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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Characteristics of aerosol pollution during heavy haze events in Suzhou, China" *by* M. Tian et al.

Anonymous Referee #1

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Review of "Characteristics of aerosol pollution during heavy haze events in Suzhou, China" by Tian et al.

The authors presented field observation results of PM2.5 mass concentration, water soluble inorganic ions in PM2.5, OC/EC analysis of PM2.5, as well as reconstructed light extinction coefficient during haze events in January 2013 in the city of Suzhou, China. (1) Sources and processes leading to PM components were discussed with both back-trajectory analysis and the potential source contribution function (PSCF) method. (2) To investigate the contribution of PM species to visibility reduction, the authors attempted to link the two with the revised IMPROVE algorithm and the Koschmieder equation. (3) Finally, gas-to-particle conversion in secondary inorganic species (sulfate and nitrate) formation was briefly discussed.

The dataset itself is definitely interesting and worth exploring, and the methodologies employed by the authors were also scientifically sound. There are two reasons I don't see this version of the manuscript is publishable yet. First, for the three points summarized in the first paragraph, point (1) is just standard and should be in a short description only; point (2) is totally unnecessary since it is a two-step linkage from chemical composition to visibility (to me, it looks more like a number game if no measured extinction coefficient is shown); point (3) should be explored in greater detail, together with the distinct feature of the third haze event (30, January). This first reason will be elaborated point-by-point below in the major comments. Second, the language of the current form of the manuscript needs lots of work to enhance its readability. This second reason will be supplemented with some minor comments below (for those I have spotted). With these reasons, I suggest a major revision before the manuscript can be published in ACP.

Major:

1. Section 2. (a) The authors used data of SO2, CO, and NOx quite a bit later in the Results and discussion section. Measurement of these criteria pollutants should be mentioned here as part of the methodology. (b) A brief description on URG 9000 IC would be beneficial to readers since it is not as standard as TOEM. (c) A brief description on OC/EC analyzer and what method of OC/EC splitting was used (thermal OC/EC or optical OC/EC)?

2. Section 3.4. In this section, the authors tried to link the sources with both chemical composition and light extinction by back-trajectory analysis and PSCF. I don't see a great value of this sub-section for the following two reasons. First, if this analysis is useful, then the results just basically invalidated the authors early assertion that secondary formation was the dominant "source" for high PM levels observed (which I believe in). Second, the discussion of this sub-section is just too confusing and difficult to follow. I would suggest to talk generally about air mass origins in the general characteristics sub-section while not pushing too far to pin-point sources of those mainly secondary

species.

3. Sub-section 3.2.3. The authors used revised IMPROVE algorithm to reconstruct light extinction coefficient and used Koshmieder equation to "reconstruct" visibility. The discrepancy is large (a factor of two), owning (in my opinion) to this two-step linkage with both steps involving a number of assumptions and uncertainties. If there is no measured extinction coefficient to support, I do not see what value this analysis would add to the manuscript.

4. Section 3.3. The discussion of secondary inorganic species formation is informative but need some cautions. (a) correlation between RH and SOR does point to the importance of aqueous-phase formation of sulfate, but RH is an indication of gas-phase water after all. It is suggested that liquid water content in PM to be estimated using E-AIM or ISORROPIA. (b) the authors cited Pathak et al., 2004, Pathak and Chan, 2005 to back the statement that homogeneous reaction between HNO3 and NH3 was important in nitrate formation. But these two papers talked about sampling artefacts for filter sampling when particles and gases can interact for 24 hours, while the authors used continuous measurement technique to measure SNA. I don't see that is relevant. (c) excess ammonium is of course one way to look at nitrate formation, but partitioning equilibrium between NH4NO3 and HNO3 and NH3 is also important to considered given the low temperature and high RH in the studied period.

5. The authors briefly mentioned the uniqueness of the third haze events compared to the first two in a number of places, but did not elaborate them. From the high POC and dominated air mass origins of C2 (short circulating trajectories), I believe this event was mainly contributed by primary emission from the local (surrounding) areas. It is suggested that this episode to be discussed in contrast to the other two with respect to primary/secondary fractions and meteorological parameters.

Minor

P33409, L3: change "public" to "the public".

P33409, L15: what is "artificial sampling"? should be "filter sampling"?

P33409, L26: change "close" to "similar".

P33410, L3: change "salt" to "salts".

P33410, L22: change "investigate the" to "investigation of".

P33411, L1: suffered should be suffered from.

P33414, L20: change little to slightly (also in P33416, L6).

P33417, L14-16: this sentence is not convincing to me. It is stated that there is difference between Suzhou and Beijing. But here it is asserted that nitrate formation may be also affected by re-volatilization of NH4NO3 as that in Beijing (similarity?).

P33418, L16: change migh related to might be related.

P33418, L28: change "similar profile" to a profile similar".

L33149, L3: I don't agree that all the secondary aerosol species were "affected" by O3. O3 is just one of those oxidants that can oxidize precursors and lead to SIA formation. In fact, it is the secondary nature of O3 that makes its diurnal profile some similar to those of the secondary aerosol species.

P33149, L11: also responsible should be also be responsible.

L33419, L12: level should be levels.

L33419, L19: "because of" should be "be due to".

P33419, L21: favored for should be favored.

P33419, L22: always southwest wind? The discussion later for the first and second haze events suggests otherwise.

P33420, L2-7: I don't see it is necessary to repeat the equation here. In fact, I don't see it is necessary to have this analysis of reconstructed light extinction coefficient, as

shown above.

P33420, L14: “similar temporary trend” and “significantly correlated” are referring to the same thing.

P33420, L16: were should be was.

P33420, L26: were reduced should be reduced.

P33421, L28: how come only NO2 (not NO + NO2) was used in calculation of NOR?

P33422, L4-6: should be two sentences.

P33422, L11-14: show the correlation.

P33422, L27: a variety of cities (change to different cities?); a little (change to slightly?)

P33424, L27: remove “differently”.

P33425, L13: suggest should be suggests.

P33425, L14: but not should be but does not.

P33426, L10: severe should be efficient.

P33426, L18: were should be was.

P33426, L24: But distinctively should be However.

P33435: what are the shaded areas with different colors?

P33437: it is difficult to be convinced that CO is a precursor of aerosol species; O3 might be one type of oxidants in secondary aerosol formation, but not a precursor.

P33439: if it is a ratio, then show a ratio, not a percentage.

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 33407, 2015.