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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 #2

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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:

- 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 refernce 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. 19, l. 11-12: The authors should comment on whether they expect higher variability of T^* in DOMINO or STREAM.
- p. 19, l. 17: underestimation "by", not "of" DOMINO
- p. 21, l. 9-11: Might the longitudinal dependency in STS_{EMAC} partly be caused by the temporal sampling of the EMAC model? The LT difference between East and West end of the OMI swatch is rather large, and using a fixed EMAC time might introduce longitudinally varying biases
- p. 21, l. 28-29: GOME/SCIAMACHY/GOME-2 might have systematic differences in the CTP products, which might in turn influence the STS results. Furthermore, the spatial resolution of the different instruments should lead to different pdfs for the cloud fraction, again possibly influencing the STS results. It would be good if the authors could comment on this issue.
- p. 22, l. 24: "MOZART" should be replaced with "MOZART-2"
- p. 24, l. 20: Also, the smaller pixel size will lead to a higher fraction of purely clouded pixels. The authors should briefly comment on the implications.
- Sect. 5.5.2: Sentinel-4 is the name of the satellite, not the instrument. The authors should instead write something like "UVN onboard Sentinel-4" or the like.
- p. 26, l. 8: Please add "on average" before "negative T^*, because single negative T^* are expected, as the authors already noticed elsewhere.
- p. 26, l. 31-32: The authors should note that applying STREAM to other trace gases, where the bulk of the profile cannot be expected to lie within the boundary layer but rather in the same altitude ranges as clouds is at least challenging.

- 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 ΔT^* 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.

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