Atmos. Meas. Tech. Discuss.,
doi:10.5194/amt-2016-14-RC4, 2016

Atmospheric Measurement Techniques Discussions

Interactive comment on "Dual-polarization radar rainfall estimation in Korea according to raindrop shapes using a 2D Video Disdrometer" *by* H.-L. Kim et al.

Anonymous Referee #4

Received and published: 28 February 2016

1 General comments:

This paper presents work to determine and test appropriate dual-polarisation radar rainfall-rate algorithms with for an S-band radar, based on radar, 2DVD and rain gauge measurements. Adding additional drop-size/axis-ratio relationships to the published literature is always potentially useful, as is the results of practical attempts to compare different possible dual-polarization rainfall-rate estimation algorithms. Given that many national meteorological agencies are in the process of implementing dual-polarization rainfall-rate estimation algorithms, the publication of what one particular organisation is doing is likely to be very helpful to others.

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Discussion paper

However, I have some serious reservations about the core of this paper: the need for a specific drop-size/axis-ratio relationship for Korea. I agree that the raindrop axis ratio is one of the parameters to which dual-pol radar measurements are particularly sensitive. However, the need to have different drop-size/axis-ratio relationships for different regions still seems to be an open research question. You appear to conflate the well-documented regional variation in DSDs with variation in drop-size/axis-ratio relationships, and assume without comment or explanation that there is indeed significant regional variation in drop-size/axis-ratio relationships.

The model given in the Beard and Chuang 1987 paper that you cite essentially gives that drop shape varies with temperature and pressure. Average temperatures and pressure do of course vary from region to region, and would therefore give rise to some variations in drop shape around the world. Some (unpublished) calculations of my own, based on an implementation of the Beard and Chuang 1987 model suggest that a pressure change from 97325 to 105325 Pa results in a 0.006% change in axis ratio for a 6 mm volume-equivalent-sphere diameter drop, which is quite insignificant and can be neglected. For a temperature change from 0 to 20°C I find a percentage change of about 1.1%, which is potentially a little more significant. However Thurai and Bringi 2005 report a standard deviation in their measured axis ratio for 6-6.5 mm diameter drops of around 11% of the axis ratio. I therefore suggest that temperature variation in drop shape is likely small compared to the natural variation in drop shape at that temperature, and so is not likely to be a major factor.

The only works that I have come across in the literature that specifically address regional variation of drop-size/axis-ratio relationships are Marzuki et al. 2013 and Gorgucci et al. 2009. The former is concerned with 2DVD measurements in Sumatra, and does find a somewhat different drop-size/axis-ratio relationship than others in the literature, although they also express concerns about the siting of their 2DVD. They suggest that atmospheric conditions are unlikely to be a significant factor, but suggest that variations in the amount of raindrop collisions may be a possible cause. Gorgucci et al. 2009 present a method for determining drop-size/axis-ratio relationships from radar measurements, and report similar relationships from Brazil and Italy, but a different relationship for Florida (albeit all in line with other, previously reported values in the literature), but I don't think their results could be taken as definitive. They also suggest drop collision and the resultant oscillations as a possible source of the variability.

Additionally, the work of Thurai and Bringi 2005 was carried out under very controlled conditions, with careful repeated calibrations of the 2DVD. I am concerned that a subset of a year's worth of data under less controlled conditions (especially for wind speed and calibration) is unlikely to be as accurate as Thurai and Bringi's work and that, whilst it is possible that a notably different drop-size/axis-ratio relationship exists in Korea, the measurements here may not be sufficiently good as to be able to prove it. I would also point out that in Thurai and Bringi 2005 and Thurai et al. 2007 a significant amount of effort in to explaining their calibration and interpretation of the 2DVD measurements made, aspects that you cover in a lot less detail.

At the moment, whilst you see clear benefits to your new relationship when comparing (what I take to be) purely 2DVD derived parameters (table 3), you see very little improvement in the actual radar measured values (table 4), which suggests to me that the different axis ratio is either not different enough to be significant, or that the measured difference is at least partly an artefact of the 2DVD data and not fully representative of the real rain. The very small improvement that you do see may be down to deriving the relationship for these rain events at the disdrometer, and then carrying out the evaluation on the same dataset.

I think there are two possible options here. The first is to provide a robust analysis demonstrating that you believe your measurement values to be accurate to a level that proves that the axis ratios that you have determined are accurate to a level that allows significant differentiation from the other axes ratios in the literature (of course, assuming that is the case). This would also require a significant beefing up of your explanation of how you determine axes ratios from the 2DVD data, or at least the uncertainties associated with that. You would also need to provide more motivation for the regional variability of the relationship - references, possible mechanisms for the variability, etc. A positive demonstration of significant regional variation in drop-size/axis-ratio relationships would in my opinion be a very interesting result.

Alternatively, you could treat the measured axis ratio relationship more as a slight variation of of those already in the literature (it is very close). You could then focus the paper more on the dual-polarisation algorithms being developed and deployed in Korea - information on what a national meteorological organisation is implementing is in my opinion interesting and worthy of publication in its own right.

2 Specific comments:

Page 2, line 29: I don't agree that "[t]he raindrop shape is defined by the shape-size relationship of a raindrop". The shape is rather defined by interactions between the drop and the atmosphere (and other drops). However, the average shape of a raindrop can be inferred from its size.

Section 2.1: Additional information about the siting of the 2DVD should be given here: things like closeness to buildings could be significant.

Page 5, line 1: You say that you use the 0.0° elevation "to avoid effects from beam blocking and ground echoes". Surely that would be further reduced by using the 1.6° elevation data? You should say why you opted for 0.0° rather than 1.6° (to have measurements as close as possible in space to the 2DVD?).

Figure 3: You seem to be showing some of the best available cases. Better might be to show a representative sample, including some of the best and some of the worst.

Section 3.2: You should give more information on the dataset you were working with here. What size of diameter bins were you using, what was the minimum number of drops in a bin included in the final fitting, what were the axis ratio standard deviations, etc.?

Page 8, line 12: It may just be a typo, but I think you should be using a mean canting angle of 0° and a standard deviation of 7°, not the other way round.

Section 3.4: It is not quite clear here whether you are applying these calibration corrections only in table 5, or also in table 4. If in table 4, then assuming the new axis ratio calibration values in all cases would perhaps unfairly disadvantage the other axis ratios considered. Additionally, you should explain why you consider only light rain events for the calibration.

Section 4.2.1: I think that here Re is being determined by applying the various algorithms to radar polarimetric variables derived from the 2DVD data. Is that correct? Either way, you should make this clearer.

Table 3: For the Kdp, Zdr algorithm, I suspect the 0.1 correlation values are wrong (compare with figure 6).

Section 4.2.2: Why do you carry out this validation only on 18 of the rain events and not all 33?

3 Technical corrections:

Page 1, line 26: I think you mean different not differences here.

Page 6, line 10: I think you mean that the drop fall velocity formula used was that derived by Brandes et al.

Page 7, line 1: In situ rather than "in suit".

4 References:

Thurai, M. et al.: "Drop Shapes, Model Comparisons, and Calculations of Polarimetric Radar Parameters in Rain", J. Atmos. Oceanic Technol. 24, 1019-1032, June 2007

Gorgucci, E., Chandrasekar, V. and Baldini, L.: "Can a unique model describe the raindrop shape-size relation? A clue from polarimetric radar measurements?", J. Atmos and Oceanic Technol. 26, 1829-1842, September 2009

Marzuki, Randeu, W.L., Kozu, T., Shimomai, T., Hashiguchi, H. and Schönhuber, M.: "Raindrop axis ratios, fall velocities and size distribution over Sumatra from 2D-Video Disdrometer measurement", Atmos. Res. 119, 23-37, January 2013

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