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Atmospheric Measurement Techniques Discussions

Interactive comment on "Cloud-microphysical sensors intercomparison at the Puy-de-Dôme Observatory, France" *by* G. Guyot et al.

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General Comments:

The authors have presented an ambitious campaign to compare a number of instruments, some of them fairly new, for measuring cloud properties in-situ. Since older intercomparison studies focused on older instruments, this is worth publishing and suitable for AMT if the manuscript can be improved. I am somewhat disheartened that the results don't show better agreement between the instruments and that we are still resorting to simply normalizing results to a single instrument to produce a unified data set. If the paper is better organized and more clearly assesses uncertainty, as suggested below, hopefully these results will lead to a better path forward.

Some important results include that rapid changes in airspeed do not affect some instruments (if the speed changes are accounted for), and that other instruments are strongly dependent on alignment. Conditions affecting other instruments–for example, mean inlet speeds (p. 22) and splashing (p. 16) are likely manifested differently with droplet size. However, limited comparisons based on droplet size are made (except for the alignment and SPP/FSSP comparison studies). It would be useful if this were done for the dataset as a whole. Likewise, droplet number concentration may affect the results due to coincidence, and some stratifying by number might also yield interesting results.

The structure of paper could be better organized and tightened up. There is some exact duplication of text, as noted below. In addition, some of the effects of operating conditions–low airspeed, location, etc, are discussed in different sections and perhaps could be better combined in one place. The authors should also consider whether other existing discussion, tables and plots can be condensed, especially since additional information will likely be added to satisfy the referees.

It is generally unclear which results are new in this manuscript and which just corroborate what others have already published. A better reporting of past intercomparison studies is needed in the Introduction, of published uncertainties in the Methods section, and of related results/characterizations in the Results section.

Specific Comments:

Line 104: Given the uncertainties that still exist, perhaps "to obtain precise information" should be changed to "to attempt to obtain precise information".

Page 6: I think a sentence or two are needed before going forward to Section 2. The authors should explain that the Section 3 results show the differences between the measurements, and Section 4 tries to explain the reasons for differences that are outside the normal uncertainty windows cited. Otherwise, the Results section seems inadequately explained when one is reading it, when actually some answers are given

in experiments described in the subsequent section. Some of my comments/questions on the Results section reflect this.

Instrumentation Section: Pre and post-calibration and maintenance procedures were not discussed. Please summarize.

Lines 202-203: From which probe was this approximate range?

Line 227 and throughout: "Accuracy" seems to be used interchangeably with "uncertainty" and "error"; for most of the statements, "uncertainty" would seem to be the most correct term.

Pages 8-9 and page 12: Were the old-style or new aerodynamic probe tips used for the FSSP and SPP?

Lines 235-236: "Theoretical air speed" is not explained; this discussion should be combined with lines 314-315. It's not clear why the speed in the FSSP is 15 ms-1 and in the pump is 15 ms-1. Is this based on an area ratio between the pump and the FSSP? How much uncertainty is added due to this speed measurement? Update: finally found this info on p. 27. Please move it up to where it's first discussed.

Lines 317-327: Duplicate material from above.

Line 370: "PVM1" has not been defined–apparently there were two PVMs operating at different sample rates? Please explain.

Lines 363-364: "The two CDPs were installed in the wind tunnel"–duplicate material from above.

Table 1: Was the irregular coverage of various instruments due to problems, or were some instruments swapped out for others in the same location?

Lines 410-411: This needs further explanation–was the instrument later found to be dysfunctional for some reason, or did it simply not agree?

Lines 424-425: A factor of two difference between two of the same model instruments (CDPs) mounted near each other is disturbing. This points to some systematic problem, possibly with effective sample volume as suggested by Lance et al (2010), which would need to be addressed through testing and conversations with the manufacturer. In the two years since this experiment, has any resolution been made?

Lines 450-451: Is there any indication that this problem was more prominent when larger cloud particles were present? Also "slashing" should be "splashing".

Sentence on lines 484-486: Duplicate information discussed above.

Line 500: "Such discrepancy" should be quantified–it looks like about 30% to me.

Lines 509: "comparable" doesn't seem like the right word, since actually the slopes of 0.35 to 2.6 cited suggest the instrument results are not very comparable.

Line 512: "constant" would be better as "systematic", I think.

Line 513: Please define "number calibration coefficient". Given the discussion later, this is apparently a factor that would correct the number concentration data for some physics that we don't yet understand.

Lines 547-548: I don't think this has been addressed. Why is the airflow more accurately monitored in the wind tunnel than in the ambient air? Add in Methods section.

Lines 570-572: Aren't the depth of fields for some instrument types more uncertain than for others? This should be addressed to help better understand the results.

Line 599-600 and lines 952-952: Really? Discrepancies of 300- 500% are expected for instruments based on similar light-scattering principles during co-axial sampling? This is not something I would expect. The authors may be overstating the acceptance of uncertainty here, even for number concentration. See suggestion for quantifying uncertainty better in Table 2, below.

Table 2: This is somewhat simplistic, as some of the uncertainties are for optimal con-

ditions and some are for non-optimal conditions. While difficult, a detailed assessment of known uncertainties is critical in evaluating your results, and would be an important summary for the community. I suggest that you should expand the table to cover the known range of uncertainties and when to expect them (perhaps as “optimal” and “non-optimal” values, with references). For example, the PVM is less accurate when the MVDs are higher; the CDP less accurate under higher droplet concentrations; the FSSP data degrades if not axial and isokinetic, etc. Then perhaps your later statement that a 300-500% difference between the different instruments “was expected” can be better evaluated, based on the known uncertainties and actual operating conditions. Or, perhaps it will show that there are still unknown sources of uncertainty.

As another referee suggested, expected uncertainty envelopes on the correlation plots would be useful to evaluate the comparisons.

Lines 612-615: Unclear–why would a ground-based instrument be functionally different, as long as it is aligned and has the proper airflow moving through it? Or perhaps that is the point, that this is difficult to achieve for the FSSP.

Lines 632-638: If the droplet speed inside the inlets is a major uncertainty, then it might be expected that the results depend on droplet size. This should be explored.

Lines 663-669: Is this a new result? Or does the manufacturer already warn of this limitation?

Lines 768-770: Again, is this a new result or is this outside the operating regime recommended by the manufacturer?

Lines 779-780: Some discussion of why this is the case, with depiction of the geometry of the two instruments would be useful.

Line 782: Section 4 is titled Discussion, but is really a description of a specific set of experiments with only some of the instruments to try to explain results, and should be labeled accordingly (perhaps moved into section 3)?

Lines 862-864: It would be expected that particle speed would vary with particle size in changing flow conditions. Haven’t computational fluid studies been done of these inlets that could help understand and possibly correct for this? Also, why not use the actual transit speed rather than the mean speed to calculate concentrations?

Lines 873 to 882: Confusing–were these calculations done in the Gerber study or in this study?

Lines 890-892: “generates spurious droplet concentration” makes it sound like the instrument is creating droplets, which it is not. This sentence also seems out of place and a similar statement is made on lines 871-872. Suggest deleting it.

Lines 988-991: The concentration effect of the mean flow is also likely playing a role, and should be mentioned.

Fig. 1 caption: Define “LOAC” shown in the image or remove the label.

Minor Corrections:

The phrasing is a bit awkward in many places. A number of changes are suggested below.

Line 50: ROSEA is used without definition. For the Abstract, use something like “this experiment” instead.

Line 125: “others” should be “other”. Likewise on line 409.

Throughout: “m.s-1” should be just “m s-1” (no period). Likewise for g.m-3 and m3.s-1.

Line 214: I don’t believe “GERBERS” is part of the instrument name.

Line 231-232: “size” should be changed to “diameter” and “of the diameter” should be deleted.

Line 257: “his” should be “its”.

Line 262: “size” should be “diameter”.

Line 297: “precipitations” should be just “precipitation”.

Line 349: “improve” should be “improves”.

Lines 363-364: Awkward; change to “instruments collected data at 1 Hz.”

Line 373: “show” should be “shows”.

Line 413: “exhibit” should be “exhibits”.

Line 467: “orientated” should be “oriented”.

Line 545: Period needed after “accuracy”.

Line 562: “coefficient” should be “coefficients”.

Line 566: No comma after “Even though”.

Line 567: “that” should be “the”.

Line 730: “larger to” should be “larger than”.

Line 735: Change “higher decrease” to “more of a decrease”

Lines 765-767: “largely lower than for larger droplets” and next sentence–awkward phrasing.

Line 899: “absurd ratio values”–change to “extremely high ratios” or something similar.

Lines 982-983: This is not a complete sentence.

Interactive comment on Atmos. Meas. Tech. Discuss., 8, 5511, 2015.