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Atmospheric Measurement Techniques Discussions

Interactive comment on "Comparison of Aircraft Measurements during GoAmazon2014/5 and ACRIDICON-CHUVA" *by* Fan Mei et al.

Anonymous Referee #5

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Mei et al. provide a comparison of datasets from two research aircraft obtained during a coordinated comparison effort. Comparisons between calibrated instruments are quite useful for evaluating whether the estimated uncertainties for each instrument do accurately represent the data quality, which is of course paramount to the usefulness of the data. Ideally, analysis of such comparisons could be used to better understand estimated uncertainties and possibly reduce those uncertainties.

This paper takes on a significant effort because the authors compare all of the possible measured parameters between these two aircraft (> 10 parameters). In general, I feel that the paper would be more useful if the scope were somewhat smaller with more significant analysis and discussion of the differences between a subset of the measurements. There are a few useful recommendations for measurements going

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forward, but also some of the disagreements between measurements which might be considered significant are not explored enough to understand if the measurements can be reconciled. At the same time, I don't think it is reasonable to ask the authors to change what they see as the purpose of the paper, but suggest that in the future such comparisons may better serve the community by going more in-depth on a smaller group of the measurements.

I have some suggestions and changes that I would like to see the authors address. These are listed below.

Table 3: Recommend instead of highlighting only slope and R2 that the systematic differences in measurements are calculated (two measurements could be perfectly correlated with a slope of 1 yet have a huge offset and differ on average by a large fraction). Can you also include something about the expected agreement based on the uncertainties of each instrument?

Line 365: Was the fact that the G1 sensor data point bad here known before the comparison and would it have been thrown out? If so, recommend removing this point from the figure as it does represent what is thought to be good data.

Section 3.2: Ozone: Table 3 shows a minimum ozone value of 0.5 for G1. Is this correct or a typo? It seems there is a slope and offset between the ozone instruments. Difference between the means is about 17%, which I think exceeds what is expected ($\sim$ 5% each instrument). I doubt the explanation that sampling losses in the tubing could account for the difference as O3 is not too difficult to sample. Please state clearly whether the differences observed between the O3 instruments exceeds what is expected for the sensors themselves, and what evidence there is to suggest sampling loss is to blame. Possibly, a leak of cabin air into the sample line affected one of the instruments.

CO: Recommend removing the outlier CO point if you have good reason to believe it was not coincident. At the same time, I don't see how the explanation on 389-391 about

"different operation principles" has anything to do with lack of coincidence between the measurements. Please clarify if the disagreement is because of bad coincidence or if you think the instruments really do not measure the same thing.

Line 418: Kind of weak discussion here about CPC difference. Seems like HALO is systematically lower. It would be useful to understand something about the difference rather than just state that it can be attributed to the typical uncertainties and other unknown factors. The comparison between UHSAS does not support it being an issue with the isokinetic inlets.

Figure 9: Why does HALO UHSAS look so much noisier?

Line 488 / Fig. 11: I don't see the value of this comparison. It is stated in the text that the UHSAS < 50 nm is not a measurement, but rather an extrapolation of the distribution down to sizes the UHSAS cannot measure, and that this extrapolation could easily be invalid during e.g. a nucleation event. Therefore, I don't understand when the extrapolated UHSAS data would ever be of use for scientific analysis. The fact that the extrapolated UHSAS distribution deviates from the FIMS measurements sometimes does not even require the UHSAS instrument to determine this. One could just extrapolate the FIMS data using the UHSAS sensitivity range and look at the difference between the FIMS measurements.

Line 512: What is referred to here had been done for decades on other aircraft and has been referred to as NMASS. Recommend citing the relevant papers for that here and earlier in the paper (i.e. lines 478 – 487). Most recently: Williamson et al., AMT 11, 3491-3509, 2018.

Line 519/section 3.3.4: There is no actual discussion of the chemical composition, just the mass/volume. Recommend removing the reference to chemical composition here and earlier in the paper (e.g. abstract and introduction).

L 650: How about a calculation with TUV to test whether the different sensitivity ranges

can account for the 10%?

———- Editorial type notes:

Line 101: issures -> issues

Line 146: change comma to period

Line 304: ‘paten’ -> ‘pattern’?

Line 326: ‘Tables’ -> ‘Table’

Line 418: ‘ rest of the 10-15. . .”

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