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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Evaluation of OMI operational standard NO_2 column retrievals using in situ and surface-based NO_2 observations" *by* L. N. Lamsal et al.

Anonymous Referee #2

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In this manuscript, Lamsal et al. evaluate the most recent operational NO2 product from OMI by comparison to a number of validation measurements (from aircraft, Pandora, MAX-DOAS and surface in-situ instruments) as well as by indirect validation with the US NOx emission data base. Their main result is that the OMI NO2 product is in reasonable to good agreement with all the validation sources used, but that individual retrievals can show large differences for a number of reasons including a priori data used, spatial sampling, and measurement uncertainties in the validation data. The paper is well written, reports on the validation of an important satellite data product and provides a number of interesting and convincing new results. As already stated in my

quick review, I think it would have matched the scope of AMT better, but I can also recommend it for publication in ACP. There are however several important points which the authors should consider before submitting a revised version of their manuscript.

Major Points

1. **Limited geographical coverage:** The main problem of this paper is that it tries to provide an evaluation of the global operational OMI NO2 product but only uses aircraft spirals over 6 sites in Maryland during July 2011, a seasonality of Pandora measurements in Hampton, VA, MAX-DOAS measurements at two sites in Japan, and 2 (arbitrarily?) selected SEARCH surface sites. While this is better than many previous studies, it cannot provide serious constraints on the uncertainty of a product covering most of the globe in different seasons and under widely varying cloud, aerosol, NO2 profile and surface reflectivity conditions. I think the authors have to acknowledge clearly in the abstract, text, conclusions, and if possible also the title of the paper that their results are limited to certain regions, seasons and conditions.

2. **Extrapolation of aircraft profiles:** In their analysis, the authors extend the aircraft derived profiles towards the surface using the last measurement point and the gradient of the model profile. As is obvious from Figs. 2 and 3, the NO2 value in the lowest layer has a large impact on the shape of the NO2 profile and thus the column and the AMF derived from it. It is based entirely on the (shape of) the monthly GMI profile as none of the aircraft profiles shows indication for such an increase in NO2 towards the surface.

   As I expect most of the spatial and temporal variability of NO2 in the lowest layer, the method used will systematically underestimate the effect of profile assumptions on the AMFs and thereby on the tropospheric columns in Fig. 4.

   I think the method used for profile extension and the implications this has on the interpretation of results should be discussed in more detail.

3. **Statistics:** In spite of the large number of spirals flown and Pandora measurements taken during DISCOVER-AQ, there only are around 10 values per location in Figs. 4 and 6. I'm not convinced that computing the correlation makes a lot of sense for data sets having so few points, in particular if they are all from a period of less than 30 days in a limited geographical region.

   I'd therefore suggest adding two more panels to Figs 4 and 6 each, showing the full data sets in a scatter plot such as in Fig. 8, separately for standard and aircraft a priori profiles.

4. **Model comparison:** I do not see any added value in section 5. Numerous comparisons between OMI NO2 data and different model runs have been published, most of them applying proper data sampling and averaging kernels. I do not see anything in this section that extends upon what is already in the literature. In particular I do not see how this section justifies the statement in the conclusions reading *"Finally, we investigated the potential improvement of the retrievals that could be realised using a high resolution model, with updated emission inputs, as a source of a priori profiles."*. Improvements can only be documented by comparison to independent results and attribution of improvements can only be done if one thing at a time is changed, not everything (model, resolution, emissions) in one step.

   I'd therefore suggest removing section 5 and all figures and references linked to it.

Minor Points

- page 14524, line 11: I don't think this manuscript adds anything new on "objective methods to compare model-simulated NO2 columns with satellite retrievals"
- page 14526, line 3: I know that this is not the topic of this paper but I find the given uncertainty of 2E14 molec cm-2 for the separation between troposphere

and stratosphere really optimistic. If the authors believe this number, they should remove all the later statements pointing at this step of the retrieval as one of the possible sources for the differences observed with other data sets, as these differences are all more than one order of magnitude larger.
- section 2.4 – it would be worthwhile to already mention here how the temperature dependence of the NO2 cross-section is treated in the Pandora retrievals
- page 14531, line 11: As discussed above, the lowest layer in the "measured" profiles is based on model assumptions. I therefore disagree with the statement: *"Both the measurements and the model suggest that 20–30% of the tropospheric NO2 column is located near the surface"*
- section 3.2.4: While Figure 9 looks great, the reader wonders why these two SEARCH sites were selected and how the comparison looks for the other sites. Is there a good reason for this selection and the omission of all the other results?
- section 4: It would be good to make the link between scattering weights and averaging kernels here for readers not familiar with the differences in these two concepts.
- page 14540, line 10: While the differences are larger than stated in Boersma et al., they are in line with other estimates of high resolution a priori profile effects (Heckel et al., 2011, Russell et al., 2011).
- page 14544, line 13: What is a factor of 2 change in profile shape? I think a better measure would be the day to day change in tropospheric NO2 AMF
- Blond et al. reference: Typo in SCIAMACHY
- Crawford et al. reference – this doesn't look like a proper reference to me
- caption Fig. 3: circles show => circles shows

- figure 5: Please don't use dashed lines for error bars. It would also be nice if you could introduce an x-offset to the Pandora values to avoid overlapping of error bars with the aircraft data
- figure 8, left: Add 1:1 and 25 (or 30)% lines in the scatter plot

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Interactive comment on Atmos. Chem. Phys. Discuss., 14, 14519, 2014.