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Atmospheric Chemistry and Physics Discussions

Interactive comment on "NO_x emission estimates during the 2014 Youth Olympic Games in Nanjing" *by* J. Ding et al.

Anonymous Referee #1

Received and published: 15 April 2015

This paper describes the use of satellite observations of NO2 to examine the effectiveness of NOx emissions control measures for the August 2014 Youth Olympic Games in Nanjing, China. NO2 observations from the Ozone Monitoring Instrument (OMI) are used in combination with the DECSO emissions estimation algorithm and the CHIMERE CTM to estimate emissions, and the authors conclude there is a 25% reduction in NOx emissions from controls implemented for the Games.

The paper is clear and well-written, and suitable for publication in ACP. There is a lot of useful detail included on previous literature and descriptions of several techniques used in the study that I think would be quite interesting for others using similar techniques, particularly for looking at the significantly polluted region of eastern China. In situ data is particularly difficult to come by in China, but the authors have done an interesting job

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backing it out of the AQI data.

My only general concern might be that the results could be somewhat overstated. The last two figures (9 and 10) of the paper show the main results. The in situ results are discarded earlier as they don't match the hypothesis, and Figure 9 does not clearly indicate the satellite results show a decrease in NO2 during the period of the YOG, so the authors need Figure 10 (NOx emissions estimates) to prove their point and quantify a 25% reduction in NOx from the emissions controls. However, from both figures 9 and 10, it's clear there is quite a lot of year-to-year variability. In fact, both 2013 and 2014 in that figure are consistently lower for the summer/fall period than the average (almost looks like a downward trend over previous years?). Also, the only significant reduction shows up in September 2014 (after the games period) due to the lack of observations during August 2014. I realize there is probably an issue of computational power, but an indication of the variability of emissions in Figure 10 from other years (2005-2012) or in the text could be useful. I believe these concerns could be assuaged with some more caveats in the text and the abstract, and I would recommend publication in ACP.

Specific Comments:

6338,2: Change "has taken" to "applied"

6338,17: Change "were successful" to "were successful in reducing NOx emissions"

6338,24: Change "of" to "on"

6338,23: This sentence is a bit vague, maybe give example.

6339,3: Change "is" to "was"

6339,7: Change "of" to "in"

6339,12: Change "is" to "has been"

6339,15: Remove "with"

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6340,1: Change “Also for” to “for”

6340,2: Change “comply the” to “comply with the”

6340,8: Remove “with”

6341,1-4: Martin et al used observed columns, not concentrations for top-down emissions.

6342,3: Change “Yangtze” to “the Yangtze”

6343,4: Is there a reference for the “industry” partitioning?

6343,7: What is the source of the climatological profiles?

6343,12: Awkward sentence. Change to “In particular, the calculation speed has been improved in this update.”

6344,3: Change to “results in a swatch width of 2600 km, providing”

6344,20: Lin et al. 2014 is missing from reference list.

6345,11: Remove “with”

6345, Section 2.3: This is an interesting way to back out an “in situ NO2”, which seems hard to find for China, particularly for non-Chinese speakers. Can you give a reference for the Technical Regulation manual? Also, is it complicated to do this? I think it would be useful here to briefly describe this process. Is the AQI a direct function of the NO2 amount, or is it a single value that includes contributions from the other species (O3, SO2, PM). How do you back out the NO2 amount? Also, you mention errors. Since you use the data to “validate” improvements in your model in Figure 1, but then ignore the data for the analysis of the YOG emissions, I think it would be instructive to give some numerical examples of the uncertainties, maybe by referencing other papers that use in situ data.

6346, Section 3.1: Figure 1 shows hardly any bias, but this seems like it might be

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more coincidence than anything since as you noted earlier, the errors in the in situ data could be large, and also why would that in situ data be representative of the 0.25x0.25 model grid but you ignore the data later on for looking at emissions changes in August 2014 as non-representative of the area? Again, I think it is important to emphasize the uncertainties in the in situ data, even if they match the model well.

6348,10: Change “calculate” to “calculated”

6348,21: I’m surprised these might not be systematically biased one way. Can you elaborate on the causes?

6350,6: I’m confused about what you mean by “removed in a single day”

6350,22: 13:30LT is the overpass time at the equator. What is the typical overpass at Nanjing? Is it closest to 13:00 LT in Nanjing? Since you only look at in situ data by itself, and not in combination with OMI data, why not use all 24-hour data to look for reductions in NO2? (Conversely, if you did plan to use the in situ data in combination with OMI, you would want to consider using only data in coincidence with OMI overpasses to avoid day-to-day sampling issues, or use a CTM vs. observations scaling factor to correct for OMI sampling.)

6351,3: This statement implies you know the answer before there is any data to support your hypothesis. Reword this statement.

6351,13: Not clear what is meant by a small trend. Do you mean upwards, downwards, 2013+2014 lower than others, etc...? Expand on this statement.

6351,20: Why are concentrations lower for the following months? You discuss the timeline of regulations in Table 1, but nothing past August 31. The lifetime of NOx isn’t such that concentrations would stay low after August. Were regulations kept in place? Elaborate here.

6353,13: You attribute the high values in August 2014 vs September to cloudy weather and lack of observations. I think it would be instructive to mention here how many

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OMI observations you actually get for each month. Also, the errors in Figure 10 are fairly consistent month-to-month. I'm not sure exactly how the assimilation works, but wouldn't one expect the errors to be larger for months that have very little observational data, so that August 2014 would have large error, but September would have small error?

6353,15: Change "this" to "these"

6354,1: The noise in the observations is not discussed earlier, they are just dismissed as not supporting the conclusions. Elaborate on the dismissal of in situ data earlier in paper, and "noise".

6354,14: NO2 is only deposited through dry deposition, not wet deposition.

6354,17: Change "has" to "have"

6354,22: Again, mention how few observations you have during this period.

6355,16: Change "analysis" to "analyze"

6356,8: Change "with at least" to "by at least"

6356,9: Again, not clear that it really is reduced from Figure 9. Lots of other months in 2013 and 2014 look low as well.

Figure 1: I'm confused about what this figure indicates. Is it pure CHIMERE modeled NO2 or is it OMI-assimilated (as indicated in legend)? Note this in caption.

Figure 5: It is difficult to see the land borders in panel (b). These figures would be easier to read if the data were plotted with the same limits and scale side-by-side.

Figure 8: This may seem picky, but there is no purple in the figure. "Inland water" all seems to show up as blue "ocean".

Figure 9 and 10: Line colors are reversed for 2013 and 2014 in Figure 9 and 10. It would be easier to read the figures if they were consistent between the two figures.

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