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Atmospheric Measurement Techniques Discussions

Interactive comment on "Raindrop Fall Velocities from an Optical Array Probe and 2D-video Disdrometer" *by* Viswanathan Bringi et al.

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This paper describes results from two measurement campaigns with a Meteorological Particle Sensor (MPS) and a 2D-Video Disdrometer (2DVD). The analyses presented in this paper are focussed on the fall speeds of droplets measured by the different instruments, and whether these deviate from results from laboratory experiments (super- or sub-terminal fall speeds). Observed sub-terminal fall speeds are then linked to turbulence intensity. I think this is an interesting paper. It contributes to the scientific discussion on the puzzling super-terminal small raindrops by showing results where these were not observed. However, the paper would benefit from a clearer description of its aims, and, if possible, stronger conclusions. As far as I understand it, there are three main messages in the paper: 1) there is no evidence of super-terminal raindrops

(contrary to Montero-Martinez et al., 2009 and Larsen et al., 2014); 2) the fall velocities of real drops closely follow the relations found by Gunn and Kinzer (1949) down to very small drops; and 3) there is a clear effect of strong turbulence on the mean and the standard deviation of fall speeds of drops of a given diameter. If this is indeed the case, then I think this should be more clearly stated in the introduction, and should be discussed more elaborately in the conclusions. So I think that after revisions, this paper is suitable for publication in *Atmospheric Measurement Techniques*. Specific comments are given below.

Specific comments

1. In the introduction it should be more clearly stated what the exact aims of this paper are.
2. On lines 66-74, the use of a DFIR is discussed along with its effects on the local wind field. For studying the relation between turbulence intensity and raindrop fall speeds, how does this double fence affect the turbulence just above the instrument? I can imagine that by reducing the average wind speed, the turbulence is also reduced. On the other hand, as stated on line 74, the fence itself also generates up- and downdrafts. I think that the effects of the use of a DFIR on the results presented in this paper should be discussed and, if possible, quantified.
3. In Fig. 1 (especially panel a) the MPS seems to detect slightly (but systematically) lower fall velocities than the 2DVD in the Greeley data. This is not the case for the Huntsville data (Fig. 3). Please give an explanation for this.
4. On lines 208-215, the correlation between E on the one hand, and the mean and standard deviation of the raindrop fall speeds on the other is discussed. I agree that this correlation is there. However, judging from Fig. 4, I think there is also

some correlation with the rain rate R (especially the peak at 10 UTC). Please elaborate on the role of the rain rate for these correlations.

5. On lines 224-225, the observed near-linear decrease of the mean fall speed with turbulence intensity (or at least its proxy E) is mentioned. How significant is this relation?

6. On lines 247-254, the results presented by Montero-Martinez et al. (2009) are compared to those presented in this paper. What could be the explanation for this difference? Could it be something similar to what is discussed in the next paragraph (lines 255-262) about the findings of Larsen et al. (2014)? Please elaborate on this.

7. On lines 272-281, the relation to the findings of Stout et al. (1995) are discussed. Is there an empirical relation between E and the rms velocity fluctuations due to turbulence? If so it would be interesting to see whether the 35% reduction in mean velocity is observed at similar rms velocity fluctuations-to-terminal fall speed ratios (0.8).

Technical comments

1. In Figs 2b and 4b, would it be possible to use a second y-axis for R instead of presenting $R/10$ on the existing y-axis?

2. On line 275, “greater that” should be “greater than”.

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