

## ***Interactive comment on “RH and O<sub>3</sub> concentration as two prerequisites for sulfate formation” by Yanhua Fang et al.***

**Anonymous Referee #1**

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The manuscript by Fang et al. provides a nice year-long dataset of PM<sub>2.5</sub> along with chemical composition and some important precursors, which would be of interest in improving the understanding of pollution evolution in Beijing. Throughout the manuscript, the authors focused mostly on the observed relationships between SOR and O<sub>3</sub>/RH, and made conclusions that O<sub>3</sub> and RH are two “prerequisites” of sulfate formation. These conclusions, however, are predictable. RH and O<sub>3</sub> together provide almost all the necessary conditions for sulfate formation: for gas phase oxidation, they are sources of OH, and for aqueous phase or heterogenous phase oxidations, water and oxidants (O<sub>3</sub>, H<sub>2</sub>O<sub>2</sub> (O<sub>3</sub> was a precursor of H<sub>2</sub>O<sub>2</sub>)). This is saying, that the authors focused on the relationship between SOR and O<sub>3</sub>/RH and concluded on multi-phase reaction by H<sub>2</sub>O<sub>2</sub> oxidation dominate (or major) sulfate formation is over concluded, or

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more like a speculation, especially given the absence of H<sub>2</sub>O<sub>2</sub> data.

In addition, there should be seasonal difference on the formation route, for example, in summer, pollution was the lowest and SOR was the highest, given the data presented, one cannot judge that multi-phase reaction by H<sub>2</sub>O<sub>2</sub> oxidation should be responsible for sulfate formation: won't the gas-phase oxidation also enhanced in summer? In fact, for multiphase reactions, AWC might be a better indicator, however, as shown in Figure 7, SOR is not well correlated with AWC but better with RH. This for me is a good if not strong indicator that gas-phase oxidation (promoted by high O<sub>3</sub> + RH + insulation) is important for at least summer high SOR.

Other Suggestions: 1) The fact of no correlation between SOR and NO<sub>2</sub> could make a good argument on the role of NO<sub>2</sub> in sulfate formation, I suggest to emphases this point. In addition, comparing SOR, NO<sub>2</sub> and NH<sub>4</sub><sup>+</sup> (it would be better if NH<sub>3</sub> is available), and see if there is any clue on the role of NH<sub>3</sub> in aerosol pH and the promoted NO<sub>2</sub> oxidation route as proposed by earlier studies.

2) It looks the authors dealt with SOR as a sole local phenomenon (local emission and local oxidation), but how about the difference in the regional transport of SO<sub>2</sub> and SO<sub>4</sub><sup>2-</sup>? What would this do to SOR?

3) There is observational data on the relationship of H<sub>2</sub>O<sub>2</sub> concentration and temperature in Beijing (Fu, A.: Study on peroxides concentration and its influencing factors in the urban atmosphere, master of engineering, College of Environmental and Resource Sciences, Zhejiang University, Hangzhou, China, 56 pp., 2014 (in Chinese) ), the authors can derive the H<sub>2</sub>O<sub>2</sub> concentration from the temperature data to better constrain the role of H<sub>2</sub>O<sub>2</sub> by comparisons with O<sub>3</sub> and SOR data.

4) Atmospheric oxidation capacity is a rather vague (or big) definition when related to specific oxidation route of chemicals. Try to avoid

5) The manuscript need a little bit more tuned, e.g., line 31-32: what is “a given RH

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threshold”?

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