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Atmospheric Measurement Techniques Discussions

Interactive comment on "Close-range radar rainfall estimation and error analysis" *by* R. van de Beek et al.

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General Comments

This paper does an excellent job of separating the potential errors involved in estimating rainfall rates from single polarization radar reflectivity observations. The concentration on a single range gate (very) close to the radar is an original way of removing the effects caused by elevated beam heights such as brightband and wind drift. I believe it adds significantly to the methodology of operational rainfall retrieval using radar.

I believe that the paper is worthy of publication but could benefit from a more complete discussion of a couple of issues.

Specific comments

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Discussion paper

I think the authors should discuss to some extent the uncertainties in the calculations of Z and R from the observed DSD. Most notably for a 1 minute sample how many drops are typically observed? An example of this is shown to some extent in figure 4 (in a very nice way), but as we know that Z is very sensitive to the concentration of larger drops if there are small numbers of such drops counted in 1 minute then the uncertainty in Z can be large. Also the authors use only the Parsivel data in their analysis but the drop size bins of the Parsivel are pretty wide for large drops, so assigning a suitable size to an individual (or small number) of drops in these bins is problematic. Can the authors comment on this?

One of the final conclusions is that using the overall Z-R found from all the events using Z and R values calculated from the observed DSDs improves rainfall retrieval. How is this different from finding a local Z-R from comparisons of radar reflectivity and rain gauges? On the other hand, I can see the advantage of the Z-R steps method of intra-event DSD calibration, but can't think how this could be applied operationally, and even if it were it would have limited areal applicability as the inherent assumption is that the rain DSD is spatially variable. Can the authors please comment on these issues?

I'd like to see the information shown in the legends of the plots in figure 9 presented in a table. In particular a table of a and b coefficients of the Z-R would make the range of value combinations easier to assess (the print is also very small and hard to see). I think this is important information as it would allow readers to see how close these come to the alternatives to the Marshall-Palmer Z-R, such as the US National Weather Service convective Z-R.

Also, for the relationships found from these plots, is there statistical evidence (R2 values for example) that the nonlinear fit is better than the linear. I would agree that the nonlinear fits look better and often the statistical tests are inconclusive due to the concentration of points near the origin, but it would help justify the choice.

Page 9: Is it not possible to use a second elevation to fill in clutter-contaminated range

gates? Would this be better than trying to subtract what could be a varying clutter signal from the observed reflectivity?

Technical Corrections

Page 8, line 1: Is this calibration exactly 1 dB or was this value approximate or chosen for simplicity? Page 8, line 8: ‘speed” should be “velocity”.

Page 9, line 8: Should read “an operational Doppler. . .”

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