Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2017-1226-RC2, 2018

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Foreign and domestic contributions to springtime ozone over China" *by* Ruijing Ni et al.

Anonymous Referee #2

Received and published: 24 February 2018

The manuscript presents a modeling analysis and attributes ozone in China to anthropogenic emissions outside China. Basically, it represents a breakdown of background ozone in China to different foreign regions. Although this breakdown analysis has its value, my main concerns are (1) the analysis was limited to the seasonal mean ozone attribution rather than high ozone events, (2) the modeling was based on a single, non-recent, year (2008), and (3) the nonlinearity in source attribution seems to be large and needs to be assessed more carefully. These issues need to be addressed and corrected before this work can be accepted by ACP.

Major comments

1. The last sentence of the abstract, "Global emission reduction is critical for China's ozone mitigation", should be removed. The reported contribution of foreign emissions

on ozone in China is essentially the background ozone. It has been well established (e.g. by several HTAP reports and references therein) that background ozone is substantial (20-50 ppbv) everywhere in the northern mid-latitude continents. For long-lived air pollutants such as ozone, essentially every country pollutes others and vice versa. To effectively mitigate ozone pollution in China, the key is to understand which source region drives the variability, especially of the high ozone days. I would be surprised if the foreign contribution is a primary factor for day-to-day changes of peak ozone over the majority of China. It appears that the paper only focuses on the seasonal mean contributions from foreign sources, thus the last sentence is a premature statement and may be interpreted misleadingly that domestic emissions control is not important.

2. To follow up the previous comment, by reporting just seasonal mean contributions of foreign sources on ozone in China, I feel the paper does not add much new knowledge to the field, especially considering their analysis was based on a single year's simulation (see my next comment). The paper would be interesting if they had analyzed the foreign contribution to peak ozone events (during pollution episode) in addition to the mean ozone.

3. I have concerns about the choice of a single, non-recent, year (2008) used in the paper for the whole analysis. The exact magnitudes of ozone mixing ratio attributable to different sources depend on meteorology and emissions, both linked with the year of simulation. How would these ozone values change if another year is chosen to conduct the analysis? The authors stated that routine ozone measurements were scarce before 2013 (pg 6, line 203), so why not a simulation year after 2013? This would be more desirable to take advantage of more observational data for model evaluation. In particular, the increase of ozone pollution is a more recent concern in Chinese cities, after high PM events are on the decline.

4. I am also concerned with the statement that over the polluted eastern China, "Chinese anthropogenic emissions lead to reductions (instead of enhancements) of surface ozone" (pg 10, line 374-375). The authors attributed this to the ozone titration effect

by freshly emitted NO. The phenomena do occur in urban areas, but the GEOS-Chem simulation used in this study has a relatively coarse grid cell even for the nested-grid option (∼50km x 50 km). This resolution would substantially smear out NOx emissions in a grid, leading to muted titration effect. My interpretation of that statement is that it suggests the nonlinearity in the zero-out simulations is strong (because it leads to negative ozone changes) and needs to be tested via different sensitivity runs and dealt with carefully. For example, the authors could try zeroing-out foreign anthropogenic emissions instead of Chinese anthropogenic emissions or try reducing Chinese emissions by a certain percentage rather than a complete zero-out, and then analyze if the different perturbation runs give consistent results over North China.

Minor Issues

Pg 5, line 190-195: The description of the weighting method to account for nonlinear chemistry is very vague, and I don't understand the scientific basis for this method. It should be expanded and explained in a way such that it is understandable to readers who have not read the original Li et al (2016) paper.

Pg 3, line 108-109: this sentence is confusing. Do you mean there are 10 producing regions and 8 source regions? Why and how are they different?

Language Issues: The paper has a few grammar errors and language issues, some examples listed below. I would suggest the authors proofread it more carefully during the revision stage.

Pg 1, line 6: "mean bias at 10-15%" should be "mean bias of 10-15%"

Pg 2, line 38: "at surface" should be "at the surface".

Pg 10, line 356: "nature ozone" should be "natural ozone".

Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2017-1226, 2018.