**Atmospheric
Measurement
Techniques
Discussions**

Interactive comment on "A comparison of rainfall measurements by multiple instruments" by X. C. Liu et al.

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This paper describes a comparison of the performance of six different rainfall sensors over a 10-month period. Three of these sensors were designed to measure rainfall amounts (rain gauges), two to measure drop size distributions (disdrometers), and one to detect present weather (present weather detector). Comparisons are presented for rainfall intensities, rainfall duration, and (in the case of the disdrometers) drop size distributions. The topic of the paper is certainly relevant. However, there are several issues that need to be addressed. The way in which the accuracy of the different instruments is actually assessed needs considerable improvement, and conclusions about the accuracy of the different instruments are missing. Specific comments on the paper are given below.

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Specific comments

1. In the introduction, I think the authors should discuss the results presented by Sieck, L. C., S. J. Burges, and M. Steiner (2007), Challenges in obtaining reliable measurements of point rainfall, Water Resour. Res., 43, W01420, doi:10.1029/2005WR004519.

2. On p. 522, line 17, the uncertainty for the tipping bucket rain gauge is given as "± 0.2 mm under 250 mm h^{-1}". It is not clear to me what this uncertainty means. Is this the uncertainty per tip? If so, this is an uncertainty of 100% ! I think the authors should make clear what this uncertainty means.

3. In Section 2.1, the authors should discuss how the time series of tips of the tipping-bucket rain gauge are converted to rainfall intensities. This is very important as it greatly influences the results presented in the remainder of the paper.

4. In Section 2.1, could the authors note whether the gauges are heated?

5. In Section 2.2, the measurement principles of the present weather detector are not clear to me. I think an understanding of the measurement principle is very important for proper interpretation of the results, so please include a clear description of these measurement principles.

6. In Section 2.3, line 5, the authors state that "JWD's output is proportional to the size and fall velocity of the impacting drops". Although the output depends on both the size and fall velocity, it is not proportional to these. See Joss, J. and Waldvogel, A. (1977), Comments on "Some observations on the Joss-Waldvogel rainfall disdrometer", J. Appl. Meteorol., 16, 112-113 and Salles, C. and Creutin, J.-D. (2003), Instrumental uncertainties in $Z - R$ relationships and raindrop fall velocities, J. Appl. Meteorol., 42, 279-290 for more information on this.

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7. In Section 2.3, line 17 of p.524, I think the authors should cite Salles, C. and Creutin, J.-D. (2003), Instrumental uncertainties in $Z - R$ relationships and raindrop fall velocities, J. Appl. Meteorol., 42, 279-290 here.

8. In Section 2, a discussion should be included on the effect of the installation in general and the wind specifically on the different instruments. An inspection of Figure 1 reveals that there is a great difference among the instruments in how they are installed. For example, the tipping bucket rain gauge is installed on the ground, whereas the weighing rain gauge is installed about 0.5 m above the ground, and has a wind screen to avoid wind effects.

9. In Section 3, the time interval chosen for the analyses presented in this paper should be given (I believe it is 1 minute), and a brief discussion of the reason for this choice should be included.

10. All of the analyses of rainfall duration depend heavily on how the tips of the tipping-bucket rain gauge are converted to rainfall intensities and the chosen time interval. I assume that it is raining in a given time interval if the TBRG gives at least one tip, the WRG records a difference in weight, and the other instruments record at least one drop. Because the difference between a single drop on the one hand and a volume measurement on the other (a tip or a change in weight) can be large in very light rain, a comparison of rainfall duration based on these criteria does not seem very relevant to me.

11. On p.525, line 20, it is stated that "obvious discrepancies of observations are excluded". Please give a short description of how these discrepancies were detected and what caused them.

12. On p.526, line 5, it is stated that for the computation of the relative bias, "R_1 is the bigger one". I don't think this is a good idea, because the result will always be

positive. The information contained in the sign of ΔR on whether it is an under- or overestimation is then lost.

13. On p.526, the discussion of Fig.2 (p.541) on lines 10-17 implicitly suggests that TBRG is used as a reference. Although it is clear from p.528 that the authors do not intend this to be the case, I think it would be good to note this explicitly.

14. On p.527, lines 8, 9, and 11 (2x), the use of the word "bias" makes it sound like the differences are attributed to errors in the sensors. I suggest rephrasing, making use of words such as "difference". This also holds for p.530, line 21 (twice).

15. On p.527, line 13, the errors in the TBRG are attributed to the tip resolution of 0.2 mm. However, with different conversion of the tip times to 1-minute rainfall intensities, these errors would be much less severe.

16. On p. 528, an algorithm is presented for generating a reference rainfall intensity based on a weighted average of measurements from five sensors. The weights are proportional to the rainfall intensity itself. This algorithm makes no sense to me. There is no reason to believe that the reference rainfall generated by this algorithm is any better than simply taking one of the sensors to be the reference or taking the average over all sensors. Because of the higher weight on larger intensities, this algorithm is likely to create a reference that is biased toward higher intensities. I strongly believe that simply choosing a single sensor to be the reference (or possibly two sensors; each one for a different intensity regime) based on known performance characteristics from literature is better.

17. On p.529, Eq.(5), the second part (the sum) is incorrect. It should be something like

$$\sum_{i=1}^{N_{\text{class}}} N(D_i)D_i^x \Delta D_i,$$

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where N_{class} is the number of diameter classes and ΔD_i is the diameter class width of class i.

18. On p.529, Eq.(6), expression for N_0, explain that $\gamma(m+4)$ is the gamma function with argument $m+4$ (generally referred to as $\Gamma(m+4)$).

19. On p.529, Eq.(6), note that these variables can be computed using any combination of three moments of the DSD. The choice of moment orders 3, 4, and 6 make sense as these are related to the highly relevant variables rainfall intensity (R) and radar reflectivity (Z).

20. On p.530, line 1, "where $G = M_4^3/M_3^2 M_6$" is ambiguous (and even incorrect if interpreted according to mathematical conventions). It should be "where $G = M_4^3/(M_3^2 M_6)$".

21. On p. 530, line 7, I assume that by "drop numbers" the authors mean "number density (N_d).". If so, please put this in the axis label of Fig.6b (including correct units) as well.

22. On p.530, line 10, "Fig.3b" should be "Fig.4b".

23. On p.530, lines 18-19, I don't think you can conclude that the 2DVD "shows a better ability to measure large-size raindrops". It simply measures more of these raindrops (it is not necessarily better able to measure these).

24. On p.530, line 25 and on p.531, lines 8-9 it is stated that the JWD measures more small-size drops than the 2DVD. Because there are simply much more small drops, this would mean that the JWD measures more drops than the 2DVD. This contradicts the results shown in Fig.6b, where the 2DVD consistently measures more drops than the JWD. I think this discrepancy should be thoroughly discussed.

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25. On p.531, lines 13-22 and Table 4, I don't think this is very relevant information.

26. Section 4 (Conclusions) just contains a summary of the paper, and no conclusions are drawn.

27. In my view, the conclusions on p.532, line 26 through p.533, line 2 about the inaccuracy of the 2DVD cannot be drawn based on the results presented in this paper.

Interactive comment on Atmos. Meas. Tech. Discuss., 6, 519, 2013.