

Interactive comment on “Strong anthropogenic control of secondary organic aerosol formation from isoprene in Beijing” by Daniel J. Bryant et al.

Anonymous Referee #2

Received and published: 11 December 2019

The paper by Bryant et al. entitled as ‘Strong anthropogenic control of secondary organic aerosol formation from isoprene in Beijing’ shows the observational data for isoprene-derived organic aerosol (iSOA), especially focusing on organosulfate. Although observational data of iSOA in Beijing itself might be unique, the reviewer believes that the manuscript needs to be re-structured and more carefully prepared to meet the publication criteria for ACP. Discussion of the data could have been deeper and more convincing if the authors have analyzed the data in more detail and carefully. There were numerous errors in the reference list. Some technical descriptions were not clear. I can understand that the authors have put significant effort to obtain this dataset. So, the manuscript should have been prepared more carefully.

Major comments Figures 3 and S1. L331 ‘OH chemistry is still an important loss route

C1

at night (>30 %) owing to night-time OH sources, such as the ozonolysis of alkenes.’ The authors estimated that the contribution of OH radicals on the loss process of isoprene is more than 50% even during nighttime, as the concentration of OH radical is close to 2×10^6 molecules cm^{-3} even at around the midnight. However, this concentration seems to be higher than OH concentrations in other urban areas during nighttime (Kanaya et al., 2007; Heard et al., 2004). Although I am not familiar with other recent field campaigns for OH radicals in Beijing, a recent modeling result also indicated that OH radical concentration in Beijing should be orders of magnitudes lower than what the authors have reported (Tan et al., 2019). If the night-time OH sources in Beijing is so important for isoprene oxidation chemistry, the potential sources need to be discussed in more detail by employing observation data or by appropriately citing necessary publications.

L421 ‘The correlation of [O3] [pSO4]’/Figure 5 It seems to me that the authors have assumed that the formation of organosulfate is approximated by the second order reaction with a constant reaction rate. However, it is a multiphase chemistry. In addition, the importance of particle phase acidity and phase are known to influence the rate of such chemical reactions. So, I am not sure if it is a good idea to assume that the rate constant is a fixed value. Figure 4 indicates that the concentrations of all the organosulfates are high during pollution episodes. Although the authors did not provide time-series data for sulfate concentration, I speculate that it must have also been high during these time periods. Considering that the ozone concentration was less variable than particle phase species (Figure 1), I suspect that the correlation shown in Figure 5 is dominantly driven by accumulation of particle phase species (i.e., sulfate and organosulfate) during pollution episodes, although there must have also been in-situ production of these species. The discussion will need to be thoroughly revised, including reconsideration of the metric.

Other comments L300 ‘OH, HO₂ and RO₂ concentrations were measured using Fluorescence Assay by Gas Expansion (FAGE) and NO₃ concentrations were measured

C2

using Broadband cavity enhanced absorption spectrometry (Zhou et al., 2018).’ I checked Zhou et al. (2018); however, I was unable to find the corresponding information (maybe, I missed)?

L321 ‘There was strong a correlation between the isoprene mixing ratio measured at 8 m by the DC-GC and at 102 m using the SIFT-MS ($R^2 = 0.77$). The SIFT-MS measurements were therefore used to investigate the correlation with iSOA tracers when no DC-GC data was available.’ Being correlated does not mean that they are quantitatively the same. Further detailed descriptions would be needed.

Section 3.3 The reviewer believes that the content is more suitable for the experimental section.

L402: ‘Zero-dimensional box modelling indicates on some days up to 35 % of the isoprene-derived RO₂ radicals can react with HO₂ in the afternoon (Newland et al., 2019).’ As Newland et al. (2019) has not been published yet, I am unable to evaluate the validity of this description.

Acknowledgement It seems that at least one of the authors is also included in the acknowledgement.

References There are numerous errors. For example, names of some authors are duplicated. Information about issues/page numbers are unavailable for some references. Abbreviations for some journals are not following the standard style. Titles of some papers are missing. Some references are appearing twice (i.e., duplicate). The authors will need to carefully check the reference list again.

Figure 1 It seems that all the data points are connected by line, even if some data are missing. I suggest to change it so that the readers can tell for which time period the data is/is not available.

Figure 3 (caption) What is the unit for the reaction rate constants?

References: Heard, D. E., Carpenter, L. J., Creasey, D. J., Hopkins, J. R., Lee, J.

C3

D., Lewis, A. C., Pilling, M. J., Seakins, P. W., Carslaw, N., and Emmerson, K. M.: High levels of the hydroxyl radical in the winter urban troposphere, *Geophys. Res. Lett.*, 31, 10.1029/2004GL020544, 2004. Kanaya, Y., Cao, R., Akimoto, H., Fukuda, M., Komazaki, Y., Yokouchi, Y., Koike, M., Tanimoto, H., Takegawa, N., and Kondo, Y.: Urban photochemistry in central Tokyo: 1. Observed and modeled OH and HO₂ radical concentrations during the winter and summer of 2004, *Journal of Geophysical Research: Atmospheres*, 112, 10.1029/2007JD008670, 2007. Tan, Z., Lu, K., Jiang, M., Su, R., Wang, H., Lou, S., Fu, Q., Zhai, C., Tan, Q., Yue, D., Chen, D., Wang, Z., Xie, S., Zeng, L., and Zhang, Y.: Daytime atmospheric oxidation capacity in four Chinese megacities during the photochemically polluted season: a case study based on box model simulation, *Atmos. Chem. Phys.*, 19, 3493-3513, 10.5194/acp-19-3493-2019, 2019.

Interactive comment on *Atmos. Chem. Phys. Discuss.*, <https://doi.org/10.5194/acp-2019-929>, 2019.

C4