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Atmospheric Measurement Techniques Discussions

Interactive comment on "OMI/Aura Nitrogen Dioxide Standard Product with Improved Surface and Cloud Treatments" *by* Lok N. Lamsal et al.

Anonymous Referee #1

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In the manuscript "OMI/Aura Nitrogen Dioxide Standard Product with Improved Surface and Cloud Treatments", Lamsal et al. report on a new version of the heavily used OMI tropospheric NO2 product, which has recently become available. The main changes with respect to previous versions are the use of the GLER surface reflectivity approach by Vasilkov et al., 2017 for both the AMF calculations and the cloud retrieval (Vasilkov et al., 2018). In addition, surface pressures are now calculated in a more consistent fashion as described in Qin et al., 2019 and the treatment of snow / ice covered surfaces was changed. The new data version is described, and differences to the version 3.1 data are discussed in detail for one day of measurements. In addition, differences are shown for two seasonal means over major emission regions. In terms of validation, comparisons are made with data from the PANDONIA network and with NO2 profiles

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Discussion paper

from the DISCOVER AQ campaign.

The manuscript is clearly structured and well written. A description and evaluation of the new OMI NO2 product is an important documentation needed for anyone planning to use these retrievals, and it fits well into the scope of AMT. I think however that the evaluation of the product is not sufficient as it stands, and therefore can only recommend the paper for publication once this part of the manuscript was improved and extended.

Major comments

I have three major concerns with this manuscript:

1. While this document would make a very good ATBD for the new OMI product, it is not a very good paper in my opinion. The reason is that most of what is described in section 2 is a summary of what the authors have already published elsewhere, and while it is good to summarize everything in one place for data users, I did not see anything new here. If I missed something and the algorithms as now implemented in the OMI processor deviate from what was published in Vasilkov et al., 2017, Vasilkov et al., 2018 and Qin et al., 2019, then this should be highlighted.

2. What data users need to know is how the product changed relative to the last version. Some nice analysis is done on this as shown in Figs. 3 – 6, and I found this very interesting. However, this is only based on one day of data and does not differentiate by region, and I actually took a wrong message from this analysis, namely to expect a very significant (20 – 40 %) and consistent increase in tropospheric NO2 columns, in particular for large NO2 columns. However, as can be seen from Figure 12, this is not universally true, the differences for all Pandora stations been much smaller than what is expected from Figure 6! This is also evident from Fig. 11, where the version differences for Greenbelt have a

clear seasonality.

I think that the authors should pick a couple of regions representative for different NO2 scenarios (polluted places, very polluted places, biomass burning regions, soil emission regions, lightning regions) and present differences between the two data versions as a function of season as done for Greenbelt in Fig. 11. This would give the reader a much better idea of which changes to expect where and when, and such an analysis should be relatively simple to do.

It would also be nice to see an example of BRDF effects on the NO2 columns outside of sun glint regions – this is a major improvement of the new data version and it would be interesting to see if it has a noticeable effect on the tropospheric NO2 columns.

3. The authors call this OMI NO2 data version "improved", and I tend to agree that the GLER surface treatment is an improvement over the use of a static reflectivity database not covering angular effects. However, the validation data shown is inconclusive, and to me it looks as if any changes in the product are within the combined uncertainties of retrievals, validation measurements and representation errors. Based on these results, there is little reason to move to the new data version! It would therefore really be nice if the authors could find an example of where the new NO2 product performs clearly better than the last version.

Minor comments:

1. Add product version number to title
2. line 21: Not sure what the authors refer to by "regional" here – as far as I can see, the improvements presented here are for the global product while the most important improvement for regional products (high resolution a priori NO2 profiles) has not been addressed. I would suggest rephrasing.

3. line 24: While the GLER was conceptually new when proposed by Vasilkov et al., 2017, it is not in this manuscript. I would suggest rephrasing.

4. line 31 / 32: I would hope that all inputs to the AMF scheme are of high quality! I also don't think that a "new NO2 AMF scheme" is presented just because the AMF module reads other inputs. I would suggest rephrasing.

5. line 36 / 37: Nothing is said in the manuscript on emission and trend analysis of NOx, let alone of other trace gases. I therefore suggest removing this sentence.

6. line 43 – 45: I think this sentence fits better to an outreach leaflet than to a scientific paper.

7. line 71: bseen => been

8. line 92: "day-to-day (orbital) variability in surface reflectance" – I find this formulation confusing as in my view, it is not the surface reflectance which is changing from day to day but the viewing geometry which leads to a variation in reflectance at TOA.

   62: What was done for SZA > 70 where use of MCD43GF is not recommended?

   69: is => are

   74: is => are

   69: delete "retrieved"

   16: Differences in vertical sensitivity – isn't that already corrected for by the AMF?

   68: "to relatively OMI's large pixels" => "to OMI's relatively large pixels"

   1: What are the Ps coming from the GLER module?

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