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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "RH and O_3 concentration as two prerequisites for sulfate formation" *by* Yanhua Fang et al.

Anonymous Referee #2

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The paper deals with the mass concentration and chemical composition of PM2.5 in Beijing during 1 year from filter samples and its correlation with pollution classes (clear days, slight, light, medium and heavy pollution). Most of the paper is devoted to the two prerequisites for sulfate formation based discussion. This is certainly a positive feature of the paper. Although the article has a clear logical structure, I strongly recommend to make the text more concise, to clarify statements, and to delete redundancies. Most importantly, in the absence of data on hydrogen peroxide, all speculation seems weak. The main idea of the article is still in the cognition of previous studies, and no more innovative conclusions have been put forward. In a word, this article is full of paradoxical conclusions and cannot provide a powerful help to the scientific community. Therefore, I don't recommend the publication in ACP journal in current status.

(1) The author name should be Weili Lin. (2) "threshold of RH and ozone" Where is this statement coming from? Is it a definition/estimate of the authors? If the threshold changed with different locations and seasons? What is the effect of these thresholds? (3) Redundancy: Page 1 line 15-16 and line 24-25. Line 13-14 and Line 17-18. (4) Section 2.1.2. Please add the steps of weighing after sampling. (5) Page 4, line 27. Should be annual standard. (6) Page 5, line 2. The method to calculate POM should be introduced in previous section. (7) Overall, section 3.1 is not necessary, because it has nothing to do with the main idea. If this section is deleted in the main article, it will not affect the presentation of the article. For example, the authors described the measurements of ions, organics and metal. However, ions except SNA, organics and metals except Fe didn't help the discussion of your topic. Therefore, the method and results section should to be streamlined. (8) Section 3.2. I strongly recommend the authors discussing the relationship between sulfate and RH/ozone in different seasons. The threshold should be changed with seasons. (9) Page 7, line 12-16 repeats the previous statement. (10) Page 7, line 14. What is the atmospheric oxidative capacity? From your statement, does ozone concentration correspond to this? Is it correct? Do you have some references to support your opinion? The authors should clarify this question because the same definition is also used in Page 9, line 20. (11) Page 7, Line 23-24. Since you couldn't exclude NO2-based reactions as major route of sulfate formation, the analysis of the relationship between SOR and NO2 is not necessary. (12) Page 9, line 2-3. The authors described on page 7, line 7-10 that the self-catalytic nature is beyond the scope of your study. However, you illustrate the importance of the self –catalytic in this paragraph. I think it's self-contradictory. (13) Page 10, line 21. Should be Zhejiang University.

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