

Review of “Evolution of Anthropogenic Air Pollutant Emissions in Guangdong Province, China, from 2006 to 2015”

This paper calculated a multi-year emission inventory (from 2006 to 2015 and a future year 2020) using the method of activity data and the emission factors. And then the author discusses the emission trends, source contributions and pick out some examples to explain the background reasons. This is a good try to show the audience a data set and comparison. However, this paper is more like a government report instead of a top scientific research paper. There are no sound scientific findings in the manuscript. The results of the manuscript are almost a repeat or quite similar to previous publications:

(1). Zhong Z. (2018) Recent developments of anthropogenic air pollutant emission inventories in Guangdong province, China, *Science of Total Environment*.

(2). Yin, X. H., Huang, Z. J., Zheng, Y., Yuan, Z. B., Zhu, W. B., Huang, X. B., and Chen, D.H.: Source contributions to PM_{2.5} in Guangdong province, China by numerical modeling: Results and implications, *Atmospheric Research*.

Zhong’s paper is comparing with 2006, 2010 and 2012 GuangDong emission inventory while the current manuscript is analyzing the data from 2006 to 2015. The source categories of these two papers are more or less the same although the author claimed in the manuscript that the source categories follows another publication: Pan et al. 2015 (line 144). The unabated emission factors of current manuscript are more or less the same with that in Zhong’s paper. Moreover, there is inconsistency in factor values or total emission amount for a corresponding source/pollutants using the same model such as IVE model for mobile source. In terms of the emission validation using observation and satellite data, it is a good way to validate the bottom-up emission inventory utilizing this kind of data. However, the sparsity of the satellite needs to be considered. The carelessness comparison in this manuscript is very simple, not much extra information. Finally, although the manuscript is evaluating the control measures, The main results (figure 9) is more like a repeat of section 2.3 Evolution of source emissions in Guangdong Province for the

year 2006 to 2015. To sum up, I recommend a direct rejection because no meaningful results or new scientific findings can be found through the manuscript.

The following is the detailed comments for the manuscript.

1. **Abstract section:** Line 39-43: “The declines of SO₂, NO_x, PM_{2.5}, and PM₁₀ emissions are mainly attributed to the control-driven emission reductions in the Pearl River Delta (PRD) region, especially from power plants, industrial combustion, on-road mobile sources, and fugitive dust, and partly to the shift of industries from the PRD to the non-PRD (NPRD) region in GD.” It does not show any new findings here, especially the idea of “shift of industries from the PRD to the non-PRD (NPRD) region in GD” comes from a published paper on Atmospheric Research (AR) titled: “Source contributions to PM_{2.5} in Guangdong province, China by numerical modeling: Results and implications” by X. Yin (2017)

Line46-48: “this might be one of the reasons that led to the slight upward trends of ozone concentrations in GD.” is this finding a reliable result or just a guess? It is not appropriate to put an ambiguous answer in the central part of the abstract.

2. **Introduction section:**

Overall review: For a publication in a top journal such as ACP, the research motivation of the current manuscript is not strong enough. The literature review is too weak. Too many self-cited references were present.

Line 84-91: I do not see the long term emission inventory comparison could resolve the controversial issue of whether the emission or the meteorology plays the main role of pollution. Please explain how the long term emission inventory could help differentiate the causes of the emission or the meteorology?

Line 96-112 introduces the history of emission inventory development in PRD and GD. Too many self-cited papers are included. Due to differences in source categories or data

sources for different geometry, using a unified method to calculate a multi-year emission inventory is fine, but what is new here? Is the technique original or are the data sources new? What are the advantages/Strength of the method/data in this manuscript comparing with the previous publications? Only calculation of a multi-year EI seems too weak to be published in a top journal ACP.

3. Methodology section.

General review: No new method or new data sources were found in this manuscript comparing with the previous Emission Inventory publications in the PRD region. The calculation of marine emission is outdated, and its uncertainty is considerable. The validation method of the emission totals is too simple to be believed.

Table S5: is the table for PM10? Please be specific.

Line 212 – 215: The dealing method of marine may under-estimate the marine emissions which are quite important for Guang Dong coastal cities.

Line 222- 223: Please specify how typical are the annual average vehicle kilometers? How many cities and what types of roads in Guang Dong were involved in the field survey?

Line: 225-227: The vehicle ratios of fuel use used in this manuscript is too outdated to be used. Moreover, the vehicle ratios should be differentiated for PRD and NPRD. The pattern of the vehicle population in GD changed quite a lot from 2009 to 2015.

4. Results section

General review: It is just the description of the changes and the sectoral contribution of the multi-year emission inventory. Relevant policy measures were used to explain the sharp drops or increases. It is more like a government report instead of a research paper published in a top journal.

Line 343: I did not see “emission trends in PRD and NPRD are shown in Fig. 2”, Fig 2 only show the trend of PRD instead of NPRD. Please specify.

Line 346-348: PM2.5 was not shown in figure 2. Please specify.

Line 349-365: Spatial maps of differences between different years (Figure 3) for the validation are better to see the changes. In term of calculating the emission changes, did you count all the grids or just the typical points in PRD and NPRD region? Please specify.

Line 395: figure 5a is showing the result of PRD, not GD. Please specify.

Line 437: "On-road mobile was also a major contributor to NOX emissions in GD (Fig. 5b)."
Fig. 5b is showing results of SO2 for NPRD. Please double check.

Line 673: it should be 2.4.4