Review comments on "Ozone Pollution over China and India: Seasonality and Sources" by Gao et al.

This manuscript provided a study investigating the seasonality of ozone pollution and its sources in China and India. This study found different seasonal futures in NCP, YRD, PRD regions of China, South and North of India. This manuscript pointed out that different emission sectors dominate the ozone formation in individual region. The topic is applicable for Atmospheric Chemistry and Physics. The text is concisely written and well documented. However, this manuscript lacked lots of details which are important for the conclusions. First, the authors discussed that the WRF-Chem model may not correctly represent the nighttime ozone chemistry, which leads to the high bias of nighttime ozone levels. If that is the case, in my opinion the study should focus on the daytime ozone concentrations (such as daily maximum 8-hr ozone) when the ozone formation is sensitive to the emissions. The daily mean ozone concentrations used in this study could not correctly reflect the photochemical formation of tropospheric ozone, but might include the effects from nighttime ozone destruction. For instance, the negative contribution from Power and Transportation emissions may be due to the nighttime titration of NO-O3, but may not come from the non-linear chemistry of ozone production as suggested in the manuscript. Second, this manuscript extensively discussed the sensitivity of seasonal ozone to individual source (Figure 5), the authors calculated the contribution from the sensitivity experiments (Table 1) using the 'zero-out' method. Due to the highly non-linear characteristics of the ozone production, it may introduce some uncertainty. The manuscript stated that Sillman (1995) value was used to identify the NOx or VOC-limited region, but I did not notice where the ozone chemical regime was discussed. The value from Sillman (1995) study on the U.S. may not be applicable for China. Third, the current Section 5 lacked discussion of the uncertainty of this study and potential weakness of the modeling platform. Revision of the Summary is suggested. Lastly, there are some missing and errors in the references, which need to be corrected in the revised manuscript. In summary, major revisions as indicated in the comments and remarks below are needed before consideration of publication in ACP.

Detailed Remarks/Suggestions for Revision

Line 98: 'Lu et al. 2018a' is not listed in the bibliography. Two 'Lu et al. 2018' are listed in the reference, so the revised manuscript should list 'a' and 'b' in the bibliography.

Line 94: Stratospheric intrusions can only influence surface ozone levels in high altitude region, while most the regions in both China and India discussed in this manuscript may not be able to see this impact.

Line 97 'J. Gao et al., 2016' is not the correct format for ACP. Please correct all of them.

Line 97: 'Lu et al. 2019' is missing.

Line 122-124: In my opinion, both the spatial and vertical resolutions of the WRF-Chem simulations are too coarse. How many vertical layers of the total 27 layers are in the PBL?

Line 124: A table showing the major physical options is suggested, either in the main article or in the supplementary material.

Line 125: 'M. Gao et al., 2016', same format problem as indicated above.

Line 169: 'Hunan', 'Hubei', and 'Jiangxi' are all labeled 'Hainan' in Figure S1

Line 181: The emissions inventory was developed for year 2012, so why the year of 2013 was simulated?

Line 207: As mentioned above, the authors should try variables such as daily maximum 8-hr ozone for the evaluation.

Line 211: It is hard to figure out if the model can capture the site observations. A scatter plot is needed here.

Line 215: Figure S2 needs more explanation. How the OMI NO2 product was used here? Criteria to filter the row anomaly? What is the solar zenith angle used to filter out data here? When computing the WRF-Chem NO2 columns, how the model results are sampled? For instance, all the grids collocated with missing OMI data points due to cloud in OMI NO2 should not be used. How the OMI averaging kernel was used here? A detailed explanation is suggested.

Line 217: In Figure S2, it is hard to conclude that the NO2 column from WRF-Chem is less than OMI NO2 column. A different plot or ratio plot is needed here. Also the emission deficiency may not be able to fully explain the high bias of nighttime ozone, because the nighttime nitrogen chemistry may not be explicit in WRF-Chem. Again, I suggest to re-do the analysis using daytime ozone to eliminate the impacts from nighttime ozone.

Line 261: 'Lu et al., 2018b'?

Line 287: This figure shows the Beijing-Tianjin-Hebei area is significantly influenced.

Line 292-293: The change of circulation is important because of the regional transport of air pollutants, but the change of cloudiness and precipitation pattern also plays an important role in the formation of ozone.

Line 311: As discussed above, a paragraph is needed here to explain how the sensitivity was calculated.

Line 314: I disagree with this statement. From Fig 5, looks like emissions from Transportation sector contribute more than the Industry sector in China.

Line 317: Li et al. (2017) is missing.

Line 341: Is 'biomass burning' here equivalent to 'Fire' sector in Figure 5?

Line 347: Looks like the biogenic emissions dominate the ozone production in India.

Line 352: I disagree with this statement. Due to the VOC-limited ozone production, biogenic emissions has the largest contribution (Figure 5S).

Line 771 Table 2: Does the 'Outside' stand for outside of China? Need clarification.

Line 779 Figure 1: It is very hard to tell the Purple sold circle in the map. I suggest using a different shape such as solid star or triangle here.

Line 783 Figure 2: As mentioned above, it is hard to see the model performance. A scatter plot is suggested here.

Line 787 Figure 3: The scale in y-axis is improper. Please re-plot the figure with y-axis from 0 to 80 ppbv or 100 ppbv. Also the current plot shows little difference between these two lines. A table shows some statistics such as NMB and RMSE is suggested.

Line 797 Figure 4: what is the ‘near surface wind field’ here? 10 m or 850 hPa wind?