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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Source apportionment of $PM_{2.5}$ in Cork Harbour, Ireland using a combination of single particle mass spectrometry and quantitative semi-continuous measurements" *by* R. M. Healy et al.

Anonymous Referee #2

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General Comments:

This manuscript describes a thorough and careful analysis of the sources contributing to PM2.5 in Cork Harbor, in Ireland. The authors use established methods, especially PMF, to work with data from a variety of instruments including single particle mass spectra (ATOFMS), quantitative EC, OC, sulfate, PM2.5 mass, and particle number. They carried out PMF using the temporal trends observed after clustering the single-particle data, which is emerging as the most useful way to include single-particle data in PMF models. The authors include single-particle measurements of potential sources

(smokeless coal, peat, and wood) to better characterize their contributions in the PMF model. The paper is within the scope of ACP and will be of interest to ACP readers, although this could be enhanced as described below. Overall, while this paper does not break new ground in methodology, it applies an established method to a new location in a new way. This detailed study of a shipping harbor should be of great interest to those trying to characterize the sources of pollution from such locations, and the authors should do much more to focus in on this aspect of their work and to put it into the context of other work that examines particles in such locations (e.g. Ault, et al., Environ. Sci. Technol., 2009, 43, 3500.).

The authors have done a careful job of presenting the work they did, and the manuscript is generally well organized and well written (see detailed comments below). It is an appropriate length, and all of the figures are important. In the course of their analysis, the authors note trends in the detection of single-particle spectra that correspond with their wood, peat, and smokeless-coal particle types, with associated nitrate in the mass spectra. They note that the addition of nitrate occurs temporally in that order and propose that this might be a measure of the relative hygroscopicity of the particle types, with the more hygroscopic particles taking up nitrate more quickly. This seems to be borne out by the data that they reference, but it would be nice to test this hypothesis with laboratory measurements of the hygroscopicity. The authors also note that oligomers are detected in particles that they say correspond with relatively fresh domestic solid fuel combustion classes. They note that the particles appear to be formed rapidly as compared to reported rates of detection in smog chamber experiments. The authors should note that in those referenced experiments, the particles were nucleating in the absence of seed particles, and thus the observed formation rates for oligomers include the nucleation of particles as well. The spectrum shown in Figure 11c indicates that the oligomers are found in particles that contain other materials (sulfates, nitrates, etc.), and thus it could be likely that they form on pre-existing particles.

Overall, I would like the authors to focus their conclusions on the broader implications

of their work, rather than on the methodology – how applicable is their work to other ports and how does it compare to the characterization of other ports that are in the literature, what are the prospects for using this method for characterizing larger areas or more complex areas, and how much does their conclusion rely upon having the authentic sources measured?

Specific comments:

1. Overall, I would suggest that the authors consider replacing the exhaustive lists of ions observed in the single-particle mass spectra that are included in the text with tables. This will decompress the text somewhat, make the text more readable, and make it easier to compare the ions observed in the various classes of particles.

2. p. 1041, line 13-14: The authors discuss the separation of EC-phos-fresh and EC-phos-aged, but do not comment there about whether these classes are different in any way other than size. The table suggested in comment 1, above, would make it easier to see this, and I would recommend adding a comment in the text as well.

3. p. 1043, line 10: The authors state that the number of factors used in their PMF calculations was varied until the “most reasonable” results were obtained. This phrase should be defined.

4. p. 1044, line 11: As there is expected to be relatively constant transmission through an aerodynamic lens in the size range for which it is designed, the authors might want to explain why there is expected to be little effect for not scaling for size-related transmission efficiency into the ATOFMS.

5. p. 1045, line 18: The authors discuss the strong signal for sulfate observed in the freshly emitted combustion particles from their source tests, which they note is different from the measurements observed by other researchers. However, they do not discuss the potential effect of their source tests being done in an outdoor stove. Could the outdoor air influence their observed spectra?

6. Throughout the paper, the authors should more closely relate their findings to those that have been published, especially related to their source profiles. Coal has been sampled before with single-particle mass spectrometers (and should be referenced), but is this smokeless-coal different in some way? Similar questions arise with wood, which has been sampled and reported in numerous papers. Peat, I believe, is new to this work.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 1035, 2010.