

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.

H.-R. Hannula et al.

henna-reetta.hannula@fmi.fi

Received and published: 12 April 2016

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 state-

[Printer-friendly version](#)

[Discussion paper](#)



ments supported by references that are not the most commonly used in literature and should be considered, and I miss some other reference that may be useful for the discussion. 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 hypotheses 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.

Thank you for the detailed review of the paper. All comments will be now answered and each comment is followed by the response. The revised version of the paper will be submitted after the author’s response for the referee comments have been submitted. We agree that the analysis of the snow spatial variability has been left narrow and the links with previous studies are largely missing. The purpose of the scale analysis was supposed to be made in the special context of the SnowSAR-2 airborne campaign (Di Leo et al., 2015) and this has not been clearly expressed. As suggested by the Referee#1 we will expand the presentation of the data itself and we will compress the scale analysis, retaining only the most significant findings describing the collected snow dataset. The significance of these findings will be explained more elaborately, following suggestions made by the reviewers and will be better explained in the context of the FMI work. In addition, analysis of the uncertainty, introduced by using only one density value to estimate SWE over large area, will be added.

Detailed comments:

Line 29: Accurate snow: please check font size.

The font size will be corrected.

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

The sentence will be reworded.

[Printer-friendly version](#)

[Discussion paper](#)



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

The abbreviation of “SD” will be replaced with “snow depth” throughout the paper. However, using “SWE” makes sentences more compacted, and is generally known and used abbreviation, so we would like to keep that also in the revised version of the paper.

Line 134: 22100 measurements

The space will be removed.

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.

The international literature representing the role of canopy density on snow distribution will be studied to find more appropriate and more cited studies to refer.

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 think 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?

The error introduced by the density simplification will be estimated and discussed following the suggestions of the reviewer #3.

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.

We will replace “sampling frequency” with “sampling spacing” in the revised version of

[Printer-friendly version](#)[Discussion paper](#)

the paper.

Line 239: Should be “snow depth on the lakes and rivers”?

“in” will be corrected to “on”.

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

The sentence will 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?

This was related to Fig. 5 where the variation in snow density within each land cover group was presented. It was mentioned that it was difficult to find distinct differences in snow density between different forested land cover groups; often the median of snow density in dense forests on mineral soil was lower than in the other forested land cover groups, but this did not hold throughout the campaign period, as noted on next sentence starting on line 285. We suggest that we remove this part and mention only that no clear difference in snow density between the forested land cover groups was found.

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

We will deepen the explanation of the Fig. 6. As in the revised version of the manuscript we will concentrate more on the description of the dataset, and will also concentrate on the compressed scale analysis in the context of this specific measurement campaign, we see these snow pit measurements as a valuable information for later interests of analysis.

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

The break point will be added.

[Printer-friendly version](#)[Discussion paper](#)

References:

Sturm et al., 2010. J. Hydrometeorology: Density and SWE variability in different landscape classes and the impact of errors in density estimation of 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.

References added by the authors:

Di Leo D., Coccia, A., and Meta, A., 2015: Technical Assistance for the Development and Deployment of an X-and Ku-band MiniSAR Airborne System (SnowSAR). ESTEC No. 4000106761-CCN1. (<https://earth.esa.int/web/guest/campaigns>).

[Interactive comment on Geosci. Instrum. Method. Data Syst. Discuss., doi:10.5194/gi-2015-37, 2016.](#)

[Printer-friendly version](#)[Discussion paper](#)