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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.

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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.

RC1: Review of Hojatimalekshah et al., "Tree canopy and snow depth relationships at fine scales with terrestrial laser scanning"

Relating tree structure to snow depth dynamics is a very important research area with cryospheric and hydrologic implications. It is also a very challenging interaction to quantify and this study uses a Terrestrial Laser Scanning (TLS) dataset to examine such dynamics at a selection of site's from Grand Mesa in 2017. Interesting relationships are revealed in terms canopy and topographic controls. This paper would make a better contribution with a number of changes, generally around clarifying the storyline and focusing the results presented, and therefore I would recommend major revisions at this point prior.

My review begins with my major comments and followed by some specific comments.

At this point, I will not make technical comments.

Literature context in Introduction:

Overall, I enjoyed the introduction and how it established the context for the rest of the paper.

1. This would further be strengthened by a concise description of the physical processes (interception/sublimation and blowing snow differences between forest and clearing) to help interpret the findings later.

Thank you for the suggestion. We will add details to the Introduction re: physical processes between forest and non-forested areas.

2. As well, there is a focus on airborne lidar and terrestrial lidar as the only tools by which to quantify snow-forest interactions. There is a large chunk of recent literature that has been starting to employ drones (with structure from motion- many including Buhler 2016, Harder et al., 2016; Vander Jagt et al., 2015; De Michele et al., 2016, Walker et al., 2020. SfM does have trouble with dense vegetation but would still be a method in the sites with more sparse tree cover. More recently, lidar on drones for snow depth (Harder et al., 2020 and Jacobs et al., 2020) has been demonstrated to deal with vegetation better than ALS). Drone scales bridge TLS to airborne scales so is relevant in this discussion and should not be ignored. For full disclosure I do work with UAV's and snow and this is not reviewer coerced citation for selfish purposes– UAV scale and capabilities can and do work on these same scales/problems and should not be ignored. This is a good point and we appreciate the references! We will add a sentence or two to both the Introduction and Discussion to include recent studies and

demonstrate the value of UAS in snow-veg studies.

TLS for forests:

1. The path of photons from a TLS are oblique and so point densities quickly diminish as one moves further away the exposed side of a tree/tree stand and point densities diminish from the occlusion in vegetation. This is evident from the footprint of the analysis and the snow depth maps reported in the appendix. This is not discussed as a limitation anywhere that I can recall in this paper. TLS is not a new method to capture edge of forest snow interactions and am not discounting previous work. I guess that I would like to see a discussion/acknowledgment of this. Especially when we are asked to consider the relationship with respect to side of tree. Won't this dataset be biased to capture interactions in greater detail on the exposure side rather than the obscured size. Will this bias the results/ relationships?

We utilized multiple scan positions to avoid occlusion of the trees we used for analysis; however as you point out, there are some instances where occlusion occurred. We avoided this for our canopy edge analyses by only using tree polygons with sufficient coverage (see Section 2.3.4). We agree that it is important to add a statement about occlusion in our Discussion as a challenge of TLS, and we could also discuss this in the context of the complementary nature of UAS and airborne lidar.

2. How do the vegetation metrics respond to a TLS scan which captures one side of a tree better than the other? Also is there a threshold to determine the extent of the analysis and how is that determined?

These are good questions and we'd like to clarify. We did not test for vegetation metrics based on a subsampling approach (on one side of a tree versus the other). To avoid unbalanced samples from different sides of a tree we used the whole tree to compute the related metrics. In addition, each tree was scanned from at least three different scan positions. However, there is still uncertainty about trees being evenly scanned, specifically within dense canopies which we can mention in the Discussion. Specific to our directional analysis, we used tree heights (only) to correlate to snow depth per each direction. We do not expect tree height to be affected by any sampling or occlusion issues. We will clarify this in the Discussion.

3. Considering these ideas should this be paper/title be rather reframed as an analysis of snow depth-tree relationships for sparse/gappy areas. As it is presented it would seem that these findings should be pertinent to all forests but from the snow depth maps we can see that extent of the datasets are very much limited to isolated/sparse trees and edge of forest areas?

We understand what you mean given that our images show plots that capture edges. Our analyses use individual trees in rather dense canopies (please see Fig B9, which will likely become a table in the revision) and we utilize edges of canopies - we will ensure that this is clear in a revision. We will also ensure that our Discussion indicates that our interpretations cannot be used for general assumptions / forests with different characteristics of this study area. For these reasons we feel that our title is appropriate as is.

Results:

1. There are a lot of results represented in various figures and tables that is on the overwhelming side of things. Can this be significantly pared down to the most important findings or the conversion of some large tables into figures (Table a1)? With 32 possible descriptor variables that have variable levels of description and no hypotheses presented it implies more of a fishing expedition which is not ideal. Are there metrics that have been used previously besides tree height? Could you focus on metrics that are a bit more common – ie LAI or sky view factor are some that come to mind- so that these findings could have broader applicability. FHD is not a commonly used metric yet. In a revision could this be flipped and some specific hypotheses be tested? Would help to focus things.

Yes, we will review the large tables (1 and 2) and attempt to put appropriate information in a figure. The use of the different vegetation variables are in response to question 1 of the manuscript. FHD is used in ecology and while it may not be commonly used in cryosphere studies, we hope to integrate these fields. In our revision we can provide more context for the use of FHD.

Yes, we can add hypothesis in lieu of, or in addition, to our questions if need be.

2. Need more help with interpretation of Figure B8, B9 (text way too small), 7 (why are the positive and negative scales split).

Thank you for the suggestion. We can provide more detail in the captions in Fig B8. We will change Figure B9 into a table for clarity. We split the negative and positive scales for easier interpretation. However, we will merge those to one.

3. Figure b1 should be more prominent – very useful for interpretation of all of this data.

Thanks for the suggestion. We will move it to the manuscript.

4. Could the results and discussion be grounded more strongly in the physical process descriptions – correlations themselves in specific situations can be hard to parse.

Yes, we will revise the Discussion to ground our observations within the context of physical processes.

Specific comments:

1. I struggled to understand what was being communicated in the paragraph in lines 63-68. What does "observed the high contribution of storms in defining the snow accumulation pattern" mean in this context? Can it be clearer what proper scales are and how that is related to the controlling processes?

Thanks. We will clarify these sentences - we were trying to emphasize the need for fine-scale TLS data.

2. Line 120: Can you elaborate on “thus we reclassified these points manually using the software TerraScan”. Not reproducible without knowing what the manual procedures implemented were.

We will clarify in the text that manual classification included visually separating snow under the trees from tree trunks. Yes, we agree this isn’t completely reproducible but adding these details will help.

3. Section 2.3.2: perhaps a graphic could be used to explain M3C2?

Yes, we can do this and add to the Supplementary material.

4. Line 130-131: Can you elaborate on how transition zones were classified?

Yes, we will clarify that the transition zone consisted of defining a 10 m outer buffer beyond each tree polygon in the direction of the open.

5. Line 155: “contain a minimum on 10 snow pixels.” The tree polygons are variable in size yes? Perhaps this would be more robust if a percentage of area needed snow pixels? Is this how low density snow depth areas are removed from the analysis?

Thank you for pointing this out. We will consider this in our analysis.

6. Section 2.3.6: What tool/software was used?

We will clarify that we used ArcMap.

7. Section 3.6: Did it result in deeper snow? You have the data to test this, correct?

Yes, the mean snow depth is presented in Table A2.

8. Line 250: how representative are findings that are limited to one side of a dense stand of trees to make a comment on tree-snow directional dynamics. A limitation with TLS. Can findings be modified to account for the bias?

Thanks for pointing this out - we need to clarify that our comments are specific to site A and refer the reader to Fig 6b, and reiterate the sampling limitation of TLS.

Site naming:

1. Could the sites be named in a way that can simply convey some of their main features. Lettering doesn't convey much and would make tracking the relationships a bit easier.

We agree though we would prefer to keep the lettering as is, as they correspond to the NASA SnowEx project naming convention.

2. Observation temporal extent: Can it be emphasized more clearly that these were single measurements (not multi-temporal) and primarily reflect snow accumulations processes. Things will obviously be different if needing to account for snowmelt dynamics.

Yes, thank you and we will more clearly point this out in the methods and discussion.

I appreciate the challenges in relating snow depth and vegetation metrics at fine scales with real world data and look forward to seeing your response.

Thank you and we appreciate these helpful comments and references!

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