Atmospheric
Chemistry
and Physics
Open Access
Discussions

Interactive comment on "NO$_x$ emission estimates during the 2014 Youth Olympic Games in Nanjing" by J. Ding et al.

Anonymous Referee #4

Received and published: 17 April 2015

This paper by Ding et al. focuses on the estimation of NOx emissions during the 2014 Youth Olympic Games in Nanjing. They constrain daily NO2 column observations from OMI and simulations from the regional CHIMERE model to infer NOx emissions. I agree with one of the reviewers that the most significant results are presented in Figure 9 and 10, which I am most concerned with. I do not think this paper is suitable for publication in ACP unless substantial revisions are made.

I agree with most comments from Reviewer #2. I have few additional comments:

1) MEIC inventory as well as Zhang et al [2009] inventory suggests small monthly variation in emissions. Emission estimates from the DECSO algorithm is suggesting ∼50% higher emissions in July than January. Small drop in February is explained by

previous publications by Zhang et al., but the seasonal variation in Figure 10 looks unrealistic based on all existing bottom-up inventories over China. It is most likely coming from deficiency of the DECSO algorithm or the CTM they use. Satellite retrievals may have seasonal bias, yielding seasonal biases in NOx emissions. There are several factors that could lead to the biased inversion. Either exploring those factors or providing enough justification why bottom-up emission is wrong is necessary.

2) Use of OMI data: There might be some limitations in the understanding and use of OMI data. I think, DOMINO algorithm accounts for aerosol effect through not just cloud information as discussed in the paper but also surface reflectivity (OMI LER). Exclusion of scenes with high aerosols may remove polluted days since high aerosols may occur for days with high NO2 pollution. Results from Lin et al., who use MODIS reflectivity and model aerosol, may not provide sufficient justification as the study did not examine the relationship between Kleipool LER with LER calculated from MODIS reflectivity and observed aerosol. Discussions on the application of averaging kernel are necessary since the idea here is to replace the TM4 profiles used in retrievals by DECSO profiles. Section title "Improvement of the satellite data" is misleading because this work does not improve any aspect of retrieval algorithm and satellite data product. Better title would have been "Data selection and pre-processing" or something similar. 70% cloud radiance fraction threshold is higher than many previous studies use. Since cloudy observations have larger errors, inversion is more error prone with higher threshold. Criterion for OmF is very subjective. Why choose the range [-5, 10] e15? Why not [-5, 5] e15? Why not relative value rather than absolute value? Why not percentile range? Is the selected range applicable to any region or just over China?

3) Use of surface data: I do not understand the logic of using surface data. There is a big unknown about the quality of the surface NO2 data they use for validation of the model results. How the (comparison) exercise is going to be insightful if the accuracy of the data used in the analysis is unknown? In addition, the comparison of NO2 at a surface site with model results at 0.25x0.25 is not really helpful.

4) Data analysis: Based on information presented in Table 2, it is more logical to focus the analysis for May-September period examining how each regulation was effective in reducing pollution level. From Figure 9, it is difficult to link the changes observed in the YOG period to regulations in place as the results are similar for 2013 and 2014. Authors state in introduction that derived emission is better to study the effectiveness of the air quality measures, but it is unclear to me how satellite-derived emission is better than satellite observations themselves as the model is not providing any additional information regarding regulations. In fact, one might introduce model errors in the inferred emissions. For the nature of the work presented in the paper, I do not see much advantage of the chosen approach.

5) Several statements in the "Model improvement" section require citations. Please, use NO2 columns consistently instead of NO2 concentrations and NOx emissions instead of NO2 emissions.

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 6337, 2015.

C1667