Comment on "Seasonal variations of high time-resolved chemical compositions, sources and evolution for atmospheric submicron aerosols in the megacity Beijing" by Wei Hu et al.

This manuscript by Hu et al. presents a comprehensive study on chemical compositions, sources and evolution for atmospheric submicron aerosols in the megacity Beijing in four seasons. Following typical AMS analysis, the source and evolution process of aerosol, especially OA in different seasons are discussed. The contributions of primary and secondary PM are also examined. With the wealth of AMS and ACSM studies in many locations including Beijing city, I was hoping for some unique discussions or scientific insights that were not available in the literature already. In particular, there are a lot of ACSM studies in Beijing in the literature that provide very similar analyses and results of the current paper. The additional analysis of OSc etc with the HR data is very similar to what has been published by many others. While the paper is well written and generally clear, the paper needs to be improved in emphasizing more on new science and insights of the work beyond our current understanding of PM in Beijing.

Some other comments below:

1 Page 7, Line 11-12, please show the satellites data in the supporting information.

2 Page 7, Line 14-15, have the authors examined the contributions of organic nitrate to the high nitrate concentration associated with biomass burning?

3 Page 8, Line 1-3, it would be useful to show the correlations of nitrate with RH under high and low RH conditions in addressing the point that aqueous reactions could contribute to nitrate remarkably in highly humid and static air.

4 Page 9 and Page 10, please clarify the calculation of the particle growth rate.

5 Have the authors tried more factors in PMF or using ME-2 to resolve a BBOA factor in spring and winter? In Page 22 and Figure 10, it seems that in both spring and autumn, a large number of data points affected by biomass burning. Further the author mentioned that in the satellites data, they identified some days with intense biomass burning activities in spring as well. In winter, it seems that HOA and CCOA spectra also bear some BBOA features.

6 Page 16, Line 7-9, the authors state "the peaks of OOA (or LO-OOA) coincided with the peaks of primary emitted COA (spring, summer and autumn) and HOA (winter) in diurnal patterns, probably because strong primary emissions favored the partitioning of oxidized gas precursors to particulate phase" However, on Page 17, Line 7-8, the they also say "In both autumn and winter, the fractions of OOA slightly increased around 100 μg m^{-3}, implying that POA probably transformed to SOA more effectively within this range." Please clarify if it was POA transformed to SOA or primary emissions favored the partitioning of oxidized gas precursors to particulate phase for the increase of OOA.

7 Page 20, Line 12-19, please explain the use of OA/ΔCO ratio rather than ΔOA/ΔCO and ΔPOA/ΔCO used in the literature.

8 Figure 3, Please add the standard deviations in the diurnal plots.

9 Figure S5, I suggest making use of the OM:OC ratio in the AMS to convert OA to OC or OC to OA in the comparison with EC/OC analyzer.