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Comment

Interactive comment on “Intercomparison of snowfall estimates derived from the CloudSat Cloud Profiling Radar and the ground based weather radar network over Sweden” by L. Norin et al.

Anonymous Referee #2

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The manuscript presents a quantitative assessment of snowfall estimation from two observing systems: one based on a ground radar network over Sweden and the other based on a space-based profiling radar (CloudSat). Method of assessment is by statistical analysis and evaluation of each system with reference to the other. Unfortunately, there is no superior independent ground truth reference to evaluate each system's performance. To compensate for this natural limitation, the authors perform an extensive statistical analysis that overall leads to convincing and correct inferences and results. The conclusion that each system has problems with light or shallow snowfall identifi-

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cation has important implications since light snowfall is a common occurrence, or that CloudSat profiling radar is insensitive to heavy snowfall, or that distance to radar station significantly impacts the quality of radar observations of snowfall. Based on these general strengths the manuscript would be important to the scientific community dealing with this very challenging problem and thus should be considered for publication.

A weakness of the manuscript that I think the authors can fix is poor English in some instances. Overall, the manuscript reads well, but here and there there are grammatical errors that need to be fixed.

Another general weakness is that sometimes it is hard to understand the fact that the performance statistics, e.g., false alarm, hit rate, etc, are relative, but to which system are they relative to? The authors should make very clear when they describe these metrics, that for instance "false alarm" rate of ground-based radar (please specify!) is with respect to CloudSat and vice versa. Here, there is no "ground truth". Again, it is hard to find a better ground truth for snowfall rate than radar-based systems, but the authors need to emphasize this when they refer to performance metrics. Or, they should clarify this head-on.

Also, I noticed that the authors refer to "clear"sky statistics. Do they mean "no-snow", the latter including cloudy and non-cloudy atmospheres?

Question/suggestion to the authors" Would it be possible to also include in the manuscript general meteorological conditions of the dataset? Surface temperature, relative humidity, etc? These are important since "cold snowfall" and "warmer" snowfall regimes for example have different responses - See the paper of "kongoli et al. 2015 in JGR, Atmospheres".

I would also have liked to see one or two cases of specific snowfall storms analyzed in more detail. That would have provided more depth and insight to the manuscript to make the points the authors rightfully make. Despite all this, the manuscript has publication merit and quality and therefore is recommended for publication.

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Interactive comment on Atmos. Meas. Tech. Discuss., 8, 8157, 2015.

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