Manuscript Title: Characterizing Spatial Structures of Field-Scale Snowpack using Unpiloted Aerial System (UAS) Lidar and SfM Photogrammetry

General review: Cho and other authors measure the spatial heterogeneity of snow across a study plot in New Hampshire while evaluating the performance of UAS structure for motion (SfM) photogrammetry against lidar and in-situ observations during one snow season. This effort was conducted in New Hampshire, USA, and the study plot includes both a forested region and open field. The authors determined that the open areas tended to have deeper snow than forested areas and that lidar and SfM generally performed better, compared to in-situ observations and each other, in the open areas of the study plot. Static landscape variables, such as vegetation type and slope, impacted the distribution of snow consistently across the snow season. This project provides a new evaluation of UAS SfM as a tool to measure snow depth (a lower cost option compared to lidar). Much of the analysis, and thus manuscript text, includes a very clear description of the methodology used. However, the presentation of results in figures could be refined for clarity, and the results also require a more in-depth discussion around the performance of the select UAS tools, given the first two objectives of the study. The following line-by-line comments, which vary in "major" versus "minor" feedback, should provide more clarity and direction regarding this review, with the hope of better emphasizing the importance and value of this work.

Line-by-line comments

Abstract: Throughout, it is initially unclear and confusing what "spatial structure" is referring to. The words "patterns" and "spatial variability" are only used at the beginning of the introduction, which provide more clarity. Suggest briefly including a definition in the abstract, given the use of "spatial structures" in the manuscript title. Otherwise, suggest replacing "structure" with "variability, ""heterogeneity," or "distribution," which are more commonly used in the literature (including in the citations provided within this manuscript), whereas "structure" is often associated with the vertical microstructure of the snowpack.

Line 27-28: It would be beneficial and more complete to report, at the very least, the direction of the correlations.

Line 36: Here "snowpack structure" is ambiguous, where, to some readership, the term insinuates the vertical, microscale structure of the snowpack.

Line 70: Unclear what "these transition periods" are referring to. Please define.

Line 73-75: It would be impactful for the authors to include why this type of forest/snowpack was chosen. For example, a number of the previously cited UAS works take place in other climates/forest types.

Line 89: Reference error. There are a number of these throughout – also associated with figure references – thus I will only note this one.

Section 2: Suggest further emphasis on why this area might be ideal for this type of study (shallow snow depths, type of forest, historical data, etc.). Obviously, there are many other locations which offer open versus forested regions.

Figure 1: It would potentially eliminate preemptive readership questions if the authors stated that the derivation of the variables shown in Figure 1b-g is explained in the following section (3).

Table 1: The concept of a mixed forest is not also shown in Figure 1b. Is this referring to a blend of both coniferous and deciduous trees? If so, it is unclear the fraction of coniferous and deciduous used to determine the mixed forest area (50/50?).

Line 131: Can the authors provide insight on which ground conditions resulted in more returns versus less?

Line 174: Suggest a Sturm citation for the magnaprobe.

Line 180: As written, it is unclear if the 9 in-situ measurements were at random within the 1x1m or consistent across each survey. And can the authors please elaborate on why full sampling was not conducted during each flight (time/personnel constraints)?

Figure 2: It is currently challenging to determine the main takeaway of this figure – is it simply to observe the timeseries or to compare across the field versus forested areas? For example, it appears that air temperature and precipitation/cumulative precipitation are the same, which would make sense given data availability, but is thus redundant. It is particularly challenging to follow the 3x y-axis labels on the right side of each figure. Suggest reformatting as a sequence of timeseries – with only 1x precipitation/cumulative precipitation panel, 1x air temperature panel, and potentially 2x snow depth panels for each area (forest versus field), including the in-situ observations.

Figure 3: Are N-values the same across the two panels? Please add.

Line 278-279: Suggest including the sub-areas of the field when introducing the field and an explanation as to why there the authors created a division here (e.g., what led to the decision making for a NW vs. W vs. E sub-area of the field?).

Figure 5: Suggest a more intuitive color scheme for snow depth. The difference color scheme makes sense (negative = red vs. positive = blue). For just snow depth, suggest purple leading to blue and then red (or something similar where red is not a color in the middle of the color bar).

Figure 8: It is unclear what 1-5 (low to high) represents – this should be stated in the figure caption. Further, are there any statistical differences? If so, please note here and in the paragraph above with relevant p-values.

Line 338: Here and throughout the manuscript, the terms “modestly” and “higher” read subjectively and would be more impactful if numerical values accompanied them and/or if there was a defined threshold for what the authors considered “modest” versus “high.”

Discussion: From the results section (e.g., Figure 4a, Figure 5), readers are led to believe that using SfM for snow depth generally is not a feasible option (without significant uncertainty) except for the west side of the field, and there isn’t much of an explanation as to why. It is unclear what makes the west side of the field different from the rest of the field? Differences to the forested portion of the study plot are perhaps more obvious but are also not stated explicitly. Further, what might the authors suggest doing differently to reduce the numerous erroneous SfM measurements? The only plausible explanation currently provided is insufficient number of point clouds. It is stated in the introduction that this methodology is still an emerging one, thus this seems like an opportunity provide insight into how UAS SfM for snow depth measurements may still evolve.

Line 344: It would be helpful to connect the western portion of the field in this study to the subsequent sentences on past studies – e.g., does the western portion have a different vegetation type or other static landscape characteristic/combination of note (nothing particularly stood out in Figure 1)?

Line 353: Can the authors indicate what likely caused the erroneous values of 150+ cm of snow depth as measured by SfM?

Conclusion: Suggest restating the error values when referring to “lower error”

Line 431-433: Suggest explicitly restating the relationship – e.g., x vegetation type leads to deeper [or shallower] snow depth.