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Atmospheric Measurement Techniques
Discussions

Interactive comment on "Discrete-wavelength DOAS NO_2 slant column retrievals from OMI and TROPOMI" *by* Cristina Ruiz Villena et al.

Anonymous Referee #2

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General comments

This manuscript presents a simplified NO2 slant column retrieval approach, which makes use of a limited number of wavelengths in an otherwise classical DOAS retrieval framework. The approach is tested on sample data from the OMI and TROPOMI sensors, and results are discussed in terms of their consistency with standard retrievals. It is concluded that retrievals based on strongly sub-sampled spectra (only 10 wavelengths are used) still provide good NO2 slant columns. Although this result is not surprising as such (given the high quality of the original measurements), the small reduction of the noise on the retrieved slant columns is in my view a bit unexpected and worth pointing out (and possibly explain). So we come with the conclusion that retrieving NO2 columns from 10 spectral points is feasible. There are however a number

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of drawbacks and limitations in doing so, and one may wonder whether the potential advantages of reducing the spectral information would actually compensate these drawbacks.

The fundamental motivation behind the study relies on the postulate that reducing the spectral information would allow to simplify instrumental design (of future satellite missions) leading to potentially improved spatial resolution at low cost. Statements along these lines are given at several places in the manuscript, but without any further elaboration, e.g. what kind of instrumental solution could be adopted? More importantly, key requirements on spectral accuracy and stability that would need to be considered for such a design are not mentioned at all. It is basically assumed that spectral performances equivalent to those of OMI and TROPOMI can "easily" be obtained with low-cost imaging systems suitable for integration on small satellites. To my opinion, the lack of such a discussion significantly limits the relevance and impact of the study. I therefore recommend publication only if these questions are better addressed in a major revision of the manuscript.

Specific comments

Pg. 2, line 25: not all sources of tropospheric NO2 are anthropogenic in nature. Please complete.

Pg. 2, line 43: in addition to in-situ and satellite techniques, also ground-based remote sensing constitute a key component of the atmospheric composition monitoring system. This includes e.g. the Network for the Detection of Atmospheric Composition Change (NDACC) or the emerging Pandonia/PGN network.

Pg. 2, line 48: current satellite instruments are limited in resolution, but TROPOMI is already doing much better than OMI. This should already be mentioned here, with a mention that ultimate resolutions in the range of 1x1 km2 are needed to allow for individual source identification.

Pg. 2, line 56: The current resolution of TROPOMI at true nadir is 3.5 x 5.5 km2.

Pg. 3, line 71: the Brewer instrument is cited here as an example for a NO2 measuring system based on a few wavelengths; however it is well-known that Brewer NO2 measurements are dramatically lacking sensitivity. This was actually at the origin of the development of the Pandora instrument, which uses simple (low-cost) grating spectrometers to (strongly) improve the quality of NO2 column measurements.

Pg. 3, line 75: what are the “specific viewing geometries” that prevent usage of the NO2 camera for space applications? Please clarify.

Pg. 4, line 116: describe in short the interpolation method used by Buscela, and its added value for this study

Pg. 5, line 123: this introductory paragraph is a bit misleading. To my understanding the critical aspect of selecting appropriate spectral channels for NO2 fitting is not related to the complexity of the radiative transport, but only to the nature of the differential cross-sections and the presence of interfering species.

Pg. 5, line 131: replace “mean optical depth” by “differential optical depth” (or difference in optical depth)

Pg. 6, Figure 2: how important is it to include liquid water cross-sections in the fitting? In the spectral range of interest, this cross-sections seem to be very unstructured and may correlate strongly with the polynomial function.

Pg. 6, line 144 (very minor comment): the choice of “discrete wavelength DOAS” as a name could in fact be questioned, since fundamentally all DOAS schemes use discrete wavelengths (it is just that in your case, their number is smaller)

Pg. 9, line 199: how can local variations in surface albedo explain differences between retrievals from same satellite pixels? Please clarify the meaning of this statement.

Pg. 13, Figure 5: why such a discontinuity in the NO2 map of 30 Oct 2005 (at 20°S)?

This looks like an artefact apparent in both QA4ECV and DW-DOAS results (considering the difference plot)

Pg. 13, line 265: the explanation given for the striping problem is inexact. Stripes in NO2 SCDs are not related to inhomogeneous illumination of the entrance slit, but merely due to small issues in spectral calibration, dark current correction and detector sensitivity (see e.g. Boersma et al., 2018)

Pg. 13, line 260-264: the small difference in noise despite the large difference in the number of spectral points considered for the retrieval is somehow a (good) surprise to me. I would have expected much larger differences. It would be interesting to further investigate and explain the fundamental reason for this lack of dependence of the noise on the number of spectral points.

Pg. 14, Figure 6: my guess is that most of the outliers in the correlation plots are related to the SAA. This could be easily verified by excluding the SAA area from the comparison.

Pg. 17, line 285: this sentence is maybe not necessary here, since it is a repetition of what has been said before.

Pg. 18, line 290: as already mentioned in my general comments, this part of the manuscript lacks more details on the instrumental challenges (or difficulties) associated to the potential new instruments. In particular, for DOAS retrievals of tropospheric NO2, it would be essential that the instrument allow for perfect spatial co-location of the 10 wavelengths and that all of them are recorded simultaneously. Any deviation with respect to these requirements might lead to spectral distortion affecting the accuracy of the slant column retrieval.

Pg. 19, line 299: as pointed out in the paper, possible wavelength calibration inaccuracies are a potential source of error. It would be interesting to investigate the sensitivity of the algorithm to such errors. This would also provide an idea of the associated requirement on instrument design.

Pg. 20, line 306: note that O4 retrieval is not needed for cloud fraction retrieval, but only for cloud top height retrieval. For a sensor working at high spatial resolution, a good working option would be to rely only on cloud free pixels (without the need for a cloud correction).

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