

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.

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Dear Editors and Reviewers,

We wish to thank the two reviewers for their insightful and helpful comments on our manuscript “LiDAR measurement of seasonal snow accumulation along an elevation gradient in the southern Sierra Nevada, California”. The Reviewers raised a number of points that have enabled us to improve upon the original submission. I believe we have made the requested changes and corrections, or addressed any questions that were raised. In particular, we have reorganized the discussion section and added

C3625

text as suggested and clarified the methods describing bright band radar. In addition, we identified some minor typographic errors and edited two sentences that referred to sensor configurations that were not used in this study, on pages 5335 and 5336. Our responses to Reviewer two’s comments are below and the reorganized discussion section has been uploaded as a supplement . With the proposed revisions we believe our manuscript is ready for publication in the special issue “Precipitation: measurement and space time variability”.

Sincerely, Peter Kirchner, on behalf of all authors

Response to: Anonymous Referee #2

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.

-Text has been added to the discussion section and the reference cited.

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

C3626

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 discussion section has been reorganized and clarified as suggested.

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

-We feel that the text, and references on the methods, for processing the LiDAR data are sufficiently clear as presented.

Figures are of high quality! Specific comments Page 5331, line 11-13: questions (i) and (ii) are basically the same

-The text has been clarified to refer to variability along the transect and at each elevation studied.

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

-Thank-you, this has been changed

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?

-Thank-you for catching this typo now corrected in text, 0.75 is the horizontal accuracy and 0.30 is the nominal height accuracy. However, higher accuracy for these flights was achieved, as described on the next page.

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

-The tallest tree in the domain, a Giant Sequoia, has been measured from the ground

C3627

at 87 m, given the low probability of returns from the highest part of the canopy and the uncertainty of both measurements this seems very accurate.

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

-A linear variogram is a standard method used by the National Center for Airborne Laser Mapping on high point-return-density 1 m DEM's this method is commonly used and sufficiently described here and in the supporting citations.

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.

-Thank-you this has been corrected

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

-We have rewritten the section for clarity.

Page 5335 – 5337: The authors mention ultrasonic gauges and snow pillows, but it is unclear when what is used exactly. -Thank-you this has been clarified in sections 2.4 and 3.4

Page 5337: Although references are given a better description on how freezing levels can be derived from bright band radar would be appreciated. -The text has been rewritten for clarity. 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. -This has been added to the discussion section as suggested.

Page 5358: Figure 2A: I do not see the bright band freezing level? -Statement refers to comparing Figures 2A and 5; the text has been clarified.

C3628

Please also note the supplement to this comment:
<http://www.hydrol-earth-syst-sci-discuss.net/11/C3625/2014/hessd-11-C3625-2014-supplement.pdf>

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