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Interactive comment on “Highly time-resolved urban aerosol characteristics during springtime in Yangtze River Delta, China: Insights from soot particle aerosol mass spectrometry” *by* J. Wang et al.

Anonymous Referee #3

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General comments: The paper deals with highly time-resolved urban aerosol characterisation during two weeks period in spring in Nanjing (China) using soot particle high resolution aerosol mass spectrometry. The work makes use of most possibilities that SP HR-AMS gives and the whole text is clearly written using good English. Although the topic is important, there are several major issues that should be answered before publishing the paper.

First, although the use of collection efficiency 0.5 was common in the past, nowadays a composition dependent collection efficiency (Middlebrook et al., 2012) should be

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used or at least tested as a composition is highly variable during sampling, as seen on diurnal variability of major species. This is even more important when no reference data for comparison with AMS total mass or any species are present.

Second, as automatic using of CE equal 0.5 induces higher uncertainty for determination of PM1 mass, it should be stated when it is compared to PM2.5 mass. The average ratio PM1 to PM2.5 equal to 0.54 is rather low especially when relatively low (for China) average concentrations were present during sampling period and at the same time average RH was not high.

Next, although using oxidation ratio of sulphur probably makes sense, the similar ratio for nitrogen (line 266) has no meaning as gas phase nitric acid is not included and there are also other nitrogen oxides than NO2 that are not accounted for.

The explanations about mainly photochemical origin of sulphate (lines 271-275) is misleading, the ratio SO42-/NO3- has nothing to do with sulphate origin. At the same time, although the maximum in oxidation ratio of sulphur in the afternoon may suggest influence of photochemical oxidation of SO2, it does not prove it. The same effect can be expected during increased mixed boundary layer period from down mixing of older aerosol from upper boundary layer in which the most of SO2 was oxidized some time ago. The size distribution of sulphate presented in Fig. 5 actually supports liquid phase formation of sulphates (Hering and Friedlander 1982).

The source apportionment as presented here is not very clear. The decision why the authors use four and not five factors seems little subjective. At the same time high correlation of “SV-OOA” factor with rBC, CO and NO2 (very similar to HOA factor) together with its very local origin (Fig. 11) does not seem to agree with its secondary origin. The doubts about SV-OOA in this work are also confirmed in the text and Table 1 because of its low correlation with SV-OOA from two other cities in China. The reviewer suggests testing five factor solution more thoroughly.

The text in lines 457-460 at least partially contradicts the conclusions based on Van

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Krevelen diagram.

More attention should be paid to the differences between dual vaporizer and a tungsten vaporizer data, as it can bring more light on the influence of refractory organics.

The corrections and doubts described above should be also corrected in the Abstract (e.g. lines 54-55) and the Conclusions (lines 545, 553-554, 561-574).

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