The manuscript "Seasonal variation of oxygenated organic molecules in urban Beijing and their contribution to secondary organic aerosol" addresses the seasonality of oxygenated organic molecules (OOM) in urban Beijing measured by a nitrate-CIMS over one year. And their potential contributions to secondary organic aerosol (SOA) via vapor condensation were estimated using an aerosol dynamic model. OOM concentrations were found to be highest in summer and generally followed the trend of radiation and temperature. OOM were classified into IP-OOM, MT-OOM, aromatic and aliphatic OOM. IP-OOM concentration vary strongly among seasons with lowest concentration in winter. Isoprene-OOM accounted for a large fraction in summer while in other seasons aromatic and aliphatic-OOM were the two major components. MS Profile of MT-OOM, aromatic and alphatic OOM were similar among different seasons. Condensation of aromatic- and aliphatic-OOM contributed most to SOA throughout the year.

Observations OOM in urban environment are important to elucidate the SOA formation and particle growth but are scare as most field observations of OOM were conducted in forested regions. Especially long time observation regarding seasonality of OOM is lacking. This study investigated one-year OOM composition and concentrations systematically, which provides very valuable information of OOM seasonality and potential contribution to SOA. The manuscript is well written and fits well the scope of ACP. I recommend its publication after addressing the following comments, which mainly work to clarify some important details and improve the readability and discussion of the manuscript.

Specific comments
1. L60, I suggest explicitly mentioning the definition of OOM.
2. In this study only mz 200-400 was analyzed. Why other mz such as higher mz were not included? What about their contributions? Are they too low to be important? It would be worth clarifying and noting the mz ranges in the conclusion.
3. In the method part (L173, Fig. 1), it seems that the authors used a IP-OOM list to differentiate whether a compound belongs to IP-OOM. It may be helpful to include the list and describe what experiments/observations the list is based on.
4. Also in this part and Fig. 3, have the authors considered the possible contribution of isoprene to C10-OOM via accretion reactions and contribution of monoterpene to C5-OOM via fragmentation?
5. Fig. 3, considering nitrate-CIMS does not detect OOM with low O number, are the OOM with nOeff<=4 mostly organic nitrates?
6. Fig. 5, it is not completely clear how the spectral profiles are obtained.
7. L262-264, Fig. 5, and Fig.7, analyzing day and night MS spectra separately and plotting the diurnal variation for IP-OOM and MT-OOM may be helpful to estimate the relative contribution of nitrate OOM from NO_x termination and NO_3 radical.
8. L403-405, why CHO accounts for so low fraction compared to other OOM classes?
9. Fig. 11, how the volatility was calculated? As volatility highly depends on the method to derive it, it would be helpful to clarify and maybe add a short discussion on the influence of the methods adopted.
10. L493, "the dominant formation pathways of OOMs stay the same during the year", it might be easier to follow to add "…each OOM stay constant during the year", otherwise it would sound that all OOM are formed in the same pathways.

Technical comments:

1. L173, in Fig. 1, "nO=10" or "nC=10?
2. Fig. 3, the order of the legend is not consistent with the order of appearance. It might be easier to follow by keeping it consistent. And there is no legend for light blue.
3. L443, a space is missing before "94 %".