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Interactive comment on "Modeling study of impacts on surface ozone of regional transport and emission reductions over North China Plain in summer 2015" *by* Xiao Han et al.

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Anonymous Referee #1 China is facing serious air pollution with high PM2.5. Recently ozone (O3) becomes the premier pollutant in summer replacing PM2.5. This study investigates this important issue using the regional air quality modeling system RAMS-CMAQ. The ISAM module is used to track the O3 from major pollution regions for the VOC and NOx-sensitive O3. The brute-force method is used to examine the sensitivity of O3 to the reduction of precursor emission from different sectors, which can provide scientific basis for O3 mitigation strategy. This work is in general a solid contribution to understanding of O3 formation and transport at regional scales. I have

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Discussion paper

the following major and minor comments on the manuscript. After the authors address my comments, I would recommend the acceptance of publication.

Major comments: 1. I would like the authors to add some discussions of the novelty of this study. In the introduction, the authors mentioned many previous studies on the similar topics. How does this study differ from previous studies?

R: Thanks for this comment. It is true that the purpose of this study which tried to investigate the related source contributions and precursor sensitivity features of O3 in NCP was similar with some of the previous researches. However, it can be seen that the method and tools applied in this study should be different from other studies. The basic modeling system RAMS-CMAQ was developed by our group with more than ten years and several processes in the model was modified for improving the accuracy of simulation in China, such as using more precise underlying surface information (Chen et al., JGR, 2018), improving the description of secondary organic aerosols (Li et al., Atmospheric environment, 2017), developing the chemical and physical processes of nitrogen pollutants (Zhang et al., Tellus B, 2007) and dust (Han et al., Aerosol and Air Quality Research, 2012). Therefore, the tools we applied should be unique here. On the other hand, we kindly think that the discussion about the O3-NOx-VOC sensitivity feature was not be using over NCP. Compared with the traditional "Empirical kinetic modeling approach", this method could provide more clearly sensitive features with high temporal resolution. The discussion of the pollution control which released more appropriate sequence of emission reduction was more efficient should be barely mentioned by other studies as well. We added some of the statement about the update of modeling system in Line 134-136, please see if it is OK.

2. it looks that the model still has obvious biases (shown in Figures 3 and 4). I would recommend the authors to add some detailed discussions on the potential factors for the model biases: emission, chemistry mechanism, physics, or model grid spacing? Is it possible to add a plot (figure) on VOC (or CO) validation of model results with observations (besides NO2 and O3 in Figures 3 and 4)?

R: Thanks for this comment. Sorry we did not gave a clearly expression. Here the underestimation generally referred to the model missed some extreme high values from observation, and mainly appeared in January. It can be seen that the mean of modeled mass concentration was very close to the observed data in June as shown by Table 1 and Table 2 (broadly same at Beijing and Jinan), and the research work was generally focused on the situation in June. Therefore, we kindly think that the modeled results were able to be applied for analyzing below. However, we may provide unclear expressions which caused some misunderstanding about the model simulation accuracy, and we have modified the sentences in Line 204-212. Please check if they are OK. On the other hand, we are sorry that the observed data of VOCs was hard to get because it is not the routine monitoring object, and only few specific field campaign measurements had the comprehensive data, but not released in public. In addition, some of the modeled VOC and OC aerosol data by this modeling system have been evaluated in another study (Li et al., Atmospheric environment, 2017), we kindly think that the modeled VOCs result also can be used for analysis.

3. The result of regional contributions of NOx- and VOC-senstivie O3 from different regions (Figures 6 and 7) is interesting. Will different regional contributions add up to be 100% at one given location (i.e., local and non-local contributions)? I would suggest to add a table to show the relative contributions to O3 in several regions (e.g., Beijing, Hebei..) from different local and non-local regions. This will give the readers the idea of O3 sources in different regions (local formation versus precursor transport).

R: Thanks for this comment. Yes, the contributions from all traced sources are equal to the total mass burden of base case, which means the results are 100% conserved. This is an important feature of the ISAM. In addition, we agree that the specific percentage of regional contribution is needed to be shown. Thus, the regional contribution percentage of major regions was added in Table 3, and the related discussion was also added in Line 238-242. Please check if it is OK.

4. Please give the reason for the non-linear change of O3: why does O3 increase in

many locations when power-plant O3 precursors are removed (Figure 9j)?

R: Thanks for this comment. The result shown in Figure 9(j) was derived from the brute-force sensitivity tests that could capture the nonlinear effect due to emission reduction. As shown in Figure 8, the regions of O3 increase due to remove of power plant emission were broadly all covered by the “VOC control” type, and generally coincide with the location of strong power plant sources. Thus, the emission of NOx should be saturated and plentiful NOx mass burden would restrain the O3 formation: O3 + NO $\rightarrow$ NO2 + O2 In addition, the emission of VOCs from power plant was very small, which means the VOCs mass burden would almost be invariant with reduction of power plant emission. Therefore, in our opinion, the ambient environment would be benefit for the O3 formation when the NOx mass burden decrease due to reduce of power plant emission in the regions shown in Figure 9(j). We added this explanation in Line 297-301, please check if it is OK.

Minor comments: 1. Line 66. change "play a role" to "play an important role". R: Thanks for this comment. We modified the sentence.

2. Line 77. change "deeply analyzed" to "through analyzed" R: Thanks for this comment. We modified the sentence.

3. Line 85, change "severe" to "strict". R: Thanks for this comment. We modified the word.

4. Line 89. "The amount of surface O3 is expected to continue increasing as the particulate mass loading decreases due to the emission control strategies employed in the NCP". why? can you explain? R: Thanks for this comment. Yes, this sentence may lead to misunderstanding, and we have modified the expression here.

5. Line 103. "statistical response surface method". This is not clear. R: Thanks for this comment. We added the explanation here.

6. Line 150. "TSSA"? R: Thanks for this comment. We added the explanation here.

7. Line 168. "grid distance" to "grid spacing" R: Thanks for this comment. We modified this phrase.

8. Line 225. "this observation". not clear. R: Thanks for this comment. We modified this expression.

9. Line 268. "Figure 7f" should be "Figure 8f". R: Thanks for this comment. We modified this word.

Please also note the supplement to this comment:
https://www.atmos-chem-phys-discuss.net/acp-2018-209/acp-2018-209-AC1-supplement.pdf

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