Hydrol. Earth Syst. Sci. Discuss., 7, C209–C214, 2010
www.hydrol-earth-syst-sci-discuss.net/7/C209/2010/

Hydrology and Earth System Sciences Discussions

Interactive comment on "Assessing the application of a laser rangefinder for determining snow depth in inaccessible alpine terrain" *by* J. L. Hood and M. Hayashi

Anonymous Referee #1

Received and published: 16 March 2010

The paper "Assessing the application of a laser rangefinder for determining snow depth in inaccessible alpine terrain" is an important contribution to the snow hydrology literature. Snow is extremely variable in space and time, and notoriously difficult to measure. This paper presents a clear and novel method for mapping snow depth in terrain that is particularly difficult to map. The approach and methods are applicable, but more details are needed before this can be published. Several important details are lacking in the experimental setup and validation procedures, and a number of references should be added to the introduction. Below is a detailed criticism of the paper, though many of the points are minor.

Introduction:

p418,l20-21 This sentence would be much stronger with a reference for both the first and second parts

p419,l5 References would make this stronger.

l5 These measurements are also useful in validating 3d snow redistribution models (e.g. SnowModel Liston and Elder, 2006)

l6 It is worth listing one or two routine snow measurements (e.g. SNOTEL in the US) or intensive field campaigns (e.g. CLPX)

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 (US$200-500k?) vs their laser rangefinder (US$100? 1k? 10k?)

Laser specs:

p421,l21 They state that 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.

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?

l25 What is the beam divergence angle? reference Tbl 1.

p422,l5 How is their system "more" suitable for "this application" Is cost the only benefit?

l7 How much higher resolution is the TLS data set vs their method?

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" (should be lasers? or systems") have a smaller beam divergence.

l14-16 They make a leap from spatial to vertical resolution. "Although high spatial resolution is generally desirable... the present study... does not necessitate a vertical resolution of centimeters." Please clarify or reword.

HESSD

7, C209–C214, 2010

MAJOR CONCERN:

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 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.

Study site:

p423,l3-5 The geology of the water shed is largely irrelevant to the present study.

l26 An estimate of the roughness of the different surfaces (1m? 10cm, 10m?) would be useful, talus slopes come in many forms.

MAJOR CONCERN:

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 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 their is no way of telling what the

HESSD

7, C209–C214, 2010

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.

Data Collection:

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.

l21 rather than a point density, some measure of the average spacing between points would be more useful (e.g. 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 7m 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?

l25 Some discussion of the pros and cons of a polynomial interpolator vs krigging vs ? would be useful here.

Validation:

p425,l4-5 Please present the average variance of the 4 observations.

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?

l10 is the "calculated" snow depth the same as the "modeled" snow depth and the "laser" snow depth? Please use consistent terminology throughout the paper, figures, and tables.

Results:

p426,l1 How does an RMSE of 0.21 compare to the mean standard deviation of each

HESSD

7, C209–C214, 2010

of the sets of 4 observations?

l22-23 They state that remaining variations are probably result from depressions in the surface topography. They should be able to test this easily with their "snow free" data set.

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.

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 neglibible. This is an important result either way, and would greatly increase the scientific contribution of the paper.

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.

Conclusions:

If they believe that wind redistribution is important (e.g. section 3.2), then they should include a discussion of that in the conclusions.

Figures:

HESSD

7, C209–C214, 2010

tbl 2: min and max distances would be useful, as would the average distance between points instead of the "point density"
tbl 3: please be consistent in nomeclature re: "measured" vs "manual", "calculated" vs "modeled" vs "laser"
fig. 1: please add the location(s) measurements were made from.
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
fig 6d: is there a reason 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.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 7, 417, 2010.