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Atmospheric Chemistry and Physics Discussions

Interactive comment on "A high spatial resolution retrieval of NO_2 column densities from OMI: method and evaluation" *by* A. R. Russell et al.

Anonymous Referee #1

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Improved retrieval of tropospheric NO2 columns is important as the product has been widely used to get insights into emissions and surface air quality. Russell et al. implemented retrieval parameters at high resolution and demonstrated the improvements in data quality. Although this is not a new research, the intent of the work is very good and will be of interest to users and therefore it is worthwhile to publish the manuscript in ACP. However, I would like the manuscript be revised addressing the following comments:

GENERAL COMMENTS:

1. The authors present the Berkeley High-Resolution (BEHR) product as the third OMI NO2 product. Use of high resolution monthly NO2 vertical profiles derived from WRF-Chem is stated to be a major cause of the improvement. As WRF-Chem is an

important part of the manuscript, additional details on model are necessary. Little information on WRF-Chem given in Section 3.3 is hidden, but it has to be made visible with more information in a separate section. The BEHR should not be presented as the third product. The manuscript as it stands tells me that a user who is investigating African or Asian emissions using standard or DOMINO NO2 product could switch to the BEHR product. It is available to the user who is interested to investigate ship emissions. That apparently is not the case. Emissions and meteorology determine the shape of NO2 profiles in model. What is the resolution of anthropogenic emissions in WRF-Chem? What are the extensions in latitude and longitude? Do the emissions have diurnal, weekly, seasonal, and annual components? What about other emission sources? Does it have lightning NOx emissions? What is the resolution and domain of the meteorological field? Unless these questions are addressed, we cannot be convinced that NO2 vertical profile shape from WRF-Chem is more representative than those from other global model. Please state clearly in both abstract and conclusion that the product is available just over a domain centered over California. Title of the manuscript needs to be changed.

2. The methodology is not clear. What is the starting point for your retrieval algorithm? Which radiative transfer model do you use to calculate AMF? What prompted the authors to implement the same method of stratospheric subtraction as for the Standard Product which has been critical in the past? How could you come up with the numbers: – 20% to +20% for terrain pressure, -40% to +40% for albedo, and -75% to +10% for NO2 profile shape? I assume, it will be very difficult to come to the conclusion without using the same algorithm as there are many steps/components that could differ between two independent algorithms. Please describe clearly how the study was carried out.

3. You state that MODIS albedo is not available over the ocean. This would mean that the method described here cannot be applied in the operational algorithm. If the main motivation is to obtain high resolution albedo database, wouldn't it be more log-

ACPD

11, C4942–C4945, 2011

ical to create high resolution (say 0.1x0.1 deg2) albedo product from OMI than using MODIS-based albedo? OMI-based albedo may be more suited to trace gas retrieval than MODIS BRDF due to retrieval consistency, same measurement time, and more representative spectral bandwidths. Describe why MODIS albedo is chosen for OMI NO2 retrieval algorithm?

4. I wonder if the retrieval and conclusions drawn here are based on observations in the month of June. The effect of albedo and profile shape could vary seasonally. Would -40% to +40% for albedo and -75% to +10% for NO2 profile shape still be valid for winter?

SPECIFIC COMMENTS:

1. Remove redundancies in description of OMI NO2 retrieval algorithms in introduction (Page 12413, line 14-27) and Section 2.

2. Page 12413, line 28: Following my earlier comments, it is probably not a development of new retrieval product, but a kind of sensitivity study.

3. Terrain pressure effects: I am surprised to see a large difference in terrain pressure. Terrain pressure in standard and DOMINO product might be based on ETOPO5 or similar, which generally has better spatial resolution than OMI observations. Why the GLOBE topographical database averaged over OMI observations should differ systematically by 5-20% being terrain pressure used in standard and DOMINO product generally higher? I wonder if the differences arise from the method of conversion from terrain height to terrain pressure.

4. Page 12417, line 1: What does the “effective terrain pressure” mean? How does it differ from “average terrain pressure”

5. Section 3.5: Complete new retrieval of OMI NO2: What does it mean? When it has same stratospheric field as in the standard product, how can it become complete new retrieval? Does it treat stripes and temperature correction differently?

ACPD

11, C4942–C4945, 2011

6. Page 12422, line 24: I wonder if the assumption of a constant 40 ppt NO2 would be valid over areas with heterogeneous boundary layer NO2 field. Because of advection, wouldn't the free tropospheric NO2 higher in polluted areas than in clean areas?

TECHNICAL COMMENT

1. Page 12418, line 28: Remove comma in front of "mean".

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 12411, 2011.