Interactive comment on "Chemical characterization of fine organic aerosol for source apportionment at Monterrey, Mexico" *by* Y. Mancilla et al.

Anonymous Referee #2

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The subject manuscript presents new data about the chemical composition of organic aerosol in the third largest city in Mexico. Organic tracers, measured by GCMS, are used to identify source types and to drive chemical-mass-balance (CMB) source apportionment modeling. The approach has been used previously in Mexico City and by the authors in a range of locations. To a large extent, the limitations of the resulting data are noted. A major shortcoming is a lack of discussion of the source apportionment results in the text. A stronger presence in the text and through tables and figures is warranted. A number of improvements are needed prior to publication.

Specific comments: 1) Given the emphasis on source apportionment in the title, in-

troduction, methods, and model performance metrics ; it was strongly anticipated that source apportionment would have a strong presence in Section 3. In the current version, the only presence of these results is a single sentence pertains to organic carbon apportioned to primary is as a multi-year and season average in Fig. 6 and one sentence reference to this figure. Relative and absolute abundances are neither presented, nor discussed and compared. A paragraph or two of text needs to be added to discuss these results more rigorously in section 3.6. In addition, a Table should be added that summarizes source contributions in different years, seasons, and day v. night (if warranted). Comparison to other source apportionment studies in the region in terms of the gas/diesel split, fraction of BB, vegetative detritus, etc. is warranted.

2) The lengthy discussion of molecular markers in sections 3.2-3.5 should be edited and shortened. For example, pages 17982-17984 and Tables 3 and 4 are devoted to using diagnostic ratios to identify PAH sources. These data really boil down to PAH implicating a mixture of combustion sources that require a more advanced tool (i.e. CMB modeling) in order to distinguish between their sources. The present discussion over-interprets the PAH data, suggesting that diagnostic ratios can be used for source apportionment and that correlations of PAH with OC and indicate the presence of "non-combustion sources." It is best to use the CMB model for source apportionment, which should be discussed in an expanded section 3.6.

3) The very low levoglucosan levels observed in this study are surprising, and among the lowest reported for a heavily urbanized location. It would be valuable to corroborate the levoglucosan data with other biomass burning tracers. Page 17988 lines 5-10 should be expanded to a quantitative discussion of deyhydroabetic acid, pimparic acid, and isopimaric acid concentrations. Please address whether or not these levels support a very small role for biomass burning?

4) As less than 10% of PM2.5 organic carbon is attributed to individual chemical species in this study, the results should not be presented as a "broad characterization" as done on page 17969 (line 1) and in the title. These should be revised to clarify

that only molecular markers were targeted.

5) The method by which the authors refer to the study periods as "first year" and "second year" in section 3.2 makes it difficult to connect the text and figures. Instead, it is suggested to use the years of sampling (e.g. "spring 2011").

6) The nature of biomass burning that occurs in and around the sampling site should be discussed. A relevant source is the surveys of fires conducted by Yokelson et al. (ACP pp 5569-5584, 2007). The authors should clearly discuss the limitations of the profiles utilized in CMB modeling in representing regional biomass burning.

7) In the introduction, the historical review of molecular-marker based source apportionment on page 7970 (lines 22-26) is inadequate. To accurately represent the development and advancement of this approach, further discussion is needed with respect to profiling sources, identifying elemental and molecular markers, and developing source apportionment methods. To suggest only "sparse efforts" in the 1990's is not representative of a decade when extensive source testing occurred within the Los Angeles Air Basin, allowing for assessment of the sources of photochemical smog.

8) The description of analytical methodologies is quite lengthy. The authors should remove all redundancies with Brown et al. (2002) to shorten this section. If no redundancies exist, then the additional information that can be accessed in Brown et al. (2002) should be stated.

9) What are the expected accuracy and precision of organic species measurements?

10) What are the analytical uncertainties in organic species measurements? This is an important consideration when comparing day/night, inter-annual, and seasonal differences. Further, these uncertainties are key inputs to the CMB model. Their calculation should be clearly presented.

11) References for PAH diagnostic ratios should be integrated into Table 3 in the same row as the corresponding information. This will clarify to the reader which information is

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drawn from which reference. Then, these refs may be removed from the figure caption.

12) Throughout section 3.4, in particular, nomenclature should be consistent and conform to IUPAC standards. Eladic acid is not commonly known, and switching between C18:1, and octadecanoic acid is confusing.

13) The non-cooking sources of cholesterol should be stated (page 17986 line 23).

14) Throughout the manuscript, the meaning of +/- is not clear.

15) Are any day/night trends statistically significant?

16) How many CMB results were averaged to create Fig. 6 versus how many data were discarded due to poor model performance (page 17989, line 4).

17) With so much OC collected, what is the need for sample compositing?

18) Further, what is the rationale for compositing non-consecutive time periods (day/night) and days (e.g. 30 May, 9 Jun, 11 Jun)?

19) Mass of OC collected is defined by air flow rates and sampling times. The last column of Table 2 is not useful and should be removed.

20) Also in Table 2, the columns containing of D/N and the subsequent number need to have column headers.

21) All multi-part figures should have parts A, B, C, etc. clearly labeled.

22) "Other OC" should be added to Fig. 6 so that 100% of the circle is equal to measured OC. This will visually clarify the fact that a large fraction of OC was not apportioned and is likely due to secondary or uncharacterized primary sources.

23) Figure 2 should have the same chemical species in each panel for easy comparison.

Minor comments: A) Suggest revising page 17970 line 4 to: "diagnostic ratios between homologs in series of biomolecules. For n-alkanes and carboxylic acids, the odd- and

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even- carbon preferences are indicators of biogenic sources (ref). A lack of carbon preference is indicative of fossil sources." B) Page 17984 line 9, $p < .1$ and $p < 0.05$ is unusual. Suggest reporting p values for each. C) Page 17984 line 9, report r with sign to indicate if these are positive or negative correlations. The direction of the correlation is lost when squaring. D) Figure 4 – rescale panel A so PAH can be compared in vertical columns.

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