

Interactive comment on “Hydrometer classification from 2 dimensional videodisdrometer data” by J. Grazioli et al.

Anonymous Referee #4

Received and published: 20 April 2014

The paper by Grazioli et al. presents several issues that make me difficult to recommend publication.

1) The English should be greatly improved before submitting the paper to a reputable journal such as AMT. Starting from the first word of the paper (it reads Hydrometer instead of Hydrometeor), there are a large number of typos and incorrect and careless sentences that do not build confidence on the work.

2) There are several unsubstantiated statements in the paper, thus for instance the comment "Some commercial disdrometers (i.e., PARSIVEL), originally [sic, typo] designed for rainfall studies, provide an estimation of the precipitation type associated with each measurement [sic, typo] by making assumptions on fall velocity and and [sic,

C586

typo] equivalent rainfall intensity. For this reason, they are prone to unreliable estimates in complex site conditions.". One issue is the precipitation type classification and a different one is the spatial variability of the estimates. The sentence mixes both problems and does not provide any support for any of the two statements. Besides, there are 3 typos in just one sentence. Moreover, the same sort of PARSIVEL pre-processing and filtering is also required for 2DVDs (p6, sentence starting "The raw images need to be processed before being employed.")

3) Disdrometers are usually deployed in pairs to check for consistency. The paper does not provide enough information on the actual setup used in the three campaigns to evaluate the consistency of the estimates. A comparison of the results of two back-to-back 2DVDs would make the case for the method, but if that is not the actual setup then the whole exercise falls apart.

4) The wind effect needs to be addressed in relation to the representativeness of the estimates.

5) Section 3.2 is unnecessary, as it only reports information already published elsewhere which is not original from the authors, and of a general character, i.e. not specific to the case. Stating that "We used the SimpleMKL algorithm (Rakotomamonjy et al. 2008)" and then provide the parameterization would suffice. The same applies to figure 4, which is well-known and unnecessary for this paper.

6) More detail is required on the manual classification method.

7) The temperature interval in the top of figure 9 (and 10 and 11) is too wide, and therein useless.

8) Fig. 12. "... and temperature data are given by closely-located weather stations". Given the spatial variability of the estimates, one would expect an in-site estimate of the temperature, not a 'close' one.

9) Concerns about computational costs are not helpful without the actual details (how

C587

long does it takes, etc.).

10) My main concern is about the validation step (section 4.3). The authors use 300 samples for the training step and 100 for validation (actually, testing in the literature), but it seems that they do not use independent data from another episodes for a true validation of the generalization abilities of the algorithm. This would be a common pitfall meaning that you are capable of successfully discriminate different instances on your current case, but not necessarily on a different one. Also, since a 2DVD will always collect just a sample of the whole, far larger population, this is a critical point even for the same episode. As currently described in the paper, the work cannot say anything about the ability of the algorithm to generalize and that makes the method not really useful for the intended main application (ground-based quantitative evaluation of products coming from polarimetric weather radars; section 6).

Moreover, even if 100 cases were actually used for validation, that would mean little since the collecting area of the instrument is quite small. Training with 300 samples and validating with 3000 would make some sense and would help to make the case for the proposed method (always providing that a twin instrument 2 meters apart would yield the same results).

Interactive comment on Atmos. Meas. Tech. Discuss., 7, 1603, 2014.