Review of the manuscript titled "Ozone Pollution over China and India: Seasonality and Sources." (By Meng Gao et al.)

The paper presents the results of an WRF-Chem modeling and analyzed the seasonality of O3 and its sources in both China and India. They derived the modeling results from the sensitivity tests (authors named this as 'a factor separation approach') and they explained that the importance of industrial sources in summer in China and the transport vehicle sector in all seasons in India. Also authors described the contributions from other regions.
The content of the manuscript is reasonable and the results also seem convincing. However, all the discussion is mostly based on modeling results, and thus the evaluation and validation of WRF-Chem results have to be made more rigorously. Also some of the descriptions in the text should be changed to be in more quantitative way, and authors can provide a statistical and quantitative modeling (or observational) results on the relations between O3 and its precursors.

(Major Comments)
1) Line 166, 23 source regions: In most figures, regions are divided into four areas (NCP, YRD, PRD, and India) for current analysis. However, in Fig. S1, authors provided only province borders in China. Please redraw the boundaries of four regions (and 8 additional regions, as employed in Fig. 6) in the Figures.

2) In Fig. 2, in winter, for example, it is considered that the O3 concentration level in East China shows considerable biases. It is suggested that at least the statistical results of four regions (NCP, YRD, PRD, and India) should be explored. Statistical results such as RMSE, IOA, etc (together with correlation coefficients as indicated in Line 228) should be tabulated (or described in the manuscript) for at least this four regions.

3) In Fig. 3, if the y-axis scale is lowered to 80 ppb (or even lower), the differences (between modeling and observations) is unlikely to be ignored. It appears that the spring season (Mar. Apr, May) in NCP is so low that the model result looks nearly doubled, and also 3 months (Oct Noc Dec) in PRD show considerable bias. India may drive higher O3 measurement in April, but the model is not simulated to be as high as measurement. Explain the reasons why the monthly measured average is high in India but why not simulated in April, through WRF-Chem model.

4) In this study, as a Monsoon Index, only wind field index was chosen (Li and Zeng, 2002). Of course, we know that the wind field is an important factor that causes precipitation by the Monsoon cycle, but the distribution of precipitation itself is one of the most important controlling factors in dealing with O3. Collect reanalyzed precipitation data (i.e., GPCP data) and plot horizontal distributions in supplementary material, and analyze both precipitations vs. O3 modeling patterns to explore the regional characteristics more directly.

5) It seems reasonable to analyze O3 during the night and day separately, to see the over- (or under-) estimation of titration effects, day and night separately, as referee 1 pointed out.

6) Add quantitative emissions for gas such as SO2 NOx, NH3, and VOCs together with particulate matters

for the whole 2013 year. Also, describe quantitatively the total of biogenic emission and biomass burning emission over both China and India for 2013 year, and compare to other emissions (i.e., SO2 NOx, NH3, and VOCs)

(Minor Comments)

1) Line 133: Year 2010 isn't so old ? The changes in emissions should be mentioned briefly in the revised manuscript.
2) Line 245 Fig. S3 → Fig. S4?
3) Line 246, satellite NO2 column (Fig. S2) indicates only day time ? If this is the case, in Fig. S2, modeling results also should be only day-time results?
4) Do not you have any VOC observations?