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Interactive comment on "Shallow snow depth mapping with unmanned aerial systems lidar observations: A case study in Durham, New Hampshire, United States" *by* Jennifer M. Jacobs et al.

Anonymous Referee #2

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In this study, the investigators mounted a small airborne lidar on a drone and flew several test flights to map snow depths across a small flat farm in New Hampshire that contained fields and forest. They then chose one flight to examine in detail. Most of the paper is concerned with the accuracy of the resultant snow depth maps, with comparison of those derived depths against on-the-ground probing (n=130), and with an extensive analysis of accuracy vs. ground point spacing from the lidar.

My overall impression of the paper is that a single acquisition flight in a single landscape, with a quite limited ground collection campaign, is too thin a reed on which to base a full journal publication. Such a limited comparison leaves open too many questions, like what the results would be if the ground was sloped, how the results would vary if the forest canopy was conifer vs. deciduous, what would happen if the snow had surface relief or other characteristics not tested in this work. In fact, the authors Figure 1 indicates a complex forest with openings and variable canopy density (a snow season air photo here would have been nice), but no attempt has been made to see if the results from one part of the forest look like those from another. No attempt was made to test how well the ground and air results match each other as a function of canopy and ground characteristics. Lastly, while the lidar and ground measurements matched beautifully in the open field, they showed a large discrepancy in the forest, which was then ascribed to over-probing through a duff layer. Perhaps that is the case, but this then ought to have been the focus of more analysis and scrutiny. The conclusion is certainly possible, but Figure 2b suggests there is also lidar sampling bias problem in the forests, and the core depths referred to in the text against which the depth probe depth was compared are never discussed, even to the extent of how many were made.

The other problem with the paper is that it is too equipment/system specific. Not everyone reading this paper will have the same drone, the same lidar etc., so what does the paper offer them? It is perhaps necessary to be equipment-specific in this type of paper to some extent, but to maximize its use to the wider community, the authors need to strive to separate what is inherent in the methodology used with the specific equipment test to what might be more universal. They try this in the discussion section with some lessons-learned statements, but these too general and read a bit like "be careful when you drive" rules. I am not sure what would be best in this regard, but some improvement is definitely needed.

Lastly, considerable space in the text is given to thin, shallow snow covers, and other lidar and airborne methods of mapping snow. While clearly when there is a fixed error in snow depth mapping (e.g., $\pm$3 cm), it is a more serious problem in thin snow. Ultimately this is a methods paper, and nothing described in the accuracy and operation of the lidar is limited or specific to thin snow.

I am going to recommend that this paper be returned for major revisions and specifically the inclusion of more extensive testing across a wider set of snow and terrain conditions. In revision, I would suggest that the focus of the paper be honed to be squarely focused on the methodology and not waste journal space on issues related to thin snow covers, for which no real new information was presented.

Recommendation: Return for major revisions and strengthen with more flights over a wider range of terrain and vegetation.

Detailed Comments

Abstract: First three sentences could be deleted.

Lines 1 to 98 could readily be deleted with no loss to the topic of the paper (thin snow discussion).

Figure 1: Nice graphic. . .very clear.

Line 84: Ground control points are mentioned, but I don't see any indication that they used control points for the SfM maps beyond the 200hz measurement rate, and I don't understand how that works.

Line 158: DTM not defined, which reflects a certain unevenness in the technical level of the paper. Who is this paper for? The new practitioner or the veteran GIS and UAV group? There are many acronyms in the paper all of which should when first presented be defined.

Line 166: Ground probe sampling method was a 5-sample cross pattern, with a GNSS GPS point in the center of the cross, but the authors wait until line 175 to tells us they averaged these 5 samples. What was the logic behind the sampling protocol and why only 5 points per 0.4 m sampling pixel, when the lidar was producing between 25 and 90? Surely more could have been measured? Also, later in the paper a core tube

(Federal s ampler?) is mentioned but no other details about it. About here in the paper it would also be good to mention the nature of the ground surface and depth of freeze, instead of later when trying to explain the discrepancy between the forest and field measurements errors.

Line 240-Figure 4: The maps look quite good, and the inclusion of the confidence map is to be commended. But several aspects shown on this figure go unremarked. Specifically, how was the location of the ground validation determined, and why so few ground data? It is unfortunate that for the field ground data, other data from the shallower area bracketing the road wasn't obtained so that a second thinner field comparison could be made. As for the confidence map, the very high confidence area in the center of western forest is at the nexus of all the flight lines. . ..is that why the confidence is high there? Conversely, comparing Fig. 1 to 4a and 4b, there are gaps and openings in the trees in both east and west forest where the confidence drops considerably, yet one might have expected these to function like the open filed. Why does it drop?

Figure 7: OK. . .but anyone new to airborne lidar will not understand it, and anyone already doing SfM or lidar will not need it. Think of who you are writing for.

Line 286 to 316: This is the first time that large vs. small UAVs are differentiated, though the weight of the lidar package would suggest a larger UAV was in use. But a quick scan of the web suggest that the drone used can handle about 14 kg. . .and recent some heavy lift drones are getting near 100 kg. Much of the discussion here seems like lessons learned that anyone trying to fly these larger drones probably already knows. It could be helpful, but they aren't detailed enough to really guide a newcomer to a successful mission. See the general point of trying to write a paper that is generic rather than specific. . .. which for rapidly changing tech can be challenging.

Lines 333 to 334: Heavy payload=short flight duration=small area mapped, hence better ground point density. While that makes sense, can't that be achieved by slower speed, closer passes etc.? And mapping extent, of course can be larger if more missions are used. So, I was puzzled what this paragraph was really trying to say.

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Discussion paper