

Interactive comment on “Molecular distributions and stable carbon isotope compositions of oxalic acid and related SOA in Beijing before, during and after the 2014 APEC” by Jiayuan Wang et al.

Anonymous Referee #2

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This manuscript investigates aerosol chemical compositions before, during and after the APEC event. The authors also provide some valuable measurement data, e.g., particulate organic matters on molecular level and isotope compositions, to testify the effect of emission reduction, temperature and RH on secondary aerosol formation. Overall, this paper is definitely of interest for the scientific community and the findings are relevant to a better understanding the influence of pollution control on atmospheric aerosols. However, some revision work as listed below should be done before publication could be considered.

General comments:

The authors try to ascribe the low concentration of and relative abundances of SIA,

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WSOC, TDOC and C2 during and after APEC to the low temperature condition. However, as shown in figure 6 in the manuscript, the backward trajectory cluster analysis demonstrates that there are more stagnant air masses before APEC than those during APEC, and “. . .the resident time of the air masses within Heibei province is very short, thus aerosols. . .is of local characteristics and relatively fresh”. This is self-contradictory. If the backward trajectory cluster analysis is right, all the secondary features before APEC can be ascribed to the more time for the formation of secondary aerosols, instead of high temperature which favour the secondary formation. The authors need to clarify this contradiction.

Specific comments: 1. Line 232, “In comparison with those before-APEC the decreased ratio C2/TDOC and increased ratios of Gly/TDOC and mGly/TDOC during- and after-APEC, together with a decreased ratio of WSOC/OC discussed previously, further suggest a reducing production of SOA during the whole campaign.” This sentence is very obscure. 2. Line 254, “As seen in Fig. 5d, H+_{ae}r correlated well with SO₄²⁻ (R₂ = 0.58), probably suggesting SO₄²⁻ is the key factor controlling the aerosol acidity.” The logic of this sentence is questionable. The acidity of aerosols is decided by the anion-cation balance, not a single ion. 3. Line 262, as discussed above, good correlation between the temperature and C2/TDOC may be because the different origin of air mass. 4. Line 279, is it possible that the change of $\delta^{13}\text{C}$ in C2 is due to the different source of C2 precursors? 5. Line 288, I am a little confused about the definition of the pollution type. Do the authors find higher relative concentration of primary pollutant, e.g., BC, CO, in the local type? Also, the air masses from Siberia should be very clean. Is there any evidence of the pollutant transport during the periods of this type? Otherwise, the use of “transport” in the definition will be misleading. 6. Line 308, still, is it possible that the difference of $\delta^{13}\text{C}$ in each type is due the different source of C2 precursors? 7. Line 315, the authors compared two pollutant episodes before and after APEC, and conclude that the difference is mainly due to the temperature. However, as it is also discussed in Part 3.3.2, the two episodes are in different pollution types with totally different air mass origin. The authors need to exclude the influence of air mass

before making such conclusions.

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