We thank the reviewers for their helpful comments. Our point-by-point response can be found below. The reviewers' comments are in *italics* and changes made to the manuscript are in blue. The line number mentioned corresponds to the tracked-change version. All the changes made do not affect the conclusions in the manuscript.

Referee#1

1. Samples were collected using different filters, samplers, flow rates, and protocols. Although blank correction is mentioned, please describe campaign-specific field/procedural blanks to demonstrate cross-site comparability.

Reply: Thank you for the comment. During each sampling campaign, field blanks were prepared by placing blank filters in the sampler for several minutes without activating the sampling pump. The filters were subsequently retrieved, wrapped in aluminum foil, and stored under the same conditions as the collected samples until analysis. All field blanks were subjected to the same pretreatment and analytical procedures as the particulate matter samples. We have added this description in section 2.1 of the revised manuscript.

Line 139: During each sampling campaign, field blanks were prepared by placing blank filters in the sampler for several minutes without activating the sampling pump. The filters were subsequently retrieved, wrapped in aluminum foil, and stored under the same conditions as the collected samples until analysis. All field blanks were subjected to the same pretreatment and analytical procedures as the particulate matter samples."

2. Lines 176-186: Horseradish peroxidase assay is not specific to H2O2, did you ever consider organoperoxides or other oxygenated species that act as efficient electron-acceptors? The quantified H2O2 can only be treated as equivalent result

Reply: Thank you for the comment. The assay used in this study was the Sigma-Aldrich Fluorimetric Hydrogen Peroxide Assay Kit (MAK165), which is based on the Amplex Red/horseradish peroxidase (HRP) reaction and is specifically designed for H_2O_2 quantification. However, we agree that although the assay is widely used for H_2O_2 quantification, some organic hydroperoxides can also serve as substrates for HRP and contribute to resorufin formation. Particularly in chemically complex aerosol extracts, potential contributions from other peroxide/oxygenated species cannot be completely excluded. We have clarified this potential limitation in the revised manuscript.

Line 191: Although the assay is designed for H_2O_2 quantification, potential contributions from other peroxide or oxygenated species in chemically complex aerosol extracts cannot be completely excluded.

3. In method section, QA/QC should be discussed or informed

Reply: Thank you for the comment. We have provided information on the quality assurance and quality control procedures for the chemical and ROS measurements in the Methods section of the revised manuscript.

Line 210: 2.6 Quality assurance and quality control

For UHRMS analysis, blank samples were analyzed to identify and remove background features, and molecular formulas were assigned using the mass-error and chemical-plausibility criteria described above. a mixture of standards of 12 organic compounds covering a wide range of molecular weights (122-230 Da) and polarity (retention time from 1.34 to 7.12 min) was regularly measured to confirm the stability of the analytical method. The mean variability of retention time was less than 10 seconds, while the mean relative standard deviation of the peak intensity was less than 15%.

For H_2O_2 measurements, concentrations were determined using calibration curves prepared with H_2O_2 standards, with blank measurements used for background correction. For EPR measurements, blank samples were analyzed under the same conditions as the aerosol extracts to account for background signals. All samples within each analysis were processed using the same analytical protocols and data-processing procedures to ensure consistency across sites and experiments.

4. Line 213-217: The MCR boundaries were developed primarily for CHO compounds, whereas the present study applies them to CHON, CHOS, and CHONS formulas. Please justify this extension and provide a CHO-only or formula-class sensitivity analysis.

Reply: We thank the reviewer for raising this important point. We agree that, although MCR can be mathematically calculated for a molecular formula of $C_cH_hO_oN_nS_s$, the MCR = 0.9 boundary was established and structurally interpreted primarily using CHO compounds. The rationale for extending the calculation to CHON, CHOS, and CHONS formulas is that MCR is a formula-based descriptor derived from DBE and the number of oxygen atoms, with nitrogen explicitly included in the DBE calculation. It can therefore be calculated consistently for these heteroatom-containing formulas. In the current study, the resulting OOC/UOC classification is used as an operational descriptor of the balance between oxygenation and unsaturation rather than as definitive identification of the actual functional groups present in each molecule.

Nevertheless, we acknowledge that nitrogen- and sulfur-containing functional groups may affect oxygen content and unsaturation differently from the corresponding functionalities in CHO compounds. Therefore, mathematical applicability alone does not demonstrate that the

MCR = 0.9 boundary has the same structural meaning for every formula class. We have clarified this limitation in the revised manuscript and avoided implying that the boundary has been independently validated for all heteroatom-containing compounds.

According to the reviewer's suggestion, we performed a CHO-only sensitivity analysis for the ambient fine PM samples, since the laboratory-generated SOA samples mainly consisted of CHO compounds. The relative peak-area-weighted fractions of OOC and UOC and their ratio (RF_{OOC}/RF_{UOC}) were recalculated using only CHO formulas while retaining the MCR = 0.9 boundary. This sensitivity analysis specifically tests whether the composition–ROS relationships depend on applying the MCR boundary to CHON, CHOS, and CHONS formulas.

The CHO-only results retain the relationships reported in Figure 4. As shown in the new Figure S8, for the ambient fine PM samples (Mainz, Beijing, Xi'an, Guangzhou, and Shanghai), the CHO-only RF_{OOC}/RF_{UOC} ratio showed positive saturating relationships with both total ROS yield ($R^2 = 0.87$) and H_2O_2 yield ($R^2 = 0.86$). When all six fine PM samples, including Hyytiälä, were considered, the positive linear relationship with radical yield was also retained, with $R^2 = 0.96$. Although the correlations with total ROS and H_2O_2 were somewhat weaker than those obtained using all molecular-formula classes ($R^2 = 0.94$ and 0.98, respectively; Figure 4), the same overall relationships were observed in the CHO-only analysis. This supports that the observed relationships are not dependent on applying the MCR = 0.9 boundary to CHON, CHOS, and CHONS formulas.

We have added this analysis as Figure S8 and added related discussion in Section 3.3 in the revised manuscript.

Line 508: Since the MCR = 0.9 boundary was developed primarily for CHO compounds, we further performed a sensitivity analysis using only CHO formulas to assess whether the relationships described above were affected by its application to CHON, CHOS, and CHONS formulas (Figure S8). For the five ambient fine PM samples (Mainz, Beijing, Xi'an, Guangzhou, and Shanghai), the CHO-only RF_{OOC}/RF_{UOC} ratio showed positive exponential relationships with total ROS ($R^2 = 0.87$; Figure S8a) and H_2O_2 yields ($R^2 = 0.86$; Figure S8b). When Hyytiälä was included, a positive linear relationship with radical yield was also observed ($R^2 = 0.96$; Figure S8c). These relationships were consistent with those obtained using all molecular-formula classes ($R^2 = 0.94$, 0.98, and 0.96, respectively), although the associations with total ROS and H_2O_2 were somewhat weaker. Thus, the observed composition–ROS relationships do not depend solely on the inclusion of heteroatom-containing formulas in the MCR classification. However, this sensitivity analysis does not independently validate the MCR = 0.9 boundary for CHON, CHOS, or CHONS compounds.

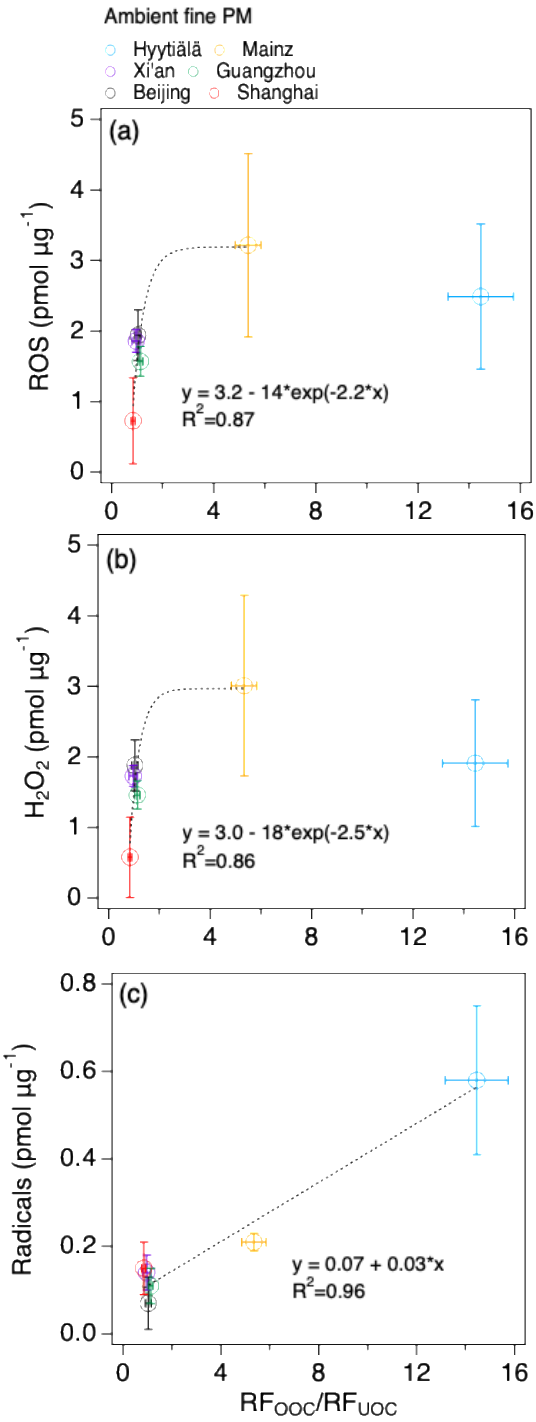

Figure S8. Correlations of mass-specific ROS yields (a), H_2O_2 yields (b), and radical yields (c) with CHO-only RF_{OOC}/RF_{UOC} ratio in ambient fine PM samples. The RF_{OOC}/RF_{UOC} ratio was recalculated using only CHO molecular formulas. The dots and error bars represent the mean values and standard deviations, respectively.

5. Lines 372-382: This interpretation is overstated. ROS-forming efficiency depends on composition, medium, pH, and assay endpoint. The present results represent water-extractable H2O2/radical yields, not general "intrinsic ROS activity." Tong et al. (2018) demonstrated medium-dependent (water vs. surrogate lung fluid) ROS yields for naphthalene SOA, while Li et al. (2022) showed strong pH effects on the peroxide content and oxidative potential of biomass-burning HULIS. Aqueous ROS yields also should not be extrapolated directly to cellular ROS responses. Please qualify this conclusion throughout the manuscript.

Reply: Thank you for the comment. We agree that the measurements reflect water-extractable H_2O_2 and radical formation under the present assay conditions and should not be interpreted as a general measure of intrinsic ROS activity or extrapolated directly to cellular ROS responses. We have therefore qualified the relevant statements throughout the manuscript and clarified that ROS-forming potential can depend on factors such as extraction medium, pH, and assay endpoint. We have also added discussion of the medium-dependent ROS yields reported by Tong et al. (2018) and the pH-dependent peroxide content and oxidative potential reported by Li et al. (2022) to further acknowledge these limitations.

Line 401: Specifically, aerosols enriched in highly oxygenated, biogenic SOA components are expected to exhibit greater aqueous ROS-forming potential,

Line 406: However, ROS-forming potential can depend on experimental conditions, including the extraction medium, pH, and assay endpoint. For example, Tong et al. (2018) reported medium-dependent ROS yields for naphthalene SOA in water and surrogate lung fluid, while Li et al. (2022) showed that pH strongly affected the peroxide content and oxidative potential of biomass-burning HULIS.

Line 418: This contrast highlights the need for a molecularly resolved metric to link aqueous ROS-forming potential with OA chemistry.

Line 522: … we show that the aqueous ROS-forming potential of fine PM is governed not by particle mass, …

6. Line 459: Replace "higher intrinsic reactivity" with "greater aqueous ROS-forming potential under the present assay conditions."

Reply: This has been replaced as suggested.

Line 498: …with increasing oxygenation directly translating into greater aqueous ROS-forming potential under the present assay conditions.

7. Several key correlations are based on only five or six averaged observations. Please report sample sizes, regression statistics, confidence intervals, and individual data where possible, and moderate claims such as "robust predictor" and "fundamental compositional control."

Reply: Thank you for the comment. We have added Table S3 to report the regression models, *p* values, and 95% confidence intervals for the correlations shown in Figures 2 and 4. In addition, the sample sizes and individual data for H_2O_2, radical, and total ROS (H_2O_2+radical) measurements underlying the site-averaged values are now provided in Table S4.

Figure 2 caption: … The regression models, *p*-values, and 95% confidence intervals for the correlations are provided in Table S3.

Figure 4 caption: … The regression models, *p*-values, and 95% confidence intervals for the correlations are provided in Table S3.

Line 350: The individual H_2O_2, radical, and total ROS measurements underlying these site-averaged values are provided in Table S4.

8. LC-ESI-MS peak-area fractions are signal-weighted rather than quantitative mass or molar fractions because ionization efficiencies vary among compounds. Please revise the terminology and acknowledge this limitation.

Reply: We have revised the terminology throughout the manuscript where appropriate to avoid interpreting peak-area fractions as quantitative abundances and have clarified this limitation in Section 3.1.

Line 263: Note that because ESI response can vary among compounds, these peak area-weighted fractions should not be interpreted as quantitative measures of the actual abundance or mass fractions of OOC and UOC. Instead, they are used here as relative molecular-composition metrics based on the detected compounds. The resulting relative fractions show distinct differences in OA composition across precursor types and environments (Figure 1b).

Line 305: … while the RF_{UOC} is strongly correlated with the relative fraction of aromatics (R^2 = 0.88, Figure 2b).

Line 482: … where exceptionally high RF_{OOC}/RF_{UOC} coincides with a high relative fraction of HOMs (Figure 2).

Line 524: The relative fraction of OOC compared with UOC is a key determinant…

Minor corrections:

1. Line 122: correct to "field campaigns"

Reply: This has been corrected.

2. Line 145: delete repeated "while"

Reply: It has been deleted.

3. Line 275: change "MR-VK" to "MCR-VK"

Reply: This has been corrected.

4. Line 393: change "Figures 4" to "Figure 4"

Reply: This has been corrected.

Referee#2

This manuscript investigates the relationship between the molecular composition of organic aerosol (OA) and reactive oxygen species (ROS) formation using an MCR-VK framework. The authors combine ultrahigh-resolution mass spectrometry with ROS measurements across a wide range of ambient environments, including several Chinese megacities, a semi-urban site in Germany, a boreal forest site, and laboratory-generated SOA. The combination of field observations and laboratory experiments is a major strength. I find the manuscript generally well organized and scientifically interesting. The manuscript is potentially suitable for publication in Atmospheric Chemistry and Physics after revision. I have several comments that I believe could further strengthen the manuscript and clarify the interpretation of the results.

Reply: Thank you for the positive evaluation of our manuscript and for the constructive comments. We have carefully addressed all comments and revised the manuscript accordingly.

1. One point that I think deserves some clarification is the use of RFOOC/RFUOC as an indicator of aerosol oxidative activity. As I understand it, these values are calculated from the relative LC-MS peak areas. Because different compounds can have very different ionization efficiencies in ESI, the peak-area fraction does not necessarily correspond to the actual abundance or mass fraction of OOC and UOC. I think this limitation should be stated more clearly.

Reply: Thank you for the comment. We agree that differences in ESI response among compounds limit the quantitative interpretation of LC-MS peak-area fractions. We have clarified this point in Section 3.1.

Line 263: Note that because ESI response can vary among compounds, these peak area-weighted fractions should not be interpreted as quantitative measures of the actual abundance or mass fractions of OOC and UOC. Instead, they are used here as relative molecular-composition metrics based on the detected compounds. The resulting relative fractions show distinct differences in OA composition across precursor types and environments (Figure 1b).

2. The OOC category appears to include a chemically diverse group of compounds. Some highly oxygenated compounds may be efficient ROS precursors, whereas others may not be particularly reactive. Therefore, it is worth emphasizing that OOC is a molecular-composition descriptor rather than a homogeneous group of ROS-active compounds. This would make the mechanistic interpretation of the results more balanced.

Reply: Thank you for the comment. We agree that OOC represents a chemically diverse group of compounds rather than a homogeneous group of ROS-active compounds. We have clarified this point in the revised manuscript by noting that compounds within the OOC category may differ in their ROS-forming activity.

Line 438: It should also be noted that OOC covers a chemically diverse range of compounds, which may differ in their ROS-forming activity.

3. Lines 270-272: I don't think HOMs/Aromatics can really be considered independent validations. Both HOMs/aromatics and the MCR-VK classification are derived from the same UHRMS molecular-formula data. I suggest replacing "independent structural indicators" with something like "complementary molecular indicators" or otherwise clarifying this point.

Reply: We have replaced "independent structural indicators" with "complementary molecular indicators".

Line 296: … we further examined its relationships with complementary molecular indicators derived from the UHRMS data.

Line 302: Importantly, the MCR-VK-based classification shows strong quantitative agreements with these complementary molecular indicators.

4. Lines 396-402: regarding the exponential relationship in Figure 4a, I am not sure that the fitted relationship alone is sufficient to support the saturation behavior. The nonlinear relationship is

interesting, but the saturation could simply describe the empirical distribution of the data. I suggest either providing additional evidence for the proposed mechanism or making this interpretation more tentative.

Reply: We agree that the fitted relationship alone is insufficient to establish the mechanism underlying the saturation behavior. We have therefore made this interpretation more tentative and noted that further studies are needed to better understand the mechanisms underlying the nonlinear relationship.

Line 431: This behavior suggests that ROS formation may initially be limited by the availability of oxygenated organic functionalities and become progressively constrained by competing chemical pathways and interactions as oxidation increases. This saturation behavior may reflect the chemically complex nature of urban fine PM, in which diverse OA constituents, including unsaturated species, aromatics, and redox-active components, interact to regulate ROS generation efficiency. Further studies are needed to better understand the mechanisms underlying this nonlinear relationship.

5. Lines 505-510: While the authors acknowledge the potential role of transition metals in ROS formation, it should also be emphasized that the current analysis does not quantitatively separate the contribution of OA from that of inorganic components.

Reply: Thank you for the comment. We have added a statement to clarify that the current analysis does not quantitatively separate the contributions of OA and inorganic components to ROS formation.

Line 563: However, the current analysis does not quantitatively separate the contributions of OA and inorganic components to the measured ROS formation.

Minor comment:

1. There appears to be a wording error in line 145: "while it was while 0.5–1 ppm for isoprene and naphthalene."

Reply: We have deleted the duplicated "while".

2. Lines 415-419: Mismatch between the text and Figure S7; I actually think this discussion can be deleted.

Reply: This discussion has been deleted.

3. Line 195: Provide references to support the statement that short-lived radicals are "assumed to rapidly react with BMPO".

Reply: We have added relevant references to support this statement.

Line 202: …were assumed to rapidly react with BMPO to form stable radical adducts suitable for EPR detection (Villamena and Zweier, 2002; Mitchell et al., 2013).

4. Line 492: The statement that the MCR-VK framework is "broadly applicable to chemically diverse atmospheric aerosols" seems somewhat stronger than supported by the current dataset, particularly given the deviation of Hyytiälä from the urban relationship. I suggest softening this statement.

Reply: We have softened the statement to avoid overstating the general applicability of the MCR-VK framework.

Line 544: This feature highlights the potential of the framework for application to chemically diverse atmospheric aerosols and offers a scalable approach for assessing aerosol oxidative activity.

5. Line 494: There is an extra period in "oxidative activity.."

Reply: This has been corrected.

6. Line 499: Replace "further validates these conclusions" with something slightly more cautious, such as "provides further support for these conclusions."

Reply: We have revised "further validates these conclusions" to "provides further support for these conclusions" as suggested.

Line 551: Laboratory-generated SOA provides further support for these conclusions.

References

Li, C., Fang, Z., Czech, H., Schneider, E., Rüger, C. P., Pardo, M., Zimmermann, R., Chen, J., Laskin, A., and Rudich, Y.: pH modifies the oxidative potential and peroxide content of biomass burning HULIS under dark aging, Science of the total environment, 834, 155365, 2022.
Tong, H., Lakey, P. S., Arangio, A. M., Socorro, J., Shen, F., Lucas, K., Brune, W. H., Pöschl, U., and Shiraiwa, M.: Reactive oxygen species formed by secondary organic aerosols in water and surrogate lung fluid, Environmental Science & Technology, 52, 11642-11651, 2018.