Atmos. Meas. Tech. Discuss.,
doi:10.5194/amt-2019-466-AC2, 2020

Atmospheric Measurement Techniques Discussions

Interactive comment on "Automated precipitation monitoring with the Thies disdrometer: Biases and ways for improvement" *by* Michael Fehlmann et al.

Michael Fehlmann et al.

michael.fehlmann@unige.ch

Received and published: 17 June 2020

Specific comments

2.1 Figure 2 has to be improved. The size of 2DVD picture may be reduced.

Response: Thank you for this comment. I reduced the size of the compound image to 0.7 of its original width and also tried to visually separate the two subfigures. I am not sure in what other way I should improve the figure in your opinion. Please bring this up again if you had other changes in mind.

Changes in manuscript (Figure 2): Figure reduced to 0.7 of its original width.

2.2 Page 3, line 27: please, modify the sentence because the 2DVD is not based on a

Printer-friendly version

Discussion paper

similar principle than Thies.

Response: Thank you for this comment. We removed this statement in the corresponding sentence of the revised manuscript.

Changes in the manuscript (section 2.1): The 2DVD, developed by Joanneum research, [statement removed] is able to derive more direct and more detailed information about individual hydrometeors than the Thies disdrometer.

2.3 Page 4, lines 29-30: it is not clear if the Eq. 1-5 are applied in the Thies precipitation classification algorithm or if different relationship are applied. Please, clarify this.

Response: Thank you very much for this comment, this was indeed not stated clearly enough. Unfortunately, the exact empirical relations used in the Thies precipitation classification algorithm (except Gunn and Kinzer, 1949) are not reported by the manufacturer. We were also not able to get more insight into other details of their algorithm. We made this now more explicit in both the description of the Thies distrometer (2.1) as well as in the description of the classification algorithm used to process the 2DVD data (2.2). Following a suggestion of reviewer #1 on this topic, we also applied our classification method to the raw data of the Thies disdrometer and mention implications of this analysis in the discussion of the revised manuscript (please see corresponding comment of reviewer #1).

Changes in the manuscript (sections 2.1 and 2.2): 2.1: The exact functioning of this classification algorithm as well as other equations used are thereby not reported by the manufacturer. 2.2: To investigate the effect of applying different classification methodologies on obtained results, the classification algorithm described above was also applied to Thies data. Given the binned data, the mean velocity and diameter of each V-D class were used for the classification rather than information about individual particles.

2.4 Page 6, lines 16-20: in my opinion, the correlation coefficient is not one of the best indicators for this type of analysis (the Table 4 confirm this, showing high CC values before and after the Thies correction). Figure 7 shows that the data are distributed along a straight line, but this is not close to the one-to-one line (as should be). A more indicative indicator to associate to the bias could be the root mean square error.

Response: Thank you very much for this comment. The advantage of the CC compared to many other metrics (including the RMSE) is that it is independent of any bias, i.e. reflects the scatter between the two observations independent of any systematic deviations. Our original intention was to provide the CC as a metric of how this scatter can be reduced by an adjustment of the Thies PSD to the 2DVD. However, as you point out correctly, the CC only changes very little before and after the adjustment to the 2DVD, i.e. the correction is mainly affecting the bias. Also, in case of the linear adjustment to the OTT pluviometer, the CC remains by nature unaffected. As the paper is actually focusing on biases and the added value of including the CC is limited, we agreed to remove the CC from Table 4 and we agree that this helps to keep the paper more focused. However, we still mention in the text that a slight improvement of the CC can be achieved with respect to snowfall intensities when using the adjustment to the 2DVD.

Regarding the characterisation of the bias, we would however like to stick to the used metric of the absolute bias for the following reasons. The advantage of this metric is that it is unaffected by the scatter and furthermore can be easily interpreted by the reader. The RMSE, on the other hand, can increase with both increasing bias or increasing scatter, and is a more complex measure probably less intuitive for the reader to interpret.

Changes in the manuscript (Table 4, sections 2.3, 3.2 and 4, Equation 7): The correlation coefficient in Table 4 and related descriptions in section 3.2 are removed. Also, the description of the methodology in section 2.3 is shortened and equation 7 is removed. Note that we keep the statement related to the improvement of snowfall intensity estimates in the discussion (please see 3rd paragraph of section 4 in revised manuscript).

2.5 Page 6, lines 27-28: what does it mean "…with respect to precipitation detection…"? Is it a minimum precipitation threshold or what? And, is it referred to the OTT pluviometer or to the Thies? It is almost impossible to understand by reading the text.

Response: Thank you for this comment. It means that we have investigated the capability of the Thies disdrometer to distinguish precipitation from no precipitation (binary variable). The hit and false alarm rates are given for the Thies disdrometer as stated in this sentence. The OTT pluviometer is used as a reference (i.e. representing the 'ground truth'), which is stated multiple times in the manuscript, e.g. in the first sentence of 3.1, page 6, line 23. The hit and false alarm rates described in this sentence refer to the comparison without introducing thresholds. The effect of applying minimum precipitation thresholds is described in the following paragraph (page 7, lines 1ff.). I have tried to reformulate the sentence in the manuscript.

Changes in the manuscript (section 3.1): The capability of the Thies disdrometer to distinguish precipitation from no precipitation is described in terms of its hit and false alarm rate when using the OTT pluviometer as a reference. In a first step, hit and false alarm rates are calculated over the whole time series and are indicated with circles in Fig. 4 (left) for different integration times. […] In a second step, we tested the application of minimum precipitation thresholds to the Thies disdrometer observations in order to reduce false alarm rates for longer integration times.

2.6 Page 7, lines 1-3: by looking at Figure 4, the combination of hits and false alarm can exceed or not 100%. Obviously, when the sum is lower than 100% it is because of miss and/or correct negative, but what about when the sum exceed 100? Is it always because they are calculated with respect to precipitation detection? This reviewer (and this could be true for a reader) is not familiar with ROC, but the text should allow to understand the methodology.

Response: Thank you for this comment. You are right that we have not explained the concept of a ROC curve sufficiently in the original manuscript and we added a more detailed description in the revised manuscript, also following a more technical comment (1.6) of reviwer #1. Regarding the concept of hit and false alarm rates, we would like to keep the reference to Jolliffe and Stephenson (2012) as we think with the given example a reader will get the correct understanding of these concepts.

To clarify your specific question: yes, the sum of hit and false alarm rate can exceed 1, as they both can take values from 0 to 1 independent from each other. Considering the following example: an overly sensitive measurement device which always reports precipitation will achieve a hit every time there is actually precipitation and no misses. The hit rate = hits/(hits+misses) will be 1. However, the same instrument will always produce a false alarm every time there is actually no precipitation and no correct negatives. The false alarm rate = false alarms/(false alarms+correct negatives) will also be 1. Thus, the sum of hit rate and false alarm rate will be 2. When imagining the opposite, i.e. a totally insensitive instrument which never reports precipitation, it will be clear that both the hit rate and the false alarm rate will be 0. In reality, the combination lies somewhere between these extremes, depending on the capabilities of the instrument, and can be further changed (ex-post) by introducing a minimum precipitation threshold for the instrument. The theoretical optimum (hit rate of 1 and false alarm rate of 0), however, can usually not be achieved.

Changes in the manuscript (section 2.3): For the evaluation of categorical variables, i.e. precipitation detection (yes/ no) and precipitation phase (rain/ mixed/ snow), hit and false alarm rates with respect to the reference instrument are calculated according to Jolliffe and Stephenson (2012). In the case of precipitation detection (yes/ no), we further investigate the effect of minimum precipitation thresholds applied to measurements of the Thies disdrometer on hit and false alarm rates by investigating the so-called Receiver Operating Characteristic (ROC) curves (e.g., Jolliffe and Stephenson, 2012). A ROC curve thereby depicts the variation of hit and false alarm rates with the variation of such a threshold. For example, using a threshold of 0 mm/h for precipitation detection (i.e. always reporting precipitation regardless of the measurement) will result in both a hit and a false alarm rate of 1. On the other hand, choosing an indefinitely high minimum precipitation threshold will result in both a hit and a false alarm rate of 0. Between these extremes, the resulting hit and false alarm rates depend on the capabilities of the Thies disdrometer to detect precipitation as compared to the reference instrument, while the theoretical optimum (hit rate of 1 and false alarm rate of 0) can usually not be achieved. To establish ROC curves for different integration times we use the fixed thresholds THROC = {0, 0.001, 0.002, . . ., 0.05, 0.1, 0.15, . . ., 1, 1.2, 1.4, . . ., 3} in mm/h.

2.7 Page 7, lines 8-9: I am always skeptic when an instrument like a disdrometer or pluviometer is considered to be able to detected so weak precipitation.

Response: Thank you very much for this comment. Of course, we are also sceptic towards the capability of these instruments to detect so weak precipitation – although according to the user manuals, the OTT pluviometer can detect precipitation > 0.01 mm and the Thies disdrometer even provides minimal intensities of 0.001 mm/h for drizzle.

With the analysis provided in the manuscript we are nevertheless able to show that the two instruments agree quite well with respect to precipitation detection. Also, we can show that – when using the OTT pluviometer as a reference – an even better agreement is achieved with the introduction of minimum precipitation thresholds for the Thies disdrometer. Given the difficulties of measuring so weak precipitation, we agree, however, that it is difficult to determine the real ‘ground truth’ or to make absolute statements about the capabilities of each instrument with respect to this ‘ground truth’. That is also the reason, why we state in the discussion that “false alarm rates might be affected by the sensitivity of the reference instrument, but are [at least] comparable to findings of Bloemink and Lanzinger (2005) who use human observations as a reference”.

Note that also reviewer # 1 asked to include more information about minimum precipitation amounts detected by the OTT pluviometer, which we included in the revised manuscript.

Changes in the manuscript: (See corresponding comment of reviewer # 1 for more information about minimum precipitation amounts detected by the OTT pluviometer.)

2.8 Figure 5: a logarithmic scale on the y-axis could be better.

Response: Thank you for this comment. Indeed, boxplot ranges for low precipitation intensities can be better read when using a logarithmic scale on the y-axis. We changed the two subfigures accordingly and added a hint to the logarithmic scale in the figure caption.

Changes in the manuscript (Figure 5): A logarithmic scale is used on the y-axis and the following hint is added to the figure caption. "Note that a logarithmic scale is used to display precipitation intensities."

2.9 Page 8, line 8: or "…described above. Whereas…" or "…described above: whereas…"

Response: Thank you for this comment. We changed the manuscript according to the second suggestion above.

Changes in the manuscript (section 3.2): "…described above: whereas…"

2.10 Page 8, line 11: the mean ratio of what?

Response: Thank you for this comment. We were using the terminology of Raupach and Berne (2015) here, apparently without explicitly stating it. The mean ratio is defined as the reference mean divided by the observed mean, while the 'mean' refers to the mean over a certain number of time steps. Alternatively, this can be expressed as the ratio of the reference to the observed precipitation sum in the calibration period, which is probably somewhat easier to understand for the reader and has been changed accordingly in the revised manuscript.

Changes in the manuscript (section 3.2): We thus propose to use the ratio of the OTT pluviometer to the Thies disdrometer precipitation sum as a correction factor and to distinguish between rain and snowfall. Using the first year of measurements, the resulting correction factors for rain and snowfall intensities are 1.20 and 0.96, respectively.

2.11 Page 8, lines 15-16: the PSD shown in Figure 8 are obtained by averaging all the 1-minute PSDs collected during the two years?

Response: Figure 8 actually shows the distribution of the summed (not averaged) 1-min PSDs over two years, which is explained in the figure caption as follows: "Comparison of particle size distribution (PSD) obtained by the Thies disdrometer and the two-dimensional video disdrometer (2DVD) during the whole time series (two years). Left: Summed PSD during all observed rain and snowfall events. The separation into rain and snowfall events is based on the recorded dominant precipitation type by the Thies disdrometer (1 min)." To make this more explicit also in the text, we revised the corresponding sentence in the text of the revised manuscript.

Changes in the manuscript: A comparison of the summed PSD between these two instruments is shown in Fig. 8 for all rain and snowfall events during the whole time series (two years). The separation into rain and snowfall events is based on the recorded dominant precipitation type by the Thies disdrometer (1 min).

2.12 Page 8, lines 34-35: I basically agree that the impact of both correction methods are comparable, but the "2DVD correction" gives higher bias than "OTT pluviometer correction". This could indicate a slight overestimation of the precipitation by 2DVD if compared to the OTT pluviometer.

Response: Thank you very much for this valuable comment. A similar comment was also made by reviewer #1, and we have indeed not interpreted Table 4 detailed enough. It seems indeed that for liquid precipitation, the adjustment to the 2DVD introduces a positive bias of roughly the same magnitude as the negative bias without adjustment in the validation period. As stated in your comment, this could indicate a slight overestimation of liquid precipitation by the 2DVD when compared to the OTT pluviometer. Therefore, we would clearly recommend to apply the more robust adjustment to the OTT pluviometer. We adjusted the description of Table 4 in the result (section 3.2) as well as the discussion (section 4) accordingly.

Changes in the manuscript (sections 3.2 and 4): 3.2: As can be seen in Table 4 and Fig. 10, the most robust result is achieved by the adjustment of rainfall intensities to the OTT pluviometer, which successfully reduces the underestimation of liquid precipitation in the validation period. The adjustment of rainfall intensities to the 2DVD, however, results in a positive bias in the validation period. For snowfall, both correction methods have a smaller impact and even result even in slightly higher negative biases than are present without any adjustment. 4: To reduce the underestimation of rainfall intensities by the Thies disdrometer, we established an adjustment to 2DVD measurements following the methodology of Raupach and Berne (2015). However, when applying the resulting adjustment in the validation period, we introduce a positive bias, which could indicate a slight overestimation of liquid precipitation by the 2DVD when compared to the OTT pluviometer. A more stable correction is achieved by applying a linear adjustment to the OTT pluviometer. This method is thus proposed as the preferred correction method in this study, especially when the PSD itself is not of interest to the user.

2.13 Page 9, lines 13-14: the sample size information should not be reported here but at the beginning of Section 2.3.

Response: Thank you for this comment. We have moved the corresponding sentence from section 3.3 to 2.3 where the comparison between the two disdrometers with respect to precipitation type detection is described.

Changes in the manuscript (sections 3.3 and 2.3): The following sentence is moved from section 3.3 to 2.3: “Furthermore, we only consider pairwise complete (1 min) observations of both instruments with either rain, snow or mixed precipitation, resulting in a time series of 2,533 h of precipitation.”

2.14 Page 10, lines 18-21: to state that the correction method proposed by you and the one proposed in Raupach and Berne (2015) are consistent you should apply their method to your data (only because Thies and OTT Parsivel are based on the principle).

Response: Thank you for this comment. I am not sure if I understand your comment fully, but would like to provide some clarifications before I come to the changes made in the revised manuscript. The application of the exact same correction method as proposed by Raupach and Berne (2015) to our data is not really possible, as they establish their correction to Parsivel disdrometers (generations 1 and 2) and we are evaluating the Thies disdrometer. However, as Raupach and Berne (2015) highlight in their article, the "the correction can be trained on and applied to data from [. . .] any disdrometer in general." So rather than applying their method, we adopt their methodology. This distinction was made more clearly in the revised manuscript.

Furthermore, in the original sentence, we only state that our result is consistent with the result in Raupach and Berne (2015) in so far as the correlation coefficient is only slightly affected by their correction of the Parsivel as well as our correction of the Thies disdrometer. However, as you have expressed yourself critically towards the use of the correlation coefficient in an earlier comment, we have removed this statement from the revised manuscript.

Changes in the manuscript (section 4): (Please see response to earlier comments for corresponding changes in the revised manuscript.)

2.15 Conclusions: the first part pf the Conclusions (i.e. page 11, lines 21-32) is a summary of Section 4. I suggest merging the two sections in only one that could be titled "Discussion and Colclusions".

Response: Thank you for this comment. You are right and we merged the two sections as proposed. We thereby removed lines 21-32, keeping only one statement of it to the new, merged section (see below).

Changes in the manuscript (sections 1 and 4): the Following statement was kept from lines 21-32: “hit rates reaching 99.7% for rainfall and 95% for snowfall using the 2DVD as a reference”, the rest was removed. Furthermore, the description of the structure of the paper in the Introduction was changed accordingly: “In section 4, results are discussed and conclusions are drawn with respect to the operational monitoring of precipitation with the Thies disdrometer as well as potential applications in a hydrological context.”

Interactive comment on Atmos. Meas. Tech. Discuss., doi:10.5194/amt-2019-466, 2020.