Review comments on “Primary and secondary organic aerosols in 2016 summer of Beijing” by Tang et al

In the manuscript the authors apportioned the primary and secondary sources of the organic aerosols using a chemical mass balance (CMB) and trace yield methods based on 144 kinds of quantified organic species, including both primary and secondary tracers. The effectiveness of control measured on primary and secondary sources were assessed based on the obtained results. Back trajectory cluster analysis was also conducted to evaluate the influences of air mass directions on the organic aerosol sources. Environmental factors, such as temperature, O3 concentration, aerosol liquid water content, and particle acidity were also investigated to elucidate the formation mechanisms of secondary organic aerosols.

The topic of the manuscript fits very well into the Atmospheric Chemistry and Physics and the manuscript is well written. Generally, I recommend the publication of the manuscript.

However, there are some technical details that might change the conclusion of the manuscript, which I think need to be addressed before its publication.

1. The authors spend the whole section 4.3 “Influencing factors for secondary organic formation in the summer of Beijing”, discussing the factors that could influence the anthropogenic SOC (Figure 4). To get their point, they did correlation plot of the anthropogenic SOC loading with different factors and positive slope indicating enhancing effects. I found this not reasonable. What the authors really need is “multivariate analysis” or “multivariate regression”.
   Otherwise, one factor could have influenced the behavior of the other factor and change the sign of the slope, leading to an opposite conclusion.
   For example y = f(x1, x2) = x1 – 0.5 * x2.
   y is positively correlated with x1, but negatively correlated with x2.
   You made some measurements at x1 = 1, x2 = 0 and x1 = 2, x2 = 1. The two y’s you will obtain are 1 and 1.5. Then based on the authors method, one will obtain y is positively correlated with x1 (with slope of 0.5) and x2 (with slope of 0.5).

2. The authors did show in Figure 1 that the governmental control changes the Organic aerosol apportionment a bit, however, the total organic aerosol loading does not change much, or even increased (from 8.9 ug/m3 to 11.0 ug/m3) (as shown in Table 1 too). The total $PM_{2.5}$ loading has decreased from 92.3 ug/m3 to 45.5 ug/m3. Then this leaves the reader wonders what have been decreased mostly? The sulfate? Nitrate? Ammonia? Or something else. The authors need to add the loading of these into Table 1. The decrease of EC from 3.3 ug/m3 to 1.8 ug/m3 is not enough to explain the more than 40 ug/m3 decrease in $PM_{2.5}$.

Besides the above two comments, I also have some minor comments as listed below.

1. Line 119, by “filters” does the authors mean “quartz filter” only. Or the authors analyzed both “quartz filter” and “Teflon filter”.

2. In Figure S7, are the vertical lines the measurement error bars or they indicate the daily ranges? As this could change the statement of line 270 stating that hope at PKUERS site were much higher than that of CP.
3. Line 305, the concentrations of what in CP were lower than that of PKUERS? Please clarify.

Overall, the manuscript is well written and data presented is extensive. I recommend its publication in ACP after the aforementioned questions be addressed.