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Interactive Comment

Interactive comment on "Microscale variability of snow depth using U.A.S. technology" *by* C. De Michele et al.

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Dear Prof. Steven R. Fassnacht,

We appreciated your comments on the manuscript. In particular, we agree with you that more details are needed to justify some choices. Please find here point-by-point answers to your questions. Please refer to the revised version of the manuscript we are going to submit for details.

Overall this paper has a lot of potential. It can make an important contribution to illustrate how inexpensive (much cheaper than lidar) methods can be used to estimate snow depth remotely at fine resolution (sub metre), possibly over large extents; this paper shows a small extent but there seem to be no limitations to going to much larger *domains. This is especially true in remote and/or inaccessible areas. This type of data collection system has great promise for snow and ice mapping, building upon work other earth science applications*

We agree with you that U.A.S. systems (also known as drones) can be a potential alternative to existing techniques to run distributed surveys in many fields of geosciences. In particular, their capability to run semi-automated, cheap and quick surveys of a study area allows for a repeatable monitoring of many natural phenomena.

However, there are some substantial problems. Crucial components are not explained or poorly described and the comparison of UAS to manual measurements is too simple. The paper needs to be rewritten and re-focused. It reads like a technical note, as the comparison dataset is very sparse (1 point per 160m or 25,000km2). The authors should consider evaluating the spatial patterns of snow distribution, especially since this dataset is much finer than other similar extent dataset, such as those collected using airborne or terrestrial lidar (e.g., Lopez-Moreno et al., 2015; Hydrol. Proc.; doi:10.1002/hyp.10245). A dataset covering 300,000 m2 (or 500m x 500m) does not exist at this resolution (5cm) in the literature. Pattern analysis would illustrate its utility. The authors suggest the importance of such fine resolution, and while hydrologically this may not be crucial, it is relevant in the context of sampling (e.g., Lopez-Moreno et al., 2013; Advances in Water Resources; doi, 10.1016/j.advwatres.2012.08.010). Contrary to what the authors say on page 1053, line 15-17, there is literature on how many points are need to be representative. The author should consider the NASA Cold Lands Processes Experiment (see Elder et al., 2009; J. Hydrometeorology).

In the revised version of the manuscript, we provide a wider context for this work within the existent scientific literature in order to avoid this to read as a technical note, only. As an example, we reshaped the Methods section to add more details about the UAS platform, the processing technique and the comparison with point data. The Results section have been enlarged, while results implications are discussed in a wider way within existing literature. In the Introduction, we now report in a clearer way that our purposes are twofold: on the one hand, we aim to evaluate if these devices can be used to return a quantitative estimation of snow depth over a small area using photogrammetry (hence an evaluation using point data). On the other hand, we also aim to assess whether a finer-than-usual spatial resolution provides a benefit for hydrological applications. As visible from Table 3 (TCD version of the manuscript), a very fine resolution with respect to usual distances between data point seems to have a relevant influence on hydrological evaluations (since the volume of snow one would obtain from interpolation of point measurements can be severely biased with respect to the UAS-based volume).

However, we agree with you that more evidences are needed on this point. For this purpose, we included an additional test to existing Tables 2 and 3, that shows how maps with an increasing cell size (obtained by resampling the original one at 5 cm) keeps only a small fraction of the original spatial variability of snow depth. We also quantify this loss of information by making the differences between the original snow depth map and the one sampled at 50 and at 100 m. Moreover, we also show how some basic statistics (mean snow depth, standard deviation, CV, maximum and minimum values) change with increasing spatial resolution. As you correctly suggested, we justify this test by considering the fact that a database with a so fine spatial resolution does not exist in the literature, to our knowledge. As a consequence, the investigation of statistical properties of snow depth at a sub-metric resolution is an important step forward that can be reached.

We agree with you that, in the literature, it is nowadays known how to determine the number of points that would theoretically be needed to get an estimation of mean snow depth at a point within a certain error range (see references in the text as examples). On the contrary, to our knowledge, no specific rule or common practice exists to determine the minimum number of manual snow depth measurements, over a given area, needed to evaluate whether a given remote sensed technique returns satisfactory performances or not. In the revised version of the manuscript, we now stress the fact that

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the evaluation we run here is still preliminary, and that a more exhaustive evaluation is needed. In the revised version of the manuscript, we added a preliminary comparison between the precision of this technique with respect to existing methods (taking laser scanner as an example given the abundancy of literature on this topic).

The biggest problem is likely the comparison to the manual measurements. Only 12 measurements were made for the one date when snow was present. It is not possible to go back in time and collect more data, but this could be a fatal flaw of the paper as it is currently presented. It is stated (p1057, line 10) that there is a "slight difference" between the UAS and manual measurements. There is no mention of the horizontal accuracy of the manual depth measurements. I assume that a GPS unit was used to determine the coordinates of the manual measurement. If so or if not, this need to be explained. I highly doubt that the manual measurements are at the same 5-cm resolution UAS pixel. See Lopez-Moreno et al. (2011; The Cryosphere; doi:10.5194/tc-5-617-2011) for 1-m resolution variability and Fassnacht et al. (2009; Ecol. Complex.; doi:10.1016/j.ecocom.2009.05.003) for crystal to metre scale resolution variability.

We agree with you that more details were needed on this point. As we have already said before, we are now much clearer on the fact that the evaluation we run here is still preliminary, and that a more exhaustive evaluation is needed. However, note that the coordinates of the points were taken by total station theodolite observations referred to GPS baselines that were surveyed by static approach (40 minutes sessions). The accuracy of the obtained coordinates is of the order of 2-3 cm (i.e., comparable with the spatial resolution of the DSM at the maximum resolution). This allows for a direct comparison between UAS-based and probes-based readings of snow depth. We added this specification in the revised version of the manuscript.

The three interpolation maps are not shown, likely since they are too simple and not realistic.

We chose not to report the three interpolation maps in the manuscript for the sake of

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brevity. However, the main purpose of these interpolations is to provide an evaluation of snow volume to be compared with the UAS-based estimation. This information is already reported in Table 3.

The swingletCAM system is proprietary (sensefly ®) and not explained well. The 3-D locating is mentioned, but with the "georeferencing" present later, its relevance is not stated. We do not all have access to such hardware, so insight would help those who want to build such a system, or justify its rental or puchase. Later the Agisoft software is used to created the Digital Surface Maps (DSMs). While this is being used by many, the specifics should be explained.

We added some additional details about the swingletCAM system and the Agisoft software in the revised version of the manuscript.

The contour intervals (10-m), presented from the "local regional administration," are too coarse for the graphical comparison presented in the paper. It is stated on page 1056, line 21 that the UAS DSM is in agreement with the "local regional administration." While those figures are too small to truly compare (make them bigger), they do not appear to be in agreement. I suggest that a digital elevation model (DEM) for the area (e.g., SRTM) should be used to compare the "agreement" quantitatively. This may require transformation of one of the datasets (UAS DSM or SRTM DEM).

We added a more exhaustive validation of the autumn DSM. In particular, we added a new picture where U.A.S.-based contours are directly superimposed on those reported in the topographic map (see Fig.6 in the revised manuscript). These show a very good agreement. However, we agree with you that a more effective evaluation is needed. For this purpose, we have also compared this DSM with the 20m x 20m DSM of the Lombardia Region. The statistics of the differences are coherent with the accuracy of this DSM (i.e. with a standard deviation of 1.2 m). We also enlarged all the pictures to guarantee a clearer evaluation.

In places the writing is quite choppy. For example, the words "automatic" and "au-

tonomous" should be replaced by "automated." Since the authors may not be native English speakers, I recommend that a native English speaker review/proof read the paper before resubmission. There are numerous other examples throughout that I will not highlight

We revised our use of English and grammar.

Specific comments

We took care of your specific comments while revising the manuscript. We provide here additional answers to some of your comments (when needed). However, we are willing to add additional details about those points that are not reported here, if this is needed.

I don't like the title. By microscale the authors mean centimetre scale. Also, U.A.S. is not a known shortform - The second survey is at the end of accumulation. This can be misleading, Perhaps say that it is around the time of peak accumulation

We modified the title. The new title we are going to propose is "Using drones to map snow depth variability at cm resolution: an evaluation at peak accumulation".

p1051, l1: is it truly bare soil?

We changed this statement. In the current version of the manuscript, we specify that the autumn DSM is the DSM of the ground. Very sparse vegetation and rocks characterize it.

p1051, l20: what is the basis for the "criteria?"

We removed the explicit inclusions of these criteria, since they can cause confusion to the Reader. Those points represent some of the features of the study area that allowed us to run this survey. In particular, being at a quite high elevation, it is usually covered by seasonal snow in April, while its easy accessibility allowed us to reach it during both the surveys. We did not report explicitly these criteria in the current version of the

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manuscript since they are merely logistic.

p1058, l1: at what scale are the "micro-topographic differences?"

We consider as "micro-topographic" resolution the one we use to map snow depth (i.e., from 5 to 20 cm).

p1059, l4: "snow density ... measured" - provide more information about this.

In the revised version of the manuscript, we added a specific paragraph in the Methods dealing with the details of snow density measurements. On April, 11th, i.e. the same day of the April survey, a snow pit was excavated, and a snow density profile was measured through gravimetry (using a cylindrical samples holder, 15 cm long and with a 7.5 cm diameter). Measurements were taken at around 20 cm intervals along 210 cm of snow depth at that point. Although one measurement for the entire area could look limiting, note that bulk snow density has usually a reduced variability in space (at this spatial extent), since it changes mainly according to the season, or climate (see Mizukami and Perica 2008, Jonas et al. 2009, McCreight and Small 2014 or De Michele et al. 2013, citation in the text).

Table 1: can't directly compare the manual measurement to UAS due to error in locating the manual measurements and their support (see Hood and Hayashi, 2008; The Cryosphere).

As already said, the coordinates of the points were taken by total station theodolite observations referred to GPS baselines that were surveyed by static approach (40 minutes sessions). The accuracy of the obtained coordinates is of the order of 2-3 cm (i.e., comparable with the spatial resolution of the DSM at the maximum resolution). This allows for a direct comparison between UAS-based and probes-based readings of snow depth.

Table 2: how were these different resolutions of UAS based data derived?

As we now specify in the text, the three UAS-based maps were obtained from the cloud of points obtained from the UAS. Note that, as we now specify in the text, the main novelty of UAS system resides in the features of the support (especially, self-conduction and low cost), while the method use to retrieve points coordinates (e.g., photogrammetry) are usually rather traditional.

References:

Jonas, T., Marty, C., Magnusson, J. (2009). Estimating the snow water equivalent from snow depth measurements in the Swiss Alps. Journal of Hydrology, 378(1), 161-167.

McCreight, J. L., Small, E. E. (2014). Modeling bulk density and snow water equivalent using daily snow depth observations. The Cryosphere, 8, 521-536.

Mizukami, N., Perica, S. (2008). Spatiotemporal characteristics of snowpack density in the mountainous regions of the western United States. Journal of Hydrometeorology, 9(6), 1416-1426.

Interactive comment on The Cryosphere Discuss., 9, 1047, 2015.