

Interactive comment on “Investigation of NO₂ vertical distribution using two DOAS retrievals for GOME-2A measurements in the UV and vis spectral range” by Lisa K. Behrens et al.

Anonymous Referee #2

Received and published: 15 November 2017

Behrens et al. present a study showing the relationship between UV and VIS tropospheric NO₂ VCDs and how it can relate to information on vertical distribution of NO₂ in the troposphere. Although the paper is well written, it appears to be rather deceiving. I find the paper too qualitative and the reader could expect an attempt to effectively derive some information on the vertical distribution of NO₂ from the combination of UV and vis NO₂ measurements. The authors provide a number of possible reasons for the differences between UV and vis NO₂ VCDs (a-priori profiles, effect of clouds/aerosols, etc) that are all plausible (and speculative) but there is no clear way forward. They almost conclude on the current impossibility to derive profile information. A weak point is that it is difficult to separate possible errors in the retrievals (in the UV spectral fits)

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from real effects.

To be published in AMT, the following points need to be addressed:

- a comprehensive error analysis on the UV retrievals needs to be undertaken.
- section 2.2: the effect of T on Uv+vis NO₂ retrievals is not well discussed. NO₂ cross sections are varying with T but only one T cross-section is included in the fits (both for UV and VIS DOAS fits). What is the impact on the results and conclusions of this study?
- section 2.6: what is the uncertainty due to errors in SSR?
- section 3.3, p14, l20: it is written that VCD differences are small but it is hard to judge as only absolute values for the differences are shown. It would be better to incorporate relative differences as well as proper error calculation (see comment above).
- p15: it is not clear how the CTM profiles should be changed to reconcile the vis, uv and modeled VCDs.
- p16, l25-26: the reading nearly suggests that it could be better to use UV retrievals but it is quite unlikely given the larger uncertainties.
- p18, l35: It is stated that ‘concurrent measurement of the same air mass from different view geometries could yield insight on aerosols and vertical distribution’ but these measurements do not exist.

Minor comments:

- p2, l34-35: I find misleading that the examples on past studies on vertical profiling are mostly unrelated to NO₂ retrievals as given in the present paper (which considers optically thin atmosphere as for NO₂).
- p3, l13: ..path within the NO₂ layer relative to the vertical path.
- p3, l15: environmental effects is too vague.

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-throughout the manuscript, the author often use the word 'visibility' to express the idea that NO₂ signal is more clear 'visible' in some spectral range. It is ambiguous as it might be interpreted by 'visible wavelength'.

-p3: the example of bAMFs should be presented here.

-p3: the effect of BRF on NO₂ retrievals is not developed enough as possible reason between UV and vis

-p3, l34: sofar → so far.

-p5, l 15: "strong absorption lines" -> "strong differential absorption lines"

-section 2.5: for the SCDs stratospheric correction, are the averages performed for both UV and VIS data separately?

-p10, l16-17: this is a bit contradictory. If it is below the detection, then how meaningful is the 0.6 SCD ratio?

-p13, l8, first word: India->China?

-p13, l18: SZA is lower-> SZA is higher?

Interactive comment on Atmos. Meas. Tech. Discuss., doi:10.5194/amt-2017-326, 2017.