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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Atmospheric submicron aerosol composition and particulate organic nitrate formation in a boreal forestland–urban mixed region" *by* L. Q. Hao et al.

Anonymous Referee #1

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This paper presents the PMF analysis of AMS data in a semi-urban location in Finland. This type deployment and analysis is not particularly new, however the authors go into some detail exploring an apparent organic nitrate signature. The results are possibly a little ambiguous, but given the potential importance of this chemical category, this result is relevant to ACP and still worth reporting. The manuscript is generally well written and scientifically sound, with only two major problems (see below), however fixing these merely entails caveating and toning down the conclusions and providing more supporting data from the PMF analysis, so I would recommend this be published subject to minor corrections.

General comments:

The method of calculating organic nitrate on page 17278 is not quantitatively sound. To begin with, the authors are not explicit as to whether they are calculating the mass of the nitrate functional group present or the mass of organic molecules containing nitrate. But regardless of this detail, the authors omit to discuss the relative ionisation efficiency (RIE) of organic nitrate, which is not very well constrained in general, owing to the complications associated with thermal desorption and the dearth of laboratory data. This is discussed in more detail in Aiken et al. (doi: 10.1021/ac071150w) and Farmer et al. (2010), but it basically means that the mass concentration analysis presented here cannot be viewed as quantitatively accurate. I would regard the quantitative outcomes of these calculations as highly uncertain and they should be presented as such. However, they are still of interest in a qualitative sense.

The authors must provide more data in the supplementary material to justify their choice of PMF solution. Specifically, why the 4-factor and 6-factor solutions were rejected and why a nonzero value of fpeak was used, given that Paatero et al. (doi: 10.1016/S0169-7439(01)00200-3) recommends that fpeak=0 is used for environmental data.

Specific comments:

Page 17275-6: One cannot apportion OOA to biogenic and anthropogenic this way. What is reported by PMF as 'LV' and 'SV' OOA do not represent organic types in themselves but end members in a continuum of a highly complex chemical composition that is entirely dependent on the range of organic aerosols observed at a specific location and time. As such, their exact nature will vary dataset to dataset and no quantitative inferences regarding precursors can be derived from their abundances.

Page 17278, line 4: The mere presence of NOx ions in an organic PMF factor alone does not prove that they are organic in origin, as there could be a level of covariance between an organic component and inorganic nitrate for whatever reason, although the

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high 30/46 ratio in factor 4 in particular would indicate that it is not ammonium nitrate.

Technical corrections:

Page 17265, line 16: The technique employed by Zhang et al. (2005) was not strictly PCA.

Page 17269, section 2.3: The instrument models should be specified for the supporting Measurements

Page 17270, line 7 (and elsewhere): 'motorway' does not need hyphenating

Page 17274, line 15: Fix "Allan et all." to "Allan et al."

Page 17275: It is not clear why there are bracketed numbers in this paragraph. I recommend making this clearer or removing them.

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 17263, 2014.