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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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Review of "Orographic and vegetation effects. . .." By Zheng, Kirchner, and Bales

In this paper, the authors use airborne LiDAR data to assess primarily how the snow pack depth increases with elevation on the west side of the Sierras, and secondarily, how other factors (canopy cover, slope, aspect) affect the distribution of snow. The main conclusion (increasing snow depth with elevation to 3300 m) is not novel, nor is the basic technique of using airborne LiDAR to measure snow depth, but I suspect that what the authors have done is reach their conclusions regarding snow depth gradients using better and more comprehensive data than heretofore has been available. Un-

fortunately, the paper as written does not make clear what is novel and what is not in the study, and the paper suffers from too much detail in discussing secondary effects (slope, aspect, canopy), obscuring the main conclusions about elevation gradients. There is also a somewhat offhand attitude in the discussion of the choice of sites and why those might constitute an "upslope transect" along the western side of the Sierras. While the choice of sites may (or may not) have been chosen for the purposes of the present study, the paper would be improved if the authors were up-front in examining the choice of study sites, comparing and explaining why these sites can reasonably be used together. I think this approach works because storms come from the west, while the range runs north-south, thus the orographic lifting effect can be thought of as a two-dimensional problem. . . ..but the authors need to explain and document this if it is true for the readers.

One other area of confusion needs to be improved in the paper: the authors introduce 4 linear models of increasing depth with elevation (or at least I think it is 4 models (Table 3)). What is the point of having 4 models? For large scale studies, wouldn't a single, averaged linear model be of more use? And if four models is what is needed, what is the use of these models? This part of the paper would be improved if the linear model was actually presented as a formula, and more care was taken in explaining how the residuals (Figures 6 and 7) were computed.

Lastly, unless I missed it, there is no discussion of the accuracy of the snow depth measurements. . ..no check of the LiDAR results compared to ground measurements. I suspect the accuracy is order $\pm$10 cm, but the authors need to address this question.

My recommendation is that the paper could be acceptable with major revision. The authors need to rewrite the entire introduction, focusing on what until now was know about the snow depth variation on the west side of the Sierras, and explaining how their new study using LiDAR is able to advance this knowledge by improving/refining that understanding (deleting a lot of boiler-plate material currently in the introduction). That will solve the problem of what is new and novel in the work. As for the rest of the

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paper, the authors should strive to simplify and clarify the results as much as possible, while addressing the omissions I detail above. I found the flow from orographic control to vegetation controls to slope/aspect to be confusing, and the detailed discussions of the effect of vegetation too involved to follow well. Lastly, the authors should expand the Study Area section, addressing why sites not along a single transect can be used in the fashion they are in the paper. I think these sort of changes, along with some effort to condense and clarify, will result in an paper acceptable for publication.

Detailed comments and suggestions follow.

Abstract: I found this longer than needed and needlessly confusing. It seems like the first 7 lines were fine, then it bogged down in details that are not first order. For example, that canopy cover decreases from 80% to 0% with elevation is hardly a new result. Does it need to be in the Abstract? The last 7 lines make little sense until one reads the paper. I suggest deleting this or making clearer the meaning of the data.

Introduction: Get right to it: this paper is not about ALL orographic systems. . ..it is about the Sierras. Plunge in and talk about the current state of knowledge for the west side of the Sierras, and tell us what has been lacking in those data and how this study will fill that gap. On pages 4379 and 4380, you name the studies that have been done, but not what was found and why that information might be deficient. Tell us what the current numbers are for the snow gradients and why these numbers might be in error, then why your LiDAR data can help fix the problem.

Page 4381, end of Introduction: It seems to me that the three state goals of the paper could be restructured a little differently and perhaps better. The prime goal could be the orographic gradient. In order to get that gradient, you have to deal with the other influences (slope, aspect, canopy), so you do. Also, there is a lot of discussion in the paper of canopy gaps vs. under-canopy snow. You should explain why knowing this is important (for example, is it to produce meaningful areal averages?).

Section 2.1: Study Area: (see main comments). It is important that here you address

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why these areas were used, and why they can be used in concert. Looking at the map, they define a line parallel (rather than perpendicular to) the Sierras, which makes one a little suspicious. I was also struck by the differences in the areas. Two are nearly flat; Providence is almost below the rain line, and Wolverton has a huge elevation range. Without it, I suspect it would have been hard to reach the conclusions currently in the paper. You need to explain why you have confidence in using these sites together. Also, add to Table 1 the mean elevation and elevation range for each site.

Section 2.2: Data Collection. It states that met data was used to determine if it snowed during the 4 days of data collection. Did it?

Section 2.3: Data Processing: Define all acronyms. Page 4383. I ended up drawing a little sketch to clarify the various surfaces. Maybe it is worth adding such a figure. Also, a little more descriptive names might help. For example, why a "Surface Model". Why not a "Canopy Top Model" and a "Snow Surface Model? Finally, some discussion of snow depth accuracy is needed. See the new paper in The Cryosphere by M. Nolan, C. Larsen and M. Sturm for a detailed discussion of this topic.

Section 2.4: Penetration Fraction: Perhaps I failed to understand this completely, particularly the section on under-canopy vegetation. It seems like you are deciding that there is only one canopy (tree tops?) and if the laser gets below that canopy, you discount any shrub-like vegetation? Also, your test of the fraction seems incestuous. Did you test it against independent data?

Page 4384, Line 17: This statement (". . .elevation was selected as the primary topographic attribute. . . " confirms what is effectively true for the whole paper and is why I suggest revising the paper so that primary goal is clear, and treating canopy, slope and aspect as variables that need to be dealt with in order to clarify the main elevation control.

Page 4386, Line 8: Is the decrease in snow depth above 3300 m real?

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Discussion, Page 4387, line 7: This “linear model” seems important, but nothing is said about its use. I assume it is used for water balance studies and the like, so it is important to have it as accurate as possible. You should explain why this model is important, and if developing a better model was a goal of the work. Also, why have 4 models? Why not a single, average model? Be sure to include the model as a formula so it is clear how it was done.

Discussion of vegetation effects, Page 4388: First, there are many types of vegetation. I think this section really refers to trees and tree canopies. Best to use more precise nomenclature here. Second, it felt like you were trying to glean too much from the data. There is some interaction between canopy density and snow depth, so that the decreasing canopy density with height is convolved with the increasing depth with elevation. I wonder if it is really necessary (and supported by the data) to dissect this combined effect in too much detail? I also wondered about rain-on-snow, which is not discussed much. It must also affect the interception by the canopy.

Table 1: Don’t list LiDAR that wasn’t used. Add mean elevation and elevation range.

Table 3: Put the gradients first, then the r2’s.

Figure 1: Nice figure.

Figure XX: It seems like a sample of a snow depth for at least one of the sites should be included so the reader can see what the end product is like.

Figure 2: Nice figure. Change the x-axis labels to “per m2”.

Figure 3: English in caption is bad. This would be a far more useful figure if there was a 4th panel that had the canopy density based on independent data against which the penetration factor could be compared.

Figure 4a: This is the main finding of the whole paper and is very interesting. I would be tempted to separate it from 4b, c, and d and focus the readers attention on it (make it as big as possible. In some ways the beautiful alignment of the 4 sites might just be

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the best argument for why you can use these sites in concert. Please clarify whether the results in Fig. 4 are open areas only.

Figure 5: Figure 5c the y-axis needs units (%?). There is a lot going on in this composite figure.

Figures 6 and 7: it is unclear whether the panels with standard error are needed. More useful would be to label the sections of the residual panels in a way that helped explained the effect being illustrated. For example, in Figure 6a, above 32° slopes, perhaps wolverton is showing the North Slope effect? Also. . .perhaps my ignorance, but shouldn't the residuals balance around the zero line with as much weight above as below the zero line?

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