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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Comparison of precipitation measurements by Ott Parsivel^2 and Thies LPM optical disdrometers" *by* Marta Angulo-Martínez et al.

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General comments

The paper presents a comparison of two optical disdrometers: the OTT Parsivel2 and Thies LPM. The work is well written and generally clear, with a good review of existing literature and the instruments compared. The results are of interest to researchers using optical disdrometers. This work should be published, but there a few revisions required to strengthen the manuscript. In particular, more filtering of raw data is required, the GLMM results need better explanation, and you should reconsider highlighting the results for which the DSD size classes

are similar (the "filtered" results).

We thank the reviewer for the insightful review and useful suggestions. We have tried to improve the manuscript, including new (extended) explanations, most especially of the results of the Gamma GLMM analysis. We have also stressed the results using filtered data.

Regarding the filtering scheme, in addition to fixing the drop size to a common range (which was, in fact, 0.250 to 8 mm), we have implemented a filtering + correction scheme. Unlikely combinations of size and velocity were removed, and the sensing area has been modified for each drop size class. This implied repeating all the analyses. For assessing the influence of this filtering, and of the required adjustment of the sensing area, we have also included results without this filtering + correction scheme, which are shown in the Appendix (Figures A.1, A.2, A.3 and Table A.1).

We have included the corrected manuscript as an attached pdf file.

The writing contains numerous small English errors, so I suggest a thorough proof-reading of the paper to fix these. There is a lot of repetition between the introduction (Section 1) and the discussion (Section 4). It reads as though Section 4 was written separately and put into the manuscript after the rest was written. I suggest that you carefully combine these sections so that the introduction contains background information about disdrometers, instrument types etc, and the discussion is more a discussion of your results with relation to previous findings in the literature. Please also better explain what the results mean for the community and researchers interested in using these instruments.

We have carefully checked the manuscript for language errors, and we believe that the language is now better.

We agree that the discussion section was somehow repetitive, so we have completely rewritten it. Some of the references have been moved to the introduction section, were they make more sense.

We also have better explained what the results mean for the community and researchers interested in using these instruments.

For the time steps tested and when bulk rainfall variables are calculated, I think it is important to carefully filter the data returned by the instruments. Two filters should be applied - the first to ensure no solid precipitation is included in the results and that the instrument lasers were functioning correctly (Parsivel flags can be used for this), and the second to remove particles that are unlikely to be raindrops (using a relationship between particle size and expected velocity, as per for example in Jaffrain and Berne (2012)).

We filtered out records when there were error flags or did not correspond to liquid precipitation (rain). We have highlighted this in the revised text (section 2.1), and we have also reported the number of minutes with errors and the number (and proportion) of minutes with rain in Table 3.

The only point where we use the bulk variables as given by the devices firmware now is in Figure 2, and we make this clear. For the rest, we have computed our own values based on the PSVD data, according to formulas provided explicitly in section 2.2.

Except for discussing the effects of filtering, which are shown in the Appendix, all the results presented correspond to filtered and corrected PSVD data, as suggested. We agree that this allows for a fairer comparison between the two devices. We therefore implemented a filtering scheme as suggested, which included: i) restricting the comparison to common drop size classes, i.e. between 0.25 and 8 mm; ii) filtering out highly unlikely drop size and velocity combinations, according to the theoretical fall velocity model of Beard (1976); and iii) correcting the sampling area of the disdrometer as a function of the drop size. This is explained in detail in section 2.2.

It appears that one of the main differences between Thies and P2 disdrometers

shown here is that Thies records many more small particles (and lower velocities) than Parsivel. But, Thies can record from 0.125 mm and Parsivel can record from 0.25 mm. I think it's important to carefully show which differences arise from this simple instrumentation difference. You have done this with your filtered results, but I feel that the filtered results are mentioned rather as an aside when they are in fact a fairer comparison between the instruments. Indeed in the abstract you mention that Thies records nine times as many particles as Parsivel for some rain rates, yet in the paper this reduces to about three times if the different class definitions are taken into account by your filtering. It should, at the least, be emphasised that some of the differences shown can be explained by the different drop size ranges.

It is true, and that is the reason why we repeated the analysis using a filtered dataset in which we removed the data from the first size of the Thies PSVD, so both devices started at 0.25 mm. Since we have re-done the analysis with further filtering, now the main results refer to the filtered data, which incorporates the common lower and upper detection limits. The comparison between the two disdrometers ***without filtering***, i.e. using the raw PSVD data, has been moved to the Appendix (Figures A.1, A.2, A.3 and Table A.1).

You use a Gamma generalised linear mixed model (Gamma GLMM) to analyse whether the differences between the instruments and location are significant. This is a nice, rigourous idea, but then the writing in the paper does not analyse the results in enough detail. I have the feeling that while the GLMM results are shown in tables, most of the conclusions shown in the paper were rather drawn from kernel densities which are easier to interpret by eye. It's important to explain the results so that for example the meaning of the different coefficients found using Gamma GLMM are clear. For example, possible random differences caused by the mask are controlled for, but there is no discussion in the text of the influence shown by this random variable and therefore it is difficult for the

reader to interpret the results shown in Tables 4, 5, and 6.

We agree again with this comment, and have very much improved the description of the results of the statistical analysis in the revised manuscript. We have further developed the tables showing the results of the analysis (for instance, there was only one p-value when in fact there should be two, one per model coefficient, and we have better expressed the random effects by stating the standard deviation attributed to the mast location and to the random residual). We have split the results in four tables (Tables 4, 5 and 6). And we have extended the interpretation and discussion of the results of the analysis, carefully explaining the interpretation of the model coefficients.

Specific comments

1. Page 2, line 30: Please carefully define what you mean by PSD here. Your point is that pressure disdrometers do not measure velocities, which is correct, but the PSD is often used to refer to volumetric particle size distributions which are calculated using a velocity (either measured or estimated).

We agree that the phrasing was not totally clear, and we have rephrased these lines.

2. Page 3, lines 10–15: the 2DVD is perhaps considered a reliable reference, but it should also be noted that it has been found unreliable for small drops (see e.g. Tokay et al. (2013) and Thurai et al. (2017)). Is the 0.3 mm limit mentioned on line 13 from Tokay et al. (2013) or another article that can be cited?

The reference is taken from Tokay et al., 2013, p. 17: ' Since the 2DVD severely underestimates the number of drops in the first size bin, 0.3mm should be considered the minimum drop diameter'. We have added the missing reference, and also cited Thurai et al., 2017.

3. Pages 3 and 4: Your literature review showing the different version histories of Parsivel and Thies disdrometers is excellent. However you mention that they have each been compared to more accurate disdrometers, but without saying

what the comparisons found. I think you should briefly outline the results of these comparisons.

Thank you for the comment. It is very true that the main conclusions of the latest studies that checked the performance of the two disdrometers should be mentioned here, and not in the discussion section. We have followed your advice and incorporated them here, which we believe improves significantly the introduction section.

4. Page 4, line 24: These statements on the average annual precipitation at the field site need a reference. Also, you should include a brief further description of the properties of the site – e.g. is it in complex terrain? What types of precipitation does it experience? etc.

We have included more information regarding the study site, including precise references to the pluviometry as recorded by the official AEMET (Spanish Weather Service) station, which is located in the same experimental site.

5. Figure 1: It would be helpful to label the different disdrometers in the image.

That is a good suggestion. We have included labels to identify the devices.

6. Page 5, line 13: I believe that recent Parsivels automatically remove margin fallers. Please confirm and mention this here. Do the Thies instruments also remove margin fallers?

Both manufacturers indicate in the technical documentation that unlikely drop combinations are removed, although they do not give any details of the procedure. Some information can be inferred from the literature in the case of Parsivel, but we have found nothing for the Thies. From looking at the raw PSVD data, it seems clear that Parsivel implements some sort of filtering, while Thies present the data in a rawer state. Our filtering scheme has been able to make the outputs of the two disdrometers more comparable, although substantial differences still remail. We have stressed this points in the manuscript.

7. Page 6, line 11: Define units for Pr. Equation 1 would benefit from having Nij instead of just N.

We now provide equations for all the integrated units used in the article. We used $N_{i,j}$ as needed. The units for all the symbols have also been provided, and the notation issues have been fixed.

8. Equation 1: What units does KE have? By my calculation the equation results in J m−2 cm−3 mm2 and it is not clear why the 1/12 appears. Please check this equation.

In order to simplify, we now use E only. We have used a less compact (and more self-explanatory) version of the formulas.

Regarding the development of the kinetic energy formula, it is as follows:

$KE = 1/2mv^2$

where v is velocity and m is mass of the raindrop. The mass is obtained as the product of the drop volumen, V, and the density of water, ρ, equal to 1 g cm^{-3}:

$m = \rho V$

The volume of (an equivolume spheric) drop is:

$V = 4/3\pi R^3 = 1/6\pi D^3$

where D is the drop diamater in mm, so we arrive at:

$KE = 1/12\rho\pi D^3 v^2$

The 10^{-3} term, finally, is a conversion factor to go from mm^3 (used for V) to cm^3 (used for ρ), so they cancel out.

9. Page 6, line 24: The normal sampling area quoted here is correct, but normally an adjustment is made for large drops because margin fallers are uncertain or removed. See e.g. Battaglia et al. (2010). Note that they use D/2 to account

for bias due to edge-fallers, but if margin fallers are automatically completely removed this should be D. I see that in your paper you are focussing on computing variables calculated by the instrument hardware, so I think all that is required is that you mention this adjustment at this point in the article. For variables calculated from the DSD, you should use such an adjustment of sampling area, or justify why you choose not to.

Since we have implemented a filtering scheme that involves removing the particle size and velocity combinations that were 50% or more different from a theoretical model, we also implemented a correction of the effective sampling area. This is detailed in equation 7 in the manuscript. Since we are not removing all the particles that do not fit the model (some random variation can be accepted, as done by all previous researchers), correcting the sampling area using D would be an overkill, so we have preferred to use D/2 as done commonly. However, as we discuss in section 4, the exact calibration of the filtering and the correction is something that will require further analysis, probably with the resource to numerical simulation.

10. Table 2: If these are one-minute values, what is the difference between rain rate R, mean rain rate Rm and max rain rate RM? (Same for kinetic energies and number of particles).

In addition to one-minute data, the mean (m) and maximum (M) values of these variables (*Rm*, *RM*, *KEm*, *KEM*, *Em*, *EM*, *NPm*) were computed for each rainfall event. We have made this clear in the text.

11. Page 7, line 24: The 2DVD has a resolution of roughly 0.2 mm so it can measure drops smaller than 0.3 mm; but other studies (e.g. Tokay I referenced earlier) have shown it is not reliable for these drops.

We have rephrased this sentence, and removed the reference to the 2DVD which is not needed here. We refer to the 0.3 limit in the introduction section.

12. Page 7, paragraph around line 25: disdrometers often record particles that are very unlikely to be raindrops (they could be droplets caught in spider webs, in- sects, snow etc). It is common to filter drop counts using some constraints on the particle size to particle velocity relationship, against expected velocities (see for example Jaffrain and Berne (2012)). For the variables that you calculate yourself using the raw data, I think it is important to perform such a filter. Also, Parsivel disdrometers give a weather type indicator that can be used to determine when the instrument has detected solid precipitation. They also provide laser status that can indicate if the laser is dirty or malfunctioning. Did you use these indicators to subset the data to only rainfall and remove any possible solid precipitation? Again I think this is an important filter to apply.

As explained above, we filtered the minute readings by considering the meteor type. We used the SYNOP code for rejecting any observations that did not correspond to liquid precipitation (rain). We also used the laser status flags of the Parsivel^2 and the quality flag of the Thies to remove any suspect records. We have explicitly mentioned it in the revised manuscript, and the number of minutes removed by each criterion is given in Table 3.

We also have implemented a filtering of ulikely particles, which correspond to a large part to double detections and edge events ('margin fallers', or partial detections).

13. Equation 2: please define N here; I gather it is the normal distribution, but then the symbol also clashes with your N in Equation 1.

Yes, we refer to the Normal distribution. It was a formatting mistake, it should be $\mathcal{N}$. This way it does not clash with N in equation 1.

14. Table 3: Is there a reason why the Parsivel on M2 recorded such different propertions of spring/winter/autumn records than the Parsivel on M1 (and the Thies disdrometers)? Here again I wonder whether you accounted for possible snow in winter?

We did not record any snow, which is very unlikely in the central Ebro valley. The reason why the P2 recorded a different proportion of records in the different seasons is because this device did not work due to different technical issues during part of the experiment. This has been conveniently explained, and the data has been detailed in Table 3.

Anyway, as it is explained, we only used the common minutes where high quality, rain data existed in the four disdrometers simultaneously.

15. Page 9, lines 21-22: The filter here is just removing drops below 0.3 mm in diameter, if I understand correctly. I think the differences, while slightly smaller than in the unfiltered case, are still significant between the disdrometers in this case.

Yes, the differences were still significant after removing the smaller particle sizes.

Please, note that in the new version of the manuscript we focus on the filtered data, according to the new scheme, and we leave the un-filtered results in the Appendix.

16. Figure 3: Please differentiate between M1 and M2 in the caption.

We have done it.

17. Page 9, line 26: "number of drops per minute" – is this what is shown, ie the raw number of drops recorded every minute, or are you showing Nt [m$-$3] in Figures 3?

Yes, it was the number of drops per minute. In the new version of the manuscript, however, and following recommendation by another reviewer, we have used the particle density (ND), expressed in number of drops per m-3 and mm-3.

18. Figure 4: It strikes me as strange that the theoretical velocities for large drops (the black line) are smaller than those for drops of 4-5 mm diameter. How does this model compare to other terminal velocity models?

We have done this figures again, using the terminal velocity model of Beard (1976) instead of the Uplinger approximation, which is known to do not hold true for very large drops.

19. Page 10, line 7: Can you be sure that the larger spread of particles are filtered out of the P2 output, or is it instrumental effect of the Thies disdrometers that increases the spread of velocities? As a comment, the large number of drops with low velocities recorded by the Thies disdrometers would explain large values of total drop concentration Nt [m−3], because the calculation of the DSD contains a division by the velocity.

We do not know the nature of the filtering of unlikely particles that is done by Parsivel, so we can not reject the possibility of an instrumental effect of Thies disdrometers. This is an interesting remark, and we have incorporated it to the discussion. In fact, we indicate that the smaller width of the laser beam on Thies (20 mm) over Parsivel (30 mm) plays against the former, which by geometric considerations alone is more prone to be affected by edge events. Therefore, the short answer is yes, there is combination of instrumental and (most likely) software sources of differences between both devices.

20. Table 4: Why is the sample size lower than the number of samples available? And how were the samples chosen?

We used a random sample of 250 minutes (so N=1000, since there are four disdrometers) to avoid size effects affecting negatively the statistical significance tests used in the anslysis (Type I error inflation; see, for instance, Sullivan and Feinn, 2012, or Lin et al., 2013). The samples were taken at random. Details have been added in the methods sections.

Sullivan GM, Feinn R. Using Effect Size - Or Why the P Value Is Not Enough. Journal of Graduate Medical Education. 2012;4(3):279-282. doi:10.4300/JGME-D-12-00156.1.

Lin, M., Lucas, H. C., Shmueli, G. (2013). Too big to fail: Large samples

and the p-value problem. Information Systems Research, 24(4), 906-917. DOI: 10.1287/isre.2013.0480.

21. Table 4: Very little analysis of these numbers is given in the text, and most conclusions seem to be drawn instead from the kernel densities in Figure 5. Please indicate how the reader is to interpret the numbers in Table 4: what are the meanings of the coefficients (the means for each group for Thies and Parsivel? What about for the mast?). How do you interpret the results for the mast, which show that the mast is sometimes important (e.g. for K e)?

This was a drawback of the first manuscript, and we have much worked on it. The tables showing the results of the statistical analyses have been improved, and the results are now extensively explained and interpreted. The objective is that both analysis (graphical and statistical) now complement each other in a good way.

22. Figure 5: In this plot and the discussion on page 10 the meaning of NP is unclear (note in Table 2, NP has a unit of "unit").

We have discarded NP in benefit of ND, which has units of number of drops per m-3 and mm-1.

23. Page 10, line 16: You mention that Z and R were higher on Thies but E was lower. Can this be explained physically, ie through differences in the numbers of small drops? Some variables (reflectivity, rain rate) influenced much less than others (total drop concentration) by the numbers of small drops recorded.

This is indeed a very relevant remark. The influence of the number of particles detected, their size and velocity, on the computed variables vary and some times cancel each other. We have now discussed the results in light of these influences.

24. Page 11, line 23: As shown by the filtered results, the large difference in numbers of particles can be in large part explained by the different drop sizes measured by Thies and Parsivel disdrometers. I think the fairer comparison is

shown in the filtered results, in which NP was about three times higher in Thies than in Parsivel data.

We have compared the results of the analysis with filtering and without it, now in section 3.4. The results show that the filter and correction affected largely the distribution of particle size and velocities, and interestingly for the median particle size and velocity the effect of the filtering and correction had a different sign for Thies than for Parsivel. With respect to the integrated variables, the effect of filtering was very large for some variables such as the particle density, moderate for others such as the precipitation intensity or the kinetic energy, and smaller for the radar reflectivity.

25. Page 11, line 27-28: Without another external and more accurate reference, I think you can not say whether one instrument or the other over- or under- estimated the numbers of small particles. What you can say is that there were significant differences between the two instrument types.

The intended meaning of this sentence was that one device tended to overestimate with respect to the other, but it is true that the phrasing is not adequate. We have tried to make this clearer in the new manuscript.

26. Figure 6: Are these violin plots for event totals or means or both?

The violin plots refer to event means and maxima. This has been made clearer in the figure caption.

27. Page 12, line 12: I think "complete" might be a strong word here, given the difficulties current disdrometers have in measuring small drops.

Agreed. We have replaced 'complete' by 'thorough', which does not imply completion.

28. Page 12, line 28: In more recent Parsivel disdrometers, margin fallers are detected by extra photo-diodes and removed (Battaglia et al., 2010); so please confirm whether this is the case with the disdrometers used in this study.

We have found no reference that the disdrometers used in this study have extra photo diodes to detect margin fallers.

29. Page 12, line 33: Again I believe that 0.3 mm is not built in to the 2DVD but rather a recommended lower limit.

Yes, that's right, it is a recommended limit but not a built-in one. We have, anyway, rewritten the discussion section and this sentence does no longer exist.

30. Page 13, line 5: The differences in the numbers of particles measured by the two disdrometers could possibly be due to the P2's splash shield that the Thies does not have. You mention in this paragraph that abnormal size-fall velocity pairs could be used to remove irregular particles; why not apply this kind of filter?

We agree, and we have re-analysed our dataset using the filtered and corrected data.

31. Page 13, line 15 and line 23: Two references are missing here.

This has been corrected.

32. Page 13, line 16: There are differing conclusions about Parsivel2 performance by drop size reported in the literature. While Raupach and Berne (2015) included some Parsivel2 comparisons, their study was primarily based on Parsivel 1 data and large variability meant comparisons for larger drop sizes were only performed for higher rain rates. Tokay et al. (2014) compared the Parsivel2 specifically but did not conclude that Parsivel2 overestimates small drops. The recent and in- depth study by Park et al. (2017) shows underestimation of small drops and over- estimation of large drops by Parsivel2 . I suggest you generalise your phrasing here to account for these differing results.

This lines have been moved to the introduction section. We have also rephrased them, so they better express the conclusions of the studies cited.

33. Page 13, line 27: There is no discussion in the rest of the article about hydrometeorological regimes, so the claim that there are no differences by regime is not backed up. By "small raindrop spectra" do you mean spectra that exhibit many small raindrops?

This is a fair remark, and we have removed this line.

34. Page 13, line 31: The paper by Jaffrain and Berne (2011) used Parsivel 1 disdrometers, not Parsivel2.

Right. We have removed removed the reference to the device version since this conclusion is general for Parsivel disdrometers.

35. Page 14, lines 10-11: Please be careful about statements that are speculation – it is not shown by this study whether or not the raw data matrices by P2 disdrometers are post-processed or not.

This is a fair remark, and we have removed the comment about the post-processing of the PSVD data. However, we wanted to stress that some crucial aspects of the internal functioning of both devices are hidden from the final user, which limits their usability in research environments.

Minor/typographical comments

All the minor issues and typos have been corrected.

Please also note the supplement to this comment:
https://www.hydrol-earth-syst-sci-discuss.net/hess-2017-652/hess-2017-652-AC3-supplement.pdf

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Interactive comment on Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2017-652, 2017.