

## Comment on acp-2021-197

Anonymous Referee #1

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Referee comment on "Characterization of non-refractory (NR) PM<sub>1</sub> and source apportionment of organic aerosol in Kraków, Poland" by Anna K. Tobler et al., Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2021-197-RC1>, 2021

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This paper presents an analysis of a Q-ACSM dataset from Krakaw, applying the rolling window ME2 algorithm. The methods are appropriate and the paper is overall well written, but the level of analysis is very shallow and does not extend beyond the validation of an appropriate solution set from the analysis. Given no scientific conclusions are presented, this to me seems to be a very clear-cut case of a manuscript that should be published as a measurement report rather than a research article. If the authors wish to present this as a technical development of the apportionment tools, then this could be classified as a technical note. However, for this to be a valid technical note, much more detail should be presented on the methodology and how this advances the current state of the art.

If the paper is reclassified as a measurement report (my preferred option), then I can recommend publication subject to the following comments:

- The rejected 6 factor solution should be shown in the supplement.
- Describing how eBC<sub>tr</sub> and metal were used to constrain and validate some of the factors is a little unsatisfactory. The authors should present correlations visually, e.g. with scatter plots. The same should be done with eBC<sub>wb</sub>, if only to demonstrate the weak performance of this. Related to this point, the authors should be consistent between whether they call the Aethalometer data product 'eBC<sub>wb</sub>' or 'eBC<sub>sf</sub>'.
- It's really not apparent what criteria were used to determine the AAE values in figure S2. Do these correspond to a particular level in the PDF?
- Using the 'Chl' product to constrain the CCOA factor could be problematic because ammonium chloride is semivolatile and may vary with temperature and relative humidity. Furthermore, an abundance of nitric or sulphuric acid may displace it from the particles, which would modulate the data in ways not representative of the actual coal OA. Specifically, this could put an artificial diurnal cycle on the factor. Can the authors verify this is not the case? Without evidence to the contrary, I would expect it safer not to use this as a constraint.
- The CCOA factor contains a lot of small signals at high m/z channels that do not display much of a mass spectral pattern. Can the authors be sure that this is 'real' signal and not noise?
- The percentiles on figure 6 are not visible to me.
- Were correlations between any of the metals and any of the other factors noted?