

Interactive comment on “Partitioning of hydrogen peroxide in gas-liquid and gas-aerosol phases” by Xiaoning Xuan et al.

Anonymous Referee #1

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General comments: Xuan et al. performed field measurements of the gas-, liquid- and aerosol-phase H₂O₂ in the urban atmosphere of Beijing to understand the partitioning of H₂O₂ between gas- and liquid-phase or aerosol-phase. They show that the partitioning of H₂O₂ in the gas-liquid phase can be explained by Henry's law and the residual H₂O₂ in the raindrops while the aerosol-phase H₂O₂ level is significantly higher than that predicted value based on Pankow's absorptive partitioning theory. This paper has important implications for understanding the H₂O₂ chemistry and sulfate formation in the atmosphere, so it is well within the scope of ACP. This paper is of great interest to the atmospheric community although some clarifications regarding the data analysis are required. I recommend this paper to be published after addressing the specific comments below.

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Specific comments: Estimation of effective partitioning coefficients: The authors determined the gas-aerosol partitioning coefficient instead of the effective Henry's law constant for the gas-aerosol phase. Is this due to that aerosol water content can not be accurately estimated for low RH? The effective Henry's law constant should be estimated for the high RH condition, e.g. heavy haze episodes from 2 Jan to 3 Jan 2019 and compared with the theoretical value.

Sources and sink of H₂O₂ in aerosol: 1) The authors estimated that heterogeneous uptake of H₂O₂ could account for 86% of the measured H₂O₂ in the aerosol phase in Sec 3.2.3 while stated that the heterogeneous uptake of H₂O₂ on aerosols contributed less than 0.5% of the aerosol-phase H₂O₂ in Sec 3.3. Please clarify.

2) The authors stated that the rates of the decomposition/hydrolysis of organic peroxides in the first and second types were 0.14 ng ug⁻¹ and 3.65 ng ug⁻¹ (lines 296-297) and further estimated the contribution of decomposition/hydrolysis of organic peroxides to aerosol H₂O₂ to be 32% (lines 343-346). However, these numbers seem to be the steady-state or maximum amount of H₂O₂, not formation rates. The estimation should be based on the formation and consumption rate of H₂O₂.

3) Though the heterogeneous uptake of HO₂ on aerosols is not well understood, it is possible to estimate its contribution to aerosol H₂O₂ using the reactive uptake coefficient of HO₂ to aerosol from literature and assuming the product to be H₂O₂ (Li et al., 2019). It is recommended to perform such calculations to provide more insights.

4) The authors should discuss the “salting in” effect of high ionic strength of aerosol particles on gas-aerosol partitioning of H₂O₂ though it may only have a minor contribution to the enhanced aerosol H₂O₂ concentrations.

Line 82: Are the organic peroxide concentrations corrected for the collection efficiency?

Lines 149-150: Please explain how 88% is derived.

Line 181: What is the gas-phase H₂O₂ concentration used to estimate the liquid-phase

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H₂O₂?

Section 3.2.4: The experimental details on the decomposition of organic peroxides should be provided. Is the extracted solution exposed to light at room temperature? Are these experiments conducted at atmospheric relevant conditions so that the derived rates of decomposition can be applied to ambient?

Technical corrections:

Lines 59-60: References are missing.

Equation 4: TSP or PM_{2.5} should be used instead of Com.

Line 331: “measured” should be “was measured to be”.

References: Li, K., Jacob, D.J., Liao, H. et al. A two-pollutant strategy for improving ozone and particulate air quality in China. *Nat. Geosci.* 12, 906–910 (2019). <https://doi.org/10.1038/s41561-019-0464-x>.

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