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

Anonymous Referee #1

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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 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.

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?

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

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

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

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

P1052 L12 what are "hard climate conditions"?

P1052 L27 Why was the GSD set to 4.5 cm?

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?

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 Cochrane (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.

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?

P1053 l15-28. 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).

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?

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?

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.

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).

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.

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