Review of “Spatially distributed snow depth, bulk density, and snow water equivalent from ground-based and airborne sensor integration at Grand Mesa, Colorado, USA”

Journal: The Cryosphere Discussions
Authors: Tate G. Meehan, Ahmad Hojatimalekshah, Hans-Peter Marshall, Elias. J. Deeb, Shad O’Neel, Daniel McGrath, Ryan W. Webb, Randall Bonnell, Mark S. Raleigh, Christopher Hiemstra, Kelly Elder
Manuscript ID: tc-2023-141
Review date: November 2023
Reviewer: Kat J. Bormann

Overview

In this manuscript, the authors present a method to invert spatially distributed estimates of bulk snow density from the combination of ground-penetrating radar (GPR) retrievals and snow depths generated from airborne lidar. The bulk density map is then combined with a snow depth map at 1 m spatial resolution to calculate spatially distributed SWE for February 1, 2020. The snow density, snow depth and SWE maps are evaluated with field measurements from the NASA SnowEx campaign at Grand Mesa (Colorado, USA) during February 2020. These measurements span forested, exposed, and forest-adjacent areas over relatively flat subalpine terrain during cold and dry winter conditions.

The novel component of this manuscript appears to lie primarily with the generation of spatially distributed bulk snow density estimates from the GPR data using airborne snow depth maps to make the inversion. While algorithms and techniques currently exist for the retrieval of bulk snow density from GPR data (Griessinger et al., 2018; McGrath et al., 2022) the authors present new techniques for automated layer picking and introduce the use of snow depths from airborne lidar. The resulting distributed snow density estimates (after filtering of outliers) show similar accuracies to previous studies (Griessinger et al., 2018) when compared to high-quality snow density measurements, with an RMSE of 27 kg/m^3.

The spatial patterns of bulk snow density presented are certainly revealing and will be of great interest to the snow research community. The implications of this variability on SWE retrievals speaks directly to the NASA SnowEx project goals, is an advancement in our understanding, and will be useful for the broader SnowEx community. However, the study misses an opportunity to compare the newly generated SWE maps to existing SWE maps such as those from the Airborne Snow Observatory(ies), which were produced for 1-2 Feb 2020 over the Grand Mesa – using modeled bulk snow density. With this, I think the study falls short of demonstrating improved SWE mapping (using GPR bulk density observations) by failing to compare (and contrast) the newly produced SWE product with existing data ASO that currently represents best practices in SWE mapping (despite the larger spatial resolution of 50m), and potentially other SWE estimates generated as a part of the SnowEx effort. Despite the main thrust of the paper being the GPR bulk density measurements and methods, I propose that the true scientific advancement of this work lies with such a SWE comparison.

Finally, while I appreciate the breadth of observations and careful use of many sources of field measurements that were used in the study, I found the manuscript narrative to be unfocused at times. Perhaps there is opportunity to reduce scope to include only the pertinent analyses and discussion that support the main purpose of the manuscript.

See attachment for further detail.

Major/minor comments

Scope: This manuscript covers a lot of ground, what are the specific questions that are being addressed and how does the analysis presented speak to these questions? I suspect there is information in here that is superfluous. Perhaps reconsider the title of the paper to narrow down and focus the theme.

Comparison with existing techniques and demonstration of improvement: the authors miss a fruitful opportunity to compare their SWE map with other spatially distributed estimates of SWE (such as those prepared explicitly for the NASA SnowEx campaign by the Airborne Snow Observatory(ies), and potentially other SWE maps from instrumentation deployed as a part of the SnowEx 2020 effort. While I understand the spatial resolution of 1m is quite different from the 50m ASO products, the sum of averages should allow a meaningful comparison and contrast and is an obvious omission from the work (in my opinion).

- I realize that with me being a part of the ASO Team, this comment may seem biased. However, I am coming from a place of advancement of the science and understanding – and in my opinion it is common practice to compare new methods with existing methods to demonstrate improvement. Please take my comments with this in mind. I am truly interested in what we can learn from this type of comparison :)

Snow depth map: I am wondering why the authors chose to generate their own snow depth map from the point cloud data using Multiscale Model to Model Cloud Compare (M3C2) rather than just use one of the official SnowEx products from the same lidar data produced by Airborne Snow Observatory(ies). The M3C2 method is really targeted toward benefits over rasterized data in rough and complex terrain, which is far from the flat plateau of the Grand Mesa. This also seems like a missed opportunity to connect the current research/work to existing frameworks within the snow community – for instance, it would be useful to know how these products compare and if M3C2 provides clear benefits (as raster-based subtractions for snow depth are widely used by the community).

Snow density inconsistency: Line 340 "The LiDAR–GPR inferred average snow density shows ... and greater variability in the open terrain than areas sheltered by forest canopies" I would have expected the opposite, the snow pit measurements do not support this statement (from what I can tell) and, given the MLR/RF/ANN models were trained using the Lidar-GPR densities (with high variability in the exposed areas, it is no surprise that the MLR/RF/ANN models are also disagreeing with the snow pit measurements w.r.t spatial variability of bulk snow density). Perhaps a brief explanation on this inconsistency is required.

Broader appeal: I am wondering if there were other SnowEx locations that had GPR data to replicate this study? (Not required for publication)

Line edits

These line edits are mostly questions on the method, meaning or seeking clarification. None of these comments should hinder publication though some of them should probably be addressed.

Line 225 - "We found that errors in the co-registration of these data are the leading source of error in the estimated densities." What is the magnitude of this error?

Line 245 - "At the 1-sigma level, errors of approximately ± 150 kg/m3 can be expected from this sensor integration method". This magnitude of deviation in bulk snow is of course large. I am wondering how the filtering on this would go without "SnowEx level" (high quality and many of) in-situ measurements?

Line 250 – The Grand Mesa is really flat, I'm wondering what magnitude of "geolocation errors in LiDAR-derived snow depths" you are expecting here?

Line 289 - "pixels within a 3 m buffer of LiDAR vegetation greater than 0.5 m height". With snow depths of ~ 1m across the Grand Mesa, are you also including vegetation that was buried?

Line 210: For clarification, the February 2020 flight was actually collected 1-2 Feb (over 2 days)

Line 215:"which agrees well with previous lidar error assessments " and also agrees with the ASO report that was produced for this survey

Line 215: I'm curious about the M3C2 method – were you seeing resulting snow depths of zero depth over the plowed roads?

Line 225: given "spatial registration between the LiDAR and GPR varies on the order of a few metres", and the high spatial variability of snow depth, does it make sense to use 1m snow depths to invert the GPR information? In Section 2.4.3.1 you do an error analysis and peturb TWT and snow depth - of the two perturbations which one injected the most uncertainty into resulting snow density estimate? On this, I really like Figure 3.

Line 255: what was the magnitude of the trends here? "Separate linear trends were identified in the forested and open regions traversed with the GPR"

Line 265: "1 October 2019 through the end of the SnowEx IOP on 12 February 2020" I'm a bit confused as to why you would include 3-12 Feb in your mean wind direction and speed calculations if you are trying to get at snow drifting ON 1-2 Feb. Perhaps I mis-read here.

Line 270: I thought Winstral et al., 2002 was based on the baseline snow off DEM. Why use snow on terrain? "we computed two parameters (maximum upwind slope and wind factor parameters; Winstral et al., 2002) from the 1 February lidar-derived snow surface elevations."

Line 285: Wouldn't some of that 0.5m high vegetation be buried in 1m of snow, and therefore not assert a drift influence? "3 m buffer of LiDAR vegetation greater than 0.5 m height"

Line 330: The Regression models were used to "fill-the-gaps" in the density field provided by the GPR. I'm curious as to what these maps look like.

Line 340: "however within the open and forested domains individually, LiDAR and GPR estimated snow depths are uncorrelated with in situ snow depths (Table 1)." Shouldn't they be correlated somewhat? Why are they uncorrelated with R2 of 0? what do the scatter plots look like?

Line 350: "may explain the weak correlation between estimated density and the in situ measurements" Is the magnitude of the un-correlation around 10%?

Line 405: Consider revising this sentence "which suggests that depth is primary to SWE in this environment"

References

Griessinger, N., Mohr, F. & Jonas, T. Measuring snow ablation rates in alpine terrain with a mobile multioffset ground-penetrating radar system. *Hydrological Processes* **32**, 3272–3282 (2018).

McGrath, D. *et al.* A Time Series of Snow Density and Snow Water Equivalent Observations Derived From the Integration of GPR and UAV SfM Observations. *Frontiers in Remote Sensing* **3**, (2022).