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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Source apportionment of fine organic aerosol in Mexico City during the MILAGRO Experiment 2006" *by* E. A. Stone et al.

Anonymous Referee #1

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GENERAL COMMENTS This paper describes the application of organic molecular markers to the source apportionment of fine organic aerosol in Mexico City during the MILAGRO experiment. The methods are now becoming relatively conventional and have been widely applied by some of the authors of this paper. In general, the results appear convincing and give a good quantitative description of the sources of organic aerosol at two sites in Mexico City, one in the downtown area, the other on the periphery. Whilst the paper does not break any major new ground in terms of methods or insights into atmospheric processes, results of the kind presented are not otherwise readily available and the paper will be of significant interest to an international audience.

SPECIFIC COMMENTS As noted above, some of the authors of this paper have been

associated with a number of previous publications using the organic molecular marker techniques to identify sources of organic aerosol. However, there are issues which the paper does not address, which in the view of the reviewer should have been covered. These include the following:

(a) There is insufficient description of the sampling sites and possible local influences. For example, the results suggest an appreciable influence of biomass burning but the site descriptions give no indication that this is conducted locally. What and where are the likely sources? Influences on the peripheral site are particularly difficult to discern. According to the Methods section, this site was subject to significant resuspension of dust. What is the evidence for this? Also, a peripheral site could be influenced mainly by very local emissions or alternatively by air advected from the central parts of the city, or as a further possibility, air from surrounding rural areas, obviously depending upon wind direction. No insights into this matter at all are given. It is suggested that as a minimum some analysis of meteorological data including local wind directions and airmass back trajectories is conducted for the period over which the results of daily samples are reported. This would help the reader to understand the influences upon this site.

(b) Table 1 lists the specific organic compounds analysed as part of this work. This is a shorter list than that in some other papers by the same authors. Can some explanation be given as to why this particular set of components was selected and not the longer list used in other studies?

(c) The results of the chemical mass balance modelling will depend critically upon the use of appropriate source profiles. Those adopted appear to have come largely from work in California published in the early 1990s supplemented by some more recent currently unpublished studies. The question is not addressed of how relevant these profiles are to emissions in Mexico City in 2006. There are both spatial and temporal issues that need to be considered in addressing this question.

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(d) Section 3.4 contains a considerable discussion of measurements of pinonic acid. This substance is not listed amongst the analytes in Table 1. Is the table therefore incomplete?

(e) The reviewer found the way in which the WSOC data were used particularly useful. However, no use appears to have been made of the measurements of aromatic carboxylic acids. Did any of these correlate with the estimated SOC concentrations?

TECHNICAL CORRECTIONS

(a) One of the author's names is misspelt in the footnote to page 9641.

(b) In the legend to Table 1, sites rather than sties.

(c) In Table 1, benzo(ghi)perylene rather than benzo(GHI)perylene.

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 9635, 2007.