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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Field characterization of the $PM_{2.5}$ Aerosol Chemical Speciation Monitor: insights into the composition, sources and processes of fine particles in Eastern China" *by* Yunjiang Zhang et al.

Anonymous Referee #1

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This data shows a comparison of ACSM data in Nanjing between a traditional PM1 instrument and a new PM2.5 instrument equipped with a capture vaporiser (CV), along with other instruments. These two modifications to the instrument design have been heavily anticipated for the purposes of comparability with other PM2.5 measurements and to get away from the 'bounce' artefacts that are corrected for using empirical functions. While technical in scope, the paper purports to also offer some scientific insight to atmospheric processes at this site, so is suitable for ACP. However, I do have a misgiving on how the differences between the two instruments is interpreted (see below),

and I worry that the overall quality of the data is not fully validated. I would like to see that the authors more convincingly prove that the differences are due to the size of the particles and not a technical issue associated with one or more of the instruments before this proceeds to full publication. It is particularly troubling that inconsistencies are found with a number of the comparisons (e.g. MARGA, OCEC), so these should also be resolved before strong conclusions are made concerning disagreements in the two ACSMs.

Major comments:

A core part of the analysis is the assumption that any differences between the two instruments are due to a difference in the size cuts of the aerodynamic lenses. However, there are other differences between the two instruments, most notably the CV. I also worry because I see plenty of reasons to suspect that a different technical issue may be at play. The fact that the ratio between the two instruments is so consistent would imply that the PM2.5/PM1 ratio of the particulates is effectively constant, which does not seem intuitive to me. I would also expect the fractional contribution of primary OA to the PM2.5 measurement to be much lower, given that this is generally accepted to be almost entirely submicron. In order for the conclusions to stand, it would be useful if some more validation work could be presented and allay my suspicions concerning data quality. Can any data be shown where the two ACSM instruments agree for smaller particles, e.g. when dominated by primary particles? More generally, while the r2 comparisons between various instruments are certainly impressive, it might be more informative to look at the ratio in real time and see what drives this ratio to vary, e.g. if it correlates with primary vs secondary particles. This would be more informative than simply looking at a slope and speculating.

What is extremely notable in its omission is a volume-convolved particle size distribution from the DMPS/APS data. To understand the split between PM1 and PM1-2.5, it would seem fairly logical to see if the accumulation mode straddled 1 μm point. Also, because of the breadth of instrumentation at the authors' disposal, it should be possible to do a full size-resolved mass budget by assigning components to different modes.

The discussion is a little rambling and doesn't really highlight what the new scientific advances relative to atmospheric science are. As a case in point, section 3.5 concludes that secondary aerosol are formed regionally and primaries are formed locally, which I would consider pointing out the obvious and as such, I would feel the need to question the point of presenting this. I'm not saying that the analysis should be taken out, but it should be tightened up and focused on specific points that feed into the discussion because right now, it feels like a lot of analysis just being done for the sake of it.

Minor comments:

Line 71: Chloride has also been shown to be an important contributor

Line 140: Should specify the ACSMs are the Q-ACSM type (as opposed to TOF-ACSM)

Line 157: Jayne et al. (2000) is not an appropriate reference describing the bounce effect, as it was not understood fully at the time.

Line 178: More detail is needed concerning the online OCEC measurement. Specifically the model number, the sampling duration, whether a denuder was used, which specific thermal ramp was used (specifically whether the 'abbreviated' NIOSH method was employed) and how it was calibrated. Also, from the perspective of validating the integrity of the split points, it would be useful to know the consistency between the optical and thermal EC values.

Line 182: The manufacturers and model numbers of the DMAs should be given, or if they were custom-made, the geometry employed.

Line 245: It is also likely that the size cut of the two inlets isn't identical, so this may contribute to a discrepancy.

Line 260: What did the ion balance of the MARGA data look like? If this didn't balance, this would indicate an issue with this instrument.

Line 285: While some of the reasons offered for the discrepancy between ACSM and OCEC are plausible, the fact that the correlation is so good would imply that a systematic issue is responsible. How confident are the authors that the instrument is determining the split points correctly?

Line 399: In order to show that the relationship with RH is causal, you must rule out confounding factors like changing source regions being responsible (these would have an effect on both humidity and precursor emissions). Otherwise, a caveat should be added.

Line 423: Information on the relative uses of gasoline and diesel in Nanjing should be discussed; the former is mainly responsible for the CO, but the latter is responsible for POA. While they will still correlate as an area source, the ratio will vary according to the emissions fleet (Allan et al., 2010).

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