

***Interactive comment on “Atmospheric aerosol compositions in China: spatial/temporal variability, chemical signature, regional haze distribution and comparisons with global aerosols” by X. Y. Zhang et al.***

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Responds to the comments from Anonymous Referee #2

“This paper presents a summary of major PM10 water-soluble components collected over a 2-year period from sites throughout China. Visibility data from various sites are also included. Although the data is somewhat interesting, most notably the spatial distributions, for the most part the paper present an analysis that is unclear and highly simplistic. First off, the paper needs significant editing to improve clarity. Some ex-

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amples are noted below, however, there were just too many instances of awkward or unclear sentences that not all could be identified. It was not clear why PM10 was the focus, since health and visibility concerns suggest PMfine could be more important. Presumably PMfine data was not available at the chosen monitoring sites; this issue should be discussed. Also, no discussion is included on measurement limitations associated with filter sampling and storage, especially those pertaining to the semi-volatile components associated with organic compounds and nitrate. It was never clear if the data were quantitative, most notably the ammonium nitrate data that is extensively discussed throughout the paper. It was also not clear how the mineral mass was determined.” “The SOC analysis based on OC/EC ratios is overly simplistic, as described. The primary OC/EC ratio is not even presented. Highly simplistic comparisons are also frequently made between their data and that reported in the literature. In summary, some of the data appears of sufficient quality and uniqueness that a paper could be published; however the paper in the current form needs substantial revisions before publication is recommended.”

A: Thank you for the comments. As the reviewer pointed out that, this paper contains data appears of sufficient quality and uniqueness and needs some clarifications before it can be published. We are addressing comments as follows: (1) We have done a thorough editing of the manuscript and polished the English usage as well as some minor structural re-organizations as suggested by other reviewers. (2) It is understood that PMFine is important to health and visibility. There are many studies in China on fine aerosols to investigate the health issues. However, because the CMA network was not structured to take both PM10 and PMfine analysis at that time, only PM10 is measured and presented in this paper. As the paper points out that dust aerosol takes quite a large fraction of aerosol mass, PM10 presents more over-all aerosol mass distributions in China than just the fine part. This fine fraction of particles may be the future development of the network. (3) The measurement technique used in this study is rather standard, which has been used in many network observations of PM10 around the globe [Malm, W.C. and Schichtel, B.A., 2004. Journal of Geophysical

Research; Querol, X. et al., 2008, Atmospheric Environment; Wang, H. and Shooter, D., 2001. Atmospheric Environment; Bourotte et al., 2007. Atmospheric Environment]. For example, we have also published a paper on chemical composition of PM<sub>10</sub> measurement [Zhang et al. 2002, Atmospheric Environment 36 (2002) 4189–4199] where the measurement limitations associated with filter sampling and storage have been discussed. (4) The issue with semi-volatile component is an on-going issue and has been investigated by many groups. The measurement limitations associated with filter sampling and storage, especially those pertaining to the semi-volatile components associated with organic compounds and nitrate are discussed in the revised manuscript and our previous work [Zhang et al., 2008. JGR]. (5) The mineral dust determination has been discussed in our previous work (Zhang et al. J. Atmospheric Chemistry 44, (2003) 241-257). We used Fe (4% of mineral dust) to calculate the mineral dust fraction in our PM<sub>10</sub>. We have added briefly description in the revised manuscript. (6) We have given the primary OC/EC ratios from biomass burning on the basis of our emission inventories in P16, line 6, such as “The high OC/EC ratios of 5.5 for rural aerosols are probably contributed from open biomass burning in fields, which has a calculated OC/EC ratio of 7.1 (Cao et al., 2006). We revised these texts, putting more primary OC/EC ratio, for discussing the observed OC/EC changes and SOC formation in the revised manuscript. (7) The SOC analysis is based upon reports from references [Castro et al., 1999. AE]. We agree that this methodology is rather simple and only used as a rough estimate of the SOC formations, but we do not have a better way to do this on the basis of daily OC and EC filter data. We have stated the uncertainty of using this to estimate for SOC, and have already mentioned “although here is a very rough estimate for SOC with substantial uncertainty, it still can provide some insight into the SOC contributions to the total OC.” in P16, line 15-21.

Some specifics “Pg 5 Lines 12-15 – reword.”

A: Reworded

“Pg 6, line 5, reword . . .this session, should it be this section. Line 18, other hands

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should be reworded.”

A: Agree.

“Fig 3, why not look at difference between urban rural pairs instead of the overall average of urban vs. rural. It is not clear this type of average has significant meaning when averaged over such a large region.”

A. The main purposes of this manuscript is to provide general picture of aerosol chemical compositions in China, due to the major focus and space limitations we can not discuss all things very much in details.

“Pg 9, lines 15, 20, reword. Line 23 are pets really a significant source of ambient ammonia.”

A: Revised

“Pg 14, line 21, reword.”

A: Reworded.

“Section 3.3.2. SOC. A discussion on the uncertainty with this method is needed and all results should include a +/- with each number. Also, pg 16 lines 12-13, giving a single number for OC/EC rates for various sources is a gross oversimplification. Give some indication of a range.”

A. Revised

“Pg 17, line 4, quantify “substantial uncertainty”

A: Agree.

“Last line pg 18 and first line of pg 19, also at end of Summary. The importance of noting that haze in China is referred to, as Grey Haze is not clear.”

A: Agree.

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“Pg 20 line 1, what exactly does dynamical and substantial contributions mean? Line 16, change specie to species. Line 20 gives the impression that the authors are suggesting that sulfate affects ammonium emissions, which is not correct.”

A: We changed the wording to clarify these.

“Pg 21, line 21, reword.”

A: Reworded.

“Figure 2, define symbols in plot.”

A: Done

“Figure 5 needs a legend.”

A: Okay.

Please also note the supplement to this comment:

<http://www.atmos-chem-phys-discuss.net/11/C12142/2011/acpd-11-C12142-2011-supplement.pdf>

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Interactive comment on Atmos. Chem. Phys. Discuss., 11, 26571, 2011.

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