Interactive comment on "Spatial and Temporal Variation of Bulk Snow Properties in North Boreal and Tundra Environments Based on Extensive Field Measurements" *by* H.-R. Hannula et al.

Anonymous Referee #3

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General comments:

The paper presents an interesting dataset used for validating remote sensing products; but, at the same time, it is used for analysing spatial distribution of snow characteristics in different landscapes and deriving information that may result useful for considering when planifying snow sampling strategies, specially in such high latitude environment. The paper is quite clear and the main comments I have is about the simplification of applying often a single value of snow density to estimate SWE over a large area. The error introduced by this simplification might be partially quantified using the available landscape units with more than one density measurements. In addition, some statements are supported by references that are not the most commonly used in literature

and should be reconsidered, and I miss some other reference that may be useful for the disscusion. In general, the presentation of results and discussion is often mixed. I would suggest to use the "results section" for presenting only the results, and to provide the potential hyphotesis for explaining them in the discussion section. Discussion does not provide any reference, so results are not contrasted with previous research on this topic. In my opinion this question is basic, and authors should modify this section accordingly.

Specific comments

Line 29: Accurate snow: please check font size.

Line 54-57: Probably it is possible to simplify the sentence

Line 79: I see more logical to say snow depth, snow density and snow water equivalent.

Line 134: 22100 measurements

Line 155. I think these references are not the most representatives about the role of canopy density on snow distribution available in international literature. I would reconsider to use more cited and relevant ones.

Line 165: "If more than one SWE points were measured within the same land cover group during the same day, an average of these measurements was used." I thin that if more than one swe data is available for one land class, they should be also used to assess the uncertainty of applying such simplification. For example, it can be shown the differences of density observed in land classes were 2 measurements are, or if there are more density measurements, the difference between each measurement with the average of the other density values available for that land class. Which error may induce this simplification in SWE estimations?

Line 196: I think that the use of sampling frequency is a bit confusing for the readers as is unclear if it refers to time or space. Sampling spacing could be more clear. 239. should be "snow depth on the lakes and rivers"?

Line 272: "this is explained by..." this should be moved to discussion

Line 284. At some point, authors relate the soil characteristics (mineral or organic) with snow density; what does support this assumption?

Line 290: The explanation of figure 6 is rather poor and mostly based in hypothesis, I recommend to go deepen in the explanation of the figure or remove it from the manuscript, as probably it is not very related with the man aim of the manuscript.

Figure 7, the break point to determine Lex might be marked in the figure.

I think that the following references may be considered to discuss the results: Sturm et al., 2010. J. Hydrometeorology: Density and SWE variability in different landscape classes and the impact of errors in density estimation on SWE estimations. Trujillo and Lehning (2015), The Cryosphere: Impact of number of measurements and sampling strategies on estimating snow in profiles or plots of different lengths. López-Moreno et al., 2013. Advances in Water Resources: Spatial variability of the snow and the difficulties to distribute spatially punctual observations.