Manuscript Number: 10.5194/amt-2020-393

Effect of disdrometer sampling area and time on the precision of precipitation rate measurement

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Summary:

Stochastic simulations are used to determine the precision of rain detection and the uncertainty affecting rain rate estimates from a new precipitation sensor - the Differential Emissivity Imaging Disdrometer (DEID) - as a function of disdrometer collection area.

Overall assessment:

The sensor presented in this paper is definitively interesting and the problem of determining the minimum sampling surface needed to achieve a given level of accuracy on rainfall estimates is clearly relevant. However, I have serious doubts about the soundness of the results and conclusions drawn by the authors. My main point of criticism concerns the methodology used by the authors, which neglects many other major sources of errors and uncertainties in rainfall measurements and, in my opinion, is far too idealized to draw robust conclusions. My recommendation would be to extend the current work by taking into account wind + instrumental limitations as well. Also, I think the structure of the paper needs to be improved, with better literature review, more detailed and consistent description of the methods, and more in-depth analysis/discussion of results.

Recommendation: Major review

Major Comments:

- Introduction: instead of giving a lot of technical details about how rainfall sensors work, and which theoretical performances are advertised by the manufacturers, you could cite more in-depth scientific studies about true measurement uncertainties for different types of sensors, sampling areas and types of rainfall. For example, there are plenty of relevant studies that have looked at uncertainties affecting rain rate measurements using co-located gauges and disdrometers. Similarly, there are numerous papers where stochastic simulators are used to generate large numbers of theoretical drop size distributions for studying sampling uncertainties.

- Methodology: Annex 1A of the WMO 2018 report on operational measurement uncertainty requirements clearly states that uncertainties for liquid precipitation are seriously affected by wind. Your simulation study completely neglects this aspect. Similarly, the effect of instrumental limitations (e.g., border or edge effects, and software/hardware limitations) are ignored. All these are rather well documented in the literature and could have been (partially) incorporated into the simulation study. The problem is that by ignoring wind and instrumental limitations, you are severely underestimating the true uncertainty affecting the measurements and, as a result, overestimating the sensitivity to the choice of the sampling surface. For heavy rain events for example, which are often associated with higher wind speeds, the size of the sampling area might only play a minor role compared to the magnitude of wind-induced errors (>10%). This is quite important as wind-induced uncertainties will be there regardless of the used sampling area. I get that this is not the focus of your paper. Still, I think that by ignoring these effects, your conclusions become very questionable from a scientific point of view.

- Description of DEID: It is not clear to me how the drop diameters are inferred from the DEID. In Section 3, ll 107-108 you mention that *"the value of ΔD=0.25mm was chosen to approximate the spatial measurement resolution of the prototype DEID."*. This is the first time this binning parameter is mentioned in the paper. Section 2, which is supposed to contain the description of DEID and its working principle does not mention anything about this. Please elaborate and add more details about how you actually calculate your quantities based on the DEID measurements.

- The DEID is a new rainfall sensor. The reference paper (Singh et al., pending) is not published yet and it would be nice to show at least a picture of the instrument to help the reader better understand the technical specifications provided in Section 2.

- Methodology: Section 3.1 introduces the simulation approach which is based on Exponential distributions. But later, in section 3.3, we learn that actually, you used gamma DSD. Please clarify this apparent inconsistency. My advice would be to rewrite the entire methodology section using only gamma distributions (which are more common in modern studies).

- Compared with the other sections, the Results section is very brief. There's hardly any discussion about the consequences of this work and how it could be used to improve the accuracy of disdrometers. For example, for the Parsivel2, the effective sampling area is about 50 cm2, which, according to your results, would not be large enough for higher rain rates. Yet often, people operate these sensors at 1 min resolution (or even 30s) with intensities ranging up to >50 mm/h. How large would the relative errors be in this case and how feasible are the guidelines of WMO?

- Rainfall rates are important. But for remote sensing, other weighted moments of the DSD are equally important. For reflectivity, which is a higher-order moment, I assume that even larger surfaces are necessary. Instead of focusing solely on rain rates, the paper could benefit from additional analyses and simulations looking at other moments and relevant parameters.

- Application to DEID measurements (section 4.2): This part is the most interesting scientifically speaking because you actually try to compare your calculations to real data. But honestly, it's very hard to follow and I did not understand it. There are too many missing details/explanations in the text. How many cases did you look at? How did you select your examples? and what type of rain events do they correspond to? To me, it seems like you're basing your conclusions on the superficial analysis of only two 1-min samples. There is hardly any discussion/analysis that puts these results in a larger perspective over many different events. Finally, I find it worrying that you can't really explain the differences you see between the theoretical confidence intervals and the observations. It makes me wonder whether other important factors were at play (e.g., wind, instrumental limitations or biases in the DEID measurements). Most importantly, it makes me question the reliability of the simulation studies in the first place.

Technical comments:

- Title: You could make the title more specific and more aligned with the actual content of the paper by adding the keywords "idealized simulation study" or "Differential Emissivity Imaging Disdrometer". Yes, the approach itself is quite general, but your results are heavily focused on the DEID. The current title does not reflect this and should be changed.

- ll.59-66: this paragraph could be moved from the Introduction to section 2 and merged together with the text describing the DEID. More generally, I suggest to move most of the technical parts about the DEID to section 2 and restructure the introduction to address more relevant issues related to sampling surfaces in optical/video disdrometers and pros/cons of using small/large measurement surfaces.

- ll.74-75 *“In principle, the results should converge, although the Monte Carlo approach also facilitates the calculation here of the time required to measure the “first drop" in a precipitation event.”*

> Too vague. Please clearly state whether your own results based on stochastic simulation techniques are consistent with those obtained by Joss and Waldvogel (1969). Also, there are many other simulations studies that are similar to the one you performed. Please cite them and explain how your own findings compare to theirs.

ll.82-83, *“individual hydrometeor mass is calculated from conservation of energy, whereby the heat gained by the hydrometeor is equal to the heat lost by the hotplate when evaporating water”*

> The way this sentence is formulated can be misleading. It gives the impression that you can infer the individual masses for each raindrop. However, as far as I understood it, you can only infer the total mass of water on the plate. This means that if there are multiple raindrops on the plate at the same time, you can’t get the detail of each. Or did I miss something? Please clarify!

ll.138-139: A sample of drops is generated from Eq. (5), where $\Delta t = h/v$ is maximized for the value of v corresponds with the smallest droplet diameter

> Not clear. Please reformulate.

ll.139-140: Small drops may contribute negligibly to the precipitation rate but be the first detected, so the value of n(D) is taken from a gamma distribution rather than the exponential in Eq. (3)

> Not clear. Did you mean “can be detected first”?

- ll.146-148: *“Following the collection approach taken by Marshall et al. (1947), reproduction of a Marshall-Palmer size distribution is assumed to require collection of 100 drops. The time elapsed for the calculated incidence of 100 drops is t100 (Fig. 3). If fewer than 100 drops were obtained in Ncoll , a new sample of drops is obtained from Eq. (5) with an increased value of Δt.”*

> Marshall et al. (1947) never stated that you needed 100 drops to detect the onset of rain. Their study was primarily concerned with inferring the concentration and shape parameters of the DSD; a task for which you need a minimum number of samples (100) to get reasonably accurate results. I don’t see how this connects to the detection of the onset of rain.

- ll.178-179: The Poisson assumption you use in the simulations is quite important, since in reality, the raindrops are not likely to be distributed uniformly in the volume and unlikely to arrive on the ground independently from each other. Especially at the beginning and end of intense convective events, you can have substantial temporal autocorrelation in the arrival times, which means you probably need a larger area and more drops to achieve the same precision. This is probably a minor issue compared with other sources of uncertainty. Still, I think it’s worth discussing the potential errors that could result from a non-Poisson arrival process.