processes and their spatial heterogeneity certainly constitutes an interesting approach. The paper comprises two elements: 1) introducing the time-lapse camera system and the evaluation techniques developed to retrieve quantitative data such as snow depth and surface albedo; and 2) presenting exemplary data to demonstrate the potential of using distributed camera network for snow hydrological applications. Unfortunately, I have to agree with the other referees that in its current form the paper is very weak.

The authors state in the introduction "we automated and extended the information that can be derived from the time-lapse images to derive time series of snow depth, albedo, snow interception, and the state of precipitation". But the reader is left alone with vague

statements how this has been achieved. After showing the results of the automated algorithm for snow depth in comparison with manually evaluated images for one camera and one year, the reader is informed that further data analysis rely on the manual evaluation procedure. Does that mean that the automated analysis has failed? We later read that "the observed snow depths at the two forest locations are nearly identical suggesting that exposition does not play an important role for snow accumulation under forest canopies". How could the authors possibly infer such a broad statement from single-point estimates at two locations? Based on snow depth data from 19 camera locations and snow density measurements from manual snow surveys, the authors further calculated the spatial distribution of SWE in one test catchment prior to and after a rain-on-snow event by "using a simple linear regression model". The reader has no further information as to the linear regression model, nor whether density was measured before or after the event, or (hopefully) both. Nevertheless, the statement is concrete: " Therefore, a total of 43mm of SWE melted during the event [...]. The numbers show the importance of the snowmelt for this flood event as about half of the available flood peak input resulted from the rain-on-snow melt of the pre-existing snow cover". Even if the SWE modeling procedure was accurate, what do you gain from the camera network, if all sites have to be visited pre/post storm to measure snow density?

The above comments show a striking lack of technical information and thorough discussion of the results obtained, exemplarily assembled for the parameter snow depth from section 2.3 and 3.1. Similar comments would apply to the other parameters: How would you justify equation (1) if your reference target is regarded as "perfectly white", resulting in a maximum albedo of 0.6? Why do the authors talk about "snow surface albedo" and present values below 0.6? Why do the authors assume linearity between relative RGB pixel values and albedo (I second the respective and more detailed comments raised by the first reviewer)? What does Figure 6 tell us, if we just look at weather station based albedo values above 0.6, assuming that these are the ones for situations with a fractional snow cover of 100%, is there any correlation at all? What happened to the interception level data between Figure 8 (max value of 56%) and

Figure 9 (max value of 100%), where the latter re-scaled to allow an inter-comparison between cameras?

At this point, I recommend i) that the authors start over again and rewrite the paper from section 2.3 onward before resubmission. It is important to first focus on the technical aspects, before resulting data can be used for further analysis. It seems possible to solve the problems associated with the data inferred for snow depth and maybe precipitation phase without the need to introduce further validation data. I thus recommend ii) to remove all content related to albedo and interception. Snow depth alone and the study example around figure 6 is enough content to construct a paper that merits publication. I further suggest iii) to present all relevant data, not only exemplary subsets.

Regarding the technical aspects it begins that we lack simple information such as the manufacturer and the model of the LiPo battery. We then need a detailed description, validation, and discussion on how snow depth data was derived from the images: What is the exact strategy used to automatically determine snow depth? have the authors thought about the problem that comes with preferential melt/deposition around snow stakes resulting in biased readings, in particular when the camera is looking onto and not parallel to the snow surface? would the code be available? what happens if snow is intercepted on the snow stake, or on the black part of the control surface? how do the algorithms deal with image distortion? how many images (from all cameras + all times) were unusable because of various reasons? how do the data compare against readings at the weather stations?

Regarding the interpolation of the snow data, we need a detailed description of the linear regression model developed. Evaluate existing literature on how other authors tackled this problem and discuss your own approach in this context in detail. Is snow depth converted to SWE prior to or after interpolation, why? Describe when, where, and how density data was collected and present the data. Is density correlated to snow depth, season, elevation, and is it different inside vs. outside the forest? How did you

estimate density at locations and for times at which no measurements were available? How about the elevation trends of HS and SWE? How did density evolve during the rain-on-snow event?

I hope these questions help to continue working on the paper. I would assume that all reviewers agree that it would be highly appreciated if the authors took the effort to further develop this paper to make it acceptable for further publication.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 9, 10687, 2012.