The Cryosphere Discuss., 9, C2314–C2323, 2015
www.the-cryosphere-discuss.net/9/C2314/2015/

The Cryosphere Discussions

Interactive comment on "Orographic and vegetation effects on snow accumulation in the southern Sierra Nevada: a statistical summary from LiDAR data" *by* Z. Zheng et al.

Z. Zheng et al.

zeshi.z@berkeley.edu

Received and published: 20 November 2015

Dear Dr. Harpold,

Thank you for reviewing the manuscript "Orographic and vegetation effects on snow accumulation in the southern Sierra Nevada: a statistical summary from LiDAR Data" by Zheng et al. Your comments were highly insightful and enabled us to greatly improve the quality of the manuscript. In the following pages are our responses to your comments.

Yours sincerely,

Zeshi Zheng

Responses to the major comments

1. Improvements in writing and framing: The paper could be substantially improved by better highlighting and framing the novelty of this work. The abstract does not highlight novelty; tell the reader more simply what the 2-3 most important points of the work are. The introduction seems to wander and needs to have several sentences pointing back to why this work is needed. I suggest adding a guiding statement to the paragraph beginning on line 24. Something like "The consistency of snowpack distributions across vegetation and topographic gradients is difficult to measure and predict". Just a thought, but something is needed to help lead the reader more clearly through the introduction.

The results are lacking in several ways. First organizationally they are hard to follow. Why not organize them around the same ideas in the same order as the discussion? Throughout the results more specifics need to be included. What are the statistics describing the linear correlation (4385, line 11)? How much less in cm or % (line 13, 4386)? How much does it increase and what are the fluctuations (4386, line 26)? These are just a few examples. Imagine the figures and tables are not easily seen (which they are not): tell the reader what the results are quantitatively.

Response: Thanks for your suggestions and we improved highlighting and framing of the work. Therefore, in consideration of comments from both reviewers, we restructured the findings of the work into three main take-home points and are highlighted them through the manuscript. Also, we changed the structure of the results and now is consistent with the discussion and more specifics are included in the results. Changes in the manuscript:

(1) We now structure our findings to highlight three main take-home points:

a. The fraction of pixels for which Lidar measured snow depth in dense forest depends on the pixel size, or averaging area, used when processing the raw Lidar point cloud.

b. Other than elevation, aspect and slope also control the distribution of snow depths.

c. In mixed-conifer forest, for area under the canopy, the effect from canopy overwhelms effects from slope and aspect, in most sites, and the interactions between these features could be observed from the data.

(2) The explanations in the results were rewritten to include more specifics and the structure now is in the same order as discussion. See Section 3, page 15-16.

2. Unclear methods for residual: Was the elevation-dependent snow depths estimated the same equation at each site (i.e. the average of all watersheds) or was a different relationship developed for each watershed (seems more appropriate)? If a single relationship was used, how was the decline in Wolverton snow depths are higher elevations accounted for (i.e. did it reduce the slope of the line and if so, can you fairly assume this is not a function of moisture depletion and changing orographic effects?). I am skeptical that a single linear relationship is appropriate given the steepness and orientation to typical storm tracks is not the same in all watersheds. Please justify the use of a single orographic relationship. I am unclear how the snow depth residual versus slope can nearly always be negative (Figure 6a), when it is a mixture of positive and negative as a function of aspect and penetration factor. I would double-check your data analysis used in 6a.

Response: The elevation-dependent snow depths were not estimated with the same equation at each site we used four individual models. Furthermore we compared the difference between them using four models versus one, and it does not make much difference in terms of estimation bias (See attached Figure #1). However, in the text we now present the overall linear model for all sites. For the decline in Wolverton, we plotted the raw pixel snow depth versus elevation and it seems that the variability starts to increase drastically after that elevation threshold, which is near tree line, this is likely due to wind redistribution or exhaustion of perceptible water both of which are beyond

the scope of this paper. Thus we filtered those data from our revised analysis. Figure 6a is correct because the mean of residuals is below zero does not mean there is no residual that is greater than zero. And since the residuals are averaged into different variable's bins, it is possible that the mixture is not zero. Under the new framework of the manuscript, the figure is just used for checking existence of the effect from the variable, not quantifying the effect.

Changes in the manuscript:

(1) Text on how the residuals are calculated was added in Section 2.5, starting from page 13 line 253.

(2) Justification of using either one model versus four models was added in Section 4.2, starting from page 19 line 402.

3. Relative importance of predictive variables not discussed: One of the major shortcomings of the manuscript is that it does not talk about the relative importance of the predictive variables in controlling the distribution of snow depth: slope, aspect, and vegetation canopy. I think this limits framing and describing the novelty of the work. While I realize that a full statistical model with interactions may be beyond the scope of the current work, some ability to quantify the relative importance of these variables would strengthen the paper. Beyond this, a more informative discussion of the interaction of the predictive variables is needed. This is begun in the last paragraph on line 4389, but not clearly framed around all the findings.

Response: Thanks for the suggestion and we agree that the relative importance of predictive importance should be included and discussed and in the revised manuscript. We used random forest with regression trees to check the relative importance of the predictive variables and the interactions between vegetation and other predictive variables are now discussed as well under the canopy effect, we show the interactions by filtering the northness into flatter terrain showing the snow depth difference between open area and under the canopy is more stable above the rain snow transition elevation.

Changes in the manuscript:

(1) Results and discussions about relative importance of variables were added in Section 3 (page 15 line 317) and Section 4.2 (page 19 line 405)

(2) Interactions between physiographical variables were discussed in Section 4.3 (page 21 line 441)

4. Explanation for differences between open and under canopy locations is not well supported: The authors provide one plausible hypothesis for the increasing differences between open and under canopy snow depths with increasing elevation. Namely, that the same density of vegetation intercepts more snow as precipitation increases. I find this interpretation difficult to understand and not clearly supported by evidence. I suggest making this point clearer and looking for more supporting evidence. When one looks at the figure supporting this assertion (presumably Figure 5a), you really only see a clear pattern in in one watershed (Providence) and mixed or no relationship in the others. Why is this? The authors suggest that changes slope and aspect alters the relationship in middle elevation bands for Wolverton, is this consistent with the proposed hypothesis?

I can think of an alternative hypotheses that the authors should either consider or refute: vegetation structure and forest canopy organization change with elevation in ways that affect interception, i.e. more sparse canopy coverage promotes accumulation in open areas or denser vegetation (where present) at higher elevations (in a related note, I ask for penetration factor plotted against elevation).

To me this is the most interesting result of the paper. I suggest bringing it more forward and adding more discussion (alternative hypotheses) to the discussion (and abstract).

Response: Upon consideration of this and reanalysis the snow depth differences between open and under canopy locations increase primarily with elevation in the rain-

snow transition zone, this is because the snow depth increase versus elevation is nonlinear in this region and the gradient of that nonlinear curve is larger in the open area than under the canopy. Above this elevation the difference becomes more stabilized without the influence of other predictive variables. (See attached figure #2). And the figure showing penetration fraction against elevation is shown. Also, the canopy-coverage effect on accumulation in open areas and under-canopy is quantified in the multivariate regression models of both cases. And the relative importance of variables shows that vegetation structure does exert different effects on snow in open areas and under-canopy.

Changes in the manuscript:

(1) The result is changed and we discussed about the causes of our new findings in the discussion.

Responses to detailed comments

1. See numerous comments in attached pdf

Response: Changes made to address the comments

2. What is the calculation of the standard error

3. Is snow depth normally distributed across these ranges of elevations (I doubt it)? Why not use percentiles to describe its variation at a given elevation (4384, line 21-22).

Response: Responding to both questions, we used standard deviation as the standard error. And the snow depth is normally distributed across most ranges of elevations, except for the highest elevation areas at Wolverton, we have changed the standard error to percentiles.

4. Why do you do Gaussian smoothing on the residual with a 5 m radius? Justify this better or explain sensitivity to other smoothing lengths.

Response: We did a sensitivity test to other smoothing lengths and it turns out the smoothing result is not quite sensitive to the radius when the radius is larger than 1.5 m, and the standard deviation begins to stabilize after 5-m radius, so we chose 5-m (See attached Figure #3). And have added this as a panel in Figure 3.

5. Consider using lidar (lower case) instead of LiDAR: “Let’s Agree on the Casing of Lidar”. http://www.lidarnews.com/content/view/10908/198/

Response: This link could not be opened when I checked it out. I Googled about Lidar and the title of Wikipedia is written as “Lidar”. So I agreed and I changed all the acronyms in the manuscript.

Please also note the supplement to this comment:
http://www.the-cryosphere-discuss.net/9/C2314/2015/tcd-9-C2314-2015-supplement.zip

Interactive comment on The Cryosphere Discuss., 9, 4377, 2015.

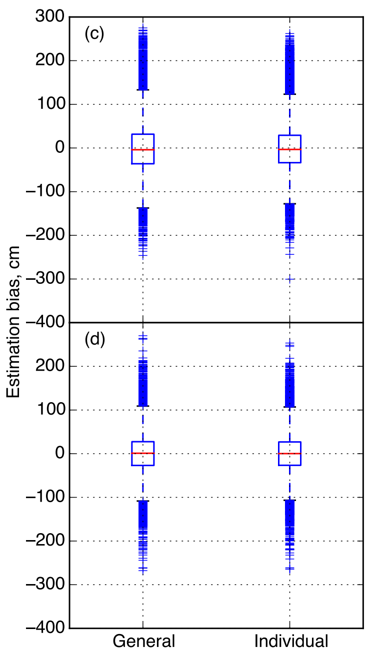
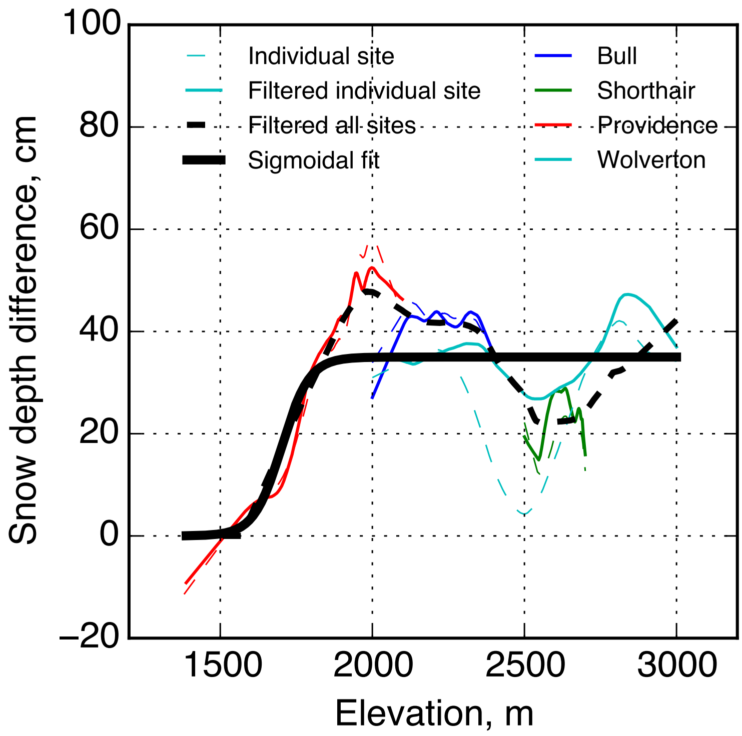
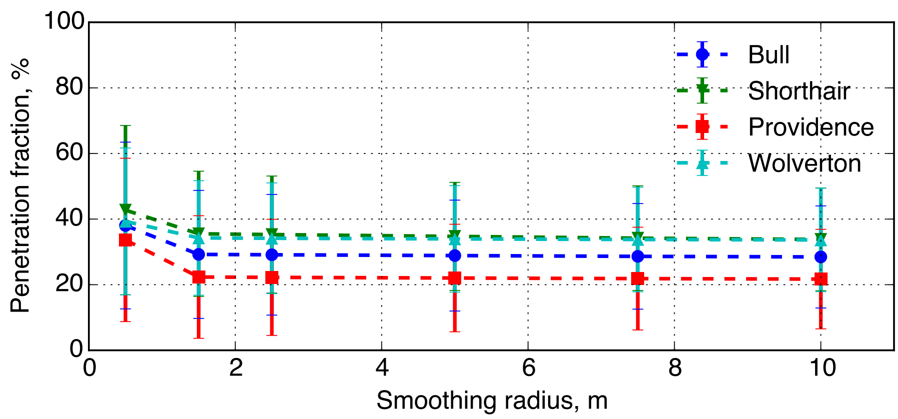


Fig. 1.



Individual site
Filtered individual site
Filtered all sites
Sigmoidal fit
Bull
Shorthair
Providence
Wolverton
Snow depth difference, cm
Elevation, m
100
80
60
40
20
0
−20
1500
2000
2500
3000

Fig. 2.



Bull
Shorthair
Providence
Wolverton
Penetration fraction, %
Smoothing radius, m
100
80
60
40
20
0
0
2
4
6
8
10

Fig. 3.