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Interactive comment on "Measuring the snowpack depth with Unmanned Aerial System photogrammetry: comparison with manual probing and a 3D laser scanning over a sample plot" *by* Francesco Avanzi et al.

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We thank Referee 2 for these useful comments on the manuscript. Please find below our reply to your concerns and our prospective changes in the manuscript.

Comment: "The authors compare the estimation of snow depth over a relatively small and homogeneous surface during a field survey carried out in April. The manuscript is clear and authors indicate the feasibility of using UAS for this purpose. As overall, I have liked the paper".

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Discussion paper

Reply: We thank the Referee for this kind comment.

Comment: "but, sincerely, I am not fully sure about the suitability of this research in a very high impact journal as The Cryosphere is (despite several works of similar characteristics to this one have been already published). My main concern is about to which extent the results are representative for other conditions or study sites. We already knew that UAS is able to provide useful information on snow distribution. Thus, I would have liked to see a paper on this topic based on more field campaigns, under different meteorological and light conditions and probably under a more variable terrain characteristics, which could allow to provide a more sound discussion about errors when using UAS for snow mapping."

Reply: As we also reported in our reply to Referee1, we realized another field campaign over the same area in April 2017 using both instruments (UAS and Multistation) and the same grid of manual measurements. We are now processing these data. Compared to the 2016 campaign, we changed the station point of the Multistation and we were, therefore, able to cover the entire study area, even though this will probably introduce some additional spurious differences due to concurrent snowmelt. We are planning to incorporate this new field campaign in the revised manuscript and therefore discuss the repeatability of our results during different surveys.

Comment: "In addition, one of the strengths claimed by the authors is the density of manual measurements taken (that implies a considerable effort), but the results suggest that manual measurements are far to be the ground truth. Thus, this is not easy to know exactly the source of the observed errors (that indeed may be high as Figure 5a shows)."

Reply: We thank the Referee for this comment, which will be better addressed in our revised manuscript. The main reason behind such a dense network of manual measurements is that previous works on this topic used sparse datasets (see the Discussion section). From this perspective, the scatter observed in Fig. 5a represents in our

opinion an important point for this manuscript rather than a limitation as it shows how relatively simple methods like manual probing may be exposed to large, unexpected errors even when performed by experienced personnel. From a broader perspective, Fig. 5a also shows that the conclusions of performance assessments of measurement techniques may be affected by the scale of the evaluation datasets; in other words, more spatially dense ground-truth datasets may show larger, local differences than coarser datasets as they sample a more representative spectrum of snow conditions and potential sources of uncertainties like ice layers (see Fig. 6). Note that UAS and MS show on the other hand consistent results and no relevant spatial patterns when compared to each other (see Fig. 3), which (1) supports the idea that non-invasive techniques may successfully complement manual measurements, and (2) shows that photogrammetry of snow surfaces and laser scanning are comparable survey techniques for the scopes of snow hydrology. We believe that all these points are relatively new for the audience of TC, but agree with Referees 1 and 2 that the main contribution of this manuscript should be better shaped and clarified. We are confident that the new information coming from another snow survey (see previous reply) and further elaborations of the 2016 survey will address the points raised by the Referee – thank you.

Comment: "Said that, the paper is quite well written and I have only a few minor comments: - In the pdf the title only show the acronym "UAS" when it should be use the full name of the technique. Similarly, it should not be used the acronym at the beginning of the abstract."

Reply: Fixed, thank you.

Comment: "I miss the paper (R. Marti et al.2016: Mapping snow depth from stereo satellite imagery, The Cryosphere) as they use also UAV data validated with manual measurements."

Reply: Thanks for the comment. The reference is added in the revised version of the

manuscript.

Comment: “Were the images processed in order to remove the distortion from the lens? Agisoft has a tool for this purpose”.

Reply: Thanks for the comment. The images were not processed using Agisoft Lens because we experienced some instability in camera calibration using this tool. Instead, we used the self-calibration tool embedded within Agisoft Photoscan to compute the lens distortion parameters during the bundle block adjustment. The task of camera calibration is fundamental in case of UAS flights because the camera suffers from impact on the ground during take-off and landings. For this reason, the parameters estimated during a standard calibration may not be representative of real flight camera conditions. We will clarify this issue in the revised version of the manuscript.

Comment: “In Figure 4a it seems that residuals never exceed 0.2 meters, however in Figure 4b, it seems that much larger biases exist. - As I mentioned before, Figure 5a shows very large differences and it is difficult to know the source of the errors (which one is closer to ground truth?)”

Reply: We thank you for this comment, which highlights a key passage of the manuscript that should be better clarified as follows. Figure 4a does not show the larger biases because these points are characterized by a very small relative frequency. This figure shows that the two distributions can be described using two Gaussian functions and this should be the main message of the plot. The summer dataset is less dispersed than the winter one because of the more difficult matching between images and possible uncertainties in laser beam response on snow. On the other hand, Figure 4b shows these larger biases, which are expected in shaded areas or depressions (see Figure 3). These errors are mostly evident in the C2 comparison, which means that they are generally the result of interpolation, i.e., DSM creation (see Section 3.2). Finally, Figure 5 reports differences between UAS and manual probing. As detailed in section 3.3, these large differences can be explained considering many different factors (presence

of vegetation, ice lenses etc.). Because data show a very good agreement between UAS and MS measurements and larger differences between these two datasets and manual probing, we can conclude that UAS (and consequently MS) measurements are closer to real ground conditions.

Comment: "The analyses is conducted with snow data from April, and Figure 1 shows a snowpack pretty metamorphosed where obtaining common points is much more feasible than under fresh snow conditions, Thus the obtained error is very likely to be lower than in most of the time along the snow seasons. This should be clearly stated in the discussion, as well as other sources of error derived from illumination, wind conditions, the effect of the shadows, etc."

Reply: Thanks for this comment. In the revised manuscript, we will clarify this issue in the discussion.

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