	Anonymous Referee #3 Received and published: 12 April 2013 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.	
3.01	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.	We appreciate the recommendation for clarity and have now added a table near the beginning of section 2 that readers can use as a reference in subsequent discussions comparing SP1 and SP2.
3.02	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.	The reviewer is correct that smaller-scale stratospheric features over polluted areas can appear as tropospheric NO_2. These and similar scenarios have been discussed in sections 4.1.1 – 4.1.3, and are now highlighted with an introductory paragraph in section 4.1. We have also added a supplemental section containing additional maps, showing daily and monthly mean retrievals from January and July 2005. These provide further illustration of how the STS algorithm handles a variety of challenging cases. We note also that the breakup of the polar vortex in the 2005-03-21 example is not handled well in either the SP1 or DOMINO algorithms, and we note this now in section 4.2
3.03	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	It is correct to say that the CTM errors are mostly not known, and that they can bias the stratosphere in clean or otherwise unmasked regions. An exception would be a strong and/or very localized tropospheric departure from the model, which can be treated as a “hot-spot” in an unmasked region and subsequently prevented from

	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!	affecting the stratosphere. Many of these issues are examined in section 4.1, and we have now added words to sections 2.4, 3.2 and 5 to further reinforce the reviewer's comments. We also note that the effects of CTM errors are addressed in detail in our companion paper Lamsal et al. (2013).
3.04	- 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.	We use box-car smoothing (moving averages) to fill in the stratospheric-field gaps, not Lagrange polynomials. We believe this is consistent with the statement in the abstract.
3.05	- 1362/15: "significantly smaller" alone does not mean "better". Please specify the region and explain why this supports SP2.	We have now added words to the abstract to more explicitly address this concern. A full comparison will be available in our subsequent validation paper (Lamsal et al., 2013).
3.06	- Please add a reference to the NOx/chemistry part of the introduction.	Two standard text-book references have been added to the introduction.
3.07	- 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.	We generally agree with the reviewer that the algorithm builds on the heritage of previous methods in using some form of interpolation to determine the stratosphere in areas masked for pollution. We would not characterize the masking as more or less rigorous – rather we are using different criteria for the masking and our own approach to the interpolation. We have added words to this paragraph to more explicitly highlight the unique features of the algorithm. Of course further details are provided in subsequent sections of the paper.
3.08	- Note that Leue et al. already focused 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.	Thank you for the suggestion - we now reference Leue et al in section 2.4
3.09	- 1366/2: "which was recently improved" - please provide more details and a reference.	We have added a few details and a reference in the form of a private communication.
3.10	- 1370/10: Why are Lagrange polynomials used for interpolation? This can cause drastic divergence, see http://en.wikipedia.org/wiki/Lagrange_polynomial	We are aware of the possibility of divergence in Lagrange interpolation. In fact, one of the significant changes

	Please comment on this and investigate alternatives.	from SP1 to SP2 was to recreate the lookup tables with node points more suitably selected to avoid divergence in a few parameter ranges where it had been found. The values interpolated using Lagrange interpolation are now within a 2 percent of the values computed using the full radiative transfer calculations in the cases we have tested. We continue to use the Lagrange interpolation
3.11	- 1372/2: "that vary about the mean" - I do not understand this.	Yes, this phrase was unnecessarily vague and has been deleted from the text.
3.12	- 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.	We agree with the reviewer, in principle. Taking account of positively correlated variables leads to an increase, not a decrease in the net error, under the usual assumptions (normal distributions, etc.). Thus, in principle, our estimated uncertainty is not an upper limit. That said, would it be physically reasonable for R, w,and pc to be correlated? I think one might be able to make a case for a small degree of correlation between \zeta and T, but it would not have much of an effect. The last 2 sentences of this paragraph were removed.
3.13	- Fig. 7: Caption: (a) should be "SP1"	Corrected