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Interactive comment on "Theoretical framework for estimating snow distribution through point measurements" *by* E. Trujillo and M. Lehning

Anonymous Referee #2

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Trujillo and Lehning present a theoretical model for quantifying errors/uncertainty in snow measurements (in this case snow depth) and demonstrate the validity of the model using LiDAR data from the CLPX project (Colorado, USA) at a forested site and at a wind-influenced area. This work provides an important contribution to snow science, as there has been minimal attention to errors associated with the design of snow surveys. To my knowledge, the framework appears to have no technical issues. The authors apply it for a number of different examples that will be of interest to snow field studies. I recommend for publication in The Cryosphere after attention to a suite of (mostly minor) comments below.

GENERAL COMMENTS

- The mathematical framework is likely to be new material to some snow scientists,

so I would argue that additional citations and an expanded background section should be included to provide resources for learning about the methodological framework. I suspect some may be unfamiliar with first/second order stationary processes, and most equations (after equation 3) may not be obvious to all readers. This would help to maximize comprehension of the theoretical framework.

- The study was conducted with LiDAR data near peak snow conditions (assuming nearly full snow cover). How might the analysis and the theoretical framework change in a situation with patchy or incomplete snow cover, such as during the middle of the snowmelt season? Please discuss.

SPECIFIC COMMENTS

- The development of the framework (section 2) would be better supported if the authors cited prior works that demonstrate snow distributions can be approximated as 1st or 2nd order stationary processes and with exponential decay (hence providing the basis for their hypothesis in the theoretical framework).

- Does the theoretical model match the observations only because the exponential decay exponent was fitted to the data (e.g., page 14, lines 2-7)? Were independent data used to fit the exponent? If not, then there is a problem here where the same LiDAR data are used for both fitting and for model evaluation. Please clarify and rectify this issue.

- It was not clear to me why the three-point profile analysis (section 4.2) and the five-point two-dimensional analysis (section 5.2) were conducted. These types of manual snow surveys may be somewhat unconventional, as it is more common to have 10 or more measurements made in a single profile. The main example that I can recall that had 3-5 point types of surveys is the CLPX experiment (see Elder et al. 2009), which was a specialized measurement campaign, so I think it is necessary to cite this study (and any other studies with similar snow surveys) in order to clarify the motivation for the analyses in section 4.2 and 5.3. The other two analyses have clear relevance for

automated snow depth stations (one point) and more typical snow surveys (n points), but the other analyses needs more definitive motivation.

- If researchers were planning a snow survey (1-d profile instead of 2-d grid), how might they estimate the exponential decay exponent (v) in order to estimate a_optimal? (page 16) The v values considered here vary by an order of magnitude and are demonstrated to have a substantial impact on the normalized squared error (Fig 10). The authors suggest (page 18, lines 5-7) that LIDAR or dense manual measurements can characterize the covariance/correlation characteristics, but what can be done if resources are unavailable for application of these detailed methods (a realistic scenario that is acknowledged by the authors, page 25, lines 1-3)? If resources are available for LiDAR surveys, then this eliminates the need for manual snow measurements in the first place (and determination of the v parameter), so this issue needs more attention in the text.

- Why is the error less sensitive to the exponential decay exponent in the two-dimensional case relative to the one-dimensional case (page 23, lines 18-21)? Does this suggest that snow surveys should always be done in a gridded fashion instead of a profile? I think this is a very useful result and so it would be helpful if the authors explained better why this result emerged.

- It would be helpful to have more information about the LiDAR data in Section 3. From what is presented in this section, it is unclear what spatial resolution is achieved with the LiDAR (though this is identified in the abstract and later in the text at page 18, line 12) and also there is no discussion of the uncertainty in the LiDAR measurements (e.g., vegetation-induced errors, slope-induced errors, errors in the snow-on vs snow-off scenes, etc.). How does uncertainty in the LiDAR measurements impact the robustness of the analysis?

TECHNICAL COMMENTS/CORRECTIONS

- Page 4, Lines 20-21: It sounds redundant to say "... use point measurements to represent snow distribution from point measurements". I recommend deleting one of

the phrases with "point measurements".

- Page 7, Line 5: Add "a" before "function".

- Page 8, Line 23: delete "at" after "represented" ("at" is already included before "which").

- Page 9, Line 26: Please clarify what the "x and y" directions represent. Is x east-west, y north-south?

- Page 9, Line 29: The phrasing "overestimate in absolute value terms" is somewhat confusing because it no longer directly conveys that the point values tend to have severe underestimation of snow depth for cases of snow depth below the mean value. Consider rephrasing.

- Page 12, Line 5 (Eq 6): I do not think that "C" has been defined. I assumed it is the covariance?

- Page 13, Line 11: It should read "shorter" instead of "sorter".

- Page 13, Line 27: It should be "consists" instead of "consist".

- Page 14, Line 21: It should read "three measurements" (plural) in the subsection title.

- Page 15, Line 14: This should either read "as an advantage" or "as advantageous".

- Page 16, Line 6: Delete "to" after "estimation of".

- Page 17, Line 3: It should read "N measurements" (plural) in the subsection title.

- Page 18, Lines 3-4: I recommend adding Sturm and Wagner (2010) to the list of citations here.

- Page 23, Lines 8-10: These results are for the two-dimensional case at RW, but how does the model perform at RW?

TABLE AND FIGURE COMMENTS

- Figure S1 (supplement) – This figure appears to be missing. There are supposed to be three figures in the supplement, but only two are provided.

CITATIONS

Elder, K., Cline, D., Liston, G. E., & Armstrong, R. (2009). NASA Cold Land Processes Experiment (CLPX 2002/03): Field Measurements of Snowpack Properties and Soil Moisture. Journal of Hydrometeorology, 10(1), 320–329. doi:10.1175/2008JHM877.1

Sturm, M., and A. M. Wagner, 2010: Using repeated patterns in snow distribution modeling: An Arctic example. Water Resour. Res., 46, 1–15, doi:10.1029/2010WR009434.

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