Wang et al. examined the role of changes in precursor emissions and PM2.5 levels in recent ozone increase in summertime Beijing. By applying box modeling constrained through extensive measurement data in Beijing, they find that PM2.5 decrease is a major driver for the past- decade ozone increase despite the reduction of NOx and VOC emissions. Supported by fast increase of J(NO2), they show that the enhanced actinic flux by decrease PM2.5 is a more important factor than heterogeneous chemistry for ozone increase.

The topic of this work is well within the scope of ACP. Recent modeling studies have demonstrated the ozone-PM2.5 linkage to explain recent ozone increase in China that is of great concern for scientific communities. This study, from an observational perspective, admirably attempted to resolve the ozone increase by taking advantage of the valuable long-term measurements in Beijing. Overall, this manuscript is well structured and easily accessible. It will deepen our understanding of ozone-PM2.5 linkage, and in particular stimulate further studies to reconcile the discrepancy between modeling and observation-based studies. I think it is publishable in ACP after my following concerns are addressed.

1. There is gap between ozone production and its concentration. A recent ACPD paper (Gao J.H. et al., doi:10.5194/acp-2020-140) and also references in the second paragraph of their Introduction Section highlighted that decreased ozone production by PM2.5 via affecting photolysis rates is much more than the reduction in surface ozone concentration. Moreover, a lot of different transport model studies (Xing J. et al., doi:10.5194/acp-17-9869-2017; Li J. et al. doi: 10.1016/j.scitotenv.2017.12.041; Li K. et al., doi:10.1038/s41561-019-0464-x) also show that the impact of PM2.5 on summer surface ozone is not important. I suggest, at least, the authors to do some detailed discussion to reconcile this important issue. This is particularly helpful for future studies.

2. Diurnal variation of ozone production. The authors take daytime average over 7:00-19:00 or (6:00-18:00?) for ozone production. I am not sure if the results may differ by narrowing the average to afternoon hours when ozone production is active and HOx levels are high. Also, Hollaway et al. (doi:10.5194/acp-19-9699-2019) show that PM2.5 impacts on the summertime photolysis of NO2 and ozone level at surface in Beijing are important before 11 am and after 3 pm but very limited in afternoon hours. I suggest the authors to show some diurnal information of simulated ozone production.

3. The authors show an important result of an increased SSA in Beijing (Fig.12). More importantly, there is a shift pattern of j(NO2) over 2006-2016 that the crossing point between J(NO2) profile of 2006 and zero AOD profile changed from above PBL to below PBL in 2016. I think this means that the role of PM2.5 may be more important under condition like 2006, but will be limited under condition like 2016 when there is offsetting effect for PBL ozone by vertical mixing. This may deserve a discussion.

4. Some other specific comments. (1) It is confused to see 2005-2016 and 2006-2016 in the text. Please clarify this. (2) I suggest to use p-value other than the r square where there is a trend analysis. (3) Line 290: Shanghai should be “the south to North China Plain”. (4) Lines 293-296: how about the role of regional contribution outside of Beijing? For example, the increasing emissions in the whole North China Plain. (5) Line 494: Please take caution when saying “ozone increase”. you mean surface ozone concentrations?