

Interactive
Comment

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.

P. B. Kirchner et al.

peter.b.kirchner@jpl.nasa.gov

Received and published: 8 September 2014

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

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



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 Dr. Blöschl 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: G. Blöschl (Referee) bloeschl@hydro.tuwien.ac.at

This is an interesting paper that explores the spatial distribution of snow water equivalent (SWE) as a function of topographic controls. I enjoyed reading the paper. The amount of detail provided by the LiDAR data is impressive. I have only a few comments to strengthen the presentation. It was not clear to me how snow interception in the canopy affects the snow depth measurements and the comparisons with cumulative precipitation. I understand that the comparison are focusing on the clearings and open land, yet one would expect C1519 some effect due to wind redistribution.

-We chose to focus this paper on broader scale processes such as the rain snow transition and orographic influences separate from forest canopy effects because there are major differences in the accumulation and ablation processes under canopy and technical issues with the analysis of under canopy LiDAR snow depth where there are fewer returns, of higher uncertainty. Addressing these points is beyond the scope of a single manuscript and the focus of a manuscript in preparation. We did find effects of wind redistribution in open areas but we concluded, locations near canopy-covered areas did not have sufficient wind speed for transporting snow after it was on the ground. These points are discussed and illustrated on 5335, 5345 section 4.2 and Fig3, and text added to 5333:23-24.

Regarding the assumption of uniform snow density throughout the domain, I am not

Interactive
Comment

sure whether it is appropriate (p. 5337). The snow pillow data used in the paper showed very little variation and no significant dependence on elevation (p. 5342), yet there are numerous studies in the literature that did find significant increases in density at peak accumulation with elevation due to stronger compaction effects of deeper snow packs at higher elevation. On p. 5344 the authors note that snow depths from the snow-pillow sites failed to capture the elevation patterns apparent in the LiDAR data. Why would you then expect that they capture the snow density patterns? An increase in snow density may increase the elevation gradients of SWE relative to the results of the paper. There is perhaps no need to change the analysis, but I suggest the authors discuss this point in their revised paper.

-Thank-you, these are important points that you will find discussed in an additional section 4.5 titled “snow density”.

Precipitation is a flux, so has units of velocity while SWE is a state variable and has units of length, yet the two variables are directly compared at many instances in the paper. I realize that precipitation has been accumulated over time periods but then it should be termed cumulative precipitation and the time periods should be given. For example, line 19 should read 6cm/time unit?, and there are many more instances of this throughout the paper. p. 5337 has “we then calculated the total seasonal precipitation” what period?

-The acquisition date and accumulation period are now defined on line 19 and cumulative precipitation is explicitly stated with a time and units elsewhere in the text.

The figure captions were not always clear to me. First, the dates or periods of the data should be given for Figs. 2, 3, 4, 5, 7, 8, 9, particularly where precipitation is compared with SWE. “Acquisition date” and “accumulation period” is not very informative, give the dates instead in all the figures. Second, the acronyms used in all figures should be explained in the captions. For example, in Fig.9 it is not obvious to which line “SWE reconstructed from daily snowmelt estimates” refers.

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



-The figure captions have been revised as suggested.

Typo: p.5337 planer – planar?

-Corrected

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

HESSD

11, C3621–C3624, 2014

Interactive
Comment

[Full Screen / Esc](#)

[Printer-friendly Version](#)

[Interactive Discussion](#)

[Discussion Paper](#)

