

Interactive comment on “A Preliminary Assessment of the Impacts of Multiple Temporal-scale Variations in Particulate Matter on its Source Apportionment” by Xing Peng et al.

Anonymous Referee #5

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Review for: A Preliminary Assessment of the Impacts of Multiple Temporal-scale Variations in Particulate Matter on its Source Apportionment Atmos. Chem. Phys. Discuss.,
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The authors have proposed to improve the identification of major sources of ambient PM_{2.5} by conducting source apportionment upon the spectral decomposition of hourly measurements of ambient PM_{2.5} and its major ionic and trace element components. I think the manuscript falls short of fulfilling the above purposes. Several issues arise after reviewing the manuscript:

a) The process of decomposing the original data in time series of different tempo-

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ral variations does not ensure that each of them are strictly positive, as they should. This is problematic for the subsequent application of ME₂ (or PMF). b) There is no quantification of uncertainties associated to each spectral decomposition. This is problematic for a correct application of ME₂/PMF. Besides, the subtractions that appear in equations (9) – (16) increase the uncertainties of individual spectral components. This degrades input data quality, potentially hampering source identification. c) From equations (12) and (16) it follows that the RBL dataset is the sum of three spectral components. Nonetheless, the authors state that the above set has “many negative values” (line 233). This is a strange result and casts doubt on the suitability of the above decomposition for generating suitable input data for ME₂/PMF. c) The dataset is too limited to validate the proposed methodology. Only two months of hourly data have been used. Monthly seasonality is absent in the analyses, and this is a serious flaw. Another limitation of this small datasets is the poor fitting of As, Cr and Se after applying ME₂ to the original dataset (lines 338-340). These trace elements may come from intermittent (point) sources that arrive to the receptor site only when specific meteorological conditions are met. The data set is too small to resolve those sources. d) The authors acknowledge that the crustal dust source was not resolved in several data sets (RD, RS, BL) (lines 492-495). This contradicts their statement (on lines 506-509) that the proposed methodology improves source identification. See also c) above. e) Actually, the point in receptor modeling analyses is not removing the noise, but properly quantifying it, so noisy data are downweighted. f) The best input data for applying receptor models is one in which data variability is captured as much as possible, to sample all potential sources impacting a site (including intermittent sources); this also implies sampling for at least a year to include seasonality. The proposed methodology goes in the opposite direction: first, decomposing original data variability into several spectral components (each with lower variability) and, second, analyze each of them separately by receptor models.

Therefore, my recommendation is to reject the manuscript.

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