

Interactive comment on “Distinctions in source regions and formation mechanisms of secondary aerosol in Beijing from summer to winter” by Jing Duan et al.

Anonymous Referee #1

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The manuscript presents online aerosol measurements performed at Beijing in three seasons and discusses about aerosol sources and formation products. It also evaluates the importance of primary vs secondary species and of photochemistry vs aqueous phase processes, in clean and polluted conditions in the three investigated seasons.

The manuscript is well written and the results appear robust. The topic can be considered adequate for ACP and of interest for the scientific community. I recommend publication after the following (major) comments have been addressed.

General comments

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The weakest point of the data discussion is the characterization of the OOAs. The authors attribute one to regional processes (RSOA) and the other one to local processes (LSOA), but this attribution is not adequately supported in the manuscript. Considering that all the discussion is based on this attribution, the authors should be more convincing under this aspect. The authors present polar plots, showing the spatial distribution of sulfate sources, resulting in a credible distinction between local and regional sulfate. I invite them at least to present the same elaborations for RSOA e LSOA as well.

I do not see the utility of Par. 3.5 in the manuscript. First, it is not clear how the selected episodes have been identified. The authors should provide the criteria that lead to select these episodes instead of others. This would help the reader in understanding the discussion. Most importantly, the conclusions derived in this section appear largely speculative, as they are based on the comparison of one episode with a couple of others, which lacks of any statistical robustness. If the authors are interested in evaluating the effect of meteorology on the occurrence of pollution episodes, they should work with the whole dataset in a statistically robust way.

The analysis of the SOA production routes is very interesting, but RH is certainly not the best tracer for aqueous phase processes. I invite the authors to make use of the aerosol Liquid Water Content (LWC) instead, which is a much better tool for this purpose. It is not difficult to calculate the aerosol LWC, based on simple models, once RH, T and aerosol chemical composition are available.

Specific comments

P7.L13. “OA dominated PM1 mass in late summer and autumn, whereas inorganic species played a more important role in early winter”. This sentence is not supported by the results: the difference in OA contribution between autumn and early winter is almost negligible (49 vs 46%). Fig. S3 shows clearly that OA is the dominant component in early winter as well.

P7.L27. It is not properly temperature that drives the boundary layer evolution. A lower

temperature is a consequence of the lower solar radiation reaching the surface, as it is a shallower boundary layer.

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P8.L15-16. Also PBL dynamics may have an effect on this.

Figure 3a. Please change the colors of the plotted lines. It is hard to distinguish one line from the other.

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Interactive comment

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