

Review of AMT-2018-63

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Manuscript title: Characteristics of vertical velocities estimated from drop size and fall velocity spectra of a Parsivel disdrometer.

This manuscript reports about the estimation of vertical air velocity by disdrometer (Parsivel) measurements. The estimation is based on the comparison between measured (by Parsivel) and theoretical vertical drop velocity. In particular, the mean measured drop velocity is calculated from Parsivel data. The estimated vertical air velocity is compared and validated with the vertical motion measured by a collocated ultrasonic anemometer. One case study, during the monsoon rainy season in South Korea, is analyzed at three different measurement sites. The characteristics of DSD parameters (i.e. radar reflectivity, rain rate, mean mass diameter, etc.) are analyzed with respect to the upward or downward estimated air motion.

The structure of the paper is linear, but at the same time many inaccuracies both scientific/descriptive and language (the paper should be checked by a native English) can be found within the paper. My main concern is related to the unsuitableness of the analyzed case study to validate the vertical air motion estimate from Parsivel measurements. The vertical air velocity mainly ranges between -0.5 and $+0.5 \text{ ms}^{-1}$, but nothing is reported about the measurement uncertainty of the ultrasonic anemometer as well as the correction of the theoretical drop fall velocity due to the air density. Because of the very low values of vertical air motion, even during convective precipitation, the analysis carried out by the authors does not clarify the doubt that the vertical velocity estimates are within the measurement and process uncertainty. Due to these general considerations, I do not retain that the manuscript is ready to be published on the Atmospheric Measurement Techniques journal. I encourage the authors to deepen investigate the methodology by considering other case studies (involving higher values of vertical air velocity). In the following, I report more specific comments.

- Line 108: the citations “Niu et al. (201)” and “Ulbrich (1992)” are not present in the reference list. Please, check all the reference list.
- Lines 116-118: referring to Tokay et al. (2009, 2014), how the Parsivel underestimation and overestimation of small and large drops, respectively, affects the calculation of the mean fall speed?
- Lines 142-146: what is the uncertainty of the ultrasonic anemometer measurements? This is a fundamental information needed to validate the air motion estimated from Parsivel data.
- Equation 2: is there a meteorological station (able to measure air pressure and temperature) collocated with the Parsivel and ultrasonic anemometer? This can be useful in a better quantification of the deviation from the drop fall velocity at sea level and in quantifying the difference between using the standard atmosphere equation and the measured temperature and pressure.
- Lines 159-160: there is no correspondence between what the authors say in the abstract (and within the text), that is the field observational site is on the Mt. Jiri at 1915 m above the sea level, and what reported here, that is the three measurement sites are at very lower altitudes. Please, uniform the information about the field observational sites.

- Lines 186-191: the authors cite about the analysis of 3D wind components as well as the vertical structure of the precipitation from dual-Doppler radar measurements, but data are not shown neither discussed. Please add an analysis on this or remove the statement. They also refer to “..a daily accumulated accumulated rainfall distribution..” but they refer to the case study (as correctly reported in the caption of Figure 4). Please, uniform the text to avoid misunderstandings in the reader.
- Figure 5 and relative discussion: I agree with the authors that the trend and Parsivel w and UVW w is similar but, as already reported in the introductory part, the very low values along the whole period cannot be useful to validate the procedure, in my opinion.
- Why Figure 6 for D4 shows three different fit lines? Do they refer to UP, DOWN and UP/DOWN together data? If it is the case, this should be mentioned in the next. Do they overlap at D1 and D2 site?
- Lines 240-242: there are several more recent papers (Tokay and Short, 1996, Bringi et al., 2003, niu et al., 2010 just to make a few examples) reporting different methodologies to discriminate stratiform and convective precipitation rather than a simple rain rate threshold.
- Lines 324-330: in my opinion this is a too strong speculation. The technique is surely promising but has to be tested in different conditions (i.e. more intense vertical winds) or the authors have to more discuss about the sensitivity of the ultrasonic anemometer used to validate the technique. “notably different characteristics in magnitude and signs and signs between the windward and leeward side...” are not so evident and in contradiction with what the authors report just below this sentence where they state the vertical wind range between -0.5 and $+0.5 \text{ ms}^{-1}$ for the case study.