

# ***Interactive comment on “ACCURACY OF SNOW DEPTH ESTIMATION IN MOUNTAIN AND PRAIRIE ENVIRONMENTS BY AN UNMANNED AERIAL VEHICLE” by P. Harder et al.***

## **Anonymous Referee #2**

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ACCURACY OF SNOW DEPTH ESTIMATION IN MOUNTAIN AND PRAIRIE ENVIRONMENTS BY AN UNMANNED AERIAL VEHICLE

Phillip Harder, Michael Schirmer, John Pomeroy, and Warren Helgason

The authors present an evaluation of snow depth retrieval from airborne photos in challenging snow environments. Rapidly evolving UAV technologies provide a relatively low cost platform to make quick repeat, spatially distributed measurements of snow height. The authors are amongst a handful of people who have attempted to provide a robust evaluation of this technology for retrieval of snow depth. Hence, due to the high

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interest in the application of this new measurement technology and the different land surface types presented (prairie and alpine), this paper should be of high value to the community. The detail provided for others wishing to follow their methodology is very useful (such as the niche combination of environmental conditions required to provide optimal accuracy) and the RMSE values as a baseline error estimate are a valuable contribution to the literature. In particular, the use of signal to noise ratios is an excellent addition to the analyses and provides a statistical estimate of error acceptability for this technique. While this is good to see, I would like to raise a number of issues for the authors to consider.

There appear to be a low number (or potentially a low number) of snow depth data used to evaluate depths retrieved from SfM. In some areas of the manuscript this is clear (e.g. observations range between 3 to 19 in the Alpine), but in the prairie, measurements 'between and at 34 snow stakes' is ambiguous. In addition, the reader is left unaware of the spatial coverage of these measurements (within each airborne measurement area) nor how representative they are. At the very least I would expect the n-value to be included in tables 1 and 2. Currently in the literature the amount of in-situ evaluation data for airborne SfM studies are highly variable, e.g. De Michele et al. (2015) tens of depths, Bühler et al. (2015) hundreds of depth, Nolan et al. (2015) thousands of depths. So while this comment should not be seen as an impediment to publication, where very low numbers of in-situ data exist, this needs strong justification or perhaps judicious exclusion from analyses.

Quantification of SCA is demonstrated in Fig 8, and only briefly mentioned in section 3.4. The authors mention this is not discussed in this paper. This leads the reader to ask why not? If data are available to do this in a more thorough manner than currently presented, then this analysis would make an exceptionally valuable contribution to the literature, increase the scientific value of this paper and should definitely be included.

Minor comments:

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While NIR imagery was attempted, as it is not used in any of the results or discussion I suggest excluding it from this paper.

While written in a very readable style, the manuscript in its current form could be shortened in many areas, losing extraneous text that is not relevant to the main thrust of the argument. This will provide room for select expansion of sections in greater detail that are currently vague. Some suggestions for sections to delete or shorten considerably are: Ln 11-14; Ln 29-32; Ln 93-97; Ln 98-104; Ln 115-118; Ln 146-149; Ln 152- 155; Ln 266-269; Ln 342-345; Ln 408-412.

Could much of the information in Ln 168- 181 be put in a table, making this section much more concise?

Ln 137: Could the size of the areas measured be explicitly mentioned?

Ln 205: Why was vegetation negligible? I'd like more information about the nature of the vegetation here to justify this claim for the creation of DSMs.

Ln 205 – 'most of the flights' – this is vague. How many flights? Did this affect the analyses?

Ln 219 – (linked to previous vegetation comment) While vegetation is said to be negligible I need more convincing that grasses, particularly on 24 July at the Alpine site after 'spring up' once the snow has cleared, would not have any impact on the on the ability to pick the ground surface from photos. I expect this concern can be allayed through local knowledge, but it needs to be made explicitly and clearly here as it has been a big issue in the past at other sites.

Ln 240: Please give more details describing what 'dynamic conditions' and 'surface characteristics' are.

Ln 242: Please define either here or very clearly in 3.3.1 how 'problematic flights' are defined. Currently this is, at best, vague.

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Ln 255: Give more explanation on what is meant by 'limited observations' and why this doesn't affect the detection of differences.

Ln 283: No correlation is presented. Do you mean 'related'? If so please change the terminology? If not, please add the statistical correlations.

Ln 325-340: Uncertain that this section on SGM is that useful. Proprietary software (last sentences of this paragraph) is always problematic for scientific understanding, but somewhat unavoidable for much SfM processing. Also, please explain what '2.5D' means.

Ln 376-381: I consider this just speculation. Suggest removal.

Ln 335: 'were' rather than 'where'.

Ln 373-375: Repetitive use of 'This'. Hard to understand what 'this' is referring to. Please re-write this section with increased clarity.

Ln 472: De Michele et al. 2015 is now in TC rather than TCD.

Ln 597 & 601: Is the mean of the absolute values not the same as RMSE? If so, then stick with RMSE as terminology.

Fig 1 c) – Is this short or tall stubble – please specify.

Fig 5 – Opening sentence of caption - introduce 'Alpine' as well as the prairie sites.

Fig 7 – Add '100' on the y-axis of both plots.

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