

Interactive comment on “Intra-pixel variability in satellite tropospheric NO₂ column densities derived from simultaneous spaceborne and airborne observations” by Stephen Broccardo et al.

Anonymous Referee #3

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This manuscript presents a study on the comparison of airborne versus satellite tropospheric NO₂ retrievals over the South African Highveld region. The study focuses mostly on the interpretation of four research flights performed in August 2007 that coincide with measurements from OMI and SCIAMACHY.

This work is highly relevant for two reasons: first because of the mismatch reported in the literature between tropospheric NO₂ columns derived from bottom-up inventories and those from satellite retrievals in this region (satellite observations provide higher values). Before one may conclude that local emission inventories need to be adapted,

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satellite retrievals should be validated locally. Secondly, this study has a global relevance in the sense that it gives valuable insights with respect to the horizontal variability of tropospheric NO₂ columns when observed at high spatial resolution. The satellite instruments OMI and SCIAMACHY are known to have a moderate spatial resolution and it is not well known how this resolution compares to the typical scale of spatial variabilities in the urban tropospheric NO₂ column field. This information is relevant for many related studies where satellite retrievals are used (e.g. in order to derive top-down emission inventories) or being validated (e.g. comparison with MAX-DOAS).

The manuscript is generally well written and addresses a relevant topic. However, to my opinion several aspects deserve more attention before publication in AMT.

1 / Four research flights are analysed and discussed in quite some detail, but the interpretation of differences between airborne and satellite retrievals could go more into depth. Based on the present manuscript, the reader might get the impression that systematic differences in tropospheric NO₂ columns between satellite and iDOAS can be explained solely (or largely) by horizontal variability in the tropospheric NO₂ columns on a scale that is smaller than the typical size of satellite pixels. What could be particularly relevant is to investigate further the potential impact of profile shape assumptions for NO₂ and aerosols in explaining the difference between satellite and airborne measurements over the most polluted regions. Close to major point sources one may expect not only to find locally quite extreme tropospheric NO₂ column abundances, but at the same locations also the NO₂ profile shape may deviate considerably from other places further away from the main sources. In this context, it may be relevant to distinguish explicitly four profiles: the true profile at the spatial resolution of the aircraft measurements ($P_{\text{true_air}}$), the profile used in the airborne retrieval ($P_{\text{prior_air}}$), the true profile at the resolution of the satellite measurements ($P_{\text{true_sat}}$) and the profile used in the satellite retrieval ($P_{\text{prior_sat}}$). Differences in tropospheric NO₂ column retrievals (space borne versus airborne) cannot be interpreted without taking into including these four profile shapes in the discussion: how much do the authors think

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$P_{\text{true_air}}$ can deviate from $P_{\text{prior_air}}$ close to the main sources (same for $P_{\text{true_sat}}$ and $P_{\text{prior_sat}}$). Furthermore the AMF is not only affected by the (different) profile shapes, but also by the block-AMFs, and these are not identical for the satellite and the airborne point of view. This should be taken into account as well.

Despite the length of this comment, I would suggest to add just one or two paragraphs addressing this point and providing some first order estimates. It could for instance be enlightening to the reader if the impact of making wrong profile shape assumptions is worked out for one hypothetical scenario. For instance (it is up to the authors to deviate from this concrete suggestion): scale height for $P_{\text{true_air}}$ is 0.2 km (e.g. close to strong isolated source); scale height for $P_{\text{true_sat}}$ is 0.4 km (averaged over a larger region the true profile is less dominated by the local source); scale height for $P_{\text{prior_air}}$ is 0.6 km (this number is used in present study); scale height for $P_{\text{prior_sat}}$ is taken from profile used in DOMINOV2 product over this region. Block-AMFs should be applied for a representative SZA and surface reflectance. When combined, this information should provide the reader with a first order quantitative estimate of local AMF fluctuations near a strong plume: to what extent can this explain the discrepancy between the satellite and airborne retrieval? Or perhaps it is concluded that - when taking this effect into account - the observed discrepancy increases even further.

2 / Although aerosols are not entirely neglected in this study, they receive little attention considering the fact that for all four flights - each covering distances of hundreds of kilometers - just one fixed value is assumed for the AOT. It is quite remarkable that the uncertainty range of the AMF is derived using a look-up table that does include variability of the single scattering albedo, but not of the AOT. Over a region where the variability in NO₂ is so large, it is almost unthinkable that the AOT can be approximated with a single value. To some extent the same argumentation as given above (in the vicinity of a strong pollution source the NO₂ profile shapes may show considerable spatial variability) can be given here as well: in the same region the AOT may show a substantial variability (although probably less extreme than for NO₂). In my opinion

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this point should at least be mentioned. It would be even better to find satellite AOT data (e.g. from MODIS) for the days of the research flights to provide more insight into this relevant parameter.

3 / In the manuscript the discrepancies found between iDOAS and OMI (SCIAMACHY) are not compared to results from other validation studies, e.g. where OMI retrievals are compared to MAX-DOAS observations. In the last years many of such studies were done, with MAX-DOAS instruments either in rural or in urban regions. It would be valuable to link the findings of this study to findings in such inter-comparisons.

4 / On section 2: please provide some more details on the iDOAS observations. For instance: the field of view, number of ground pixels in across track direction.

5 / I am missing a formula that describes how VCD's are derived precisely from the (differential) slant column measurements. In my opinion, this should be described in a more detail, although it has already been described elsewhere in full detail.

6 / The statements in Sect. 3 are quite general. The words 'usually' (p.6,l.3) and 'frequently' (p.6,l.8) suggest a large number of profiles that are measured. However, these are not shown. Furthermore, it is not clear if the profiles that are measured are representative for the plume or for more remote regions (see also the first comment).

7 / Figure 5 could be better readable if a grid was plotted on the left and right side of each cube. Furthermore it could be beneficial to use colours instead of different line styles and to provide a legend.

8 / P.15, l.24-25: "...indicating that ... 9 August". The terminology 'aged' versus 'young' might cause confusion, as some readers might wrongly think of 'photochemical aging'. It might be that what is here called an 'aged plume' is actually a region where the NO₂ profile is less shallow than for a 'young plume', and more in line with the prior NO₂ profile shape used for the OMI and/or iDOAS retrievals (see also the first comment). If this is the case, then one cannot say that OMI would be limited in its ability to capture

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the higher NO₂ gradient in the young plume because it is 'young'; for instance it could be more appropriate to say that the AMF derived using the prior profile shape used in the DOMINO product better matches the profile shape of an aged plume than the profile shape of a young plume. Please comment on this.

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