

We sincerely thank the referee for the helpful and constructive comments.

1) Line 323: For P1, the major potential source region appears to be southwest of Hyytiälä, mainly southern Scandinavia. However, the subsequent discussion focuses primarily on the previously reported influence of southeastern air masses. Are there previous studies identifying characteristic aerosol sources or processes associated with transport from the southwest/southern Scandinavia? If available, such information would help interpret the P1 event. Otherwise, please clarify that the specific sources associated with this PSCF region cannot be determined from the present analysis.

The corresponding part has been revised as follows.

“The potential source regions for the accumulation mode particles during P1 were located mainly southwest of the sampling site, the area covering southern Scandinavia and extending to the Kattegat region (northern Denmark and southwestern Sweden) (Fig. 3a). Significant anthropogenic OA precursor emissions in the Kattegat region, as inferred from large NO_x and non-methane volatile organic compound emissions there (Kuenen et al., 2022) may have contributed to the event, although the contribution from further long-range transport that was not detected in the PSCF analysis is not ruled out. Furthermore, Fig. 3a indicates additional contributions from the southeast, particularly Eastern Europe.” (Lines 319–324).

2) Table 1: Please remove the parentheses around the references in the first column for consistency with standard reference formatting.

The parentheses around the references in the first column of Table 1 have been removed.

3) Lines 365–368: Please also clarify that O:C or the degree of oxygenation does not necessarily provide a direct measure of volatility. This distinction is one reason why MO-OOA/LO-OOA terminology is preferred over LV-OOA/SV-OOA when volatility is not directly constrained (e.g., Zhang et al., 2011). Therefore, the correspondence between MO-OOA and previously reported LV-OOA should be described cautiously.

This clarification has been added at Line 372–373.

4) Lines 386–391 and 414–418: I still find the discussion of HOA somewhat disconnected in its current location. Since the later discussion shows that HOA exhibits even stronger relationships with some hydrocarbon-type ions associated with BSOA (e.g., C₅H₇⁺ and C₆H₉⁺), I suggest integrating these two discussions. For example, the paragraph currently at Lines 386–391 could be moved to around Lines 414–418, where the overlap between HOA and the less source-specific hydrocarbon fragments is discussed. This would provide a more logical basis for the suggestion that HOA may contain some biogenic influence, while also making clear that these ions are not sufficiently specific to distinguish biogenic from fossil-fuel HOA.

Section 3.2.3 has been revised to improve its logical flow in response to the reviewer’s comment. (Lines 389–421).

Other changes

The following reference has been added. All the changes, including those for formatting issues, are presented in the track-changes version of the manuscript.

Kuenen, J., Dellaert, S., Visschedijk, A., Jalkanen, J. P., Super, I., and Denier van der Gon, H.: CAMS-REG-v4: a state-of-the-art high-resolution European emission inventory for air quality modelling, *Earth Syst. Sci. Data*, 14, 491-515, 10.5194/essd-14-491-2022, 2022.