

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

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Dear Anonymous Referee 1,

We would like to thank you for the review and useful comments. We will consider them carefully while revising the manuscript, and we will try to address them at our best. Here you have a point-by-point reply to your indications and questions. Please find in *italics* your comments, and in plain text the answers.

General Comments

This is an interesting paper that in essence seeks to compare a photogrammetric approach to estimating deep snow depth (average of 1.80m) from a UAS platform with ground measurements. The authors explore comparisons between a DSM created with

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industry-grade software using stereoscopy and in situ measurements of snow depth, albeit at the deeper snow depth range. They also compare DSM snow depth and estimated snow volumes from the UAS with estimates from interpolated snow depth and volume map data. In my view, this aspect is un-necessary and does not add any substance to the paper. This is supported by the fact that they do not really comment on the volume estimates in the conclusion. Suggest it is removed form the analysis as it is quite weak.

We thank you for this comment. Probably, we were not enough clear about the reasons why we compare the DSM with the estimations of distributed snow depth one would obtain from simple interpolation techniques, so we will be much clearer in the revised version of the manuscript. We are aware that such a comparison is not the main focus of the paper, as this is testing a U.A.S. system in measuring the microscale variability of snow depth. However, we opted for including it, since the interpolation of sparse point measurements has been the traditional way used for a long time to get a distributed evaluation of SWE for operational applications. As an example, the SNOTEL network in western US was set-up in order to determine near-real time scenarios of water availability in that area by measuring this quantity at an increasing set of points. In this perspective, one could argue that using a photogrammetric technique with such a high spatial resolution to retrieve this kind of information is too time consuming, and that this does not add any clear added value to the final result. This is probably one of the reasons why SWE estimations have been mainly run at the point scale until now. However, we show here that 1) getting direct distributed estimations of snow depth can be cheap, fast and relatively safe, by using a U.A.S. system, and that the interpolation of snow depth at random point values can lead to a non-negligible difference with respect to the DSM one gets from U.A.S. It is worth noting that the mutual distances between these random points are very reduced with respect to the usual distances between gauged sites in operational applications. Starting from snow depth, SWE can be derived by measuring (or modeling) bulk snow density at the same location. We will improve the discussion on this point in the revised version of the manuscript.

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A significant question is that the application is for a one-site, one day estimate when things go well. But what is the evidence for its applicability under different landscape and snow conditions? The authors state that the site topography is homogeneous but were there trees or low-stand vegetation types present? And even if there were not, what would the implications be if they were?

We will add some considerations about these points in the revised version of our manuscript. On the one hand, an exhaustive assessment of the variability of sensor (and support) performances with landscape, snow conditions, vegetation and topography heterogeneity is probably beyond the scopes of this contribution, as this would require a much wider set of field surveys. On the other hand, we agree with you that speculating about the implications of these factors on the performances is important, and worth including. As far as we were able to see, the quantitative use of U.A.S. systems on snow has not been documented exhaustively in the literature, and this has some intrinsic complications (e.g., the difficulty in orthophotos composition due to a general reduction in topographic features during snow presence on the ground). As a consequence, we think that documenting the feasibility of such a survey, and that the expected accuracy of this survey is rather high, will contribute to trigger new studies that will investigate the important points you raised.

Overall, the paper is quite well written although the grammar is a little awkward in several places and needs to be proof-read further.

We will revise the grammar and the use of English.

Specific Comments

P1050 The authors need to better describe the distinction between UAS as a platform and how it can make a contribution to this application, as opposed to the instruments that are described in the introduction. What previous stereoscopy approaches have been adopted elsewhere and why have they been successful/unsuccessful? This will better make the case for the UAS approach since this is where the novelty of the paper

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lies; the case needs to be made more convincingly from the start.

We completely agree with you on this point. As you have correctly said, U.A.S. are a novel platform that could allow to run traditional surveys (such as photogrammetry, or even airborne laser scanning) in a semi-automatic, accurate, repeatable, and cheap way. This is the main reason why they could represent in the future a very interesting alternative to both point and “manned” remote sensing techniques. We will rethink some parts of the Introduction to stress this point in a better way.

P1051 line 21– why was 2000 m.a.s.l. selected as the threshold? How sensitive is this to the success of the project?

This threshold plays no role in driving the success of the project. While designing this field campaign, we chose the Malghera Lake area as a suitable location since this area is likely to be covered by seasonal snow in April, due to its high elevation. This is the meaning of lines 20-21 (page 1051). However, since this is just a secondary information, we will remove this numerical specification from the revised version since it is useless and can cause confusion in the reader.

P1051 L23 what does “interested by seasonal snow” mean? Do you mean “covered by seasonal snow”?

Yes, exactly. We will consolidate this in the revised version.

P1052 L12 what are “hard climate conditions”?

A U.A.S. is usually a light and quite fragile device. Therefore, it was our intention to denote as “hard climate conditions” those conditions that would endanger the use of these supports (e.g., strong wind conditions). We will improve this in the revised version of the manuscript.

P1052 L27 Why was the GSD set to 4.5 cm?

We chose 4.5 cm as GSD since this value allowed a survey at a flying elevation of

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around 130 m, which represents a good safety condition for the U.A.S. device.

P1052 Section 3.1 What is the camera wavelength and bandwidth (e.g. full width at half maximum)? What is the signal to noise ratio of the instrument and what is the sensitivity of the detectors?

We operated the survey using an optical compact camera (Canon Ixus). Consequently, the survey has been made in the visible spectrum. The camera uses a bandpass filter for the three colors RGB. These are placed ahead of the CMOS according to the Bayer filter (50

P1053 Section 3.2. The authors make some interesting observations regarding number of points needed to evaluate the performance of a technique. Interestingly, work by Snedecor and Cochran (1969) [Snedecor, G. W., and W. G. Cochran, 1967: Statistical Methods. 6th ed. Iowa State University Press, 593 pp.] introduces such methods and work we did in 2005 attempted to leverage this knowledge (Chang et al. 2005 J. Hydromet. Vol. 6: 20-33.). It would be interesting to see how this might fit with the authors' study.

We thank you for this suggestion. We will consider this approach in the revision of our manuscript.

P1053 Section 3.2. Several studies have explored spatial variability of snow at the landscape scale (much of the Arctic and Sub-Arctic snow research frames spatial domains at the landscape scale) rather than as a simple random field of variation. This is because there are inherent spatial scales of variation of snow distribution caused by those controlling factors that the authors describe in section 1. Even in Alpine areas, there is predictability of snow accumulation and redistribution that could have informed the sampling design. Can the authors explain why they adopted the approach that they did for spatial sampling?

We agree with you that the investigation of the variability of snow depth at different spa-

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tial scales is a well-documented field of research. However, as Grünewald and Lehning (2014, citation in the text) state, representative snow depth point values are rather randomly distributed and cannot be identified a priori. A similar idea drove our sampling technique, i.e. the investigation of the performances of a U.A.S. system against measurements at random locations. In this context, random is a key word. In fact, including any additional information could artificially influence the evaluation of the performances, and could have raised a number of objections. Here, we consider the worst case (i.e., no information available). We will include a mention to this issue, and a clearer statement of sampling hypotheses, in the new version of the manuscript.

P1053 I15-28. Here or in the Results section, the authors should include details on how accurate (what the errors were) in these previous studies so that their work can be contextualized. Their study is in a mountainous basin that is not glacierized whilst at least one was in a glacierized basin (Machguth et al. 2006).

We agree with your point of view. We will try to include a wider context about survey uncertainty with respect to the existing literature.

P1054 Section 3.3 The authors describe several methods for spatial interpolation that have been used elsewhere but provide no rationale for their own selected methods – why were these three methods chosen that essentially incorporate spatial weighting rather than combined effects such as elevation derivatives (slope, aspect) and vegetation type?

In the revised version of the manuscript, we will include a clearer motivation of our choice. What we would like to compare are the DSM by a U.A.S. and the estimations of snow depth by simple interpolation techniques. As already said, the main reason is that the interpolation of sparse point measurements has been the traditional way used for a long time to get a distributed evaluation of SWE for operational applications. In doing this, simple techniques are straightforward to be interpreted, and do not add additional modeling uncertainty to the problem, apart from the type of spatial weighting

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considered. Probably, they also represent the most used techniques in spatialization problems. We will be much clearer on this point in the revised version. Vegetation type is not a reliable predictor here since this is very sparse and of reduced height over the entire study area.

P1057 Section 4.3 I agree with the authors that with so few sampling points, it is difficult to make widespread generalizations about the data across all ranges, even though the data seem to agree quite well at the small upper range of snow depths encountered. The sampling points on the ground average 1.80 m with a -7.3 cm bias relative to the DSM data. But how applicable are these at low snow depths less than 1.4 m, for example, which were not sampled in the field? Can the authors provide some further insight across a wider range of depths as to how this method might perform?

We agree with you that assessing the vertical resolution of snow depth measurements which are outside our observation range is problematic. As we state, additional investigations are necessary to assess U.A.S. performances in case of, e.g., shallow or patchy snow cover conditions. However, please consider that U.A.S. is a novel support to run a well-established survey (i.e., photogrammetry). This should improve the reliability of the measurements we took. We will try to elaborate on this point in the revised manuscript.

P1058 L16. Why do the authors state 20cm as a favourable resolution for snow depth mapping? Why not 25 or larger? This seem arbitrary. Did they test coarser spatial resolutions? More evidence is needed for this assertion.

We will try to clarify this point in the revised version of the manuscript. We would like to point out that, in the current version of the manuscript, 20 cm is mentioned as a good trade-off, namely as an acceptable compromise between the push for increasing resolution (i.e., considering smaller pixels) and the amount of data to be considered in survey processing. What we noted is that an increase in resolution beyond 20 cm (say, 10 or 5 cm) does not seem to provide any added value to the survey, in this case

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study. In other words, 20 cm seems to be a good upper boundary of photogrammetric resolution in similar situations. We will be clearer on this point, thank you.

P1058 Section 4.5 Since the average in situ measured snow depths have a -7.3 cm bias, it is not surprising that the interpolated data also underestimate snow depth (and volume). The authors should include the cross validation data from their interpolations since this will provide insight into the precision of the interpolation. This section, while interesting, seems a little un-necessary since spatial interpolation methods that use spatial adjacency only, will always be inaccurate unless there is a dense network of measurement points. It would be very interesting, perhaps to compare the difference snow map with a more physically-based snow model that is better capable of predicting snow accumulation in complex terrain (e.g. CRHM or SnowTran3D).

We refer to previous replies on the same point for a more exhaustive discussion. We appreciated your suggestion about possible cross validation. However, this is problematic given the paucity of points. On the other hand, using a physically-based model in this context is difficult given the absence of input data availability in the area.

P1060 L8. Assertion (3) is not new – the interpolation methods are only biased because the very few snow depth measurements ($n=12$) have a low bias. Furthermore snow volume does not equate to SWE as implied.

We will rephrase this statement. In particular, we will mention that the evaluation of snow depth volume using classical interpolation techniques of randomly chosen point values leads to biased results, that depend on the bias in point values, when compared to U.A.S. results. Clearly, SWE cannot be assumed equal to snow depth volume, but it can be derived from this last information, once snow density is known.

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

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