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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 #3

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The manuscript "A new algorithm for retrieving vertical column NO2 from nadir-viewing satellite instruments: Applications to OMI" by Bucsela et al. discusses modifications of the retrieval of NO2 from OMI and provides a comprehensive discussion of several improvements, compared to previous implementations, including an extensive error analysis. The study fits to the scope of AMT. However, before publication, the authors have to account for the following remarks, which require major revisions.

Major remarks:

1.) Section 2:

The authors discuss the retrieval settings for SP2. In some subsections, these are

compared to SP1, but not in all. For instance, in 2.1, it is not clear to me how far the described setup refers to SP1, SP2, or both. I would appreciate if the authors could provide an introductory paragraph to section 2 where they clearly state the main changes of SP2 wrt SP1, and take care that in each subsection the respective discrimination is clearly listed in detail. It woul help a lot to have a table comparing all retrieval settings of SP1 vs. SP2.

2.) Section 2.4:

The proposed stratosphere/troposphere separation (STS) is a major component of the new retrieval. In fig. 2, the results of the STS implementation are compared to other STS for 21 March 2005, showing clear improvements. However, for STS, winter is the most challenging time of year for the northern hemisphere due to the polar vortex (see e.g. Beirle et al., 2010). Thus, the authors should provide further comparisons for different months, on daily as well as monthly mean basis. Zonal means as provided in Fig. 8 are not sufficient, as they do not resolve the 2D-patterns caused by the polar vortex in wintertime. For instance, if the stratospheric column has a maximum/minimum over the US eastcoast, which regularly happens in wintertime, this would be masked out by the applied pollution threshold, and the stratospheric estimation would be biased low/high, respectively. It would be interesting to see the comparison of various STS for such challenging cases, and how far the derived uncertainties also apply for winter.

3.) Error estimate

In section 3.2, the error of the estimated stratosphere is discussed. For the uncertainty due to the a-priori tropospheric column, an uncertainty of 1.5e14 molec/cm2 is given and labelled as "conservative", as 3e14 molec/cm2 was applied as threshold for masking. However, the error of the a-priori tropospheric column itself, taken from a CTM, is not known and could only be determined by independent measurements. If, e.g. over remote regions, the model yields columns below 3e14 molec/cm2, but the true column would be higher, as a consequence of emissions that are not appropriately represented

in the model (e.g. soil emissions, which are highly uncertain), the proposed algorithm would interpret the observed enhancement as stratospheric. This error source is intrinsic for all stratospheric estimations based on column measurements alone and should be clearly admitted. The respective error of the stratospheric column would be as high as the tropspheric enhancement. The only way to overcome this ambiguity would be independent measurements. Cloudy observations might help, but only if cloud fractions as well as heights are high enough. Thus, the conclusion that "the errors ... are comparable to nominal ... uncertainties in the stratosphere" (1390/11-12) has to be restricted in so far that it relies on "clean" regions (1390/11) - as defined by the model, which may miss something!

Further comments:

- 1362/7: "... any global zonal wave pattern" - The authors do not fit a wave pattern any more, but still, they have to somehow fill gaps of the stratospheric fields over polluted regions, which they do now by Lagrange interpolation. Thus, the abstract is misleading.

- 1362/15: "significally smaller" alone does not mean "better". Please specify the region and explain why this supports SP2.

- Please add a reference to the NOx/chemistry part of the introduction.

- 1365/20: "using only ... measurements and ... climatologies" - In addition, you need interpolation as well, which is in principle not that different from IPT or wave fitting. I see the main reason for the reduced artefacts in SP2 by the far less rigorous removal of nadir measurements. Please clarify.

- Note that Leue et al. already focussed on cloudy observations for the stratospheric estimation, an approach that the community seemingly has lost track of in the following years, but should be referenced appropriately in this study.

- 1366/2: "which was recently improved" - please provide more details and a reference.

- 1370/10: Why are Lagrange polynomials used for interpolation? This can cause

drastic divergence, see

http://en.wikipedia.org/wiki/Lagrange_polynomial

. Please comment on this and investigate alternatives.

- 1372/2: "that vary about the mean" - I do not understand this.

- 1377/18-20: The error interdepencies might reduce, but could as well increase the overall error! I thus don't agree that the overall uncertainty is an upper limit.

- Fig. 7: Caption: (a) should be "SP1"

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