Author comment on "Retrieval of tropospheric NO_2 columns from SCIAMACHY combining measurements from limb and nadir geometries" *by* A. Hilboll et al., doi:10.5194/amtd-5-5043-2012.

Response to anonymous reviewer #3 (C1650)

Andreas Hilboll et al.

30 November 2012

We thank the anonymous referee #3 for the valuable comments.

> (1) The title of the paper is misleading, since the retrievals, comparisons, figures and discussions involve mainly stratospheric NO_2. I suggest modifying it to better reflect the scope of this study.

We agree that the title should reflect the focus on stratospheric NO_2 and therefore renamed the manuscript to "Improvements to the retrieval of tropospheric NO_2 from satellite—stratospheric correction using SCIAMACHY limb/nadir matching and comparison to Oslo CTM2 simulations".

> (2) In general, I found the nadir-limb-CTM2 comparisons in the paper somewhat confusing, since the nadir measurements contain tropospheric NO_2, but limb and CTM2 do not. When the limb and CTM2 columns are corrected by an additive offset to match the nadir columns over the Pacific, they are effectively contaminated by the small amount of trop NO_2 in that region. At that point, they are no longer purely stratospheric estimates. (From my reading of the manuscript, it seems the Pacific background correction described in section 2.3.5 has not been applied in most of the figures.)
>
> The zonal plots of stratospheric VCD (Figures 9 to 13) would be easier to interpret if the authors provided an estimate of the magnitude of the tropospheric contamination in the Pacific and showed it in the zonal plots if possible. The amount should be SCDtrop/AMFstrat (which is not the amount shown in Fig. 20). Is this contamination ever comparable to the differences shown between limb, nadir and CTM?

In the revised manuscript, we changed the zonal plots to show 'more true' stratospheric datasets. For limb and modelled VCDs, we included the offset correction, and from the nadir measurements, we subtracted the tropospheric NO_2 content over the Pacific Ocean. Therefore, all three datasets now show an estimate of the stratospheric NO_2 content.

For an estimate of the tropospheric contamination, it should be accurate enough to consider the difference between these stratospheric nadir and limb datasets. For the determination of SCD_{trop}, as

you suggest, it would be necessary to perform some kind of stratospheric correction, so when using limb measurements to determine SCD_{trop}, the difference between stratospheric nadir and limb data as shown in the plots amounts to SCD_{trop}/AMF_{strat}, as you suggest.

> (3) Equations would be very helpful to show explicitly how/where the various corrections and offsets are added the column amounts.

We agree that equations are essential to exactly understand our algorithm. We have added them to the re-written methods section.

> (4) In section 2.1, the authors say their method has an advantage over that of Beirle et al [2010], since it does not require averaging several days of stratospheric measurements. However, the figures here show only monthly mean results, and the methods for dealing with NO_2 over the Pacific seem appropriate only for multi-day averages.

In the revised manuscript, we included daily plots for those four days presented in Beirle et al (2010).

> Specifically, the tropospheric AMFs used to correct for the small Pacific background are monthly means, as stated in 2.3.5. For daily retrievals, cloud amounts can significantly affect the visibility of trop NO_2.

We agree that cloud contamination of the Pacific troposphere will lead to an overestimation of the stratospheric slant columns and thus to too low tropospheric columns. We have added some discussion about this error source to the revised manuscript.

Discarding cloudy pixels over the Pacific would lead to a considerably reduced number of measurements, increasing the influence of random errors on the final retrieved quantities. Also, it is hard to correct the tropospheric AMF used in the offset correction for the influence of clouds, as over the Pacific, both clouds and NO_2 are mostly found in the free troposphere, and no reliable information exists about the relative vertical distribution of both. Therefore, trying to include cloud effects in the AMF calculations would be an error-prone correction on a small effect.

We have amended the revised manuscript accordingly.

> Also, in the reference sector method (2.3.6), the very narrow (0.125 deg) latitude bands could exacerbate errors and create latitude-dependent noise in the stratospheric estimate unless multi-day averaging is done.

We have removed the discussion of the reference sector method from the revised manuscript, as one of the anonymous reviewers has asked us to do so.

> These effects might well be small, but please add some comments on these retrieval-parameter choices and the size of the potential errors they could introduce. This might be done by referring to section 3.5.3 and adding some words there.

In the revised manuscript, we have added an estimate of the error introduced by possible cloud contamination of the Pacific troposphere.

> I also recommend addition of a figure showing stratospheric and tropospheric retrievals for a single day. This would help demonstrate the effectiveness of the limb-nadir interpolation procedure presented in this study.

In the revised manuscript, we have added plots of daily tropospheric slant columns retrieved with our algorithm. As the temporal variability of stratosheric NO_2 is rather low, we do not see much insight to be gained by showing daily stratospheric plots and have thus decided against showing such a figure.

> (5) Regarding the discussion of Figure 6 in section 3.2.2, why would the smaller nadir SCDs (relative to limb) in the tropics be the result of upper-tropospheric lightning or biomass-burning NO_2? The AMF in the upper troposphere is similar to that in the stratosphere, so wouldn't the resulting contributions of upper trop NO_2 to nadir and limb SCDs be similar?

We agree with the reviewer and have removed the mentioned paragraph from the discussion.

> (6) In the final paragraph of section 3.5.3 (page 21), the authors correctly state that tropospheric AMFs are larger in cleaner regions than in polluted ones. But the error contribution to the stratospheric correction depends on the magnitude and uncertainty in the tropospheric slant column. Because of the small NO_2 amounts in clean regions, this uncertainty is likely to be smaller than in polluted regions, regardless of the AMF (an exception would be totally cloudy scenes, where low clouds could completely mask boundary layer pollution, but enhance the visibility of trop NO_2 in clean regions). I suggest some rewording of this paragraph.

We do agree with the reviewer that the relative contribution of the stratospheric correction error to the full trop. slant column error is independent of the tropospheric AMF. The part of the total uncertainty in the tropospheric slant columns which gets introduced by the stratospheric correction is independent of the tropospheric pollution. However, the relative contribution of the stratospheric correction error to the total uncertainty of the tropospheric slant columns does depend on the amount of tropospheric NO_2 and is larger in clean regions, because there, the measured slant columns are smaller and therefore have an (absolutely) smaller uncertainty.

We have rephrased the mentioned paragraph to better reflect this line of argument.

> Although the meaning of the text is generally understandable throughout the manuscript, there are several instances of awkward grammar and cases where rewording would make the meaning clearer.

We agree that some of the used expressions can sound awkward and have tried to reword in cases where it helps understand the meaning clearer. Further language corrections will be carried out during Copernicus' copy-editing process.