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 #2

Received and published: 25 April 2016

The manuscript “The STRatospheric Estimation Algorithm from Mainz (STREAM): Estimating stratospheric NO$_2$ from nadir-viewing satellites by weighted convolution” by Beirle et al. is a very thorough description of a new algorithm for the separation of stratosphere and troposphere in the space-borne measurements of tropospheric NO$_2$. It is very well written and a pleasure to read. I recommend the manuscript to be published in Atmospheric Measurement Techniques. However, in order to further improve the manuscript, I suggest addressing the following minor comments in a revised version of the manuscript:

C1

• The number 1E14 molec/cm$^2$ is mentioned several times as tropospheric background NO$_2$ columns over the remote Pacific Ocean, and cite a publication by Valks et al for reference. However, it should be noted that other studies (Martin et al., doi:10.1029/2001JD001027, Fig. 8; Hilboll et al., 2013, doi:10.5194/amt-6-565-2013, Fig. 5) derive significantly higher background values over the Pacific. It would be good if the authors could acknowledge that the number they use is at the lower end of a range of values proposed by previous studies.

• p. 7, l. 4-5: The mean spatial distribution does not reflect the pollution probability, as claimed by the authors. E.g., the same mean value can be caused by a single extreme pollution event in an otherwise clean region, or by moderate, constant in time, pollution levels. So the notion of probability should not be used in this context.

• p. 7, l. 5-6: The authors should specify if the multi-annual mean trop. NO$_2$ column the use does include the seasonal cycle, i.e., if they have one “multi-annual mean” per month. If not, the authors should clarify how the seasonal cycle is being considered in the weight calculation.

• p. 7, l. 10: It would help the reader if the authors could give a range for the pollution proxy $P$. Otherwise, it is impossible to grasp how large $w_{pol}$ is in comparison to the other weights.

• p. 8, l. 12: The authors should explain why measurements where the strat. contribution has been overestimated should contribute more strongly to the strat. estimate.

• p. 9, l. 2: The reference to “S4.2.3” is wrong.

• p. 9, l. 3-6: The example of pixels over U.S., Europe, central Africa, and China leading to low $w_{TR}$ is not helping, since without further information, the reader
has to assume that these regions already have low weight due to \( w_{pol} \). As the
differentiation between the pollution and the trop. res. weights is not immediately
clear to the reader anyways, it might be a good idea to find an example of unusually
high polluted regions, which would not have been assigned low weights by
using \( w_{pol} \) alone.

- p. 10, l. 1: If the authors set \( W_{ij} = 0 \) in case of measurement gaps, then \( V_{ij} \)
as defined by the authors is not defined. Shouldn’t it be enough to set \( C_{ij} = 0 \)?
Otherwise, please amend Eq. 11 so that it yields a well-defined \( V_{ij} \) everywhere.

- p. 10, l. 4: The authors should clarify if the 2D Gaussian they use as CK is
defined in degree-space or in kilometer-space. If it is defined in degree-space,
they should justify the resulting inconsistency depending on latitude.

- p. 13, l. 31: There’s a spurious "see" in the reference to Jöckel et al.

- p. 14, l. 6: The authors should specify how the EMAC model determines the
tropopause height, i.e., thermal, dynamical, . . . criterion?

- p. 17, l. 5: Currently, the manuscript states that "the final Vstrat […] as weighted
mean of both [CKs]". This is not really precise, it should rather say that the final
Vstrat is the weighted mean of Vstrat calculated with both CKs.

- p. 18, l. 6: The authors write of "the" small-scale structures of strat. NO$_2$ in
EMAC. It would be good to elaborate a bit on the "the", i.e., in which regions,
in which months, . . . I suspect that these structures are mostly there at low
latitudes, but it would be good if the authors could be more explicit about this.

- p. 18, l. 10-11: The authors should clarify if the "remaining biases" are low or
high biases.

- p. 18, l. 26: "meaningful" has an extra "l"
• p. 27, l. 21-24: The authors should think about ways in which the LT of measurement being in the morning could hinder STREAM for GOME/SCIAMACHY/GOME-2.

• Fig. 8: I cannot understand how the 90% percentile $T^*$ over polluted regions can still be lower than 1 CDU. Is this really correct? Furthermore, I have trouble deriving the minimum in the $T^*$ difference over China/Japan in Fig. 9 from the statistics given in this Fig. 8.
   After seeing the definition of "polluted" in Fig. S7, this becomes clear; but it points to the misleading label "polluted" in this context.

• Figs 9, 10, 11, S19, S20, S23, S24, S26, S27b, S30-S31, S33 should have $\Delta T^*$ as colorbar label instead of $T^*$.

• Fig. S9: Isn’t increasing the cloud weight by a factor of 10 equivalent to increasing the total weight by a factor of 10, due to the definition of the total weight in Eq. 8? Why should this Figure then say something about the cloud weight in particular?

• The authors should introduce the OMI instrument before making reference to it from p. 8 onwards. In the current manuscript, OMI is only introduced later, in Sect. 3.1.