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Interactive comment on "Tree canopy and snow depth relationships at fine scales with terrestrial laser scanning" *by* Ahmad Hojatimalekshah et al.

Ahmad Hojatimalekshah et al.

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Thank you for all the helpful comments! Please see our responses (in italics) with intended revisions if this manuscript moves to the next stage.

RC2: General comments

In this work, the authors focus on quantifying the roles of tree structure (and wind and topographic characteristics such as slope and aspect) in controlling snow depth variations using terrestrial laser scanning (TLS). The authors found that vegetation structural metrics (foliage height diversity) and wind are highly influential on spatial variability of snow depth. They also highlighted that windward slopes have greater impact on snow accumulation than vegetation features.

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Overall, I enjoyed reading this manuscript. The authors identified interesting research questions and attempted to leverage the strength of the TLS data to fill in the scientific gaps. However, the current version of this work needs to be improved upon before publication in The Cryosphere is warranted. Major concerns are given below. I would recommend that this paper be returned for major revisions and specifically request inclusions of additional analysis with appropriate interpretations and reorganizing the structures of the manuscript for the Cryosphere community.

Major comments:

1. A major concern is that the methodology used to derive the conclusion in this study is entirely based on correlation coefficient in a linear regression without investigating inter-dependency among the physical variables. I do not think if the linear correlation based approach is enough to identify and to conclude the relationship between snow depth and the tree variables. For example, the snow depth with the distance from the canopy edge is not linear (see Figure 2 in Hardy Albert, 1995). A potential approach I would suggest can be regression tree or multivariable analysis (e.g. multiple linear/logistic regression analysis with the standardized coefficient) to quantify relative contribution of the vegetation metrics along with wind, topographic features (Molotch et al., 2005; Schneider et al., 2020). Also, it would be beneficial to provide variation in snow depth, key vegetation characteristics, and elevation, etc along representative transects for the sites. Please refer to Sturm Fig.2 in Sturm et al. (2001).

Thank you, we appreciate the comments and references. We will investigate the multicollinearity of the variables, and the use of a multiple linear regression, and make subsequent changes in the results and interpretations. While our analyses were performed at individual trees, we can investigate if the data will support representative transects.

Âů Molotch, N. P., Colee, M. T., Bales, R. C. Dozier, J. 2005. Estimating the spatial distribution of snow water equivalent in an alpine basin using binary regression tree

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models: the impact of digital elevation data and independent variable selection. Hydrological Processes 19 (7), 1459–1479. doi:10. 1002/hyp.5586

Âů Schneider, D., Molotch, N. P., Deems, J. S., Painter, T. H. (2020). Analysis of topographic controls on depletion curves derived from airborne lidar snow depth data.Hydrology Research.

Âů Hardy, J. P., Albert, M. R. (1995). SnowâËŸARËĞ induced thermal variations around a single conifer tree. Hydrological processes, 9(8), 923-933.

Âů Sturm, M., Holmgren, J., McFadden, J. P., Liston, G. E., Chapin III, F. S., Racine, C. H. (2001). Snow–shrub interactions in Arctic tundra: a hypothesis with climatic implications. Journal of Climate, 14(3), 336-344.

2. Regarding the comment above, another concern is the interpretation of the correlation coefficient values from the linear regressions in the result sections. For example, the authors state that “slope explained 44

While this comment was cut off, we think the reviewer is concerned about ‘slope explained 44% of the variance’ and how this is interpreted. This will be clarified in the process of responding to the above comment #1.

3. I think the TLS data’s reliability should be verified. How accuracy is TLS-based snow depth, especially under the tree canopy? Many previous studies found that there were issues in ALS, TLS, structure-from-motion photogrammetry (SfM) with observation gaps in forested regions. The return density under or near canopy can be extremely low that may not be adequate to observe spatial variations of snow depth. In Figure B1, the snow depth maps the authors provided seem to be very limited to areas near forest and under the trees. Thus, I would recommend that the author quantify the accuracy of the snow depth measurements especially under/near canopy. Have you seen comparison results with independent snow depth measurements? I know the validation work is out of scope in this study, but it would be helpful for readers to be

able to have a sense of how accuracy the TSL technique is, particularly in these sites. I believe there are various available ground-based (or other techniques) snow depth measurements because this work was part of the NASA-led SnowEx 2017 campaign. If they are not available, the authors should provide at least general uncertainties in TLS-based snow depth from previous findings, particularly under/near canopy.

Thank you for the comment. A previous paper (Currier et al., 2019) demonstrates the relative accuracy of the TLS to airborne lidar (ALS), which is likely a better measure than using the snow depth measurements that were not collected at all sites and/or same day. Currier et al., indicate that the median snow depth difference between ALS and TLS at sites A and K was less than 5 cm. In addition, ALS snow depth comparisons with field transects indicated that the median values for transects were 6 cm greater than ALS median values. Moreover, the mean absolute difference and RMSD of that comparison was 7 and 8 cm, respectively. In sum, we will reference Currier et al. in the revision in the discussion on uncertainties in TLS-based snow depths.

Currier, W.R., Pflug, J., Mazzotti, G., Jonas, T., Deems, J.S., Bormann, K.J., Painter, T.H., Hiemstra, C.A., Gelvin, A., Uhlmann, Z., Spaete, L., Glenn, N.F., Lundquist, J.D., 2019. Comparing aerial lidar observations with terrestrial lidar and snowâĂŘprobe transects from NASA's 2017 SnowEx campaign. Water Resources Research. doi: 10.1029/2018WR024533.

4. There are a few comments in terms of structure of the paper. (1) Given the three research questions in L74-78, it would be best to either rearrange the results (discussion) to better address the three questions or increase the number of questions to better reflect the structure that the results are provided. Too many subsections exist in the result section. I would recommend combining the subsections based upon the questions. Also, please consider to combine "Results" and "Discussion" (because some descriptions in the both sections are duplicated). (2) I would strongly suggest reorganizing the figures and tables in the main body and the supplementary. I think some figures and tables (e.g. Figure 4 and Table 2) in the main body would be better to be

included in the supplementary. Similarly, some figures and tables in the supplementary should be moved into the main body (Figure B1). (3) Some sections should be re-named and relocated. For example, “2.1 Study area” should not be under “2.Method”. “2.2 TLS” and “2.3 TLS Data Processing” should be combined which may be under “Data and preprocessing”. Also, I think contents in some subsections are too short to comprise an individual subsection (e.g. Section 3.6 and 3.7). It would be good to combine similar subsections into one.

Thank you for the helpful comments, and this comment aligns with similar comments by Reviewer 1. We will review the questions and better present the results and discussion in a logical order. We may keep Results and Discussion separate, but will review what we present so to minimize duplication.

Regarding Figures and Tables - we will move Figure B1 to the manuscript, and move Figure 4 and Table 2 to the SI. Based on Review 1, we will also revise the caption of Figure B8 and move the information of Figure B9 into a table and we will merge the positive and negative violin plots of figure 7.

Regarding the sections, we will simplify the sections so that they flow better and avoid duplication.

Specific comments

L41 Please add citations We understand this comment is in regard to “and understanding how best to describe forest characteristics (cover, structure, gaps, etc.) relevant to snow distribution is evolving.” and we will add citations.

L58 Please provide a range of the snow depth quantitatively (e.g. snow depth > XX mm) with general forest information (e.g. dominant types).

The dominant species was Pinus sylvestris and shallow snow here means snow depth < 0.5 m and deep snow is > 0.5 m. We will add this information to line 58.

L63-64 Would you check the reference again? Schirmer et al. (2011) do not provide

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the relationship between vegetation or canopy characteristics and wind effect and snow depth variations.

Thanks. Yes, you are right. That was from Trujillo et al. 2007. We will modify the sentence.

L200 Can you quantify what “mid-to-high correlation” mean? Also I would recommend providing correlation matrixes between vegetation metrics and snow depth for each site to identify intercorrelation among the vegetation metrics.

Thanks. We will clarify what we mean by mid to high correlation. Our response will also be modified based on the changes we make from Comment #1 above.

L203 two distributions -> two peaks of the FHD distribution

Thanks for catching this mistake, we will correct for two peaks within the distribution.

L204 0.35-0.75 -> -0.35 to -0.75; throughout the manuscript

Thank you for catching this mistake. We will correct.

L222 Remove “we propose”

Will remove.

L304-205 This is identical to the sentence above. Rephrase “more evenly spaced along an individual tree”

Thank you for catching this, we will rephrase.

L273-274 Rephrase the sentence.

Yes, we will rephrase.

L292-294 This should be in data or methodology sections.

OK we will move it to the methodology section.

Table 1 In Median Absolute Deviation, what is the constant number “1.4826”?

1.4826 is a scale factor which relates MAD to deviation of average. This is MAD with one sigma uncertainty assuming normally distributed data. We will clarify in Table A1.

Table A2 Please add the units. I think boxplots would be more suitable to present the dataset. For example, 3x6 boxplots with three different colors for canopy, transition, and open areas.

Thanks for your suggestion. We will illustrate that table as a boxplot.

Figure 4 It would be fine to move into Appendix.

Yes we will move to Appendix.

Figure B9 In the figure, site O has random distribution. But in the caption “Only site N has a random distribution pattern of trees”. Please double check. And I do not think the six distributions are needed in this figure even in supplementary info – all distributions are the same. A table including nearest neighbor values only would be more appropriate than the figure.

Thank you for finding this error. We will correct and change this figure into a table.

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