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

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Anonymous Referee #1 Received and published: 5 June 2015

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 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 ⇒ We aim to evaluate the uncertainty of snow depth measurements in a field environment. As such, measurement precision determined for a narrow range of conditions is not necessarily representative of the range of conditions experienced at a given site (e.g. ultrasonic pulses are affected by temperature). This was the primary motivation for evaluating the uncertainty of snow depth observations using error propagation. The steps proposed by the reviewer may provide a baseline, or lower limit estimate, of the precision of individual sensors; however, this is not the intent of the present work. ⇒ Revised: The measurement precision of the individual sensors was added in Table 1, since their values would be determined by testing similar to what is proposed by the reviewer (likely in a lab).

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 ⇒ We agree, and indeed, the periods without snow at the beginning of the season were removed for the analysis. Analysis of these periods, while interesting, is outside the scope of this paper. ⇒ Revised: We added "(periods without snow at the beginning of the season were not considered in the analysis)".

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? ⇒ We quantified the uncertainty of snow depth measurements from well-established snow depth

observation system: - Revised: We compared average snow depth at the same snow target to show the spatial variability of snow depth at CARE site. Thus, we added: "We can show that several snow depth observations are needed to represent snow depth only on this site". In addition, the uncertainty of manual observation was calculated. We added: "these values can provide the limitation of snow depth measurement with snow stakes". - Revised: We calculated the bias error and normalized bias removed root mean square error. Thus, we added: "We found that systematic error of individual snow depth sensor was significant although automatic sensors were well calibrated before snow season. The NBRRMSEs (8.63% to 10.1 %) mean the random errors of individual sensor with respect to manual observation that was believed as true snow depth". - Revised: We calculated the instrumental uncertainty of individual automatic sensor. We added "We can determine the reliability the snow depth measured by automatic snow depth sensors and can realize the uncertainty of automatic sensor that can be considered in snow depth studies in the future. Furthermore, these values can be used as the reference for design and construction of snow depth measurement system". - Revised: We added: "In this study, the uncertainty of snow depth measurement was calculated: these values are valuable information on manual snow measurement error because they are not well quantified in the previous studies". ⇒ We discussed the results found in the paper with other studies as below: - We added: "Neumann et al. 2006 demonstrated spatial variability of snow depth within several sites and concluded that snow distribution was related to climate, topography, and local vegetation. We can conclude that the spatial variability of snow depth within CARE site caused by prevailing wind and slope of CARE site resulted in bias between manual observations". - We added: "These results also emphasize the necessity of several manual observations within the experimental site and support that single point measurements could not represent snow depth of surrounding area as mentioned in Grünewald and Lehning 2014". - We added: "Lopez-Moreno et al. 2011 mentioned that the increase of sample size reduces snow depth estimation error. In this study, the uncertainty of 36 samples located on different bases and snow depth targets was calculated". - We added: “These results could show that established observation system can affect the uncertainty of snow depth measurements”.

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. ⇒ Revised: We changed the title of this paper from “Uncertainty Analysis for Evaluating the Accuracy of Snow Depth Measurements” to “A New Method of Analyzing Uncertainty to Evaluate the Accuracy of Snow Depth Measurements”. ⇒ Revised: We have rewritten abstract to express our purpose and method for analyzing uncertainty in snow depth measurements 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. ⇒ Revised: Fischer 2011 was added to the references provided (in place of Fischer 2008). ⇒ Revised: In the Summary and conclusion section, we added: “We conclude that the WMO criteria for the uncertainty of snow depth are met by the sensors tested, in that these values were within the 5 % for the maxim snow depth (58.9 cm) measured by automatic snow depth sensor.”

P4160 L5fff: 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. . .). ⇒ Revised: We defined for “uncertainty”, “propagation of error”, “instrumental uncertainty”, “spatial variability”, “systematic error”, and “random error” more clearly and physical meaning of them in Section 2 as below: - Uncertainty: estimated variability of the measured snow depth - Propagation of error: errors of manual and automatic measurements affect the uncertainty of snow depth measurements - Instrumental uncertainty: uncertainty of automatic sensor - Spatial variability: variation of snow depth measured at different locations - Systematic

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error: the error exists in the difference between true and measured values constantly - Random error: the error that exists in the difference between true and measured values randomly

P4162 L19,20 MAE and RMSE are not used in the rest of the paper. There is no need to show the formulas. ⇒ Revised: We removed the formulas for MAE and RMSE

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 x1 and x2 instead of x and y as in the earlier equations? ⇒ Revised: We replaced "spatial distribution" with "spatial variability" in the whole manuscript and added "a break" after "at each target". ⇒ Revised: We replaced x1 and x2 with x and y in the whole manuscript.

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? ⇒ "average total snowfall " means the maximum snow depth, since it includes both the new snowfall and snow pack underneath. ⇒ Revised: We added: (the maximum snow depth). ⇒ The climate in the season was analyzed in WMO/CIMO, 2012a, but not mentioned how they was analyzed. ⇒ Revised: We removed: "the trained human observer measures snow depth once a day, starting from the southeast side of the site during non-precipitating periods" and added: 'Manual measurements were generally conducted at close to the same time every day, irrespective of the conditions. Some measurements were performed during snowfall (when possible, a second set of measurements were conducted on days when it was snowing), which were treated the same as all other measurements in the analysis".

P4163 L5fff: 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

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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). ⇒ Revised: We added the principles and application of each sensor as below: The ultrasonic snow depth sensors emit ultrasonic pulses and measure the time from transmission to return of signal reflected by a target. This time is converted to the distance from the sensor to a target that can be used to determine snow depth. On the other hand, the laser snow depth sensor emits visible laser light and measure phase change to determine the distance to a target. Therefore, laser snow depth sensor cannot affect temperature, but generate representative error. In contrast, ultra snow depth sensor can affect temperature. Also, we included the accuracy and beam angle of each sensor in Table 1.

P4166 L2; I think that “spatial variability” would be the better expression (instead of distribution) ⇒ Revised: We replaced “spatial distribution” with “spatial variability” in the whole manuscript.

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! ⇒ Data quality was needed to remove unrealistic data (outliers and discontinuities) from analysis of sensor accuracy. It would be a good idea to include a comparison of results calculated using the method outlined in the paper and any methods proposed by manufacturers (e.g. quality numbers for SR50A). For the purposes of this assessment, a uniform, consistent method of QC was applied for all sensors, and that the development of new methods is a separate objective that will be considered elsewhere.

L17 -20 I do not understand this paragraph, please reformulate. ⇒ Revised: We removed “If all data are flagged during a one hour interval, a new base line is considered.

The data during the two hour period before the new base line are also flagged, to draw analyst attention to scenarios in which sensor performance may have been impacted" from this paragraph because we found this flagging procedure was not applied to snow depth measurements.

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? ⇒ Revised: We replaced FEL (base 20) and FEL (base 11A) with FEL (base 20, S2 in Figs. 5a and 6a) and FEL (base 11A, S3 in Fig. 7a), respectively. ⇒ The deviation of FEL 11A in Fig. 5a results from the loss of communication between the sensor and datalogger. When no response from the sensor is received by the datalogger within a specified time interval, a 'null' value of 6998 is recorded, which is subsequently filtered out by the quality control procedure.

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. ⇒ Revised: We added "(standard deviation of snow depth)" after uncertainty. ⇒ Revised: We added "Manual measurements were performed one a day with 0.5 cm resolution, and total number of manual observation during analysis period was 88".

L24 -26: In that context earlier publications on spatial variability and representativeness of snow measurements should be referenced and discussed. Examples for such studies are Lopez-Moreno et al. 2011, Neumann et al. 2006 or Grünewald and Lehning 2014; But there are many more. . .. ⇒ Revised: We already answered with respect to comment 3 (above).

L4169: L4 unclear - Is the mean of the 4 stakes of the target compared to the other targets? ⇒ That does not indicate the mean of the 4 stakes of the target compared to the other targets. We calculated the uncertainty for all pairs, on each base, and for each snow depth target as shown in Table 2. Thus, we mentioned "When comparing each snow depth target" to compare the uncertainty for each snow depth target ⇒ Revised: We replaced "When comparing each snow depth target" with "When comparing the uncertainty for each snow depth target".

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. ⇒ This statement should read 'spatial', rather than 'temporal' variation. While measurements were performed during snowfall in some cases, the spatial variability of snow depth across the site (as exemplified by the 'waved' surface in Fig. 2a, for example) is expected to be a more significant factor for uncertainty than the temporal variability, for the reasons indicated by the reviewer.

P4170 L6; what is meant with "significant" – statistically significant? Please clarify ⇒ Revised: We replaced "the differences in random errors among the different sensor types are not considered to be significant." with "the differences in random errors among the different sensor types are not great."

L7 The value stated in the text (10.7 cm) deviates from the one in Table 3 (10.8 cm) ⇒ Revised: We replaced 10.7 cm with 10.8 cm.

L8 I suggest to add, that the random error is expressed by the BRRMSE ⇒ Revised: "expressed by the BRRMSE" was added after "the random error ".

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? ⇒ The uncertainty of snow depth measured by snow depth sensors may be affected by limitations of the sensors, as noted on P4170 L15-17, The spatial variability of the surface is also likely an important factor. ⇒ We agree that it would be interesting to analyze separately the data outside of snowfall events, but as noted with respect to comment 2 (above), this is outside of the scope of this paper.

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. ⇒ Revised: We added FEL 11A – FEL 20 and FEL 11A – 12A as the distribution showing bimodal and mentioned "most of distributions were shifted below 1:1 line except for SR50A on base 11A vs. 20".

L26 spatial variability instead of spatial distribution ⇒ Revised: We replaced "spatial distribution" with "spatial variability" in the whole manuscript.

P4171 L20: The paper does not really "introduce a methodology" but is rather applies an established method. Please reformulate. ⇒ Revised: We replaced "introduced a methodology" with "applied an established method"

L22: No results of MAE and RMSE are shown in the paper ⇒ Revised: We removed equations of MAE and RMSE that were not used in the results.

L24: spatial variability instead of spatial distribution ⇒ Revised: We replaced "spatial distribution" with "spatial variability" in the whole manuscript.

L4172: The discussion should be enlarged: What do the presented results mean for the research area and how do they compare to earlier studies? L7: please add that the data were aggregated to 1 min ⇒ Revised: We already answered with respect to comment 3 (above) ⇒ Revised: We added "In addition, the data collected from automatic snow depth sensor were aggregated to 1 min".

Table1: I suggest adding a column showing the measurement precision as stated by the producers. ⇒ Revised: We added a column showing the measurement precision

Table 2, 3: units are missing ⇒ Revised: We added units in Tables 2 and 3.

Figure 1: legend and figure could be divided more clearly; this would improve read-

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ability; a scale bar should be added. ⇒ Revised: We divided legend from figure by surrounding them with a rectangular box and added a scale bar.

Figure2a on the picture the surface looks as it was waved? Is that true? What would be the reason? ⇒ The surface of the site was, in fact, waved, as shown in Figure 2a. This is believed to have resulted from the combination of the non-packed/lower density nature of snow in the surface layer, wind speed and orientation, and the resulting surface flow field due to local topography and site installations. The observers remarked that the surface layer appeared to behave like a fluid, and that this wave-like structure persisted at the site due to subsequent freezing and/or compression.

Figure 2, 9, 11: enlarging the Figure would improve readability ⇒ Revised: We enlarged Figs. 2, 8, and 10.

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. ⇒ Revised: We agree that a solid line is inappropriate and removed a solid line from Figs 7a, 9, and 11.

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

Grünewald, 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

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 12, 4157, 2015.

Please also note the supplement to this comment:
http://www.hydrol-earth-syst-sci-discuss.net/12/C5117/2015/hessd-12-C5117-2015-supplement.pdf

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