

***Interactive comment on “A one-year comprehensive chemical characterisation of fine aerosol (PM<sub>2.5</sub>) at urban, suburban and rural background sites in the region of Paris (France)” by M. Bressi et al.***

**Anonymous Referee #1**

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This paper is about a detailed study of the chemical characterization of PM<sub>2.5</sub> at five locations in urban and suburban Paris (France) and the rural region around this major European city on a daily basis over a one year period. The resulting data provide very detailed and important information about the characteristics of fine particulate matter in this large urban area. The paper is not rich in scientific novelty but its value is in the comprehensive overview about PM composition and in the spatial variability of PM<sub>2.5</sub> on regional scale. The manuscript is interesting to read and the conclusions are generally sound, it should be published in ACP after consideration of the following

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comments:

1. Although the text is generally well written, there are quite some linguistic errors (often wrong prepositions). This is disturbing and it is worth to carefully go through the text and to revise the text accordingly.

2. The boundary layer height (BLH) is used as a factor that has an impact on PM<sub>2.5</sub> concentrations. Quantitative values for BLH are used in this paper without description of how BLH was calculated. This should be explained in section 2 (Material and methods).

3. Pages 29414 and 29415: The rather homogenous spatial variability of OM is interpreted as resulting from mid- or long-range transport. This is hard to believe as the main explanation given that Paris is such a large urban environment. Can the spatial homogeneity be a consequence of secondary formation processes of precursor emissions in the Paris region (mentioned in the conclusions section but not in section 4)? The time needed for gas-to-particle conversion could explain the observed spatial variability of OM, the high observed fOC-OM values support this. SOA formation as an important factor for low spatial variability should be discussed/considered. The same is true for the subsequent discussion of SIA, the general statement that SIA is mainly due to mid- or long-range transport appears not well justified and probably is too much of a simplification.

4. The conclusions section can be shortened and repetition of results that have already been presented in the Discussion section 4 should be avoided.

Additional comments:

Page 29396, line 17, should be "URB" instead of "UR".

Page 29397, line 11, should be "have been" instead of "has been".

Section 2.3.3: What kind of thermal-optical method was applied, the transmission (TOT) or reflectance (TOR) method? Please provide this information.

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Page 29403, line 15: What does this mean that “TEOM-FDMS . . . was not corrected for semi-volatile materials . . .”? Does this mean that the reference signal of the TEOM-FDMS was used here only? Please make this clear.

Page 29410, line 15: Daily PM<sub>2.5</sub> values is compared with the annual limit value, why? It is arbitrary to compare daily concentrations with annual standards, please explain why this has been done.

Page 29412, line 15, sentence is weird, please correct and clarify.

Page 29425, line 24: Link to the emep main website as a reference for excess ammonia in the Paris region is not sufficient. Please provide a more specific link or reference.

Legend of Figure 2: The three different lines can hardly be distinguished. I suggest showing the time-series of PM<sub>10</sub>grav – PM<sub>10</sub>chem in this figure instead of PM<sub>10</sub>chem.

Legend of Figure 3 and scatter plots in supplementary material: Please indicate the meaning of the error bars (+/- 1 sigma?).

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Interactive comment on Atmos. Chem. Phys. Discuss., 12, 29391, 2012.

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