The authors used the WRF-CHEM model to investigate the contributions of the non-local emissions to the summertime air pollution episode in Beijing. By turning on/off the local and/or non-local emissions and using the factor separation method, the authors found that during the episode (July 5 to 14, 2015), non-Beijing emissions contributed dominantly over local emissions to the $PM_{2.5}$ and ozone concentrations in Beijing. Although previous studies have discussed the topic of local vs. non-local contributions in Beijing, this paper provided updated information on the topic by focusing on the summer of 2015, after Beijing has taken drastic emission control measures in recent years. My major criticism, however, is that the paper lacks sufficient quantitative analysis and scientific discussion as a research paper. Therefore, I recommend a major revision before publication.

Major comments

Note that I make a bunch of suggestions in the major comments. I do not think it is necessary for the authors to follow these suggestions completely but I do think the major questions or concerns should be addressed in the revision.

1. The authors gave a lengthy description of model evolution of $PM_{2.5}$ and ozone in the BTH region during the episode (Line 257-301, Fig. 5-9). However, these descriptions are mostly qualitative and are not scientifically insightful. The multi-panel figures (Fig. 5-9) are too complex to help a reader understand what the authors say in the text. I would suggest the author to rewrite the section and keep it succinct. It may also be a good idea to move some of the content to a supplement.

   In addition, Fig. 5-8 shows that $PM_{2.5}$ and O_3 in BTH are being transported by wind. However, this is not equivalent to the contribution of non-local emissions. First, $PM_{2.5}$ and O_3

in Beijing can also be contributed by local productions from precursors emitted outside Beijing. Second, it is also possible that Beijing emissions can contribute to the production of $PM_{2.5}$ and O_3 outside Beijing, and then these $PM_{2.5}$ and O_3 are transported back to the city. Since the authors did not provide quantitative analysis to rule out these possibilities, Fig. 5-8 cannot support the author's conclusion very well. Instead of describing the 12-panel figures, I would suggest the author to do more quantitative analysis (for example, diagnosis of the flux of $PM_{2.5}$ and O_3 across the city boundary) and discuss whether precursor transport is important.

2. For the purpose of this paper, the accuracy of the emission inventory is very important. However, the description of the emission inventory in the manuscript is too brief. What year does the emission inventory based upon? Is it for the year 2006 as in Zhang et al. (2009) or is it updated? What are emissions from Beijing compared with those from the surrounding regions? How uncertain is the emission inventory? This information are essential for a reader to assess the significance of the paper's conclusion.

   In addition, as the authors pointed out, the emissions have been greatly reduced in Beijing in recent years. Therefore, if emission inventory for multiple years are available, it would be very interesting to conduct additional simulations and calculations and see how the implementation of APPCAP affects the contributions of trans-boundary transport.

3. The authors used the FSA method to separate the impact of local and non-local emissions. I have several questions about the results that the authors present.

a. In Table 2, the background contribution f_0 varies from 32.6 ppbv to 62.9 ppbv. Why does background vary so much? In addition, f_0 and f_s' (surrounding) anti-correlates very well (R^2=0.89 based on my calculation). In Table 3, f_0 and f_s' also anti-correlates really well (R^2=0.92). Why is that? The author should give more discussion to provide insight into these interesting results.

b. In Table 2 and Figure 10(a). The authors show that local contribution to O_3 is much less than non-local contributions. Beijing, with so much traffic, should have large amount of NO_x emissions. Given NO_x lifetime in the summer should be on the order of several hours, regional transport of NO_x should not be very significant. So the question follows, why is regional contribution so larger? Is the input of regional O_3 or input of precursors? If it's the input of O_3, why do regions surrounding Beijing have such high O_3 production? Do they emit a lot of NO_x and VOCs. The authors should discuss the matter by referring to emission inventory (See major comment 2) and flux diagnosis (See major comment 1).

c. In Table 3 and Figure 10(b), the authors show somewhat counter-intuitive results that non-local emissions almost always contribute more than 50% of Beijing and f_s' follows the $PM_{2.5}$ concentration perfectly. In my opinion, the authors should show additional results using emission inventory, flux diagnosis, etc. to convince a reader that their calculation is right and consistent. Or, it may be a good idea for the authors to show simulation and FSA results outside an episode in the same summer to give readers a sense of how the "control" case looks like.

d. More fundamentally, I am concerned that the FSA results in the paper are somewhat misleading to readers because in the four simulations to derive FSA, the author turn on/off the local or non-local emissions completely. As a result of the nonlinear chemistry, these simulations cannot give accurate information about the local sensitivity of air quality to emission reduction. Theoretically, it is possible that reducing local emissions may still be more effective than reducing non-local emissions. I suggest the authors to make clear in the text about the limitation of their method.

4. The paper also lacks sufficient discussion of the results in the context of previous studies. The authors mentioned several previous studies on local vs. non-local emissions. The result of this paper stands out as reporting most significant non-local contributions. The paper will be much better if the authors can discuss their paper in context of these studies (in terms of method, results, discrepancies, or agreement, etc.).

Minor comments

5. Abstract. The words "pure local emissions" and "pure emissions outside Beijing" are confusing. Are there impure emissions? I suggest to use "local emissions", "non-local (or non-Beijing) emissions", and "interactions between local and non-local emissions".
6. Line 121. Is NCEP reanalysis only used for boundary and initial conditions? Is WRF configured to nudge the meteorology fields towards reanalysis?
7. Section 3.1. This section is only a summary of changes in air quality of Beijing during recent years, which are background information rather than research results. Therefore this section should not be within "results and discussion".
8. Line 217-219, 243-238, 290. The authors attributes the model errors to inability of WRF-CHEM to resolve convection in several occasions. However, these statements are not backed by any data (e.g., observations of convective clouds etc.). Given there are so many plausible error sources in a 3-D chemical transport model, I would suggest not to make guesses on

what leads to the model errors (emission errors, meteorology errors, etc.), unless supported by observation data or model sensitivity test.

9. Line 250. The title number should be 3.2.3