

## ***Interactive comment on “Uncertainty analysis for evaluating the accuracy of snow depth measurements” by J.-E. Lee et al.***

### **Anonymous Referee #1**

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The paper “Uncertainty analysis for evaluation the accuracy of snow depth measurements” by Lee et al. presents a systematic investigation on the accuracy of different snow depth measurements. Several types of snow depth sensors (ultrasonic, laser, manual measurements) were obtained at the CARE site during a winter season and the measurements were compared (same types of sensors and different sensors) with statistical methods (bias, error propagation). The paper is understandable and written well. The conclusions drawn are supported by the data. The topic of the manuscript is important for the snow science community and especially for snow hydrology and therefore well suitable for publication in HESS. However, I have some suggestions with – as I believe – could further improve the paper:

- 1) I think that a paper that aims to analyze uncertainty and accuracy of snow depth  
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measurements should also have a look at the measurement precision of the individual sensors. This could be done by repeatedly measuring a flat, regular artificial surface and / or by selecting a short time series when a flat snow surface did not change

- 2) It is not clear how the periods without snow in the beginning of the season were treated. Were they removed for the analysis? As measuring snow is what matters in this study (and not grass) I think that these periods should be removed and possibly be analyzed separately

- 3) The results found in the paper should be discussed in more detail. What do the results mean in a broader context and how do they relate to other studies?

Minor comments:

Title and abstract sound like “a new method for uncertainty analysis was developed”. In fact the authors applied a well-established method to analyze snow depth sensors. I suggest that is expressed more clearly.

P4159 L23 Fischer 2008 is missing in references L5-6 I suggest to include a statement (possibly in the summary) if the analysis showed that these WMO criteria are met by the sensors tested

P4160 L5ff: I suggest to clearly define the terminology used: e.g. what exactly is meant by “uncertainty”, “random error”, “propagation of error” and so on. The different types of errors could be listed and their physical meaning could be provided (e.g. instrumental, random, spatial variability. . .).

P4162 L19,20 MAE and RMSE are not used in the rest of the paper. There is no need to show the formulas.

P4163 L8 I think that spatial variability would be the better term in that context (instead of spatial distribution) L9 include a break after “at each target.” L19ff why do you now use  $x_1$  and  $x_2$  instead of  $x$  and  $y$  as in the earlier equations?

P4164 L4: what is meant by “average total snowfall” – the accumulated new snow or the maximum of snow depth? During which period? The entire winter season? How was the climate in the season analyzed? L23 what was done during snow fall? No manual measurements? How were these days treated in the analysis?

P4163 L5ff: The paper deals with accuracies of different sensors and how they compare. In my opinion, then functionality of those sensors is an essential information to the reader in that context. I therefore suggest including some more information on the sensors (e.g. measurement principles, sensor accuracy, application. . . of each sensor).

P4166 L2; I think that “spatial variability” would be the better expression (instead of distribution)

P4167 L2ff: I do not agree: Data quality is mandatory for a study that deals with sensor accuracy and the data must therefore be processed with the best method possible, especially when the manufacturers propose a specific processing. If – as the authors state – a QC method is available, that is believed to perform better, that method should be tested and possibly applied. I do not see reason to postpone that to later SPICE activities!

L17 -20 I do not understand this paragraph, please reformulate.

L26 FEK (base 20) corresponds to S2 in Fig 5&6a. I think that including this in the text would help the reader to link text and figures. (same for P4168 L 9 for FEL (11A))P4168

L8: What is the reason for the deviation of FEL 11A?

L14: I suggest to state which measure is used to express uncertainty (I guess standard deviation of snow depth). Moreover I would state clearly which data have been used (which interval, which resolution, how many observations). This has been described earlier but a repetition would improve readability.

L24 -26: In that context earlier publications on spatial variability and representativeness of snow measurements should be referenced and discussed. Examples for such stud-

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ies are Lopez-Moreno et al. 2011, Neumann et al. 2006 or Grünwald and Lehning 2014; But there are many more. . . .

L4169: L4 unclear - Is the mean of the 4 stakes of the target compared to the other targets?

L9 what is meant with “temporal variation” – as stated the manual measurement take about 20 minutes; I do not see the process which causes significant temporal variation during that short period of time- Heavy snow fall might induce some variation during 20 min but if I understood correctly manual measurements have not been performed during snow fall.

P4170 L6; what is meant with “significant” – statistically significant? Please clarify

L7 The value stated in the text (10.7 cm) deviates from the one in Table 3 (10.8 cm)

L8 I suggest to add, that the random error is expressed by the BRRMSE

P4170 L19 This could be investigated by removing all snowfall events from the data and to check how this changes the distributions and the uncertainty?

L 19-22 this section is not sufficiently described. What about the distributions of e.g. FEL 11A - FEL 20 or FEL11A – 12A – they look “bimodal”, too. Anyway, I think that bimodal is not a very clear description for the distributions. The obvious shifts of the distributions should also be discussed.

L26 spatial variability instead of spatial distribution

P4171 L20: The paper does not really “introduce a methodology” but is rather applies an established method. Please reformulate.

L22: No results of MAE and RMSE are shown in the paper

L24: spatial variability instead of spatial distribution

L4172: The discussion should be enlarged: What do the presented results mean for

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the research area and how do they compare to earlier studies? L7: please add that the data were aggregated to 1 min

Table1: I suggest adding a column showing the measurement precision as stated by the producers

Table 2, 3: units are missing

Figure 1: legend and figure could be divided more clearly; this would improve readability; a scale bar should be added.

Figure2a on the picture the surface looks as it was waved? Is that true? What would be the reason?

Figure 2, 9, 11: enlarging the Figure would improve readability

Figure 8a, 10, 12: In my opinion the points should not be linked by a solid line. A solid line induces a direct link and some kind of continuity between the observations. This is not really the case here.

References:

Lopez-Moreno JI, Fassnacht SR, Begueria S, Latron JBP. 2011. Variability of snow depth at the plot scale: implications for mean depth estimation and sampling strategies. *The Cryosphere* 5(3): 617–629. DOI: 10.5194/tc-5-617-2011

Neumann NN, Derksen C, Smith C, Goodison B. 2006. Characterizing local scale snow cover using point measurements during the winter season. *Atmosphere-Ocean* 44(3): 257–269. DOI: 10.3137/ao.440304

Grünwald, T. and Lehning, M.: Are flat-field snow depth measurements representative? A comparison of selected index sites with areal snow depth measurements at the small catchment scale, *Hydrol. Proc.*, n/a–n/a, doi:10.1002/hyp.10295, 2014.

Best regards

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