Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2017-646-RC2, 2017

Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Comparison and evaluation of anthropogenic emissions of SO_2 and NO_x over China" *by* Meng Li et al.

Anonymous Referee #1

Received and published: 25 August 2017

This manuscript describes a comparison of two bottom-up SO2 and NOx inventories over China. The authors describe some of the input data, and explore the reasons for discrepancies between the two inventories. Satellite observations of NO2 are used with the GEOS-Chem model to produce top-down NOx emissions that are also used to evaluate the bottom-up inventories. The authors find that while differences in total emissions of SO2 and NOx from the bottom-up inventories are small, discrepancies at the sector level and provincial level are large. Compared to the top-down emissions, both bottom-up inventories are found to have negative biases, although uncertainty in the top-down approach cannot be ignored.

General comment:

This study is written clearly for the most part, and brings attention to specific de-

tails/uncertainties about bottom-up inventories that should be considered when used in chemical transport model simulations. In general, the methods are technically sound and the conclusions are supported by the results. However, as a reader I was left with a larger question: What is the take-home message of this article? What is its importance to the atmospheric chemistry community? The authors do a sound job of pointing out differences between two (seemingly arbitrary) bottom-up inventories, but besides the obvious conclusion that some inventories will be different than others as a result of different methods/datasets, I'm not sure of the relevance here. The manuscript is quite technical, and in my opinion, misses the mark in terms of scientific significance. I encourage the authors to consider how their results and conclusions have larger impact. As written, it's not clear what substantial new concepts or methods have been advanced.

Specific comments:

Abstract:

1) As written, the abstract seems to focus quite a bit on the methods, and very little on the results and relevance. I encourage the authors to consider editing their abstract to include the important results and conclusions.

Methodology:

1) What is the reason for focusing on ECLIPSE and MIX? It comes across as an arbitrary choice of inventories. Are they the most recently developed? Are they the most popular in chemical transport models? Do they provide the most methodological details? Why should the readers be interested in these two inventories specifically?

2) The spatial proxies are mentioned very generally many times throughout the manuscript, but almost no detail is given the methods about the actual data used in each case. On Page 14 the authors state, "Proxies used.. are summarized in Section 2". But unless I missed it entirely, they are not summarized beyond some very general

language. Further broad strokes are given on Page 14 ("for industry and residential sector, emissions are distributed mainly on population data. Road networks and population are used as proxy for transportation emissions"), but I think at the very least, these details belong in the methods earlier on. I was frustrated by the number of times spatial proxy data are referred to with general language ("mainly"; "including"), but did not come away with a comprehensive understanding. Can the authors include a table that summarizes the source of all the spatial proxy data that is actually used in each inventory/sector? Or perhaps include maps of the different spatial data used in the Supplementary Information? A lot of attention is given to the spatial patterns, for them to be of such little importance in the methods.

3) The authors point out that OMI SO2 observations have large uncertainties. Would the observations be at all valuable in a qualitative comparison of spatial emission patterns?

Results:

1) P 14 Line 2 mentions how the industrial and residential sectors show "clear administrative boundaries". But for someone who is not familiar with the administrative boundaries, this isn't obvious (Provincial? county?). Would it be useful to include some of the boundaries they are referring to?

2) P 14 Line 17 mentions how "other" proxies are population-based. Which proxies exactly are the authors referring to?

3) P 14 Line 20 mentions the excellent correlations observed for all sectors, but then misses the most interesting question. What are the exact sources of the occasions when they are different? For example, residential NOx has a slope of 0.88, whereas the slopes for the other sectors are all very close to 1. What data has been used differently that causes this difference in the residential sector between the two?

4) P 15 Line 15 "In light of the bottom-up comparisons". Here, can the authors be

specific about what issues they are referring to? Exactly what hypotheses are the sensitivity tests set up to test? This would help understand the importance and purpose of the sensitivity simulations.

5) P 18 Line 26: This is the first indication in the entire article that IGDP is used as a spatial proxy. This is a good example of why the discussion about spatial proxies became frustrating to me. Again, I encourage the authors to lay out or list the spatial proxies comprehensively in the methods. Perhaps these details are obvious to some, but they aren't obvious to me.

Figure 1: Might I suggest the authors include the totals for SO2 and NOx from each inventory in the figure (just as a number, somewhere in the panel)?

Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2017-646, 2017.