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Atmospheric Chemistry and Physics Discussions

Interactive comment on "PM speciation and sources in Mexico during the MILAGRO-2006 Campaign" *by* X. Querol et al.

Anonymous Referee #3

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General Comments The authors have sampled atmospheric aerosol and determined its major and minor elemental composition at urban, suburban and rural sites in and around Mexico City. They interpret the results in terms of a small number of sources which are identified from their elemental signature, including the use of Principal Component Analysis, and provide some quantitative estimates of individual source contributions to PM2.5 and PM10. The methods used are very routine and it may be argued that the results of this paper contribute little which is new to the literature. The results will be of local value within Mexico, but unless their contribution to the overall results of the MILAGRO-2006 campaign is especially important, it is difficult to see a justification for publication. This kind of information is widely available from many sites around the world.

Specific Comments The work is conceptually sound but the following points need to be addressed if the paper is to be finally accepted for publication.

(a) Two different types of quartz microfibre filter were used. It is not explained why different types of filter were used for different samplers. Was any intercomparison made in terms of the collected mass of specific species?

(b) The environmental conditions under which filter weighings were conducted are not specified, and need to be. How was the optical particle counter “calibrated” against the gravimetric measurements?

(c) What is the justification for calculating the carbonate concentration from that of calcium? Later in the paper the presence of gypsum is reported and therefore it is very clear that not all of the calcium was present as calcium carbonate. This might therefore be an over-estimate.

(d) Why was the electron microscopy carried out with an environmental scanning electron microscope (ESEM)? This is a specialised instrument allowing measurements to be made at significant water vapour pressures. The conditions of operation need to be better defined.

(e) Substantial concentrations of PM2.5 (as well as PM10) are attributed to local soil resuspension. This seems quite unlikely. What is the supporting evidence?

(f) Some justification of the algorithm used to calculate concentrations of crustal material is needed. This looks very incomplete.

(g) What factor was used to convert the OC concentration into organic matter? This can have a significant impact on mass closure.

(h) Masses of secondary inorganic aerosols are reported but the species considered in these masses are not defined. Presumably it was just sulphate, nitrate and ammonium. If so, this needs to be stated.

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(i) At a number of points the authors appear to be under the misapprehension that Mexico City is in Asia. They refer to results from “other large Asian cities”. This needs to be corrected.

(j) The attribution of the mercury to a point source (an incinerator) seems unlikely given that concentrations of mercury were correlated with sulphate. Sulphate is likely to have a regional origin arising from a large number of point sources of sulphur dioxide which would give a very different directional signature to that of a point source such as an incinerator. Consequently, the results do not appear to support the incinerator being the predominant source of mercury and this needs to be considered.

(k) Mean wind directions are reported in Figure 4. How were these estimated? Averaging northerly winds of 360ž and 0ž gives a value of 180ž but this does not imply a southerly wind. Was this pitfall avoided?

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 10589, 2007.