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<tr><td></td><td>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.</td><td></td></tr>
<tr><td><u>2.01</u></td><td>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.</td><td>We have now added a paragraph at the end of the introduction giving the purpose and scope of the paper. Our goal has been to describe the overall retrieval algorithm (SP2) to serve as a general reference for NASA's current OMNO2 data product. The emphasis of the study and most important feature of the new algorithm is the strat/trop separation scheme, and we have sought to highlight this throughout the paper. However the more gradual improvements are also important and are now listed explicitly in a table at the beginning of section 2 (in addition to being described in detail elsewhere in the section).

Statistical comparisons of the data products have been now added in the supplemental section.

The reviewer makes an important point that validation is a critical component of the discussion, especially for the tropospheric retrieval. However, detailed validation, with a complete investigation of all effects of algorithm changes on the tropospheric retrieval is beyond the scope of this paper. We will address this separately in a subsequent validation paper, namely Lamsal et al. (2013). We have added words to this current paper to state this intention.</td></tr>
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2.02	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:	
2.02 (a)	• 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.	If the diurnal change in stratospheric NO_2 across an orbit is properly handled in the retrieval, there will be no associated artifacts (e.g. negative tropospheric values on one side of each orbit). We believe SP2 effectively avoids does this by preferring data from the current orbit and taking each measurement "as is", and by eliminating flagged data. The comparisons shown in section 4.2 and in the supplement suggest a reduction in artifacts relative to other algorithms.
2.02 (b)	• 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.	We agree that non-local enhancements in tropospheric NO_2 relative to the climatology may be confused with stratospheric NO_2. Scenarios like this are examined in sections 4.1.1 – 4.1.3. We have added a paragraph at the beginning of section 4.1 to draw the reader's attention to this discussion.
2.03	P 1373, l 18: What is Vinit?	Vinit is the ratio of the total slant column to the stratospheric AMF (please see Eq. 7).
2.04	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?	The log of I/F is not taken, in order to preserve the symmetry in the statistical scatter of the actual measured quantities, namely I and F (if the distribution in I/F is symmetric, then the distribution in ln(I/F) is not). In general, fitting ln(I/F) will produce smaller parameter estimates than fitting I/F itself. As to the second question, we spline the irradiance and absorber and pseudoabsorber cross sections onto the given radiance wavelength grid.

2.05	P 1367, l16: Burrows et al., 1999 is not high resolution (GOME measurements)	We now have the correct reference in the manuscript, along with a reference for the measured OMI slit function.
2.06	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(d) (showing masked areas in white) is plotted on an OMI orbital basis. Because of the way orbits overlap and are over-plotted at middle and high latitudes (e.g. over eastern USA and Europe) and because the pixels are small, it is not easy to see points that contain usable data for constructing the stratospheric field. However, some of these usable points are evident in Fig. 2(e), which is plotted on a grid-cell basis. The points show up in 2(e) as rectangular spots within the smooth field over eastern Europe, for example. These spots are responsible for the stratosphere's spatial structure within regions that appeared to have been uniformly masked in Fig 2(d). The spatial structure in the masked areas is introduced during the interpolation and final smoothing steps. In this particular example, the high Vinit values in these spots leads to the overall high values of the stratospheric field in surrounding masked parts of eastern Europe. An abbreviated version of the above comments has been added to the paper in the description of the interpolation procedure under Section 2.4.
2.07	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?	Descending orbital data are not used in the calculations. However, visible parts of the descending sections of the orbits have now been removed from Fig. 2.
2.08	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?	We use data from the current target orbit. Also ±7 adjacent orbits (for a total of 15) are employed to fill in any grid cells where target-orbit data are missing. This method is used in Fig. 2.
2.09	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.	The reviewer is correct that a bias still exists. This is due to the fact that the discrepancy between OMI and GMI is not exactly a linear function of latitude. This choice was only made for simplicity. We have added a comment to the text.
2.10	Fig. 7: P1 => SP1	Corrected