

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

Anonymous Referee #2

Received and published: 18 February 2020

This manuscript by Xuan et al describes a detailed study of the partitioning of H₂O₂ in the atmosphere through field measurements. The authors quantified H₂O₂ in the gas phase, aerosol, and rainwater (as a surrogate for cloudwater). By comparing the measured and theoretical Henry's law constant, as well as the measured and theoretical partitioning coefficient, the authors conclude that the measured values for both are higher than the theoretical values. An in-depth assessment is conducted to evaluate the influence of raindrop falling on the quantified H₂O₂ concentration, as well, a discussion on the source and sink of H₂O₂ in aerosol is provided. H₂O₂ plays an important role in the atmosphere, and understanding its partitioning in different atmospheric phases is of great importance for the atmospheric chemistry community. The manuscript is within the scope of ACP. The data analysis and calculation were per-

C1

formed with caution. I recommend publication on ACP after addressing the following comments.

Major comment: In section 3.2.4, the authors present the evolution of H₂O₂ as a function of time in the aerosol abstract, and a detailed discussion on the potential source of H₂O₂. This result highlights the challenges in making off-line H₂O₂ measurement from filter samples. Especially, when the sampling time is as long as 11.5 h (Line 100), it is very likely that the organic peroxides present in the aerosol sample is continuously decomposing on the filter. The authors categorize the H₂O₂ evolution into three types and postulate the relevant source of H₂O₂ for each type. However, in my opinion, this appears too speculative. The decomposition of H₂O₂ on filter is difficult to control, and the quantified H₂O₂ could be merely a snapshot of an ongoing decomposition process. The authors must justify whether it is valid at all to establish gas-aerosol partitioning of H₂O₂ based on the current technique.

Minor comments:

- Literature-reported Henry's law constants of H₂O₂ varies across a certain range. The authors should justify why they used 8.4×10^4 M/atm. Is this the recommended value by the JPL publication?
- Line 139 – Is the PM_{2.5} concentration a good indicator for Com in a polluted environment like Beijing?
- Line 150 – The authors state that when H_mA was less than H_tA, the samples followed Henry's law. Why? Shouldn't they agree (neither higher nor lower)?
- Line 275 – The authors report here that heterogeneous uptake can count for 86% of aerosol phase H₂O₂. Later in Line 346, the author report 0.5%. Please clarify.
- Line 370 – correct me if I am wrong. “additional source of liquid-phase H₂O₂ gradually increased” – should this be the sink of H₂O₂ due to droplet-to-gas transfer is gradually reduced?

C2

- Figure 5, and Line 263 – the authors interpret the inversely related H₂O₂ concentration and PM_{2.5}/sulfate concentrations as a result of a H₂O₂ sink by SO₂ oxidation. However, could the inversed relation be just due to dilution of H₂O₂ when aerosol loading is high?

Technical comments:

- Line 59- “easily to absorbed” to “easily absorbed”
- Line 144 – “statistically counted” appears awkward. Should probably remove.
- Line 146 – “with” 25.20 μM – is 25.20 μM the theoretical value? “with” makes the sentence unclear.
- Line 171 – “almost” less than 2000 m – should this be “always” less than 2000m?

Interactive comment on Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2020-34>, 2020.