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Atmospheric Measurement Techniques Discussions

Interactive comment on "A new stratospheric and tropospheric NO_2 retrieval algorithm for nadir-viewing satellite instruments: applications to OMI" *by* E. J. Bucsela et al.

Anonymous Referee #2

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In this paper, the authors report on a new version of the OMI tropospheric NO2 product with an emphasis on the improved scheme for the separation of stratosphere and troposphere. The new approach is described in detail, uncertainties and sensitivities to changes in the parameters are evaluated, and comparisons to the other two OMI retrievals (SP1 and DOMINO) are shown. The paper is clearly structured, well written and reports on an important new algorithm for the separation of troposphere and stratosphere in satellite observations of NO2. It therefore fits well into AMT and in my opinion should be published after addressing the points made below.

Major comments:

1) My main concern with this paper is that it mixes two issues:

- The separation between stratosphere and troposphere, for which an excellent job is done and a very promising improved algorithm is presented.
- A set of other, more gradual improvements of the tropospheric retrieval (profile climatology, surface reflectance, topography calculation) which will have large impacts on some of the retrievals but are only introduced and discussed in a very superficial way. The validation shown in Fig. 9 is addressing the latter part but is more or less worthless without a detailed discussion of which algorithm updates actually lead to the changes observed in the comparison.

In my opinion, this would be a much better paper if it would concentrate on the stratospheric separation and would then include more details, for example figures of the monthly climatology used, a more detailed statistical comparison with the DOMINO stratosphere, and validation of the new stratospheric columns with independent measurements. The other changes and their impacts on the product need to be discussed in a more detailed way which should be done in a dedicated paper and should then have an extended validation part. The present manuscript is not a good reference for the SP2 tropospheric retrieval but apparently the intention is to use it as such in the future. I'd suggest to reconsider this approach.

2) I find the new strat – trop separation scheme very convincing and the results look consistent. However, there are two aspects which are both briefly discussed in the paper but in my opinion have the potential to lead to artefacts and should be discussed in more detail:

- At mid and high latitudes, the diurnal change in NO2 in the stratosphere can lead to artefacts as it is not taken into account other than preferring measurements from the current orbit (which makes sense but doesn't help if they are flagged). See also my comments to Fig. 2.

- One of the nice results of the new scheme are the incredibly smooth and consistent near 0 values in background regions. However, in a way this is a prescribed result as in all regions where the model assumes a clean atmosphere and where there is no local hotspot, the measurements are assumed as being stratospheric and the result is basically the tropospheric column from the model plus the noise of the measurements. In many cases this will be a good value but in cases of non-locally elevated tropospheric NO2 not included in the tropospheric climatology used, the results will look nice and smooth but are not correct. Whether or not this is a serious problem can only be decided by critical analysis of a larger set of SP2 tropospheric NO2 columns.

Minor comments:

P 1373, l 18: What is V_{init}?

P 1367, l 8: Do the authors not apply the natural logarithm of the ratio? Is there a non-linear component in the retrieval to align the spectral axis between I and F?

P 1367, l16: Burrows et al., 1999 is not high resolution (GOME measurements)

Fig. 2: Looking at this figure, it is unclear to me how the spatial structure in the stratospheric field is created in the masked regions over Europe and the US. Over Africa, interpolated fields look smooth as expected, but in the other two areas, the interpolated (?) values have a lot of spatial structure and are actually always higher than the remaining measured values. Please explain.

Fig. 2: In high latitudes, clear artefacts can be seen in panel (c) from individual orbits – stripes at high latitudes in the left part of the figures, residual low values from an descending part of the orbit in the right part of the figure. This is not apparent in panel (d). Why?

Fig. 2: From the text I understood that for each orbit, the measurements from 7 orbits are used to estimate the stratospheric columns. Was this also done in this figure?

Fig. 6: If GMI is scaled per latitude to SP2 as stated in the text, then I do not understand the significant bias between panel (b) and (d) for example in equatorial regions. Please explain.

Fig. 7: P1 => SP1

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