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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.

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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.

AR: The intention of the paper is to demonstrate the potential of a distributed network of standard digital cameras as an observation tool. Therefore exemplary data is pre-

sented to show potential fields of applications of such a camera network. We absolutely agree with the referees that a revision of the paper in its current form is required in line with the other reviewers and are very thankful for constructive and helpful comments which will help to improve the paper.

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.

AR: More technical details about the image analysis approach will be included in the revised manuscript.

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?

AR: As shown in the paper (Fig. 4) and apparent from the statistical data presented in the paper (p.10692 line 26) the automated analysis has not failed. But as discussed later in this comment we used a semi-automatic approach to determine snow depth data from the images.

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?

AR: We will modify the sentence to make clear that the observation is in this form strictly speaking only valid for this location.

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?

AR: More information about the linear regression model (as well as coefficients of determination) which was applied separately for forested and open sites will be included in the revised manuscript. However, we would like to stress again that we want to show examples for application and not present a detailed analysis on the variability of SWE in catchments. Regarding the snow density measurements, we agree that snow density measurements are necessary to calculate SWE from snow depth data. However, this approach or a sole modeling approach, which is certainly less accurate, is used around the world to use continuous snow depth measurements and transfer them into SWE. The cameras provide daily or sub-daily measurements of snow depth – which is important for any hydrological modeling and prediction of snowmelt induced floods.

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?

AR: As already mentioned in comments for reviewer #1 and #2, the albedo section will be revised substantially based on the thoughtful and constructive suggestions of the first referee Dr. Javier Corripio.

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?

AR: No rescaling was conducted. With the presented approach the fraction of white pixels within defined polygons in the canopy are calculated. Therefore the distance of the camera to forest and the forest structure has an influence on the calculated results. This means, for example, that the fraction of white pixels may be larger for images where the distance between camera and forest canopy is larger even though the interception storage may be similar. As stated in the paper with this approach we gain qualitative and semi-quantitative (relative interception) information about the interception process and the subsequent unloading/melting of the snow in the forest canopy. Therefore this approach has the potential to improve existing interception algorithms. An approach to determine quantitative data would be definitively desirable and might be developed at a later stage. However, there are to our knowledge no observation methods available to continuously monitor interception in forest canopies and we believe that this approach will provide a much better continuous information about interception processes as relying solely on modeled interception.

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.

AR: We do not agree with the referee to remove all content related to albedo and interception. The goal of the authors is to present an improved version of the presented study including all possible winter applications with a camera network. We do not see a need to change the structure and the content of the paper.

Regarding the technical aspects it begins that we lack simple information such as the manufacturer and the model of the LiPo battery.

AR: Information will be included. Manufacturer: Kokam, Type: SLPB – Superior Lithium Polymer Batteries 3.7 V, 3200 mAh (20C). We did not include these information since product half-life in this sector is relative short.

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?

AR: Preferential melt and depositions are a problem when using an automated image analysis approach. That was one reason (other reasons see discussion section of the manuscript) why we decided to read out the images semi-automatically. Nevertheless an automatic determination of snow depths from digital images is possible as shown by other studies cited in the manuscript, however, errors will occur need to be corrected afterwards as well. The code would definitely be available upon personal request. However we do not feel the need to "advertise" this in the paper.

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?

AR: Further information will be given about the percentage of images at different locations that have been discarded due to several reasons (fog, strong snowfall, freezing of the camera lense, snow intercepted on the stake and reference board. . .)

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?

AR: We use standard snow tubes to manually determine snow density. Extensive snow surveys were carried out at several elevations and separately for open and forested locations at least every fortnight accounting for the concerns of the reviewer regarding density dependency on elevation, snow depth and land cover. Such a snow survey was carried out just prior to the event in question giving us very reliable snow density data. Snow depth was converted to SWE prior to the interpolation of the data. This explanation will be included in the revised manuscript. Not surprisingly, we did find strong elevation trends for snow depth and SWE which were used for the linear regression as discussed in the original manuscript. It has to be stressed again that the regression analysis presented should not be considered as a stand-alone approach for the regionalization of snow data, rather than to show the potential of the spatial distributed information gained from a camera network presented in this study.

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