Atmos. Chem. Phys. Discuss., 11, C12675–C12681, 2011
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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Receptor modeling of near-roadway aerosol mass spectrometer data in Las Vegas, Nevada, with EPA PMF" *by* S. G. Brown et al.

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Received and published: 2 December 2011

The manuscript is technically good, well written and with enough details for a comprehensive reading of the text. I consider very relevant the outcomes of the novel approach of exploring the sensitivity of results when pulling to different kind of target profiles. Also the detail of evaluating the robustness of results comparing source profiles with previous studies is appreciated. However, in my opinion, the measured levels of traffic-related pollutants are surprisingly low for being measured next to a 200,000 vehicles/day freeway. In this sense, it would be helpful to look at rainfall data in the area during the measurements.

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Interactive Discussion
Discussion Paper

We appreciate the reviewer's insights. This manuscript focuses on PM measurements such as organic matter which are typically only slightly elevated next to the roadway (e.g. from Zhu, Y. F., W. C. Hinds, et al. (2002). "Concentration and size distribution of ultrafine particles near a major highway." Journal of the Air & Waste Management Association 52: 1032-1042.). This is in contrast to other species such as BC, CO, NO2, and ultrafine particles, which are often quite high next to the roadway. In other manuscripts we are working on, we go into these other measurements in more detail. BC, for example, is $\sim$ three times higher at the near-roadway site compared to a urban site more than 2 km from the freeway. As the reviewer notes the traffic-related pollutants (such as BC, CO, etc) should and do have higher concentrations near the roadway; however, as the OM is not predominantly from direct emissions on the roadway, OM concentrations are not enhanced. There was no rainfall after January 5 until January 23, so this is unlikely to be the cause of the low concentrations. The 2000-2001 Las Vegas Valley Visibility/PM2.5 study is the most thorough other study of the area, even though it was 10 years ago. In wintertime, sulfate concentrations were on average less than 0.5 μg/m3, similar to our study (available from http://www.clarkcountynv.gov/Depts/daqem/Pages/ResearchProjects.aspx) ; including summer as well, the average annual concentrations were between 1.3 and 0.98 μg/m3 at three sites. Nitrate on average was between 0.2 and 0.6 μg/m3. There has also been some speciated PM2.5 data collected as part of EPA's CSN network, from 2002 to 2007. Using the data from this 5 year period, average sulfate is 1 μg/m3, and average nitrate is 0.89, though sulfate is lower and nitrate is higher in the winter. Concentrations are indeed low for an urban environment, but this is part of why Las Vegas is somewhat unique. There are very few sources upwind, so the amount of transported secondary organic carbon and ammonium sulfate is low.

Other minor comments are showed below: 1. Can authors justify their choice of maximum dQ of 1% in general? I am not saying this value is too high or too low but it would be very interesting to know the reason of this threshold (and of possible exceptions allowed) since the degree of tolerance can be critical when determining the spectrum

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of possible solutions/rotations.

We don't have a specific justification, other than thinking that a dQ of 1% is "small", so that changes to the solution will presumably be somewhat small, while still informing us as to any rotational ambiguity of the solution. As an example of using a higher dQ, we provided the BBOA to Chestnut Smolder profile pull using a dQ of 1% and 3%. Here Q changes by 1.3% in the latter pull (instead of 0.4% in the first pull), though the solution does not appear to improve. In future EPA PMF guidance, we plan to conduct more systematic testing to provide examples of what dQ values may be most appropriate.

2. According to this, please revise the maximum dQ allowed for the edge points pulling: table 2 says 31%, but in the text is different.

There was a typo in the text, we've changed the text to say 31% instead of 13%.

3. The average value of OM in page 22917 has changed from the previous version of the paper, but the averages of different wind scenarios did not. Please check if they need revision as well.

We apologize for this oversight, and have checked all our values and provided the correct values.

4. A modest correlation was found between OM and BC, CO and NOx. Some discussion is needed here about the formation and transport of SOA

We've added discussion on the implications from these correlations, i.e., the modest correlation among these parameters suggests that a large fraction of the OM is probably secondarily produced.

5. Nitrate does not correlate with any other pollutants (traffic-primary pollutants nor SV-OOA). Can authors discuss possible reasons for this? Too low and/or sporadic concentrations measured?

Indeed, the nitrate concentrations were generally low (median of 0.54 μg/m3), and

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were very episodic. Two episodes account for all concentrations above 2 μg/m3, and were associated with transport from California followed by stagnant conditions. In addition, there is little ammonia generated in the region, as it is entirely desert (e.g., http://nadp.sws.uiuc.edu/nh3net/). Since we are ammonia-limited, ammonium nitrate levels may be lower than expected for an urban environment; a map in Figure 2 of 2005-2008 ammonium nitrate concentrations in the U.S. suggests ammonium nitrate concentrations are on average < 1 μg/m3 in the area. If nitrate formation is limited by ammonia availability, we would not expect it to correlate well with OM or OM components.

6. There seems to be a repetition in the abstract when HOA is mentioned to be about half of OM under downwind conditions. We've revised the abstract to exclude this repetition.

7. It would be nice to look at the wind direction patterns. We do not know when the wind is blowing from the sector of the freeway. We've added wind direction to the time series plot in Figure 3 and Figure 8. We've also included a wind rose in Figure 1.

8. Please, clarify also in the methodology section that measurements were made outdoor. We've added that measurements were made outdoors to the first sentence in the Methods section.

9. Please, justify the choice of not including inorganic species in the source apportionment analysis. High S/N? We have many variables to help us understand the OM composition, with more than 100 m/z fragments, so we use these to apportion the OM. We have only total sulfate and total nitrate, so similar to the approach employed in Chemical Mass Balance, we do not try to fit them to multiple sources (i.e., we have no additional information on sulfate or nitrate to link them back to sources or atmospheric phenomena that would be useful in a PMF analysis). Note we are not trying to apportion total PM, since we are missing metals and soil elements, so only apportion OM.

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10.The additional fifth factor could be related to BB, but BB is actually increasing its contribution. Is that possible? This is a good point. If the 5th factor is related to BB, then indeed BB is under-apportioned in the 4 factor solution, meaning that some of the SV-OOA factor in the 4 factor solution is related to BB. This is certainly possible, though as we were unable to determine the provenance of the 5th factor, we used the 4 factor solution as our "final" one.

11.In Figure 3 Nitrate shows a minimum value, often repeated. Is that the Detection Limit? Nitrate concentrations were essentially zero for a number of hours; detection limits for nitrate are quite low, for example, 0.004 μg/m3 as reported in Huang et al., 2010 (http://www.atmos-chem-phys.org/10/8933/2010/acp-10-8933-2010.pdf). The y-axis minimum is zero.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 22909, 2011.

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Interactive Comment

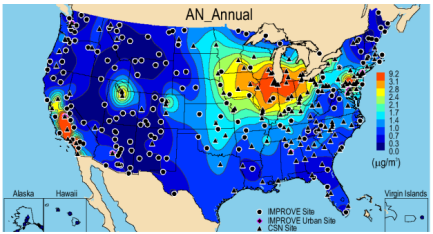

Fig. 1.

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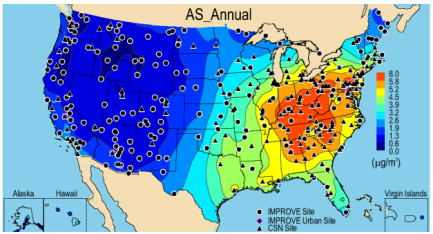

Fig. 2.

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