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Atmospheric Measurement Techniques
Discussions

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.

L. Norin et al.

lars.norin@smhi.se

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Response to Anonymous Referee #1

We thank the referee for his/her constructive comments and suggestions that have led to clear improvements in the manuscript. Below, please find a point-by-point reply to the comments (reproduced in italics).

General Comments

The authors compare two data sources of snowfall, a parameter notoriously difficult to measure with any instrument. The intercomparison shows encouraging agreement. In general, I am happy with the selected parameters (POD, FAR, HR, KSS and pdfs), the conclusions are based on results, and the amount of data is larger than in many other projects due to use of several years and several radars.

I see the weaknesses of this paper in description - and perhaps even preprocessing – of the data used, its uncertainties, and in setting the work in context. In my opinion this is an important weakness because the paper otherwise is lacks novelty compared to e.g Cao 2014, unless the differences in datasets are clearly defined, as the concepts, ideas and tools are largely the same.

We thank the reviewer for the encouraging comments. A paragraph that discusses the previous work by Cao et al. and Smalley et al. has been added to the introduction. We have emphasised that in this study we have used a completely different radar data set, over a completely different region at much higher latitudes, where meteorological regimes can be quite different.

Also I see a weakness in proofreading. With several authors from different institutions, the team has somehow forgotten to check that the different paragraphs match, that the same issues (as blind zone) is not explained over and over again and that all methods used are documented somewhere. Example of this is the weak link between Figs 3 and 4 and the describing text.

The text has been revised to ensure that the paragraphs match better and that the text flows more evenly. Figure 3 has been removed from the manuscript.

As you comment in conclusions (P8177 L24) major source of uncertainty in ground-based weather radar estimates is the selection of Z-S equation. "Quote: Our preliminary comparison of snowfall distributions employing various ZS relationships shows that there exists a large room for improvements of Swerad relationship has been one of the chronic problems, often discussed widely in the scientific community." But I can *not find in other parts of paper anything about this "preliminary comparison"*

The comment has been removed from the conclusions to avoid confusion.

The preliminary comparison was actually performed by the authors but the results were not included in the paper as they do not directly fit to the main message of the manuscript. In the comparison we examined the sensitivity of snowfall distribution to various Z–S relationship and how CloudSat can be used to constrain it. These results may be a part of a follow-up paper.

*You do not even mention which Z-S equation(s) you used to process the Swerad data here, and why. The only one mentioned in the paper is Z=aR**B with a=200 and b=1.5, very near but not equal to what Marshall and Palmer (1948) used for rain (b=1.6), in context of correcting the radar data with gauges. So, please, describe what ZS equation was used, how it was selected, and speculate of effect of selection of this equation to your results. The issue is indeed "discussed widely in scientific community", as an example there is a nice overview of these in Rasmussen et al (2003) Snow Nowcasting Using a Real-Time Correlation of Radar Reflectivity with Snow Gauge Accumulation in JAMC. If you have used the is Z=aR**B with a=200 and b=1.5, compare it to the values given by Rasmussen et al.*

The Nordrad dataset does not use a Z–S relation. One of the original reasons for this study was actually to see if Nordrad's snowfall estimate could be improved by introducing a Z–S relationship. Nordrad applies the relation $Z = 200R^{1.5}$ regardless of precipitation phase. The error introduced by this is partly corrected by the adjustment to rain gauges. We have added/emphasised this in the manuscript. A comparison of Nordrad's Z–R relation to one of the Z–S relationships, described by Rasmussen et al., has also been added.

Using pixel-to-pixel comparisons and long time series has provided the authors with plenty of data points. However the allowed disparity in time (15 min) and place (2 km) are not balanced in my eyes. If a precipitation system moves 60 km/h, it moves 15 km

in 15 minutes. Grouping to rings with different radius helps somewhat, but I think the spacing of rings (15 km) is quite small. I would like to see this uncertainty discussed more than the comment P8167 L 7 "this is likely to introduce some uncertainty".

We thank the reviewer for pointing this out. The maximum allowed temporal disparity is actually 7.5 min (as Nordrad updates every 15 min). This has been corrected in the manuscript. The average temporal disparity is thus less than 4 min, which is reasonably close to the maximum allowed spatial disparity of 2 km. We have added a paragraph discussing this.

The authors do not compare their approach or results to similar intercomparison studies, or other validation of the same instruments. An interesting reference would be the paper by Cao et al (2014), using Nexrad/NMQ as a reference. I think there should also be available some reference to the overall performance of the Swedish radar network as compared to gauges, did you look for one ?

We have added a discussion on the results by Cao et al. and included a reference to Berg et al. that discusses the performance of the Swedish radars as compared to rain gauges.

In introduction, properties of ground-based radars and CloudSat are compared to gauges. I would like to see a short paragraph about other satellite missions, especially NASA's Global Precipitation Measurement GPM, but it would not harm to comment the products of geostationary satellites either.

Indeed GPM is very useful in mapping precipitation. It is now mentioned in the manuscript. Geostationary satellites also help in monitoring precipitation, but for a very high latitude country like Sweden, their usefulness is limited due to high viewing and relative azimuth angles, especially in the northern parts of Sweden.

Specific comments

P859 L16: connection between timely information of snowfall and agricultural industries *remains unclear to me.*

Snowfall can have both positive and negative impacts on agriculture. For some crops, the sudden arrival of snowfall can have a devastating effect, while for some fruits snow cover can actually protect them from very low sub-freezing temperatures.

P861. Please clarify "insensitive to vertical location of the precipitation system?" Do you mean that if there is overhanging precipitation (virga, snow evaporates before reaching surface) satellite considers it as real snowfall ? Or are you talking about ground-based radars overshooting shallow precipitation? You mention several times that CloudSat can also miss shallow snowfall that forms in its blind zone, so none of the data sources is insensitive to vertical location of the precipitation system in my eyes.

The sentence has been rephrased.

P8163 L16. "enhanced sensitivity of C-band" might be an useful remark to American readers. In Europe, there are no S-band radars in snow region anyways, and the benefit of sensitivity difference can be overrun by beam overshooting (at distance where it would be important, the radar beam may already be above the precipitation layer). Also note that S-band typically uses 3x as high transmit power to compensate for their lack of sensitivity. You come back to this on P8176 where the "Swerad's decreased sensitivity for increasing distances" is "mainly" the reason for differences. It is actually possible to calculate at what distance the sensitivity becomes an issue, if you know the sensitivity (MDS) of the radar, and the used ZS equation. Different combinations which I tried quickly give values between 80 and 250 km.

The sentence mentioning the enhanced sensitivity of the C-band radars has been removed. The decrease in the ground-based radars' sensitivity with distance can be seen in Figure 4, where the minimum (measured) snowfall intensity is seen to increase for increasing distances.

P8163 L25: For clarity, add "The Swedish radar measure..." – this is no universal property of all the radars in the world.

Done.

P8163 L26: You write about "minimum reflectivity" being below -30 and its upper limit increasing. An established term is Minimum detectable signal MDS, which is defined at range of 1 km and then increasing with range. See Doviak Zrnic p. 60 and chapter 6.

The sentence has been rephrased to include the MDS.

Close to the Swedish radars the MDS correspond to much lower reflectivity values than $-30\,\mathrm{dBZ}$. However, the lowest reflectivities output by the radar, regardless of distance, is $-30\,\mathrm{dBZ}$. For this reason we here refer to it as "minimum reflectivity".

P8165 . You earlier mentioned the gauge network is not dense enough, here it is used for correction of radar data with no remarks. Is it a source of uncertainty? Can you find an estimate of how much the original radar values are changed with this adjustment?

The sparsity of the gauge network, in both time and space, is indeed a source of uncertainty. This is now emphasised in the description of the gauge adjustment-method, in which a week worth of data are needed to provide sufficient statistics. The gauge adjustment is based on a second degree polynomial as a function of distance to the radar. For the studied years the average parameters were: $F = \log\frac{G}{R} = 2.1\times10^{-1} + d\times1.0\times10^{-3} + d^2\times1.3\times10^{-5}$ where d is the distance from the radar. The adjusted rain rate is calculated as $R_{adj} = R_{orig}10^F$ and is shown in the figure below.

P8165 L10. Vertical profile or reflectivity is, in addition to partial or complete overshooting, also effected by microphysical properties of the snowfall process: hydrometeor in upper parts of cloud are smaller and grow as they fall (consuming cloud drops which are invisible in radar).

Correcting the radar data using VPR would most likely improve the quality of the composites. However, VPR is not available for the Nordrad dataset. A discussion about VPR has been added to the revised version of the manuscript.

P8165 L10 You mention the height of the radar beam growing with distance, but not the width of the beam. At long distances (say 250 km) the beam may be 5- 6 km wide, so the nominal resolution of 2x2 km on Nordrad data may be artificial. I know this is an issue in edges of the network and at sea areas, but is the Swerad network really so dense you don't need to care ? You could calculate this with the maximum distance from radar you use (>199 km) and Swedish beam width (0.9 degrees).

We have added the a comment about the increased measurement volume as a function of distance.

P8167 There is no discussion of Figure 3. !!!

Figure 3 has been removed.

P8167 L18 / Figure 4. "In addition to the ECDF for all distances, Fig. 4 shows the corresponding functions for the various range bins defined in Sect 2.3.". I do not see the ECDF for all distances, and it is not fully clear for me how you defined several ECDFs for CloudSat. And I can't see them either, apart from first few millimeters, as the Swerad-ECDFs are drawn on top of them. To me it looks like you have 11 lines for radar and 5 lines for satellite.

The ECDF for all distances was labelled "$> 0\,\mathrm{km}$". For clarity this has been changed to "all dist." The ECDFs for CloudSat are calculated in the same way the as the ECDFs for Swerad. For clarity, they are now drawn on top of the Swerad lines.

P8168 L20 "This suggests that either Swerad overestimates the snowfall rate for large reflectivities" ...which may be result of bright band contamination, or poorly selected ZS equation, or what do you think? Please speculate different possible reasons for such overestimation (knowing that Swerad data has been corrected with gauges).

A discussion on this has been added.

Technical corrections

P8163. In my understanding even the radars owned by the military are part of "Swedish radar network" (SWERAD, a col laboration between SMHI, FMV and the Swedish Armed Forces which was established in the 1980s to ensure the operation of Sweden's weather radar network). See https://www.defencetalk.com/saab-modernizes-swedish-weather-radar- network-42851/ is this old knowledge?

The reviewer is correct. We have reformulated the sentence.

P8164 Estonian EMHI is now EtEA Estonian Environment Agency (EtEA)

Done.

P8167 line 1 Range bins, would more naturally be called range rings to me. In radar world the word "bin" is often reserved to "a pixel in polar coordinates". Done.

P8168 (and elsewhere) Hanssen–Kuipers skill scores are, according to Wilks' (Statistical Methods in the Atmospheric Sciences)and Jolliffe's recent textbooks, more correctly caller Peirce skill Scores. I think this is becoming a standard in meteorological world.

Done.

P8176 L26. Change order of words to make sure you do not mean the clutter is detected by ground-based radars.

Done.

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

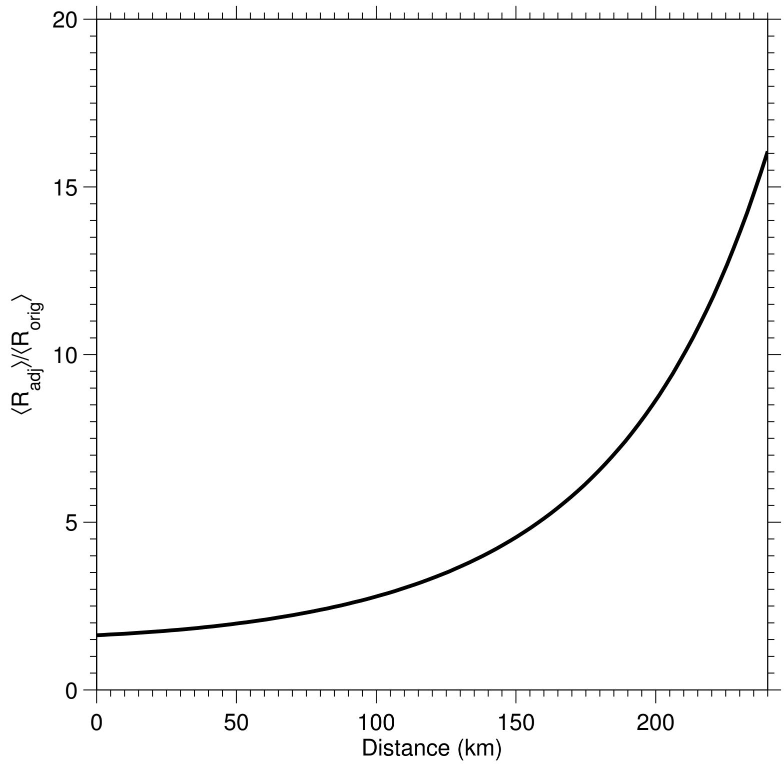

Fig. 1. Average correction to rain rate as a function of distance to the nearest ground-based radar.