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Atmospheric Chemistry and Physics Discussions

Interactive comment on "A high-resolution and observationally constrained OMI NO_2 satellite retrieval" *by* Daniel L. Goldberg et al.

Anonymous Referee #1

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Review of "A High-Resolution and Observationally Constrained OMI NO2 Satellite Retrieval", ACPD, by Daniel L. Goldberg, Lok N. Lamsal, Christopher P. Loughner,Zifeng Lu and David G. Streets

The current paper presents a modified product of tropospheric NO2 columns during the Maryland 2011 DISCOVER-AQ. One of the major uncertainties of retrieving the vertical NO2 columns originate from the calculation of AMFs. The uncertainty of AMF might resulted from several inputs, but here, the main focus is on the shape factors. The authors claimed that a high resolution model output can potentially distinguish highly polluted regions from others, because they are roughly mixed in the original OMI product. The authors further brought up an important issue in CMAQ that is its large underprediction of NO2 in the free troposphere. This issue has nicely been addressed by using P3-B measurements to constrain the CMAQ NO2 profiles. Finally, the authors

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made use of CMAQ to downscale OMI tropospheric NO2 columns to provide a very high resolution “map” using the method of Kim et al., [2016]. Although there is not a great deal of effort to advance the retrieval process, I believe that this will be interesting for environmental agencies who are looking for a very fine product, particularly for the use of health impacts. This manuscript in its present form requires significant improvement before being acceptable for publication in ACP.

The first major problem with this study is the lack of adequate comparison in terms of magnitude. I am aware that the authors used the Pandora measurements, but the comparison might have been influenced by errors in OMI stratospheric NO2 columns (which had been added to the tropospheric ones to conduct an apples-to-apples comparison with Pandora). Since you are using a more spatially detailed model to estimate shape factors, the improvement in correlation is expected. However, it is imperative to use other products such as ACAM [Lamsal et al., 2017, JGR] to show if the new product will get closer to observations with small footprints. My suggestion will become more serious for your last product (OMI_CMAQ_OD). This is a pseudo observation based on integrating a model and an observation. Thus, its accuracy should be more carefully verified.

My second major concern is about overlooking the impact of accurate NO2 (and other gases) during estimating the scattering weights. Two parts that the priori NO2 values from model are used during the retrieval are i) estimating the jacobian values from a radiative transfer model and ii) calculating the shape factors. Why the first step was not performed or not even mentioned in this paper? Would considering a more accurate a NO2 priori profile lead to a better estimation of scattering weights? This may be investigated using VLIDORT.

My specific comments follow:

P2, Line 18. "to generate tropospheric air mass factors..." might be changed to "to re-calculate" or "to modify" OMI tropospheric air mass factors.

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P2, Line 24. How about the bias compared to ACAM or other air-borne observations?

P3, Line 13. "RO2" instead of "HO2" would be broader. You might need to define it in parentheses.

P4, Line10. I suggest the authors mention about the recent changes in China in 2011-2012 [Souri et al., 2017] mostly due to using SCR for power plants. Souri, A.H., Choi, Y., Jeon, W., Woo, J.H., Zhang, Q. and Kurokawa, J.I., 2017. Remote sensing evidence of decadal changes in major tropospheric ozone precursors over East Asia. Journal of Geophysical Research: Atmospheres, 122(4), pp.2474-2492.

P4, Line 14. How about the nadir-spectrometers like TES? This sentence might be revised. The footprint of surface concentrations exists in OMI signal. But it is not easy to separate it. The current sentence leaves readers with an impression that the radiance has not been impacted by the surface concentrations at all.

P4, Line35. You might want to elaborate their works in the introduction.

P5, Line 22. How was the stratospheric slant column subtracted from total column in OMI? CTMs or based on the OMI radiance?

P5, Line 26. I would suggest adding Martin et al., 2002 for NO2 shape profile.

P5, Line 30. Please provide references. I am assuming that scattering weights are already stored in a six-dimension LUT, and for partially cloudy pixels, a lambertian surface with albedo equal to 0.8 is assumed, then they combine the results (cloudy and clear) using the IPA. Is the new product different from this?

P6, Line 1. Please clarify whether the profile from GMI model is constant over time. You may need to mention: "It should be noted that a blue light converted which selectively photolyzes NO2 was used for P3-B. As a result, there was no need to modify NO2 concentration by applying an empirical equation from [Lamsal et al., 2008]."

P7, Line 4. NO2 varies quickly by time, and using a short duration is more appropriate,

because OMI captures NO2 just in a matter of milliseconds. Please check whether reducing the time average will make the comparisons better.

P8, Line3. Please specifically mention which scheme was used for the biogenic emissions. Did the authors consider the soil NOx emissions?

Figure 2. Why no observations were shown? In the text, you claimed that CMAQ has a better performance compared to observations (P9, line1).

P9, Line 10. I am not sure both model used the same lighting NOx option. The way they treat lightning might differ. There exit myriad of reasons for the underprediction of CMAQ NO2. It can be related to vertical mixing, uncertainty from NOx aviation emissions, stratospheric sources, or lightning. The vertical allocation of emissions are also different. If you had used the GMI for the chemical boundary conditions of CMAQ, it would have been easier to compare them.

P9, Line 19. Poor grammar.

P9, Line23. This is a very important message. It means the poor performance of CMAQ in simulating NO2 in free troposphere will make a challenge for the retrieval purposes. We may have to use the aircraft to constrain it, or to use GMI models at those specific altitudes. You may want to highlight it in the conclusion.

P11, Line 18. Please explain why it is rudimentary (i.e., not considering the errors in observations, model and the priori).

P11, Line 30. The discussion is not enough. Please explain the possible reasons of large differences between OMI_CMAQ and OMI_CMAQ_O. Were AMFs enhanced largely due to larger shape factors in the free troposphere?

P12, Line 28. I don't agree with your sentence "OMI can now "see" ...". This is an illusion. You used the model to downscale the values. This is not the OMI; it is the model that provided a tool to concentrate the observations. I would call it a pseudo observation or simply a "map". We have to clarify that OMI footprint is too coarse to

see these plumes. That's why we need TEMPO and TROPOMI. This paragraph should be revised or be dropped.

P15, Line 30. It is not only about the emissions, but also the meteorological fields. Simulating surface winds in many situations is not straightforward. So the winds may be off in the model resulting in wrong distribution of final product.

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