Reply to comments of Neil Fox on Close-range radar rainfall estimation and error analysis

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 bright band 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.

The authors would like to thank Neil Fox for his comments. Below we will give a reaction to each separate comment.

1. 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?

DSD sampling effects (both in terms of number of drops per unit time and diameter class widths) are indeed relevant aspects. We will add a short discussion to the paper regarding these issues. Even though this issue is relevant, we do not think it will greatly influence the outcome of this paper (see e.g. Salles and Creutin, 2003, Tokay et al., 2005, Uijlenhoet et al., 2006, and Leijnse and Uijlenhoet, 2010)

Salles, C. and Creutin, J.-D.: Instrumental uncertainties in Z−R relationships and raindrop fall velocities, J. Appl. Meteorol., 42, 279–290, 2003.
Tokay, A., Bashor, P. G., and Wolff, K. R.: Error characteristics of rainfall measurements by collocated Joss-Waldvogel disdrometers, J. Atmos. Ocean. Technol., 22, 513–527, 2005.
Uijlenhoet, R., Porrà, J. M., Sempere Torres, D., and Creutin, J.-D.: Analytical solutions to sampling effects in drop size distribution measurements during stationary rainfall: Estimation of bulk rainfall variables, J. Hydrol., 328, 65–82, 2006.
Leijnse, H. and Uijlenhoet, R.: The effect of reported high-velocity small raindrops on inferred drop size distributions and derived power laws, Atmos. Chem. Phys., 10, 6807-6818, doi:10.5194/acp-10-6807-2010, 2010.

2. 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?

Our main reason for using DSD data to infer optimal Z-R relations is that these would be independent of all other potential radar error sources. This allows us to really investigate the effect of all of the different error sources. Using a radar-gauge comparison to infer a Z-R relation would result in a Z-R relation that compensates for all other remaining error sources.

Regarding the operational applicability of using separate Z-R relations for different types of events, we agree that this is not possible without an unrealistically dense network of disdrometers. Our purpose was mainly to quantify the magnitude of the errors when using improper Z-R relations. Having said this, one could of course think of an application where data from a limited network of disdrometers could be used to derive event type-specific Z-R relations (for e.g. convective, stratiform, etc. rain) that could then be operationally applied (reverting back to climatological relations if no rain occurs over any of the disdrometers).

3. 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.

We will include a table with Z-R coefficients and exponents. Note that the left-hand panel of Fig. 10 also shows a summary of these relations.

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.

We have no statistical evidence that the nonlinear fit is better than the linear fit. The problem with such statistical evidence is that it is probably easy to find statistics that would show that either linear or logarithmic fitting would be best. The main reason for using non-linear fits is that in most practical applications high rainfall intensities are much more important than low rainfall intensities. Using logarithmic fitting would result in the difference between 0.01 mm/h and 0.1 mm/h being as important as the difference between 10 mm/h and 100 mm/h. Furthermore, due to the highly skewed distribution of rainfall intensities, low intensities naturally receive much weight. We do not want to increase this weight even further by using logarithmic fitting.

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?

Using a higher (uncontaminated) elevation to fill in a clutter-contaminated pixel would indeed be helpful. However, for this event and pixel, we found that most of the lower elevations are still contaminated by some (side lobe) clutter, so we did not use this technique.

Page 8, line 1: Is this calibration exactly 1 dB or was this value approximate or chosen for simplicity?

The value was indeed not exactly 1 dB, but the uncertainty in this number is such (a few tenths of a dB) that rounding to 1 dB is justified.

Page 8, line 8: 'speed" should be "velocity".

We will modify this.

Page 9, line 8: Should read "an operational Doppler

We will modify this.