

Interactive comment on “Source apportionment of PM₁₀ in a North-Western Europe regional urban background site (Lens, France) using Positive Matrix Factorization and including primary biogenic emissions” by A. Waked et al.

Anonymous Referee #2

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This paper presents the PMF analysis of a comprehensive analysis of PM₁₀ samples taken over a long period at a site in Western Europe. While there is no big news in terms of new particle types being identified or new analytical or mathematical techniques being employed, the sheer size and scope of the dataset means that conclusions regarding the trends of particle types can be drawn with high confidence. Work of this nature would certainly fall within ACP's remit and contribute to atmospheric science in general.

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The analysis is organised and presented well, allowing relevant and useful information to be extracted easily. The authors also show good attention to detail regarding the individual factors derived and any technical or scientific details arising from the findings. I therefore recommend that this is published subject to minor corrections.

General comments:

No mention is made about whether the authors considered rotational ambiguities in the solution sets. If any tests were performed, it would be very appropriate to document them in the supplementary material. I am also concerned that not enough attention has been paid to documenting the reasons for choosing a 9 factor solution. The only reason reported in the supplementary material appears to be because at 10 factors starts to produce less favourable diagnostics from the bootstrapping analysis. It would be more informative if more detail on the rejected solution set could be given, such as the authors' opinions on whether the profiles were physically meaningful, or the results of any other tests (e.g. seed variation) performed on the data.

I think the emphasis placed on a 'discrepancy' between this and PMF results from the AMS is largely unwarranted for the pure and simple reason that two different size fractions are being studied here. Even if the results of the analytical methods were directly comparable (which they are not), I personally would not expect a factorisation of PM₁ (which is dominated by combustion and secondary aerosols) to yield the same results as PM₁₀ (which includes dust, sea salt and biological particles) anyway. That is not to say that the discussion comparing the techniques isn't warranted, but I would not interpret them as being in disagreement.

Specific/technical comments:

Page 25331, line 17: Why were these species excluded and not dramatically 'down-weighted' (i.e. uncertainties artificially increased)?

Page 25336: I would suggest the authors do not use uppercase sigma to denote 'total'

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in line with freeform text (e.g. 'sugars'), as combining the two makes reading difficult. Simply writing e.g. 'total sugars' or using an acronym would make it clearer.

Page 25336: mg/g or mg g⁻¹ are not legitimate units. The fractions should be expressed as a decimal or a percentage, qualified as 'by mass' if necessary.

Page 35339, line 17: Correct 'specie' to 'species'.

Page 25342: Both on- and offline measurements of PM₁₀ are known to be affected by losses of semivolatile ammonium nitrate, especially the methods that require the substrate to be heated to remove water vapour. While the authors refer to this later on, the authors should comment on whether they think that this is an issue for these measurements here.

Page 25350, line 2: The turn of phrase 'as a matter of fact' is not appropriate in this context, because the PSCF does not point to an irrefutable detail.

Page 25350, line 5: Change 'highest ships density' to 'highest density of ships'

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 25325, 2013.