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Interactive comment on "Space-based NO_x emission estimates over remote regions improved in DECSO" *by* Jieying Ding et al.

Anonymous Referee #1

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This paper by Ding et al. focuses on NOx emission estimates over remote regions using OMI observations and an improved version of the DESCO algorithm. The paper has many interesting aspects. I recommend publication after attention to the commenst below.

General comments:

Additional descriptions are needed in section 2 so that am independent author could reproduce the results from the information in the paper. For example, the prior emission inventory used and its error statistics (Pf) should be carefully enumerated. The authors refer to Mijling and van der A (2012) and to Ding et al. (2015) for detailed information, but the prior emissions used in those two studies are not identical.

The high resolution of the CHIMERE model system is a significant advantage over

other attempts to relate OMI NO2 measurements to emissions. However, the authors should acknowledge that there are known biases in the OMI product they are using that stem from low resolution elements of the retrieval. These will bias the resulting emissions by amounts that are comparable to the changes the authors derive. There will also be biases from the 25km resolution of the model that are not negligible especially for sources that are small compared to the grid scale. While it is not necessary to do new calculations, it is necessary that the paper discuss the results presetned in light of this related research.

Issues related to resolution effects on retrievals and models of NO2 are discussed in (among many other papers):

Heckel et al. Influence of low spatial resolution a priori data on tropospheric NO2 satellite retrievals, Atmos. Meas. Tech., 4, 1805-1820, doi: 10.5194/amt-4-1805-2011, 2011.

Kuhlmann et al. Development of a custom OMI NO2 data product for evaluating biases in a regional chemistry transport model, Atmos. Chem. Phys., 15, 5627-5644, doi:10.5194/acp-15-5627-2015, 2015.

Laughner, J. L., et al. Effects of daily meteorology on the interpretation of space-based remote sensing of NO2, Atmos. Chem. Phys. Discuss., doi:10.5194/acp-2016-536, in review, 2016.

McLinden et al. Improved satellite retrievals of NO2 and SO2 over the Canadian oil sands and comparisons with surface measurements, Atmos. Chem. Phys., 14, 3637-3656, doi:10.5194/acp-14-3637-2014, 2014.

Russell et al. A high spatial resolution retrieval of NO2 columns densities from OMI: method and evaluation, Atmos. Chem. Phys., 11, 8543-8554, doi:10.5194/acp-11-8543-2011, 2011.

Valin et al. Effects of model resolution on the interpretation of satellite NO2 observa-

tions, Atmos. Chem. Phys., 11, 11647-11655, doi:10.5194/acp-11-11647-2011, 2011.

Yamaji et al. Influence of model-grid resolution on NO2 vertical column densities over East Asia, J. Air. Waste. Manage., 64, 436-444, doi:10.1080/10962247.2013.827603, 2014.

Model error is an important aspect in emission inversion. As meteorological variables are not updated in DECSO, the sensitivity of observation to emissions (H) can suffer from model transport errors, which will bias the emission estimates. Discussion of the implication of model transport errors and its treatment on the emission inversion here.

The paper describes the resetting of the threshold of matrix H that represents the sensitivity of observation to emissions. The authors update the minimum value for H matrix elements from 0.05 to 0.1 hour. The motivation is that the H elements for observations located at the edge of the plume are usually small. Without explanations from a tracer transport perspective, I'm not convinced that setting a minimum is the appropriate approach to solve this problem. H elements calculated from the simplified 2D trajectories represent the contribution from model emission grid to the observations. Setting the 0.1 hour threshold could arbitrarily enhance this sensitivity for some emission points, and overcorrects the emissions which observations are not sensitive to. Some tests showing these effects are negligible and that a choice of 0.1 is optimal should be added.

The model assumption of persistent emissions is inconsistent with the behavior of biogenic, fire and lightning emissions. Additional discussion of this issue is needed.

For example, there is some knowledge of the mechanisms of biogenic emissions and models are available that represent processes. These processes vary strongly in repsonse to temerature and soil moisture. e.g. Hudman, et al.: A mechanistic model of global soil nitric oxide emissions: Implementation and space-based constraints, Atmos. Chem. Phys. Disc., 12, 3555-3594, 2012.

Do the derived biogenic emissions behave as expected in response to temperature or

rainfall?

It does not appear that lightning NOx emissions are represented in the model. Is it possible that the effects of lightning are interpreted as biogenic emissions?

Is it possible that fires are interpreted as biogenic emissions?

Details:

There are several other studies using Kalman filter and related methods to estimate NOx emissions The authors should cite them.

The equations, data and method should be provided for the calculation of soil emissions in Guangxi using emission rates from Li et al. (2007).

Figure 7: locations mentioned in section 4.2 should be marked in Figure 7 for readers who are not familiar with locations in Asia.

The authors should define "total emission" in this paper because it actually only includes anthropogenic and biogenic components. The general total emissions should include lightning NOx also. Suggestion is to rephrase it as total emissions from surface.

Support for the statement: "the errors caused by few observations on the edge of the emission plumes have been decreased" by updating H should be elaborated.

There are independent measurements from the national in situ observation network collected and maintained by the China National Environmental Monitoring Center (CNEMC).

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