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Hydrology and Earth System Sciences
Discussions

Interactive comment on "LiDAR measurement of seasonal snow accumulation along an elevation gradient in the southern Sierra Nevada, California" *by* P. B. Kirchner et al.

Anonymous Referee #2

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The authors combine LIDAR measurements, bright band radar, snow pillow and ultrasonic snow-depth sensors, local meteorological observations of wind speed wind direction and precipitation, and gridded precipitation estimates to explain variation in snow water equivalent (SWE) along an elevation profile in the Sierra Nevada in California. The dataset that the authors use is impressive and the combination makes this study unique and suitable for publication in HESS after addressing the suggested revisions below.

The study shows that there is an increase in SWE up to 3300m and then a sharp decrease at higher elevations. This pattern is mainly explained by a positive precipitation

gradient up to 3300m, after which all water seems to have precipitated. This is the main conclusion, but the discussion on the elevation of maximum precipitation is very limited and I suggest to extend this further and included a process based discussion on this. Although it is quite an old publication, see for example:

Alpert, P. (1986), Mesoscale Indexing of the Distribution of Orographic Precipitation over High Mountains, J. Clim. Appl. Meteorol., 25, 532–545.

The authors address many topics that influence SWE distribution in mountainous areas, e.g. precipitation gradients, wind redistribution, aspect, solar radiation. Although admittedly very complex, the discussion related to each of those topics could be more to the point. It is too blended now, which complicates the interpretation. This is a general weak point in the writing style of the manuscript. It needs more focus and clearer formulations.

The method section is unclear, in particular the part on LIDAR data processing. I suggest to add a flow chart showing all steps explicitly.

Figures are of high quality!

Specific comments

Page 5331, line 11-13: questions (i) and (ii) are basically the same

Page 5332, line 19: snow covered conditions instead of snow on and snow free conditions instead of snow off

P5332: A vertical accuracy of 0.75 m is mentioned here. This seems quite large in relation to the snow depths and the authors do not really get into vertical accuracies later on. Is this purely an instrumental accuracy? What about the accuracy of the gridded DSMs and DEMs?

Page 5333, line 3: That is a very tall tree!

Page 5333, line 5: It would be good to show the semi-variogram. Why a linear semi-

variogram?

Page 5334, line 12: In many occasions throughout the manuscript the authors use 1-m elevation or 1-m snow depth, while they mean a 1 m2 resolution elevation or snow depth product. This should be systematically checked and corrected.

Page 5334: The whole part on aspect intensity is not clear to me. In particular the last sentence.

Page 5335 – 5337: The authors mention ultrasonic gauges and snow pillows, but it is unclear when what is used exactly.

Page 5337: Although references are given a better description on how freezing levels can be derived from bright band radar would be appreciated.

Page 5344: I suggest to dedicate a part of the discussion to interaction between topography and precipitation, elevation of peak precipitations, type of precipitation and its relation with the findings.

Page 5358: Figure 2A: I do not see the bright band freezing level?

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 11, 5327, 2014.