Atmospheric
Measurement
Techniques
Discussions

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Interactive comment on "Intercomparison of an Aerosol Chemical Speciation Monitor (ACSM) with ambient fine aerosol measurements in Downtown Atlanta, Georgia" *by* S. H. Budisulistiorini et al.

Anonymous Referee #2

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This paper reports on an intercomparison exercise between the ACSM and a variety of other measurements over a long time period. Given that this is a new instrument suited to monitoring, thorough evaluation work such as this is important.

While this work is important in principle, I didn't really get a feel for what the outcomes contribute in terms of understanding the instrument or development of best practice. In some instances, such as the investigation of interferences with nitrate, I do not feel the problem was investigated in as great a depth as possible. In others, such as the poor quantification of sulphate, this only serves to highlight a problem that has already been identified and rectified by the manufacturers (sulphate calibrations are now standard

practice).

This is not to say that this is not publishable, but I think that in some places, a deeper analysis of various issues could be performed. The paper also needs to be clearer in its conclusions about the outcomes and recommendations of this work. It should also be very clear to identify which issues (e.g. the need for routine sulphate calibrations) have since been addressed; given that the paper features a number of coauthors from Aerodyne, one might be forgiven for thinking that these issues are still outstanding and would reflect badly on the instrument.

General comments:

While it is unfortunate that there is a lack of calibration data for sulphate, I regard the method used to retrospectively estimate the RIE to be deeply unsatisfactory, especially seeing as it relies on a reliable nitrate measurement, which elsewhere is deemed to be suffering from interferences. I would be interested to know whether better statistics could be obtained by using different combinations of parameters, such as a single value of the RIE.

No information is given as to how the vaporiser temperature and heater bias were set. Given that these are known to affect data, this should be covered.

Specific comments:

Abstract: I would consider the abstract to be too long. Consider making more concise and reducing the length.

P1185: The focus on the 1999 SEARCH campaign in the introduction is an odd choice. While this was historically highly important in establishing the AMS as a credible technique, this was merely the first of many systematic comparisons between the AMS and IC-based techniques. It would be more informative if the authors were to give other examples of previous comparisons, including more contemporary studies.

P1188, L14: The reason for not using the naphthalene data should be given here. More

information is also needed on how the m/z=28 signal was used because the ACSM does not have an 'airbeam' in the same sense as the AMS (not using a chopper). If the background signal was used, diagnostic data should be given, because this will receive significant interference from CO.

P1191, L7: Perhaps I've missed something, but it should come as no surprise to learn that the filter-corrected data correlates so well with the filter data itself.

P1195, L20: I would regard this discrepancy to be an indictment of the method used to estimate sulphate RIE. Agreement for sulphate in previous literature comparisons has generally been quite good.

P1196, L4: What is the estimated uncertainty of this technique? Personally, I would regard a 27% discrepancy in sulphate to not be acceptable.

P1196, L7: The values of OM/OC are not just high, but unphysically so. CO_2, the most oxidised form of carbon, has an effective ratio of 3.67.

P1198: The issue of nitrate interferences is very important and should really be covered in the main manuscript rather than the supplementary material.

P1200, L26: Comparing mass with volume will never yield a perfect correlation because density is variable. That being the case, why not compare SEMS volume with an ACSM-derived volume, using known densities of the components? A volume contribution from black carbon can be added based on the Aethalometer data.

Figure S5: It would seem obvious to me that the sulphate events captured by JST but not ACSM coincide with events seen in the organic time series. Has the mass spectra of these events been inspected? Is there any deviation in the sulphate ion ratios during these events?

Figure S10: It would seem obvious to me to try comparing delta30 with the organic mass loading, if an organic interference is suspected. I find similarity between this trace and the OM trace in figure S4 to be quite striking.

Technical:

P1184, L25: Hallquist et al. (2009) is an odd choice of reference here. Consider using the recent IPCC report instead.

P1185, L18: The AMS should be qualified as the Aerodyne AMS, as 'aerosol mass spectrometry' can also be used as a generic term

P1186, L6: The acronym 'ACSM' needs defining

P1186: Ng et al. (2011) needs only to be cited once; the fact the subsequent statistics are from the same paper is self-evident.

P1188, L14: The naphthalene source is effusive, not diffusive.

P1189, L21: Recommend citing Middlebrook et al. (2012) regarding the effect of acidity on CE.

P1190, L27: Was the Sunset instrument online or offline? Also, which temperature protocol was used?

P1191, L1: Were any corrections for shadowing or scattering performed on the Aethalomter data?

P1192, L5: A reference for the DMA design used should be given, as some DMAs struggle to transmit particles of sizes nearing 1000nm, even at low flow rates.

P1192, L17: Surely this should be 'below detection limits'?

S7, L20: Being an optical measurement, the Aethalometer strictly measures 'black' carbon, not 'elemental' carbon. The latter implies some form of chemical analysis.