Reviewer 1

This study introduces a novel dataset of snow depth (HS) observations for a small study site with mixed vegetation and open, non-vegetated areas. HS was measured using both, LiDAR and SfM systems mounted on a UAV at different dates during one snow season. The study further evaluates the HS maps against different manual in-situ reference measurements and compares the LiDAR and SfM products against each other. The dataset is finally analysed using the relative distance concept and by comparing the mean relative distance (MRD) to 5 different spatial features describing soil and terrain characteristics.

Novel datasets of HS distribution using UAV-based remote sensing are strongly encouraged and of great importance for the hydrologic community (especially if the presented data sets will be made publicly available). Only a few other data sets exist that provide repeated, high-resolution HS observations from UAVs. For cold-maritime environments in in the north-eastern US, no comparable data sets exist (Except for the data set presented for the same study site by Jennifer M. Jacobs et al. (2020). The data therefore has the potential to provide new insights into the specific drivers for the spatial distribution of HS in such environments and is scientifically significant. The performed analysis, however, lacks a clear scientific aim and does not give new insights. The findings of the manuscript, that i) SfM provides less reliable observations with larger data gaps underneath the forest canopy and ii) HS distribution being influenced by topography and vegetation are not new to the scientific community. See for instance Harder et al. (2020) that performed a comprehensive comparison of SfM vs LiDAR HS maps derived from UAVs and Mazzotti et al. (2023) that discussed the different drivers of HS variability at high spatial resolutions. The presented method moreover lacks a co-registration of the snow-off and snow-on maps. I see the greatest potential of your manuscript and data to further discuss the relative difference maps and analyse their temporal stability quantitatively. Understanding the consistency of spatial HS patterns is an important aspect of current snow research (Geissler et al., 2023; Pflug & Lundquist, 2020).

Thank you for your constructive feedback and the specific suggestions on our manuscript. We have addressed each of your comments and carefully revised the manuscript accordingly. The referenced literature was instrumental in enhancing the positioning of our paper within the community.

Major Comments:

Overall Style:

1. The manuscript contains erroneous links to figures and references.
2. The manuscript overall has a good structure but could benefit from a careful proofreading (See also minor comments) and a more scientific writing style.
3. Some figures are not satisfactory and need to be improved (See minor comments).

Thank you for your feedback on the manuscript's overall style, references, and figure quality. We appreciate your detailed observations, and we have carefully revised the manuscript in response to each point.

Method:

1. I encourage the authors to perform a co-registration of the snow-off and snow-on surveys. If there are no snow-free areas within the snow-on surveys to perform a co-registration with, maybe a co-

registration of the point clouds (or at least the elevation models) for each survey using SfM and LiDAR could help to reduce systematic over-/underestimation of the products. Check https://github.com/jgenvironment/cluster_snow for ideas on how to perform a three-dimensional co-registration of point clouds using vegetation. Did you check whether your snow-off surveys are comparable between both systems?

Thank you for this comment. All of the surveys were co-registered using ground control points surveyed with a Trimble Geo7X GNSS Positioning Unit and Zephyr antenna. Linear, horizontal, and vertical shifts were applied to align all digital elevation models to the GCPs. The authors have added the following text at the end of section 3.1 to clarify this:

“GCPs surveyed by a Trimble Geo7X GNSS Positioning Unit and Zephy antenna were used to co-register the UAS data. Linear, horizontal, and vertical shifts were applied to align all SfM and lidar DEMs to the GCPs.”

2. Subsequently, I would be interested in a transect – plot (of the underlying point clouds) that could give a better understanding of i) where the differences between SfM and LiDAR originate and ii) give an idea of the sub-canopy point density of the two products. Providing the point densities of the different products for the different surveys is essential. (Overall, forest, open).

The authors are creating a transect plot and summary table of the point counts for SfM and lidar similar to Harder et al. (2020) to address this comment. This content will be added to the supplement.

3. I suggest removing the different validation strategies (cameras vs. magnaprobe) from your manuscript, as the number of camera locations is not sufficient to give reliable results. Moreover, as you show, the differences are negligible. Your magnaprobe measurements are important to provide the overall accuracies of your data sets but are too small in number to provide reliable insights into potential deficits of your HS maps.

Thank you for this suggestion. Based on your feedback, the authors have decided to remove the comparison between the in-situ measurement types and instead include a brief summary of the UAS SfM and lidar validation based on the magnaprobe data alone. The study objectives presented in the abstract and introduction have been updated and additional text was added to section 4.1 to explain that the cameras were not used for validation due to their limited spatial extent compared to the magnaprobe sample locations. Additional text was also added to section 4.1 to discuss the potential sampling bias for the magnaprobe data (i.e., overprobing) in Figure 3b. All statistics presented in section 4.1 and Supplementary Table S2 (formerly Table S1) now only reference the magnaprobe data. The field camera data is retained in the updated version of Figure 2 as it provides the only continuous measurement of snow depth throughout the field season.

4. Instead, you could further examine the relative difference maps, discuss their temporal stability quantitatively and relate areas of persistent relative differences with areas of varying relative differences to your topographic and soil features. This is where I see the greatest potential of your work.

We agree. We have reduced the text in the sections on validation and are expanding the section that describes the relative difference maps to include content on the three field areas...

5. Secondly, I would try to work out explicitly where and when SfM is a suited method to measure HS and what strengths and weaknesses of this sensor are. Did you find some features/flight conditions that impacted the SfM more than LiDAR?

Thank you for this suggestion. We have revised section 5.1 to include more discussion on the relative performance of the two techniques and the limitations of SfM for the study region. We added more information on the greater potential for SfM tie point errors in forested/vegetated areas, over homogenous snowpacks (e.g., fresh snow), and in sub-optimal lighting conditions. Citations are presented for each of these conditions. In addition, further discussion was added to describe problem areas in the field at the Thompson Farm site (e.g., northwest field, east field) and potential causes for the larger differences in these areas (e.g., lack of unique features, fresh snow).

Introduction and Discussion:

1. As noted above, I would focus your work i) on the comparison of SfM and LiDAR and ii) the subsequent analysis of relative differences, including the quantitative assessment of temporal stability. For i), I would explicitly state for which conditions you can recommend using SfM. Obviously, your results show that SfM is not capable of measuring sub-canopy snow ($r^2 = 0.01$ and MAE being almost three times as high as the mean HS, see Figure 3). I am missing a discussion on potential applications where choosing SfM could be reasonable. Is SfM suited to measure shallow snowpacks? Maybe analyse your difference-maps also with regard to your topographic and soil features! For ii) a more thorough introduction and discussion of literature working with snow distribution pattern is required (e.g. (Geissler et al., 2023; Pflug et al., 2021; Revuelto et al., 2020; Sturm & Wagner, 2010; Vögeli et al., 2016))

Regarding the suitableness of the SfM, we provided our answer above. Based on your suggestion regarding a more thorough literature review, we have expanded the discussion of snow distribution patterns by incorporating relevant studies that emphasize the importance of spatial variability in snow distribution under diverse environmental controls in the introduction as follows.

"For instance, Sturm and Wagner (2010) found that snow depth patterns are stable across years due to persistent topographic and vegetation influences, demonstrating the value of empirical snow distribution patterns for improving snow model accuracy. Pflug et al. (2021) investigated the interannual consistency of snow patterns and propose a downscaling approach based on historical snow patterns, which is relevant for predicting snow distribution in years with limited observations. Geissler et al. (2023) provided a dense, high-resolution dataset that characterizes daily snow variability in a sub-alpine forest, identifying distinct snow depth clusters that can help validate and inform snow models in similar landscapes. Revuelto et al. (2020) introduced a method that combines in-situ snow depth measurements with remote TLS and time-lapse photography to produce snow depth distribution patterns over time, offering a transferable approach to derive spatial snow data from limited ground observations. Lastly, Vögeli et al. (2016) utilized high-resolution airborne digital sensors to refine precipitation scaling in a snow distribution model, highlighting the potential of remote sensing data to capture complex snow dynamics in alpine regions."

Minor Comments

Title: Here and throughout the entire manuscript. I would avoid the word 'structures' in this context as snow structures could also refer to the microstructure (e.g. grain size, type or specific surface area) of the snow. Maybe use pattern or distribution instead.

Thank you for your suggestion. We replaced the word "structures" with "patterns" or "distributions" throughout the manuscript.

L15: LiDAR not introduced and make sure that you use the same abbreviations throughout your manuscript (lidar vs LiDAR).

The introduction of light detection and ranging (lidar) was added to the abstract and introduction and all uses of the term were updated to "lidar" for consistency.

L23: This sentence is not clear, you have only a very few measurements from the cameras and the measurements were taken at different locations. Thus, it is not surprising that they differ. I would skip this analysis.

Thank you for this feedback. Based on your earlier comment, we have removed the comparison of in situ techniques from the text.

L42: Repetition of L30ff.

The repetitive wording was revised.

L43: I would improve this section with a more thorough literature review. From what Geissler et al. (2023) and Pflug and Lundquist (2020) showed, patterns are rather stable.

Thank you for this feedback. We recently completed a stand-alone review that updated Clark et al. That will be incorporated in the introduction.

L56: …and most sensors cannot measure through the canopy of trees.

Indeed! We added this text to the sentence.

L64-65: The sensors can measure the HS, not the interactions between the snowpack and land/soil characteristics.

Agreed. Additional clarification has been added to the text:

"Hence, the capabilities of UAS platforms equipped with diverse sensors can observe snowpack properties and support analyses of field-scale physical interactions between snowpacks and land/soil characteristics (Cho et al., 2021)."

L70: Not sure what you mean with transition periods.

Additional clarification has been added to the text:

"However, investigating the transition periods between snow-on and snow-off poses challenges, primarily due to the snow becoming increasingly shallow and patchy, eventually revealing bare ground."

L76: Again, you mean spatial distribution and not structure of snow depth and probably persistence and not stability.

Structure was updated to distribution.

L88: Erroneous reference. Here and many others. Check entire manuscript.

This reference, as well as others throughout the manuscript, have been corrected.

L90: Actively managed and unmown grassland – sounds contradictory.

Agreed, we removed "and unmown".

L131: You need to specify the point densities of all of your surveys and data sets (Overall vs field vs forest) together with the size of the data gaps.

The point densities of the surveys and data sets were added to the supplementary materials.

L133: remove '-'

The dashes have been removed.

L147: How and when did you rasterize your products? Before or after the substruction from snow-off data? I am missing a co-registration of your datasets. See major comments.

Snow-on and snow-off DEMs (rasters) were made independently of each other. They were subtracted on a pixel-by-pixel basis to make each snow depth product. The methods description for both SfM and lidar now specify that the snow depths were calculated as the "difference between the ground classified snow-on and snow-off elevations within each pixel."

Co-registration was performed for both SfM and lidar DEMs based on the surveyed ground control points. Please see response to major comments for more information.

L167: 'made' – rephrase.

The sentence was rephrased as follows:

"Dense clouds were produced with the quality setting set to high and depth filtering set to moderate."

L171: Style again: …DEMs are derived based on the points classified as ground within…

This statement was rephrased as follows:

"Finally, digital elevation models (DEMs) were derived from the ground classified points within the dense clouds."

L182: © and TM antenna – please check author guidelines.

Thank you. We removed © and revised "Zephyr™ antenna" to Zephyr antenna.

L194: Make sure to be consistent field-scale vs field scale.

Thank you for the comment. "field-scale" has been applied throughout the manuscript.

L196: Please clarify what variables are physical.

We clarified the text as follows.

“The variables used in this study include physical characteristics of the landscape and soil, which are directly measurable properties. These variables are slope, aspect, shadow hours, saturated hydraulic conductivity (Ksat), soil organic matter, and plant functional type. While plant functional type is a physical variable, it is categorical, representing only vegetation characteristics (Figure 1).”

L201: Formula not referenced.

The formular was referenced in the text with GLI.

“The forested area was further classified as coniferous or deciduous for the study region by applying the Green Leaf Index (GLI) (Louhaichi, Borman, and Johnson 2001) (Equation 1) to the optical three-band (red, green, and blue) orthomosaics derived from the snow-off DJI Phantom 4 RTK survey.”

L211: At what date? The incidence angle changes throughout the season. And what about sub-canopy shadow hours? More details on the underlying method are needed! Check grammar.

Thank you for the thoughtful question. The current shadow hours were calculated on February 24th. We agree that the change in solar angle between February 4 and March 7 could influence the shadow hours due to seasonal shifts in the sun's position, potentially impacting solar incidence and, thus, shadowing on the terrain. To address the comment, we considered all shadow hours within the available observations from February 4 to March 7. Although the sun's angle does change slightly during this period, our analysis assumed a consistent shadowing effect throughout the day (7 am to 5 pm) for simplicity and practical applicability. The variation in solar angle over this period is relatively minor in terms of altering shadow duration or extent across the study area. Thus, we determined that incorporating adjustments in solar angles would have a negligible effect on the overall shadow hours and snowpack conditions measured here.

However, we acknowledge that incorporating dynamic solar angle adjustments could be beneficial in studies where seasonal or monthly changes in shadowing may significantly impact outcomes, particularly in cases involving longer observation windows or areas with high topographic relief. We have clarified this in the revised text to address potential reader questions on this point:

"*The shadow hours, representing the number of hours from 7 am to 5 pm experiencing shadowing, were calculated using the unfiltered UAS lidar digital terrain model and a static sun incidence angle based on average conditions for the study period. Given the minor variation in solar angles from February 4 to March 7, any change in shadow hours was considered negligible for this study.*"

L231: Something is wrong with the references.

Agreed. We modified this with the correct referring to Figure 2.

L234: Define winter season explicitly.

The sentence has been modified as follows. “Between December 15th, 2020 and March 12th, 2021, the average daily temperature was -2oC, the maximum daily temperature was 19°C on January 31st and the minimum daily temperature was -19°C on March 11th.”

L236: I would rephrase this sentence.

The sentence has been rephrased “December and early January had ephemeral snowpacks of less than 10 cm which melted within a week. A snowpack was continuously present from late January through the middle of March.”

L249: You have not really talked about standard deviations so far – this sentence is not clear? Is it needed at al?

The sentence has been removed.

L251: You did not introduce accumulation and ablation periods so far. What are you referring to? Sentence could become clearer after rephrasing.

The authors modified the sentence as follows. “Figures 2b and 2c show that the eight UAS-based SfM and lidar flights captured both the snow depth peaks and the ablation period following the last peak on February 20th, referring to the phase in the seasonal snow patterns when the snowpack begins to melt and decrease in depth.”

L255: Could the increased variability of originate from the reduced point density/increased data gaps? -> Would become clearer with the transect plot (See major comments).

The authors are creating a transect plot and summary table of the point counts for SfM and lidar similar to Harder et al. (2020) to address this comment. This content will be added to the supplement.

L258: Remove ‘snow observing’.

This has been removed.

L262-265: Unclear – sentences should be more concise.

Thank you for this comment. These lines (formerly L262 – 265) in section 4.1 were revised to make the discussion of the UAS vs in situ comparison clearer.

L271: r^2 - check layout!

This has been corrected.

L277: remove space in snow.

This has been corrected.

L287: 0 cm?

This statement was revised:

“The difference between SfM and lidar snow depths was fairly consistent in the field and close to 0 cm on most dates.”

L291: remove daily

Removed as recommended.

L297 : ‘were some gaps’ – rephrase and be more precise. How do these gaps emerge? Using your formula 1, I assume that areas with no snow (‘patchy snow cover’) would result in a relative difference of -1. I think this needs to be further discussed as this is what makes your data set special and valuable to the community! There are not many data sets that have several revisits during one season and could be used to analyze also patchy snow covers and their evolution.

Thank you for highlighting this point. We agree that the presence of gaps in the snow cover warrants further clarification and discussion, especially given the uniqueness of our dataset. The gaps in snow cover in our study emerge due to several factors. First, variations in terrain, slope, and aspect affect snow accumulation and retention. Additionally, areas beneath dense canopy cover can experience faster snow ablation or limited initial accumulation, leading to patchy snow distribution across the field site.

Based on your suggestion, the statement has been rephrased "During the ablation period, consistent spatial patterns of the relative difference were observed despite the presence of patchy snow cover in some areas. These gaps emerged primarily due to differential melting, leading to sections with no remaining snow."

L299: over the time period – better: for all survey dates.

Replaced as recommended.

L303: AOI does not contain areas south of forests? And…

Agreed this is confusing. We were referring to the treeline at the northern part of our study error, not the forest. We have rephrased this to clarify the location and to make it less confusing.

L305: ..no forest is located in the wester field? This is very confusing. Maybe add small numbers that you could refer to Figure 7.

Agreed this is confusing. We modified the text so that we only refer to the forest area that is studied throughout. In this comment, we are referencing the forest. We are reviewing this section and may update the figure with numbers if the locations are not clear.

L311: You don't know what primarily drivers of snow distribution. This would require a more thorough analysis.

Agreed. We replaced this sentence with "In the combined areas (i.e., forest + field), relative snow depth clearly differs by vegetation type."

L330: It is well known that LiDAR outperforms SfM (Harder et al., 2020).

Additional text has been added to section 5.1 to discuss the relative performance of SfM and lidar. For example:

"While it is apparent that the accuracy of SfM-derived snow depth estimates cannot match that of lidar, the results of this study indicate that both techniques provide sufficient accuracy for monitoring the median change in shallow snow depths over time in flat, unforested land covers when there are a sufficient number of unique characteristics for SfM post processing. It is clear from the results of this study and previous ones that compared to in situ data, UAS lidar techniques produce lower errors and fewer data gaps than SfM, especially in forested land cover and over homogeneous snowpacks (Bühler et al., 2016; Bühler et al., 2017; Harder et al., 2020; Revuelto et al., 2021b; Miller et al., 2022)."

L337: What features? Can you give examples? This is where it gets interesting!

Thank you for this comment. The authors have revised section 5.1 to include more discussion on the relative performance of the two techniques and the limitations of SfM for the study region. We added more information on the greater potential for SfM tie point errors in forested/vegetated areas, over homogenous snowpacks (e.g., fresh snow), and in sub-optimal lighting conditions. Citations are presented for each of these conditions. In addition, further discussion was added to describe problem areas in the field at the Thompson Farm site (e.g., northwest field, east field) and potential causes for the larger differences in these areas (e.g., lack of unique features, fresh snow).

L340: Do you mean image overlap? You only have one point cloud for each survey.

This sentence has been rephrased to: "In SfM processing, an insufficient number of valid tie points, used to stitch together overlapping images, may degrade the accuracy of SfM snow depth data (Harder et al., 2016)."

L360: You only showed the overall relationships!

Modified the text to indicate that monitoring allows for identifying the overall relationships.

L375: Underneath rather than beneath?

The text was updated as suggested.

L386: would help the snow community.

The text was edited as suggested.

L388-389: Please give some examples for the 'numerous studies'. Not sure to what section/results you refer this time stability. This needs to be further analysed, see major comments.

References will be added to this section that indicate when snow depth patterns are consistent with or across seasons.

L411: remove space.

The space was removed.

Tables and Figures:

Table 1: These are the numbers of magnaprobe measurements per survey? Please clarify!

Upon further discussion and based on feedback from other reviewers, the authors determined that the inclusion of the full and partial sample dates together was likely to cause confusion and potentially bias the statistics computed for all dates. As a result, we have removed the extra samples collected on 2/4 and 2/24 and retained only those samples which were included in nearly all surveys. Table 1 was moved to the supplementary material (now Table S1) and the caption was revised to clarify what the numbers are referring to (i.e., number of locations where a grid of 9 samples were collected).

Figure 1: The patterns of the slope are interesting, as they vary on very different scales comparing forests and open sites. Can you confirm this from your knowledge of the sites or could this be due to some problems in the ground/no-ground classification of the point cloud? Here, again, a transect of the point

clouds could help to get an idea of the topographic characteristics of your site. For the aspect: can you confirm these ‘stripes’ from your observations? Can you reproduce them with your SfM- snow-off map?

The slopes are indeed as they appear in this figure. The variations in the forest are likely why this land was not used as pasture. We are creating a transect that will show these patterns for the supplementary materials. Regarding aspect, some of the striping is due to the color range transition between 0 (red) and 360 (blue). This part of the field definitively has more “rows” as compared to the western and northwestern field; this is somewhat present in the orthomosaic.

Move coniferous into the middle of the dark green bar in the legend. Did you explain what these outlined areas are in the middle of the Western field and in between the western and eastern field?

Great point. We added text in the figure caption that indicates the outlined areas include a small pond, the USCRN station, a small outbuilding, and a section of dense shrubs that were not representative of the field.

Figure 2:

This plot is very unclear and it is very difficult to understand what you want to show. For instance, you can assign the uncertainty ranges to the individual dots. I would completely revise this Figure. Showing the meteorologic forcing together with the HS timeseries is important, but I would reorganize this figure. For instance, a plot containing Meteo forcings and the three (!) time series from your camera locations. A comparison with your available in-situ measurements is already done in Figure 3.

The caption has some formatting problems.

Thank you for your feedback on how to revise Figure 2. We have combined your feedback with that from the other reviewers and created a new version of this figure with three subplots. The purpose of this figure is to show the reader how typical conditions at the field site changed throughout the sampling period. We hope that the context provided by this time series also aids in the interpretation of the raster timeseries shown in figures 5 and 6.

The first subplot shows the meteorological data which illustrate the seasonal conditions (precipitation liquid equivalent and average air temperature). The second shows the snow depth evolution in the field from the field camera (continuous measurement) and the median snow depth value for the two UAS measurement types (SfM and Lidar). The third subplot shows the snow depth evolution in the forest from the two cameras and the median snow depth value for the two UAS measurement types. Field camera data was retained for this plot as it provides a continuous measurement of snow depths throughout the season.

Figure 3:

I would change the x-axis range to the data (e.g. 0 – 40 cm). I assume this plot combines all surveys? Clarify! It is hard to differentiate the colors of the SfM circles. After incorporating my major comments, it might be more suited to use the color of the dots to visualize different, more relevant informations such as the survey date instead of Magneprobe vs camera.

Yes, the plot combines all surveys. We have updated the figure caption accordingly. The authors have updated both of the subplots in this figure to a range from 0 – 50 cm on the x and y axes (this range matches the change made to figure 4). Based on your previous comment, the field camera data were removed from this figure and the updated version now compares the two UAS measurement techniques against the magnaprobe measurements. The colors and marker shapes have been updated to reflect this change.

Figure 4:

Is it needed to show all the outliers of your SfM data in this plot? I would be more interested in the Scatterplots and density plots for the more relevant range e.g. <50 cm.

Thank you for this suggestion. The plots have been updated to show a range from 0 to 50 cm. Based on feedback from other reviewers, the figure has also been divided into eight subplots showing the forest and field sections individually to improve visualization of this data.

Figure 5:

It seems to me as if you sometimes have less data gaps in the difference map compared to the SfM – how is that possible? (e.g. 2/20/21).

Thank you for your comment. The authors have reviewed the figures and determined that this was likely due to a data visualization issue. The snow depth plots only displayed values within the range shown in the legend (0 to 34 cm) and any values outside of this range were not included. However, in some locations the snow depths measured by SfM or Lidar were outside this range but were less than or equal to ±15 cm different from each other. To resolve this, the new figure was produced using filtered rasters which had their pixel values trimmed to the same range as the legend. The updated difference maps were calculated from these filtered rasters. As a result, the new versions of the difference maps also exclude these locations where snow depths exceeded reasonable values.

Caption introduces SD. Either introduce the abbreviation for the entire manuscript or not.

Use of this abbreviation was removed from the caption.

Figure 7:

Are these dark-red areas along the edges of your study site potentially due to misclassifications (ground/no-ground) of your point cloud? Or can you explain them otherwise?

No, it is unlikely they are misclassified. As one approaches the northern edge of the field, there is a dense coniferous tree line. While we did not explicitly measure the processes in these locations, we were able to clearly see distinct patterns in snow depth. That said, during snowfall, we believe that interception seems to reduce the accumulation. There also seems to be a longwave effect where the warm trees melt the snow adjacent to the treeline. We are currently conducting other studies to better understand the energy balance in forest field transitions.

Figure 8: Why do you compare combined vs field and not field vs forest? Or all?

Thank you for this comment. We chose to compare combined vs. field areas, rather than field vs. forest or each area individually, because we assumed that plant functional types in forest areas primarily influence results, likely overshadowing the effects of other physical variables. However, we agree that including results for forest areas in Figure 8 would benefit readers who may be interested. The authors are currently working on adding these forest area results.

References

Geissler, J., Rathmann, L., & Weiler, M. (2023). Combining Daily Sensor Observations and Spatial LiDAR Data for Mapping Snow Water Equivalent in a Sub-Alpine Forest. *Water Resources Research*, *59*(9), Article e2023WR034460. https://doi.org/10.1029/2023WR034460

Harder, P., Pomeroy, J., & Helgason, W. D. (2020). Improving sub-canopy snow depth mapping with unmanned aerial vehicles: Lidar versus structure-from-motion techniques. *The Cryosphere*, *14*(6), 1919–1935. https://doi.org/10.5194/tc-14-1919-2020

Jennifer M. Jacobs, Adam G. Hunsaker, Franklin B. Sullivan, Michael Palace, & Eunsang Cho. (2020). *Shallow snow depth mapping with unmanned aerial systems lidar observations: A case study in Durham, New Hampshire, United States*.

Mazzotti, G., Webster, C., Quéno, L., Cluzet, B., & Jonas, T. (2023). Canopy structure, topography and weather are equally important drivers of small-scale snow cover dynamics in sub-alpine forests. *Hydrology and Earth System Sciences Discussions*, *2023*, 1–32. https://doi.org/10.5194/hess-2022-273

Pflug, J. M., Hughes, M., & Lundquist, J. D. (2021). Downscaling Snow Deposition Using Historic Snow Depth Patterns: Diagnosing Limitations From Snowfall Biases, Winter Snow Losses, and Interannual Snow Pattern Repeatability. *Water Resources Research*, *57*(8), Article e2021WR029999. https://doi.org/10.1029/2021WR029999

Pflug, J. M., & Lundquist, J. D. (2020). Inferring Distributed Snow Depth by Leveraging Snow Pattern Repeatability: Investigation Using 47 Lidar Observations in the Tuolumne Watershed, Sierra Nevada, California. *Water Resources Research*, *56*(9). https://doi.org/10.1029/2020WR027243

Revuelto, J., Alonso-González, E., & López-Moreno, J. I. (2020). Generation of daily high-spatial resolution snow depth maps from in-situ measurement and time-lapse photographs. *Cuadernos De Investigación Geográfica*, *46*(1), 59–79. https://doi.org/10.18172/cig.3801

Sturm, M., & Wagner, A. M. (2010). Using repeated patterns in snow distribution modeling: An Arctic example. *Water Resources Research*, *46*(12), Article 2010WR009434. https://doi.org/10.1029/2010WR009434

Vögeli, C., Lehning, M., Wever, N., & Bavay, M. (2016). Scaling Precipitation Input to Spatially Distributed Hydrological Models by Measured Snow Distribution. *Frontiers in Earth Science*, *4.* https://doi.org/10.3389/feart.2016.00108