

Interactive comment on “Enhancement of secondary aerosol formation by reduced anthropogenic emissions during Spring Festival 2019 and enlightenment for regional PM_{2.5} control in Beijing” by Yuying Wang et al.

Anonymous Referee #2

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Comments on manuscript entitled “Enhancement of secondary aerosol formation by reduced anthropogenic emissions during Spring Festival 2019 and enlightenment for regional PM_{2.5} control in Beijing”

General comments: This manuscript reported primary pollutant reduction but enhanced SIA formation in an emission reduction period during the 2019 Spring Festival in Beijing. The opposite trend of atmospheric oxidative capacity responding emission reduction was proposed the cause for enhanced SIA formation. Though the supporting discussion still appeared to be weak. Nevertheless, this study should call for the at-

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tention on SIA pollution control policy mitigation. I thus recommend publication of this manuscript on ACP with minor revision.

Specific comments: Lines 21-22: O₃ control regime on a regional scale is still a controversial topic. This manuscript did not intend to discuss on such topic given no VOCs measurements were present. Therefore, it is a bit risky to go such far with current data available. I suggest to delete the statement on NO_x and VOCs control strategy if no more discussion shall add. Line 56: be specific! Change to “secondary inorganic aerosol formation” Line 92: consider to revise this sentence Line 160: as shown in Fig.2, O₃ titration appeared to occur in both POL and BG period. O_x=O₃+NO₂ is thus suggested to add in Fig.2. Lines 202-203: cannot read from Figure 2 that m_{org} and m_{BC} increase by % at night from daytime is less in BG relative to POL Lines 203-204: both m_{nitrate} and m_{sulfate} varied! Lines 246-24: decreased from what? Lines 255-257: From the context, I can only get that O_{rg} and BC reduction was sharper than sulfate and nitrate. If I can accept that “secondary (inorganic) aerosol” could replace “sulfate and nitrate”, I am still reluctant to accept that O_{rg} and BC are all primary aerosol. Figure 5: The high SIA and large PM_{2.5} number in POL were mostly seen at low RH, which is against the impression that heavy PM_{2.5} pollution was usually accompanied by high RH condition in literature. The author should at least address such unusual data. Figure 6: Given the discussion on RH or ALWC in the context, I would suggest to add one of the two parameters in one column. Line 403: High O₃ concentration itself will not surely lead to strong atmospheric oxidative capacity or even O₃ production. The first reason is that O₃ was titrated in Figure 2. The secondary reason is that O₃ can be regionally transported as a relatively long-lived species. And the third, OH instead of O₃ is the major oxidant in the atmosphere, which better represents the atmospheric oxidative capacity and does not differ significantly from pollution days to clean days in winter Beijing (see Eloise et al., Elevated levels of OH observed in haze events during wintertime in central Beijing). More data or discussion are needed here.

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