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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Aerosol particle measurements at three stationary sites in the megacity of Paris during summer 2009: meteorology and air mass origin dominate aerosol particle composition and size distribution" *by* F. Freutel et al.

Anonymous Referee #3

Received and published: 31 October 2012

This paper presents a very comprehensive interpretation of the combined measurements from multiple site during the MEGAPOLI campaign in Paris. On reading the paper's title, one might be forgiven for expecting this paper to be a hypothesis-driven analysis, however it is a very weighty piece of work that explores many aspects of the data collection and analysis, ranging from technical issues to reporting statistical analysis of the results. While certainly encompassing, it does seem to ramble in places. However, it isn't a bad paper as such and does present interesting and useful statistics

regarding Paris that are compared well with other recent megacitiy campaigns. I would therefore recommend that this be published, subject to the following comments:

General comments

The running title "Air mass origin dominates aerosol characteristics in Paris" is perhaps a little bullish considering that this is only the results from one measurement campaign. To really make that statement, one would have to show it applies to long-term data. Suggest a reword.

While thorough, I would venture that the authors go a little overboard on the analysis in places. In particular, after reading sections 3.4 and 3.5, I was left wondering how these contributed to the main conclusions of the paper. While it would be difficult to change too much at this stage, the paper would benefit from being more succinct and to the point in places.

The 30% scaling applied to the Sub SW AMS is very worrysome and it is very disappointing that the authors were evidently unable to find the underlying reason why this was needed. It should be stated whether the common causes of 'bad' AMS calibrations have been individually ruled out, specifically that the calibration particles were dry, appeared to be of the correct size (using the PTOF data) and did not appear to contain any contamination (based on the mass spectrum). If the calibration was performed by comparison to a CPC (as opposed to using BFSP data), the authors must provide an estimate of the influence of multiply charged particles and technical details on the CPC and the plumbing configuration used to split the AMS and CPC flows (including whether mixing was used).

The authors do not present sufficient evidence to suggest that new particle formation is taking place. While there is certainly an abundance of smaller particles during the day, there is nothing to even suggest that anything other than primary sources are responsible. There is certainly nothing resembling the typical 'banana' behaviour seen at other sites, or the order-of-magnitude increase in CPC concentrations that would indi-

cate that a nucleation burst has taken place. Given the lack of strong SO2 sources and the large pre-existing particulate surface area, new particle formation would intuitively seem unlikely, so I would recommend that this very speculative line of discussion is taken out entirely unless the authors can present stronger evidence.

Specific comments

Page 22208, line 15: Reword “well comparable”

Page 22211, line 5: The authors need to be specific whether the Allan et al. tables were used, or the subsequently modified version presented by Aiken et al. (this can be checked against the Colorado wiki).

Page 22211, line 13 (and elsewhere): The authors need to specify whether MS or BFSP data was used for the ammonium calibrations (MS is strongly recommended).

Page 22211, line 27: The AMS vaporiser has a much stronger influence in the relative peak intensities of the organic spectra than the mass spectrometer ion transmission function. Considering this with the different sulphate RIEs, I would say it more likely that the discrepancies are due to different vaporiser function.

Page 22212, line 2: One way of interpreting the organic discrepancy between the two instruments is to surmise that the RIE of the organic fraction had drifted in the same manner as the sulphate. That being the case, it might make more sense to alter the RIE rather than apply a scaling to the data products.

Page 22213, line 6: Rather than using an average density, why not calculate a time-dependent density from the AMS measurements? This would give a more accurate volume estimation.

Page 22213, line 12: The SMPS cutoff of 500nm is based on mobility diameter, which translates to a vacuum aerodynamic diameter of 825nm, which is actually quite close to the AMS cutoff.

Page 22218, line 11: I would prefer uncentred R2 (normalised dot product) when comparing mass spectra.

Page 22218, line 12: The exact source of the reference spectra must be stated.

Page 22222, line 3: A typical wind speed corresponding to ‘fast’ should be given.

Page 22225, Line 10: This period would be sufficient if cloud processing was important during this period (which, presumably, it wasn’t).

Page 22227, line 1: Do the authors really mean ‘SO3’?

Table 1: The table caption seems excessively long. Consider presenting this information differently.

Figure 1: Could a map be overlaid on this?

Figure 5: The exponent on the CPC units has been truncated.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 22199, 2012.