

Overall assessment

This manuscript presents a comprehensive characterisation of the molecular composition, sources, and oxidative potential (OP) of PM_{2.5} during the heating season in Weifang. The combination of online measurements, molecular characterisation using FIGAERO-CIMS, PMF source apportionment, hierarchical clustering, and DTT measurements provides a valuable dataset for investigating the sources and molecular characteristics associated with aerosol OP.

The manuscript contains substantial analysis, and the overall dataset appears suitable for addressing the research questions. However, I found that several interpretations, particularly those concerning the identification of compounds and sources driving OP, are stated more strongly than the statistical analyses directly demonstrate. Since a considerable part of the manuscript relies on correlations between PM components, molecular compounds, source factors, and OP, I think the distinction between association, source attribution, and contribution to OP needs to be made more clearly. Some reorganisations of the Results would also considerably improve the flow of the manuscript.

I therefore have the following comments.

Major comments

1. Interpretation of PM_{2.5}–OA and PM_{2.5}–nitrate correlations

Around lines 302–307, the authors report correlations of PM_{2.5} with OA ($r=0.80$) and nitrate ($r=0.71$) and use the stronger OA correlation to suggest that OA is a key driver of PM_{2.5} pollution. The correlations are useful for showing that these components covary with PM_{2.5}, but I am not convinced that the difference between ($r=0.80$) and ($r=0.71$) by itself supports the stronger interpretation concerning their relative importance. This is particularly relevant because OA and nitrate are both components of PM_{2.5}, and nitrate is reported to contribute approximately 29% of PM_{2.5} compared with approximately 22% for OA.

If the intention is to establish that OA has a particularly important role in PM_{2.5} pollution, I suggest that the authors clarify the basis for this conclusion rather than relying mainly on the difference between these two correlation coefficients. If additional analyses presented later in the manuscript support this interpretation, connecting them more explicitly here would strengthen this discussion.

2. Statistical robustness and interpretation of the molecular-level OP analysis

A substantial part of the later analysis relates individual molecular compounds to OP. Around lines 660–662, compounds associated with OPm are selected from the 274 high-abundance OA compounds based on their individual correlations with OPm ($p < 0.05$). Given the relatively limited number of offline OP samples and the large number of compound correlations being evaluated, I think some additional clarification of the statistical treatment would be useful.

In particular, when a large number of correlations are tested using $p < 0.05$, some compounds may be identified as significant simply by chance. Could the authors clarify whether this was considered when selecting the OPm-associated compounds? If not, I suggest that the authors consider an appropriate statistical approach or discuss this limitation, particularly since the selected compounds are subsequently used in the source-attribution analysis

The subsequent MLR analysis (lines 655–674) also requires some clarification regarding both the methodology and its interpretation. As I understand it, the authors use the MLR to determine which of the four sources is most strongly associated with each compound previously identified as correlated with OPm. This appears useful for identifying the likely source associations of these OPm-correlated compounds. However, at lines 669–674, the manuscript goes a step further and states that a source contributing strongly to a positively correlated compound thereby exerts a “significant positive influence on the overall OP”, whereas a source associated with a negatively correlated compound may exert a “negligible or suppressive effect” on OPm. It is not clear to me how these conclusions follow directly from the analysis. An association between a source and a compound that is itself correlated with OPm does not necessarily demonstrate that the source influences OPm through that particular compound. I suggest that the authors clarify the basis for this interpretation or use wording that more closely reflects the associations demonstrated by the analysis

In addition, the MLR is introduced in the Results, but I could not identify a corresponding methodological description of the analysis in the Methods. Please provide sufficient methodological information to explain how the MLR was performed, including how the regression coefficients were obtained and normalised and how their relative magnitudes were used for source attribution.

3. Interpretation of the January 12 and February 11 high-OPv episodes

Around lines 543–554, the interpretation of the two high-OPv episodes is not entirely clear to me. On Jan 12, the authors report a secondary inorganic contribution of 5% and an elevated levoglucosan concentration ($0.59 \mu\text{g m}^{-3}$; 1.9% of OA), which are interpreted as evidence of strong secondary formation and solid-fuel-burning influence. In contrast, on Feb 11, SO_2 and metals associated with fireworks increased

approximately tenfold above the campaign average. The similar OPv values during the two episodes are then used to conclude that OP driven by OA during biomass-burning events could rival that driven by transition metals during fireworks. While levoglucosan indicates biomass-burning influence on Jan 12, I do not understand how the presented evidence (levoglucosan ~ 1.9%) establishes that the high OPv on this day was specifically driven by biomass-burning OA. Similarly, the description of a 5% secondary inorganic contribution as “high” is unclear. Please clarify the source contributions on Jan 12 and the evidence supporting the attribution of its high OPv to biomass-burning OA.

4. Interpretation of intrinsic OP measurements and the conclusion regarding “aged primary OA”

The discussion of the standard-compound measurements requires some clarification. Around line 739, malic acid is reported to have an intrinsic OPm of approximately 22 pmol min⁻¹ µg⁻¹ and is described as having negligible activity. However, the campaign-average ambient OPm is reported as approximately 24 pmol min⁻¹ µg⁻¹. It is therefore unclear on what quantitative basis an intrinsic OPm of 22 is considered “negligible.” Or is this supposed to be 2.2 pmol min⁻¹ µg⁻¹? Please clarify the criterion used for this terminology and revise it if appropriate.

I also suggest reconsidering the interpretation around lines 779–783. The association of high OPm with solid-fuel combustion, together with the moderately oxygenated character of OP-associated compounds is good. However, I am not convinced that the presented analyses uniquely demonstrate that this represents aged primary OA. For example, oxidation of VOCs emitted by solid-fuel combustion followed by SOA formation could potentially produce a similar association and molecular signature. If the authors can distinguish aged POA from combustion-derived SOA using the available measurements, the evidence should be explained more explicitly. Otherwise, terminology such as oxygenated OA associated with solid-fuel combustion may better reflect what is directly supported by the observations.

Specific and technical comments

1. **Around line 346:** EC is subsequently referred to, but I could not identify an independent EC measurement in the described methods. The manuscript describes BC measured by AE33, whereas EC is only explicitly mentioned elsewhere as a water-insoluble component excluded from the DTT analysis. Please clarify whether EC was independently measured or whether EC here refers to BC, and use BC/EC terminology consistently.
2. **Lines 377–379:** The vehicle-emission factor appears to peak at approximately 10:00, whereas the CO maximum is described as occurring between 08:00 and

09:00. Please clarify this temporal difference if the CO profile is being used to support the vehicle-source attribution.

3. **Around line 441:** The discussion uses levoglucosan to support the attribution to coal/solid-fuel combustion. Please report the corresponding correlation coefficient with the relevant PMF factor, if this correlation forms part of the source-attribution evidence.
4. **Lines 449 and 460:** Please provide the corresponding r values for the correlations discussed.
5. **Lines 591 and 597:** Please report the corresponding correlation coefficients rather than describing the relationships only qualitatively.
6. **Lines 613–619:** On 22 November and 28 March, secondary inorganic aerosol (76%) and dust (47%), respectively, dominate the PM_{2.5} mass, while OPm remains comparable to the campaign average. These observations are used to conclude that secondary inorganics and dust are not the key components driving OP. If so, could the authors clarify which components or sources are thought to account for the observed OPm on these two days? Was it still biomass burning and coal combustion?
7. **Line 680:** The terminology used to describe correlations should be made more consistent throughout the manuscript. For example, around lines 496–498, $r=0.81$ is described as a high correlation whereas $r=0.72$ is described as moderate, while around line 680 $r>0.4$ is used when describing compounds as “significantly correlated.” As mentioned in (1) of the Major comments, there doesn’t seem to be too strong of a difference between these two correlations. Also, Please clarify whether defined ranges are being used to classify correlation strength and apply them consistently. In addition, correlation strength (r) should be distinguished from statistical significance (p -value or confidence interval). Since correlation analysis is central to the manuscript, I suggest reporting the numerical r values where these relationships support important interpretations rather than relying only on qualitative terms. A supplementary table summarising the principal correlations used for source attribution and OP interpretation may also improve transparency.
8. **Lines 760–762:** Please report the relevant correlation coefficient(s) supporting the relationship discussed here.
9. **Section 3.3:** The authors first discuss correlations between individual FIGAERO-CIMS compounds and the PMF-derived sources (Fig. 3), and subsequently group 274 high-abundance compounds into six clusters using HCA and again relate these clusters to the PMF sources (Fig. 4). I found this progression somewhat repetitive. Since the HCA groups compounds with similar temporal behaviour, would it provide a clearer narrative to introduce the clusters first and discuss relevant individual compounds/tracers within each cluster? Alternatively, please clarify what distinct information the individual-compound analysis in Fig. 3 provides beyond the subsequent HCA analysis.

10. **Line 100:** The sentence "... when fresh anthropogenic mixed with biogenic emissions...", appears incomplete. Can be rephrased as "...when fresh anthropogenic emissions are mixed with biogenic emissions..."
11. **Line 172:** The Methods state that offline PM_{2.5} sampling was conducted from 15 November 2023 to 15 March 2024. However, the figures and later discussion appear to include measurements extending to approximately 30 March. Please check the sampling/measurement period and make the dates consistent throughout the manuscript.
12. **Lines 188–189:** The text currently states that "OC was calculated as TC minus OC," which appears to be a typographical error. The Methods state that BC was measured using an AE33 and TC using a TCA-08, while the Supporting Information indicates that OC was obtained from the difference between TC and BC. I therefore assume this should read $OC = TC - BC$. Please correct/clarify.
13. **Line 673:** Consider replacing "whereas" with "while"