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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 #2

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The article *NO_x emission estimates during the 2014 Youth Olympic Games in Nanjing* by J. Ding et al. is an interesting study about an important problem, namely the investigation of the effects of local legislative measures on air quality, using the example of Nanjing, China, during the Youth Olympic Games (YOG) in August 2014.

Unfortunately, the article suffers from

1. a lack of detail in some areas,
2. a lack of justification of some seemingly arbitrary choices,
3. overly optimistic / too definite conclusions.

Given the interesting nature of the subject and the promising results shown in this article, I recommend the article to be *reconsidered* for publication in ACP *after major revisions*.

1 General comments

The paper is generally well written and easy to understand; the few errors in the usage of the English language will surely be corrected during the copy-editing process.

2 Specific comments

- In the introduction, the authors cite *van der A (2006)* regarding the strong NO_2 increase over China over the past two decades. I think it would be good scientific practice to acknowledge the earlier scientific literature on this subject (Irie et al., Richter et al. both from 2005).
- On p. 6343, the authors describe that they extend the modelled NO_2 fields above the CHIMERE upper boundary at 500hPa using *a climatological partial column*. This gives rise to the following questions:
  - How is this column derived?
  - How is the AK applied to this partial column?
- Also on p. 6343, the authors describe *an updated version of DECSO*. It is not clear to the reader, why some parts of the description of the DECSO improvements are here, i.e., in section 2.1, and others are in the later chapter 3.

- On the same subject: The authors fail to give either details or reference regarding the sector-dependent injection height in the model.
- On the same subject: The authors fail to give either details or reference regarding the used backward trajectory calculations.
- On the same subject: The authors' re-definition of the observation error E_{obs} seems arbitrary. Specifically,
  - the authors fail to justify this error tuning; they simply describe the effects, but not why it should be a valid assumption to give more weight to larger columns.
  - the wording *the error [. . .] is recalculated* is misleading, as it suggests that E_{obs} is a physically meaningful error estimate. I suggest re-wording this along the lines of *DESCO v3 uses tuned synthetic error estimates derived from the original satellite uncertainties via [. . .]*.
  - I fail to see how, using typical c_{sat} of 5E15, f can be anything different than zero, which according to equation *(1)* just leads to a 50% reduction of the observational error.
- On l. 27 on p. 6344, the authors should justify why they filter out the outmost 4 pixels on either side of the scan.
- In l. 1 on p. 6345, the authors should detail which surface albedo dataset they use for the filtering.
- In ll. 8–10 on p. 6345, the authors (quite inspecifically) describe that *the selected data is still of sufficient quality*; however, they should also discuss the potential influence / additional uncertainties this modified cloud filter criterion has on / adds to the satellite measurements.

- On the same subject: The authors claim that *the number of observations increases [by] about 37%*, however, an increase of 37% on a total number of zero measurements (as given earlier) is still zero measurements.
- In l. 18 on p. 6345, the authors should give the exact URL where the *conversion table from the Technicsl Regulation on Ambient Air Quality Index in China* is available.
- On the same subject: It is not entirely clear which in-situ NO_2 mesaurements the authors actually use. From what is described in the text, it seems that they do *not* use the original in-situ measurements but rather calculate the in-situ measurements from the AQI values. However, air quality indices are usually derived from a number of different air quality indicators; the aqicn.org website lists PM, O_3, NO_2, SO_2, CO. Mathematically, the calculation of the AQI is therefore a mapping from an \$n-\$dimensional state space to a \$1-\$dimensional value. Therefore, it is not clear how the authors can derive the NO_2 concentrations which lead to a given AQI value from the AQI (and the mapping table) alone.
- In l. 11 on p. 6346, the authors write that *NO emissions cannot be negligible*. This sounds incorrect to me; maybe the authors meant to write *can be non-negligible*?
- In l. 19 on the same p. 6346, the authors write that *land use may have large differences in 15 years*. Why don't they just show the land use maps for the Nanjing region from both datasets? Also, the authors should reference their Fig. 8 in this context.
- In l. 22 on the same page 6346, the authors should write the differences between DECSO versions 3a and 3b. In the earlier chapter 2.1, the authors only talked about v3, and now there are two sub-versions, and the authors do not explicitly state their differences.

- In ll. 5f on p. 6347, the authors state that CHIMERE v2013 *improves* the NO_2 concentrations during night, while *improvements during daytime are rather small*. However, it is unclear with respect to which reference the concentrations *improve*. For nighttime, the authors have stated that CHIMERE v2013 gets rid of unrealistically low boundary layer heights which previously lead to unrealistically high NO_2 concentrations, but for the dayime, it is unclear which reference the authors refer to when they notice a *small improvement*, especially, since they did not point towards any deficiencies of CHIMERE v2006 during daytime.

- Again on the matter of *improvements* of CHIMERE v2013: In l. 16 the authors claim to *see some improvements for averaged NO_2 concentrations*; but given their own reference to *Blond et al. (2007)*, and given that the surface concentrations used in the present study are unreliable (as previously stated by the authors), maybe the conclusion of noticable *improvements* is not justified well enough.

- In l. 25 on the same page 6347, the authors see indication of *a better performance of CHIMERE v2013 in summertime*, based on the OmF they calculate. However, this conclusion would only be valid if the satellite measurements were reflecting the true NO_2 concentrations in the atmosphere. Given that the NO_2 measurements are subject to measurement uncertainties, the statistical significance of the conclusion *CHIMERE v2013 is better than CHIMERE v2006* depends on the magnitude of the measurement uncertainties. At least theoretically, it would be possible that the CHIMERE v2006 forecast were more accurate than the CHIMERE v2013 forecast; if now the NO_2 measurements were strongly biased towards the v2013 forecast, this could lead to the observed OmF values. Therefore, I do not see justification for the authors' conclusion that v2013 is better than v2006. I would appreciate at least a comment from the authors on this matter.

- In lines 6 f. on p. 6348, the authors write *[...] the NO_x emissions are almost entirely removed [...]*. This statement is not understandable to a reader who is not familiar with the DECSO algorithm. I suggest the authors elaborate a bit on this statement so that readers not familiar with DECSO can understand it.

- In line 8 on the same page 6348, the authors say that the unrealistic emission updates are *related* to large OmF values. However, correlation is not causation, so I would appreciate if the authors could modify their statement from a *relation* to a *concurrence*.

- In line 13 on the same page 6348, the authors should give reference to the PM data set they used for this observation.

- In line 17 on the same page 6348, the authors write about an *underestimation of cloud fraction [...] from OMI*. However, this statement is only admissible if it were already established that the MODIS cloud fraction is more correct / of higher quality than the OMI cloud fraction. As the authors do not give reference to any study allowing this conclusion, I believe it is not adequate to speak of an *underestimation*.

- In line 19 on the same page 6348, the authors should give proper reference to the cloud retrieval algorithm.

- In line 22 on the same page 6348, the authors give reference to *Lin et al. 2014*. However, in the reference list of the article, there is no such reference to be found; a proper study to cite in this context would be Leitao et al. 2011.

- In line 24 on the same page 6348 the authors should remind the reader that the *observational uncertainty* they are talking about to actually refers to the redefined obsertational error E_{obs} from Eq. 1.

- In lines 3 f. on page 6349, the authors should explain why *haze around the Bohai Bay [. . .] indicates that the high aerosol concentrations are near the surface*.
- In line 12 on the same page 6349, the authors should explain why they chose the filter criterion they mention, and how they arrived at this criterion.
- In line 17 on the same page 6349, the authors should explain why the longer NO_2 lifetime can lead to larger OmF values in winter.
- In lines 13-19 on the same page 6349, the authors should discuss what it means for their conclusions that the distribution of the OmF in winter is clearly not Gaussian (see the heavy tail to the right in Fig. 6b), given that they explicitly state on line 7 of the same page 6349 that /In the data assimilation it is assumed that the OmF distribution is Gaussian/.
- In lines 9 f. on page 6350, the authors write that /when the pixel size of the satellite is twice that of the model grid cell, the updates of emissions in that grid [cell] will even be doubled/. This statement again is not understandable to a reader who is not familiar with DECSO, so the authors should explain why this is the case. As a side note, to a reader not familiar with DECSO, this sounds like a serious flaw of the DECSO algorithm, so I do see the necessity to explain.
- In line 13 on page 6351, the authors write that they *include this in the SD*. This statement is not understandable. What is SD? Why do they chose to include the trend in the SD? What does this even mean?
- In line 16 on the same page 6351, the authors speak of a *small decrease in [. . .] February*. However, I do not see any decrease in Fig. 9 in February.
- In line 19 on the same page 6351, the unit *molec cm-2* cannot be correct with a number *6.6*.

- In lines 20 f. on the same page 6351, I the authors write that *consequently [. . .] NO_2 concentrations of the following months* are also lower than in previous years. Given the short lifetime of NO_2 in the atmosphere, I do not understand the causal connection implied by the authors' use of the word *consequently*. It would be nice to hear the authors' interpretation of this: Were the pollution control measures prolonged by the authorities? Were they voluntarily continued by the population? Is this a mystery? Also, the authors should define *the following months*, given that they never clearly stated their study period.
- In line 27 on the same page 6351, the authors should cite previous studies showing that *differences [. . .] can be attributed to the meteorological conditions*.
- The authors have to modify the conclusion they give in lines 12-16 on page 6353. From my understanding, the lack of observations in the second half of August 2014 means that it is impossible to decide if the emission reductions shown by DECSO for September 2014 actually occurred in August or September 2014. While I agree that it is highly probable that the emission reductions did indeed occur in August as a consequence of the implemented pollution control strategies, from a scientific point of view, it is impossible to draw this conclusion without doubt. I believe it is absolutely necessary to explicitly state this uncertainty of the time of emission reduction. Also, looking at Figure 3, it seems that there are no measurements in the Month of September, except for the last days of the month. What is the implication of this for the conclusions? And again, the statement cannot be understood by a reader unfamiliar with the DECSO algorithm, so the authors should add one or two sentences about this.
- Also the conclusion given in line 20 on the same page 6353 has to be modified, as DECSO did not detect any emission reduction *for* the YOG period, as there were almost no measurements *during* the YOG period. The conclusion has to be softened.

- In line 20 of page 6354, the authors should explain why the legislative measures which were only in place until 31 Aug 2014 (see Tab. 1) still effect NO_2 concentrations in the following month, given the short lifetime of tropospheric NO_2.
- In line 27 on the same page 6354, the authors should explain how they arrive at a resolution of *50* $\times$ *90 km*; the spatial resolution of the model was not explicitly given before.
- In lines 12-15 on page 6355 (or earlier, when introducing the concept of the OmF filter), the authors should elaborate why they chose an OmF filter and do not explicitly filter out scenes contaminated by high aerosol loads, using OMI or MODIS AOD measurements.
- In line 24 on the same page 6355, the authors write about *high electricty consumtions by power plants*. They probably mean high electricity *production* of power plants.
- In Table 1 on page 6361, it is not clear if earlier regulations were *complemented* or *replaced* by later regulations. Also, the authors should give reference to the source of information.
- In the discussion of Table 2 on page 6362, the authors should clearly state on which grounds they decided on the redistribution factors, or they are arbitrary.
- In Figure 1 on page 6363, the authors should clarify if the x axis is *sun local time* or *timezone local time*.
- In Figure 2 on page 6364, the colorbar does not have any units. What do the colors mean?
- In the same Figure 2, it is unclear what is actually shown. Are these time-averages of the whole period Jun-Aug? All invidual satellite measurements? All individual model grid cells? What is the spatial domain used for this Figure?

- For Figure 3 on page 6365, the authors should explain the meaning of the dotted horizontal lines. Also, it is not clear which spatial domain this Figure refers to. Finally, it is not clear what σ_{obs} refers to - is this the same as E_{obs} from Eq. 1?
- Figure 5 on page 6367 lacks any proper reference to the source of the images.
- In Figure 7 on page 6369, the authors should clarify if they mean *sun local time* or *timezone local time*. Also, given that the vertical bars are not *errors* but show the natural variability within one month, maybe the authors should not call the bars *error bars*.
- In Figure 8 on page 6370, it might be helpful to see a second subplot showing the effectively chosen land cover type for each grid cell, i.e., the scaled-down version of the same data.
- In Figure 9 on page 6371, again the authors write *error bars* even though the vertical bars do not contain *error* information but show the natural varibility for a single month. Also, *SD* should be spelled out as *standard deviation*.
- Figure 10 on page 6372 shows that there is a problem with the *error of the mean NO_x emission estimate from DECSO* (shaded areas): For August 2014, the minimum value of the shaded area is higher than the maximum value for September 2014, i.e., there is no overlap between the credible regions of the emission estimate for August and September 2014. However, the authors do draw the conclusion that the emission reductions seen in September actually happened in August already. I see a contradiction here: Either, the authors' conclusion is correct; in this case, the errors shown as shaded areas in Figure 10 are clearly too small. Or, the errors shown in Figure 10 are correct, but then the authors' conclusion would be merely speculation, as it would not be backed by the error estimates.

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