**Response to Reviewer #1 for the article, 'Assessing the application of a laser rangefinder for determining snow depth in inaccessible alpine terrain'.**

***P418, L20-21: This sentence would be much stronger with a reference to both the first and second parts.***

References have been added (P3, L2-3 in the revised manuscript).

***P419, L5: References would make this stronger***

The structure of this sentence was changed and references were inserted (P3, L11-13 in the revised manuscript)

***L5: These measurements are also useful in validating 3d snow redistribution models (e.g. SnowModel Liston and Elder, 2006).***

We have inserted a reference regarding 3D snow redistribution models (P3, L12).

***L6: It is worth listing one or two routine snow measurements (e.g. SNOTEL in the US) or intensive field campaigns (e.g. CLPX)***

References have been inserted for both Canadian (Derksen et al. 2002) and US routine snow measurements (Dressler et al. 2006 – Journal of Hydromet.) on P3, L14.

***P420, L9: Given that one of the major benefits to their scanning system over a full TLS system is the price, it would be appropriate to include approximate prices of TLS vs their laser rangefinder***

The laser rangefinder used in this study was purchased for $6000 CAN versus $100,000-$250,000 for a TLS system and this is now noted in the text (P4, L21).

***P421, L21: They state the maximum range of the XLRic is 1850m, but it is worth noting that the maximum range in daylight is 800m, though this could probably be improved by adding narrow bandpass filter.***

We have made clarifications to indicate under which conditions the maximum range applies and what typical daylight ranges may be expected (P5, L20-22).

***L24: Please provide more information about how the accuracy of 4.3m was calculated, what incidence angle is assumed? What rangefinder accuracy (assuming no angular error)? How does this affect the error in vertical snow depth?***

The stated accuracy of 4.3 m was the uncertainty in the position of the target using a bearing accuracy of +/- 0.5 degree, according to the manufacturer specifications (this is clarified on P5, L23-25). This is likely an over-simplification of the accuracy of each point measurement; therefore, more discussion addressing the different sources of uncertainty has been added (impact of incidence angles P8, L14-18; interpolation P8, L21-27). Information on the incidence angles has been added to Table 2 and a discussion of the influence of incidence angle has been added to the text.

***L25:What is the beam divergence angle? Reference Tbl 1.***

The beam divergence angle refers to the increase in the size of the beam with distance and this has been clarified in the text (P5, L26), and a reference to Table 1 has been provided (P5, L27).

***P422, L5: How is their system "more" suitable for "this application". Is cost the only benefit?***

Cost and the portability of the system are the primary benefits to the laser rangefinder (versus TLS). The text has been modified to include information on the weight of a laser rangefinder versus a TLS system (P6, L12).

***L7:How much higher resolution is the TLS data set vs their method?***

The TLS data set of Prokop (2008) results in data points with 3-6 cm horizontal spacing (P4, L6); whereas, the laser rangefinder data set results in data points with 1.5 – 4.1 m spacing (P8, L10).

***L11: They do not present the specs for a TLS system in Tbl 1. , thus they can not use it to back up the statement that TLS "models" have a smaller beam divergence.***

The reference to Table 1 was removed and beam divergence specification for TLS systems were inserted (P6, L6).

***L14-16: The make a leap from spatial to vertical resolution. Please clarify or reword.***

This section was deleted. The spatial resolution of TLS and the laser rangefinder are discussed on P4, L6 and P8, L10, respectively.

***L17: They state that an objective measure of satisfactory results is an uncertainty of 10-15%. They need to supply a reference that supports that this is a useful accuracy for hydrologic applications. I would argue that it is not satisfactory because 10-15% is nearly the inter-annual variability in many places, and climate trends (one of the applications they claim to help) are likely to be substantially smaller than 10-15% in the near future. Thus errors of 10-15% supplies very little useful information to water resource managers. Please at least provide the inter-annual variability at this site (from precipitation records if no better source is available). If 10-15% is for individual measurements, and the error in the calculated average snow depth is smaller, then please state this and discuss how you will evaluate this accuracy.***

We acknowledge that we had limited basis for establishing satisfactory results as 10-15%, and this has been removed. We also agree that 10-15% variability is in the range of climatic trends and acknowledge that the results of this study may not be of use to water resource managers in a climate change scenario. In light of this, we have revised the introduction and conclusion to remove reference to climate change applications– specifically, P418 L25 to P419 L3, and P428 L17-19 of the original manuscript have been removed. It remains that accurate measurement of snow depth in rugged alpine watersheds is a difficult and elusive task. The laser method gives a reasonably good and unbiased estimate of average snow depth over the scale of measured slopes, which is useful to hydrological process and modeling research, despite the uncertainty in point values. In this watershed, and others, there is no other readily available way of obtaining snow depth, and therefore we feel that the laser method provides a valuable tool for reducing the uncertainty in the amount of snow in the watershed by reducing the areas of no data. Additionally, the accuracy is on par, or greater than the predictive capacity achieved with advanced interpolation of intensive snow survey data (Balk and Elder, 2000; Anderton et al., 2004; Lopez-Moreno and Nogues-Bravo, 2006; Erxleben et al., 2002; Molotch et al., 2005).

***P423, L3-5: The geology of the watershed is largely irrelevant to the present study.***

We disagree that the geology is irrelevant to the study. The fractured carbonate geology results in frequent falling rocks, and infrequently, large catastrophic rockfall. This is a major factor in why these slopes are inaccessible for manual snow surveys. We have clarified the text to make this clear (P7, L4; P7, L27-29).

***L26: An estimate of the roughness of the different surfaces would be useful, talus slopes come in many forms.***

It has been noted that the talus consists of blocks with large relief due to the dominantly quartzite geology (P7, L21).

***L18-25: They state that the validation slope has a substantially different snow accumulation regime. They need to discuss how this may or may not affect their results. Are measurement errors likely to be any different on this slope vs the other two sites? Errors could stem from differences in talus slope roughness (one might expect the slopes under the cliffs to have larger talus blocks which would result in larger errors). Errors could also come from rock fall onto the talus slopes at the other two sites. Furthermore, errors could come from differences in the viewing angles and distances to the slopes. The average distance to their validation slope is substantially less than the average distance to the other two slopes, and there is no way of telling what the viewing/incidence angles (relative to the slope angle) are at the different slopes. While I suspect their results are still valid, these points need to be addressed.***

A section has been added to the methods (P7) discussing the differences between the validation slope and the upper and lower talus slopes. The difference in snow accumulation regime will have no impact on the study results. However, the surface roughness is an important consideration. The validation site was chosen specifically because it has very coarse, blocky debris (due to the presence of a bedrock failure scarp that is upslope) and therefore the 'snow-free' survey approximates similar conditions to the talus slopes. The reviewer is correct that the distance to the validation slope is less than the average distance to the other slopes, and this will result in a smaller beam 'footprint' on the slope. However, the incidence angles are in the same range as the lower slope, and less (resulting in poorer position accuracy) than the upper talus slopes. We have added the incidence angles in Table 2.

***P424, L14-15: The location of their laser system must be described and displayed on Fig. 1. In addition, Fig 1. Should show polygons for the regions mapped, not just stars for their midpoint locations, this is important for verification of the slope angles, min and max distances, and relevant incidence angles.***

The suggested changes have been incorporated into Figure 1.

***L21: Rather than a point density, some measure of the average spacing between points would be more useful (eg. at each point calculate the distance to the closest point in each cardinal quadrant, and take the mean? Or just take the square root of the inverse of the point density?), this would be a much more understandable number, e.g. 0.02 = a 7 m spacing between points! This seems rather large, and some justification for this point spacing is necessary, how does this compare to the spatial variability of snow depth?***

The data is now presented as average distance between points (P8, L10; Table 2), and this ranges from 1.5 – 4.1 m. The spatial variability of snow depth is expected to vary depending on the process scale of interest. In general, it is difficult to define the spatial variability of snow depth because correlation lengths for snow measurements are inherently dependent on the sample spacing (Bloschl, 1999). We acknowledge that the roughness of the surface may result in considerable variability in snow depth at a scale that is less than the measurement spacing. However, our results suggest that the mean snow depth is well captured, and therefore we feel the spacing is appropriate.

The measurement spacing at the validation slope was less (average 1.2-1.7 m) than at the talus slopes (1.5 – 4.1m). The analysis at the validation slope was repeated using 25% of the original points for both the snow-covered and snow-free survey. The mean snow depth obtained remained unchanged, which indicates that the method is surprisingly insensitive to the spacing between laser measurement points.

***L25: Some discussion of the pros and cons of a polynomial interpolator vs kriging would be useful here.***

Discussion of the rationale for the choice of a polynomial interpolator has been added (P8, L21-24). Additionally, a figure has been added (new Fig. 2) which illustrates the advantage of using an inexact interpolator.

***P425, L4-5: Please present the average variance of the 4 observations***

The average standard deviation of the 4 observations was 0.13 m, and this is now noted in the text (P9, L1).

***L9: How many data points were discarded? Was this in any way a function of slope angle or other term which could also be related to snow depth?***

Ten points were discarded and this has now been noted in the text (P9, L3). The points that were discarded had no relation to slope angle or snow depth, but were discarded because the differentially post-processed GPS positions had greater than 1m uncertainty (in X,Y position). The uncertainty in the position influences how the 'calculated' snow depth is compared to 'measured' (i.e. if there is a high X, Y uncertainty in the measured snow depth then it isn't possible to compare it to the equivalent laser-derived snow depth).

***L10: Is the "calculated" snow depth the same as the "modeled" snow depth and "laser" snow depth? Please use consistent terminology throughout the paper, figures and tables.***

The term 'laser snow depth' is now used throughout the figures, tables and text to indicate the snow depth that was determined with the laser snow method.

***P426, L1: How does an RMSE of 0.21 compare to the mean standard deviation of each of the 4 sets of observations?***

The average standard deviation of the 4 observations was 0.13 m, and this is now noted in the text. (P9, L1).

***L22-23: They state that remaining variations probably result from depressions in the surface topography. They should be able to test this easily with their "snow free" data set.***

This comparison was made, and confirmed the fact that the spatial variability in snow depth can be attributed to variations in surface topography.

***P427, L5-9: The cross-slope variation they describe as wind loading appears lobate in form and thus could be a result of a local avalanche. This stresses the importance of individual semi-random events, and is an important point to address. Snow depth on steep slopes can be highly variable in time as a result of these semi-random redistribution events, and not related to precipitation and thus the water budget. As a result it may be critical to evaluate the snow throughout the basin, not just individual talus slopes. Further discussion would greatly enhance the paper.***

We fully agree with this statement and have expanded the discussion to address these points (P11, L22-23). Work is in progress in the watershed to use several other methods (oblique photographs, satellite imagery and snowmelt modeling) to further quantify the variability that results from avalanche transport.

***L25-26: They suggest these regions are important hydrologically because of their persistence into late summer. This may be true, but at present they have not justified it. If they could calculate the total water volume stored in these types of snow deposits through out the basin (a simple extrapolation from their data would be adequate for the present paper), and show that this water volume is large relative to late summer stream flow they would have a reasonable argument. Otherwise it is possible that late summer streamflow is***

***dominantly a result of soil moisture drainage and these small areas of large accumulation are negligible. This is an important result either way, and would greatly increase the scientific contribution of the paper.***

Additional discussion has been added (P11, L11-19). Assuming the mean snow depth of 4.1 applies to all such similar regions of the watershed (8% of the watershed area), and assuming that a mean snow density of 450 $kgm^{-3}$ (as determined from a depth-density regression equation for measured data in the watershed), then the snow in slough and avalanche deposits comprises 16% of the annual streamflow, and 54% of the late summer streamflow (Aug 1 – Sept 30). Therefore, it is quite likely that these deposits do play an important role in prolonging the snowmelt season. Further work is in progress to characterize this contribution using a distributed snow melt model in conjunction with characterization of snow-covered-area using oblique photographs.

***L26: Also , Fig. 5c and 6b show that snow was still present in their "snow-free" survey, they should discuss how this influences their measurement of snow depth.***

The text has been revised to indicate that the snow-depth measurement in this case represents the seasonal change in snow depth, rather than total snow accumulation (P7, L25-26).

***Conclusions: if they believe that wind redistribution is important (e.g. section 3.2), then they should discuss this in their conclusions.***

This was omitted from the conclusions, as it is not considered to be an important component of this work.

***Figures:***

***Table 2. min and max distances would be useful, as would the average distance between points instead of point density.***

Min and max distances have been added to Table 2.

***Table 3. please be consistent in nomenclature***

Changes have been made to keep nomenclature consistent between figures, tables and text.

***Fig. 1. Please add the location(s) measurements were made from***

The measurement locations were added.

***Fig. 4. Error bars on measured snow depth would be useful. In addition, the "calculated" snow depth should be on the x-axis as it is being used to "predict" the measured/real snow depth. Ideally error bars should be added to the "calculated" snow depth too based on expected errors in view angle and x/y/z position and or the variance of the local measurements.***

Error bars for measured snow depth have been added and the axes have been switched. We agree that error bars for the 'calculated' snow depth would improve the plot. However, they have been omitted due to the difficulty of determining whether the various sources of uncertainty are additive or not.

***Fig 6d: is there a reason that this scatter plot is substantially cleaner looking that that of fig 6c? I would expect the talus to be a rougher surface than the snow. Is this a result of changes in measurement procedure? If so this should be noted.***

Both 6c and 6d are for snow surfaces. The cleaner look of 6d is a result of a greater number of points collected in April 2008 (748 points) versus April 2009 (442), rather than a change in measurement procedure.

***References:***


Anderton, S. P., White, S. M., and Alvera, B.: Evaluation of spatial variability in snow water equivalent in a high mountain catchment, Hydrological Processes, 18, 435-453, 2004.

Balk, B., and Elder, K.: Combining binary decision and geostatistical methods to estimate snow distribution in a mountain watershed, Water Resources Research, 36, 13-26, 2000.

Bloschl, G.: Scaling issues in snow hydrology, Hydrological Processes, 13, 2149-2175, 1999.

Erxleben, J., Elder, K., and Davis, R.: Comparison of spatial interpolation methods for estimating snow distribution in the colorado rocky mountains, Hydrological Processes, 16, 3627-3649, 2002.

Lopez-Moreno, J. I., and Nogues-Bravo, D.: Interpolating local snow depth data: An evaluation of methods, Hydrological Processes, 20, 2217-2232, doi:10.1002/hyp.6199, 2006.

Molotch, N. P., Colee, M. T., Bales, R. C., and Dozier, J.: Estimating the spatial distribution of snow water equivalent in an alpine basin using binary regression tree models: The impact of digital elevation data and independent variable selection, Hydrological Processes, 19, 1459-1479, doi: 10.1002/hyp.5586, 2005.