Atmos. Meas. Tech. Discuss.,
doi:10.5194/amt-2015-405-RC1, 2016
© Author(s) 2016. CC-BY 3.0 License.

Atmospheric Measurement Techniques
Discussions

Interactive comment on "The STRatospheric Estimation Algorithm from Mainz (STREAM): Estimating stratospheric NO_2 from nadir-viewing satellites by weighted convolution" *by* S. Beirle et al.

Anonymous Referee #1

Received and published: 26 February 2016

This paper presents a good and thorough description of the STREAM algorithm for separating stratospheric and tropospheric NO2 in satellite retrievals. STREAM is a logical next step in the series of increasingly sophisticated RSM- and MRSM- type STS algorithms (e.g. Richter and Burrows, 2002 to Valks et al 2011 to Bucsela et al 2013). To estimate the stratosphere, the authors use measurement-based a priori pollution amounts, cloud conditions (both coverage and height) and then make an iterative correction for tropospheric contamination that may have been missed in the previous estimate. They do extensive comparisons of STREAM relative to other algorithms as applied to OMI, GOME-2 and SCIAMACHY as well as looking at results from an in-

dependent model estimate of stratospheric NO2 from the EMAC model. The paper is well written and generally well organized and worthy of publication after the authors address the major and minor concerns below.

Major comments:

1. The inherent question in any STS algorithm is whether features in the V* field belong in the stratosphere or troposphere. With its relatively smooth stratosphere (evident in Fig 3), STREAM correctly identifies some structures in unpolluted regions as tropospheric (e.g. tropospheric transport events). But it also folds some synoptic-scale stratospheric features into the troposphere. To clarify such ambiguities, it is essential to test an STS algorithm against truth by applying it to synthetic data. The authors perform such a test and show the effective retrieval errors. However given its importance, this section of the paper seems a bit brief. It would be useful to see how the weighting factors and convolution kernel sizes can be tuned to improve the retrieval. The section on parameter sensitivities indicates the magnitudes of the effects of parameter adjustments, but it could go further in determining which are actually better and why the authors believe STREAM's method is optimal. For example, could an increase in wcld (Fig. S9) help counter the low bias in T* seen in Fig S 18?

2. Stratospheric NO2 shows significant diurnal variation across an orbital swath. This is evident in the daily OMI DOMINO and NASA stratospheres when plotted orbit by orbit. From the description of STREAM, it is not apparent to me that the algorithm accounts for this, and therefore some of the stratospheric variation in each orbit will be aliased into the troposphere (this is also a problem in the RSM and in some of the other MRSM approaches). Please give an estimate of the magnitude of this effect and discuss the consequences of ignoring it, as well as the handling of orbital overlap – especially at mid- and high latitudes. A somewhat related question is how STREAM treats OMI pixels near the swath edges. Are these weighted differently?

3. In section 4.2 the authors describe a number of sensitivity studies. But sensitivity to

the pollution weights should also be included here since it seems likely these weights would have a large effect on the retrieval.

Additional comments:

1. (pp 5-6) I think the algorithm description needs a more detailed outline than the 2-step version given at the top of page 5. Please list each step explicitly, including the names of the three weighting factors, their product and the iteration involving the TR weights. This would improve clarity, since the order of presentation in the paper is not exactly the order of data processing.

2. (p 8) In section 2.2.3, setting-description #3 says that the minimum size of a region for considering a TR weight is a grid pixel and the immediately surrounding pixels. Is this also the maximum size?

3. (p 12) In section 3.1.1, the current version of the OMNO2 data product should be v2.1, not v3.

4. (p 12) Also in 3.1.1, the authors claim that new SCDs from van Geffen et al. (2015) and Marchenko et al. (2015) would not affect the TR. This is not true: the smaller SCDs will also decrease the retrieved tropospheric amounts.

5. (p 15) As described in section 4.1, Fig. 3 shows V* for two days along with the STREAM stratosphere. Please also show also the stratospheres for RSM, OMI DOMINO and OMI NASA on the same days, since these would help indicate how much of the V* structure is assigned to the stratosphere in each algorithm. It is difficult to determine this by looking only at the highly structured daily TR maps for the respective algorithms (see major comment 1)

6. (p 16) In the discussion of cloud weight (4.2.1, part (b)), why not use a larger wcld, which lowers Vstrat and increases the magnitude and standard deviation of T* – i.e. why does the algorithm have a lower wcld as its baseline?

7. (p 19) I suggest a wording change at the bottom of p19 (section 5.1.2): "…interprets

the difference between the full total column and the (small) tropospheric model as stratospheric column…"

8. (p 20) Regarding section 5.1.2, bullet point 2, the way the stratosphere is estimated need not affect NO2 retrievals by cloud slicing (see Choi et al., ACP 2014).

9. (p 20) In section 5.1.3, please state why an additive rather than multiplicative offset is applied to EMAC?

10. (p 22) Section 5.2, first sentence at the top of the page, a misspelling: "…extraordinarily high…"

11. (p 23) Section 5.3, minor wording changes: "…overall still works well…", "…similar to GOME-2…", "…similar to OMI or GOME-2…"

12. (p 26) Section 5.7 states that STS errors cannot be directly quantified since the "true" stratosphere is not known. As the authors have already shown, this can be known to some extent using synthetic data (major comment 1). Please include a comment here to this effect.

13. (p 26) Section 5.7, last sentence, a misspelling: "…focusing…"

14. (p 27) Section 6, Conclusions, 2nd paragraph, a misspelling: "…with a high weighting factor."

Interactive comment on Atmos. Meas. Tech. Discuss., doi:10.5194/amt-2015-405, 2016.