Review "High-resolution (0.05°x0.05°) NOx emissions in the Yangtze River delta inferred from OMI"

The manuscript entitled 'High-resolution (0.05◦×0.05◦) NOx emissions in the Yangtze River Delta inferred from OMI' presents an inventive method to estimate NOx emissions on a high resolution from satellite observations. The topic is interesting and important. Important aspect is also the error analysis that has been described in detail. However, before the paper gets published, the manuscript can be improved a lot by adding more discussion of the results including the relation to other existing inventories, and the description of method should be rephrased in a clearer way.

General remarks

This inventory is presented as the only high resolution inventory for the YRD region, but in the MarcoPolo-Panda project, a high resolution emission inventory of 0.01 degree resolution has been developed for this region (see http://www.marcopolo-panda.eu/products/toolbox/emission-data/) Since the affiliations of the authors were also participating in the MarcoPolo-Panda project it is surprising that this inventory is not mentioned or used in their comparisons.

Also Zhao et al. (2015) present a city-scale emission inventory with the resolution of 3kmx3km in Nanjing, in the Yangtze River Delta.:

Zhao, Y., Qiu, L. P., Xu, R. Y., Xie, F. J., Zhang, Q., Yu, Y. Y., Nielsen, C. P., Qin, H. X., Wang, H. K., Wu, X. C., Li, W. Q., and Zhang, J.: Advantages of a city-scale emission inventory for urban air quality research and policy: the case of Nanjing, a typical industrial city in the Yangtze River Delta, China, Atmos. Chem. Phys., 15, 12623-12644, 10.5194/acp-15-12623-2015, 2015

The authors claim that very high resolution emissions are lacking, but it is not mentioned how "very high" is defined. Several regions have a high resolution emission inventory with similar resolution as PHLET: CAMS in Europe, GlobEmissions in the Qatar and South Africa, MarcoPolo-Panda in various regions in China.

On page 3, Line 13-20 other methods are shortly introduced in a very confusing way. The conclusions the authors make here are not always correct. I will be more specific:

- The authors mention that their method is not computationally expensive and can be applied world-wide, but a rough calculation shows that their algorithm will take at least 10 year to calculate the emissions for the whole world, which is not faster than many other methods they refer to op page 3.
- The authors say that the methods are limited in time period, spatial domain and horizontal resolution. This is very different for all the referred methods. The methods of Miyazaki et al and of Stavrakou et al. have already been applied on a global scale, while other methods are also not theoretically limited to a certain domain. In general, the methods mentioned can be applied to any time period as long as satellite observations are available.
- The authors suggest that only (Lin et al., 2012) and Stavrakou et al. (2013) provide uncertainties of the CTM, while they are also presented by Miyazaki et al. (2012) and Ding et al. (2017).

The paper is missing a discussion on the results, there is a section “concluding remarks” which gives a summary, but I miss the following discussions:

- What are pros and cons of the introduced method? The pros are mentioned, but what is the downside of averaging a time period of 2012-2016. Specifically in this period strong trends are appearing in NO2 over China.
- The period is focusing on the summer time. What are the expected results for the winter-period. Will this change the spatial resolution? Will the magnitude of the emissions change a lot?
- It should at least be mentioned that there is no sector information in the derived emissions, which is an advantage of bottom-up inventories
- Although the gridding is on a 0.05° resolution the actual spatial resolution of the resulting emissions seems much lower. An indication of the intrinsic resolution can be obtained from the largest gradients in the emissions. I cannot detect clear structures with a 0.05° resolution. One would at least expect some power plants to show up as clear spots in this region. Maybe the method is still limited by the OMI resolution?

The structure of the paper is somewhat confusing and therefore I suggest moving appendix C and E to the main text. Section 2.1 is too short to understand the method of determining emissions.

In section 2.3: The contributions of lightning, biomass burning and aircraft emissions are neglected. The authors explained that the contributions of these emissions are small. In the inversion method, both soil and anthropogenic emissions are derived. In section 4, it is calculated that the soil emissions contribute 0.9% of the inverted emissions. This looks like a very small amount. However, biomass burning is considered as a significant source in the YRD, especially in summer. On a scale of 0.05◦×0.05◦ lots of biomass burning activity will exist. Give some more detailed information and explain why lightning, biomass burning and aircraft emissions are neglected.

The model error is set to be the sum of the quadrature of errors contributed by several aspects. However, there is no explanation on how the authors set some errors, for example the treatment of background NO2 concentrations. The authors use wind fields from ECMWF on a coarse resolution and regridded to a high resolution. The error of regridded wind field on high resolution can be quite large. The authors consider error of wind speed, but how about the wind direction? The error set for the wind looks optimistic.

References: All the references should be carefully checked if they are in the correct format, especially the names of authors.

Many articles are missing or articles should be removed throughout the whole text. It is advisable to let a native speaker make the necessary corrections.

Detailed comments

Page 1, Line 14: lacking => missing

Page 1, Line 17: The inversion => The model used in the inversion

Page 1, Line 18: We construct a model called PHLET (..)

Page 1, Line 19 Metrix => Matrix

Page 2, Line 5 tied => related

Page 2, Line 7 features => structures

Page 2, Line 8-9: This last sentence is kind of obvious. It should be moved from the abstract to the conclusions/outlook, but I suggest to just remove it.

Page 2, Line 21: split the sentence into 2 separate sentences to make it more understandable.

Page 2, line 24: on => of

Page 2, line 24: how is a “very high resolution” defined?

Page 3, Line 1: Bottom-up emissions do not only use spatial proxies but are also based on gathered statistical information of industrial output, car emissions, etc.

Page 3, Line 4: Please define “high”

Page 3, Line 20-21: What do the authors mean by “low-cost” and “high-resolution”

Page 3, Line 22: Here it is mentioned that these inventories are important for trends and variability. I agree, but the method presented in this paper do not give the possibility to study trends and variability, which should be mentioned somewhere in the conclusion.

Page 3, Line 22-23: Why is it important to understand air pollution with the advent of TROPOMI? I would say it is the other way around: TROPOMI is important for understanding air pollution.

Page 3, line 24: Constructing => construct

Page 4, line 1 other 13 => 13 other

Page 4, line 1: explain the acronym POMINO

Page 4, line 3: change to “a model called PHLET”

Page 4, line 5: delete “concentration dependent”

Page 4, line 17: Why is this the finest spatial information possible?

Page 4, line 19: What Is SCM? This is explained much later in the text.

Page 4, line 21-22: Without further explanation this does not explain the method.

Page 4, line 22: Which fixed formula is used?

Page 5, line 1: Which nonlinear relationship do the authors mean here? There are 3 quantities mentioned: (1) emissions , (2) lifetimes and (3) VCDs.

Page 5, line 4: A long time period is mentioned. What do the authors mean, a long time period to average or multiple 5 years periods? And why are these long time periods not presented in this paper?

Page 5, line 5: It is mentioned that the calculation takes about 36 hours after necessary input data? What are the necessary input data? How long does it take to prepare the input data?

Page 5, line 5: If the inversion takes 36 hours for a 5x5 degree domain, a global calculation will take about 10000 hours, which is about 10 year.

Page 5, line 8: a reference for OMI is missing.

Page 6, line 2-7: Removing the 30 outer pixel and the row anomaly will strongly reduce the number of pixels used in this research. How may pixels are still used?

Page 6, line 8: space => grid

Page 6, line 11: The footprint does not change, the location of the footprint changes from one day to another.

Page 6, line 14: the year of the reference to Fioletov is missing.

Page 6, line 17: For purpose => For the purpose

Page 7, Line 7: The assumptions of the PHLET model are not mentioned in Beirle et al. This reference should be removed.

Page 7, Line 10: The transport from neighbouring regions is missing in this list while this is an important contribution.

Page 7, Line15-16: Can the authors give a reference for this statement.

Page 7, Line 17-20: This is quite some assumption about the background value. What is the basis of this assumption? Why is the uncertainty set to 5%?

Page 8, line 14: What is the source of the wind data?

Page 9, Line 20: space => grid

Page 11, Line 7-8: I would suggest mentioning the average number of iterations (about 60?) needed to reach convergence and remove Fig C1. Is the value of 390 chosen based on this Figure and the fact that it is stable or are there other motivations?

Page 12, Line 6: It becomes more clear if the short appendix C is just put into the text here.

Page 12, Line 11: “inverted emission” is not the correct term. The concentrations are inverted to get the emissions. This “emission inversion” and “inverted emissions” is appearing in many places in the text.

Page 12, Line 11-12: What is the value of error on the lifetime? I suggest mentioning also the values of the calculated errors in the text.

Page 14, line 1: inverted => derived

Page 14, line 14: Since there is a lot of agriculture in the YRD region a soil contribution of 0.9 % seems very small and needs some explanation. A discussion on biomass burning emissions (which occur in the agricultural regions) can be helpful here as well.

Page 15, Line 1: Figure 2e is mentioned without discussing 2a-d.

Page 15, Line 13: Please mention the basis of the coloring of Fig 2f.

Page 15, Line 19: Why are the emissions not directly compared to bottom-up inventories instead of these proxies that are used in the bottom-up inventories. For example a comparison with the MarcoPolo-Panda or the Zhao et al. inventory can give more insights.

Page 17, Line 5-6: To separate the anthropogenic emissions, GEOS-Chem is used to calculate soil emissions. What is the uncertainties of the soil emissions calculated by GEOS-Chem?

Page 17, Line 6: Figure 3ows?

Page 17, Line 8: Comparing Figure 3e and 3f is only useful if they are at the same resolution. Thus Figure 3e should be regrided to the coarser resolution of Figure 3f.

Page 18, Lines 6-15: There is some repetition of the text of the previous sections

Page 18, Line 19-20: I would remove the last sentence about the programming language, which is not very relevant in a scientific paper.

Page 22: Line 17: Since one observation of the satellite is used in several grid cells, I doubt if the assumption that covariance matrices are diagonal matrices is correct. A discussion is needed here.

References: Most references contain many spelling errors and omissions.

Figure 2d: The lifetime is very short over the ocean, contradicting to what is usually seen in the literature.

Figure 2e: Although the gridding is on 0.05 degree resolution the actual spatial resolution of this image seems much lower. An indication if the intrinsic resolution can be obtained from the largest gradients in the emissions. I cannot detect clear structures with a 0.05 degree resolution. One would at least expect some power plants to show up as clear spots in this region.

Maybe the method is still limited to the OMI resolution. I would like to see some discussion about this.

Figure 2f: The plot is more logical when the x-axis and y-axis are reversed. I also suggest drawing a line for the 100% relative error in this plot as a helpline to guide the eye.

Figure 2 caption: What are the magnitudes of POMINO that are mentioned. In Figure 2f it is too small to see.

Figure 3e: This has to be regrided to the resolution of 2f for comparison.

Figure 3: I miss information on power plants, which are a major source of NOx.

Figure 4: The crosses in the plot, indicating the amount of grid cells, are too small.

Figure D1: I do not understand why we need two colors for the dots.

Figure D2: The lifetime depends a lot on chemistry, temperature and precipitation. Therefore, the plot seems very simplified.