We thank the reviewer for their careful reading of our manuscript and for providing these constructive, specific comments. Below, we respond to each comment point-by-point. Comments by the reviewer are given in black normal font, and our response to the comments is shown in blue. Newly added and modified text in the revised manuscript and supporting information (SI) is given in italics.

Comment: Section 3.3: The analysis of O_3/H_2O_2 ratio differences between clean and polluted days provides an interesting preliminary assessment, but the authors should acknowledge potential confounding factors like VOC/NOx ratios that might influence this relationship.

Response: Thank you for this insightful comment. We agree that the analysis of the O_3/H_2O_2 ratio difference between clean and polluted days is a preliminary assessment and that other factors, such as variations in VOC/NOx ratios, could indeed influence this relationship alongside particle uptake effects. We will revise the text in Section 3.3 to explicitly acknowledge these potential confounding factors and state that while the observed difference supports the hypothesis of particle uptake, variations in photochemical regimes (influenced by VOC/NOx) could also contribute.

We have added the following texts:
Line 300-304: "In addition, differences, potentially driven by varying VOC/NOx ratios between clean and polluted days, could also influence the O_3/H_2O_2 relationship independently of particle uptake effects."

Comment: While the study effectively demonstrates the need for particle uptake, the sensitivity to the HO_2 uptake coefficient (discussed in lines 304-309) is important. The manuscript correctly notes that this influences the derived H_2O_2 uptake coefficient. Perhaps briefly reiterate this uncertainty in the conclusion when stating the H_2O_2 uptake coefficient.

Response: We agree that reiterating the potential influence of the HO_2 uptake uncertainty on the derived H_2O_2 uptake coefficient in the conclusion is valuable. We will add a sentence to the abstract to briefly restate this caveat.

We have added the following texts:
Line 18-20: "A box model with default gas-phase chemistry overestimated H_2O_2 by a factor of 2.7, and including particle uptake of H_2O_2 (uptake coefficient: 6×10^{-4}) improved agreement with observations, although we note this value carries some uncertainty related to the assumed HO_2 uptake coefficient."

Comment: The text acknowledges the underestimation of organic peroxides (lines 125-126, 244-245). While H_2O_2 is shown to dominate, briefly stating why the method underestimates organic peroxides (e.g., lower collection efficiency for some species) could add clarity for readers unfamiliar with the technique.

Response: Thank you for pointing out the need for clarification. We have expanded our explanation of why the measurement method underestimates organic peroxides in Section 2.2:

Line 126-133: "However, it is important to note that the percentage of organic peroxides reported in this study represents a lower limit, as the collection efficiency of the stripping coil technique varies significantly among different organic peroxide species. While H_2O_2 is efficiently trapped due to its high solubility (Henry's law constant: ~10^5 M atm^{-1}), many organic peroxides such as methyl hydroperoxide (MHP) have substantially lower solubilities (Henry's law constant: ~3×10^2 M atm^{-1}), resulting in lower collection efficiencies. Additionally, the catalase used to differentiate between H_2O_2 and organic peroxides may not completely discriminate between certain hydroperoxide species, further contributing to uncertainty in organic peroxide quantification."

Comment: There seems to be a minor discrepancy in the stated contribution of particle uptake to H_2O_2 loss (69% in Abstract/Conclusions vs. 64% implied by Fig S2 caption/text line 320). Please ensure consistency.

Response: We thank the reviewer for catching this inconsistency. We have verified our calculations and the contribution of particle uptake is indeed 69%. We will correct the value mentioned around line 320 (currently 64%) and ensure consistency in the Abstract and Conclusion.

Comment: Line 192-196: The manuscript highlights an increasing trend in H2O2 concentrations over time in the North China Plain. While comparisons with previous studies are provided, the discussion on potential drivers of this trend (e.g., changes in NOx/VOC ratios due to emission policies) is limited. The authors should expand on this.

Response: This is a very relevant point. We revised the text in Section 3.1 to include a brief discussion on potential drivers.

Line 201-205: "The significant reduction in NOx emissions in the North China Plain over recent years, while VOC levels remained relatively high or decreased less sharply, has likely shifted the atmospheric chemistry towards conditions more favorable for HO_2 recombination, potentially contributing to the observed increasing trend in H_2O_2 concentrations. This aligns with the known sensitivity of H_2O_2 formation to NOx levels."

Comment: Line 407: The conclusions summarize the key findings well but could include a forward-looking statement on future research needs (e.g., long-term H_2O_2 monitoring, improved HO_2 uptake parameterization) to guide subsequent studies.

Response: Thank you for the suggestion. We added some text at the end of the Conclusion outlining future research needs:

Line 457-462: "Future research should focus on long-term H_2O_2 monitoring across different environments in the region, refining the parameterization of heterogeneous uptake processes (particularly for HO_2 and H_2O_2 under varying aerosol compositions), and investigating the impacts of changing VOC/NOx ratios on H_2O_2 chemistry. In addition, further research on the interactions between gas-phase oxidants and aerosol processes will be vital for understanding the complex feedback mechanisms that influence air quality in rural and urban environments."