The Cryosphere Discuss., 9, C1684–C1687, 2015
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Comment

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.

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Received and published: 21 September 2015

I reviewed the paper "Orographic and vegetation effects on snow accumulation in the southern Sierra Nevada: a statistical summary from LiDAR Data" by Zheng et al. I found the paper to add important context to the distribution of snow in mixed conifer forests. In particular, the finding vegetation canopy effects on snow depth were a function of elevation is a very interesting insight. However, I found several major issues with the paper that I outline in more detail below: 1) some of the writing is difficult to follow and does not highlight the novelty of the work, 2) the method used to calculate the residual is unclear, as are some of the associated results, and 3) the relative impor-

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tance of different topographic and vegetation variables is not evaluated or discussed, 4) alternative hypothesis (or more support) should be presented for the processes driving differences in snow accumulation between open and under canopy. Additional smaller comments are given below and as sticky notes in the attached pdf file.

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.

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

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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.

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.

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,

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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).

Minor comments: 1) See numerous comments in attached pdf. 2) What is the calculation of the standard error? This is never explained. 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). 4) Why do you do Gaussian smoothing on the residual with a 5 m radius? Justify this better and/or explain sensitivity to other smoothing lengths. 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/

Please also note the supplement to this comment:
http://www.the-cryosphere-discuss.net/9/C1684/2015/tcd-9-C1684-2015-supplement.pdf

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

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